NATIONAL LIFE STORIES

AN ORAL HISTORY OF BRITISH SCIENCE

Professor John Nye

Interviewed by Dr Paul Merchant

C1379/22
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| **Interviewer’s comments:** |
Could you start by telling me please when and where you were born?

I was born in Hove, in Sussex, and it was in, on the 26th of February 1923.

And would you be able to tell me a bit about your mother, in terms of her life and career as you know it?

Yes. Yes, my mother came from North Wales, and her father was the man who painted these oil paintings that you see around.

Ah yes.

He was a professional oil painter, and, made enough money from his paintings to keep his family, his rather large family.

Mm.

The tradition is that he hated to sell a picture, and, every now and again his wife, my grandmother, would go to him and say, ‘Oh dear, we need some more money for the housekeeping,’ and he’d say, ‘Oh damn, I’d better sell another picture.’ So, when he died there were a lot of his unsold pictures waiting in the, in the house, and my mother inherited them. So that’s how we come to have so many of his pictures.

Mm. Where did he sell them, in London, or...

Well, he was the President of the Royal Cambrian Academy, which is like the Royal Academy but the Welsh side of it. And I have a catalogue of his, of one of the exhibitions, and you’ll see that the price of the painting was something like £400, £500, the sort of prices that you would have had in yesterday’s money but in 1910 or something like that. So they were really enormous prices. He painted pictures for Manchester cotton magnates, who wanted an imposing landscape over their sideboards, so he, he painted, some of his canvases were very large, and I’m told he
invented a way of painting large canvases outside and then folding them so that they wouldn’t smudge as he took them home. Because of course the tradition at the time was very much, you made a sketch in the field and then you came back to your studio and did the proper thing. But, these pictures I think were probably painted outside as they are.

*And these are Welsh landscapes?*

They’re all in the Conway Valley. You can see, you can see the Conway river in the background there, and in that one too. He lived in a little place called Deganwy, which is not very far from Llandudno.

[03:07]

*And what did your maternal grandmother do while her husband...?*

While he was painting his pictures?

*Yes.*

I think she was just a housekeeper and a mother. She didn’t have a job.

*Right.*

My mother was a member of a fairly large family. Well I think, well, I can’t remember actually, I’m not, wasn’t in touch with them, but about four siblings. And she was, she got married very late. My father was in the war, and, he was wounded, he was in the Royal Engineers, got the MC, and came to convalesce, and came to this big house, Vardre, in Deganwy, where my mother lived. And of course they got to know each other, and I think he probably fell in love with her then.

*Mm.*
And... But being a very proper gentleman, he didn’t... it was in the days when you didn’t marry until you could afford to keep your wife in the style to which she had become accustomed.

_Ah, which was quite stylish with the..._

Which... So he, he came, he came back after the war, and they got married I think in around, around 1919, 1920. So that’s the, that’s the background to the marriage.

_Mm._

[04:30]
But, my mother was not, was not as you might say a conventionally educated woman. She got some school prizes, but, she was, she was far from academic. But she nevertheless, she was absolutely determined that her two boys should have the best possible education. And I think it was her determination that that should happen that led to my having the best possible education.

_Where do you think that that enthusiasm for education came from?_

I really don’t know. I know that she had a couple of school prize books, so she was the sort of person I think who, who prized education as something, but wasn’t educated herself.

[05:21]
_And your father’s educational background?_

My father came from a very large family. He, his, his father was a minister in a rather obscure church called the Catholic Apostolic Church. And my grandmother on that side invented something, with him I think, called the Beresford changing bag, which was a photographic apparatus which allowed you to change your glass dark slides in the field. In those days you know, you took out a number of glass plates, and when you had exposed them, you brought them back home and developed them in your darkroom. But this way, you, you could take out a whole lot more in a package, and
then when you were in the field you could put your hands inside this bag, which was a dark bag, and change them in the field, thereby taking many more photographs.

Yes.

So they marketed this device, and as far as I can tell it brought them in a handsome income. He only had a minister’s income, which can’t have been very much, and he had a family of, I think it was nine children, and together they brought them up. And my father was not the eldest but the second eldest of that large family. He left school I think at the age of fourteen, and then went on to, as they said in those days, to better himself, by going to night school. He learnt for example elocution. I saw one of his elocution books where they have diagrams of where to put your feet and what sort of gestures you make, gestures of horror or imploring, all the, all the rhetorical gestures of politicians. And, he set himself up to be a, as I say, to better himself. And he joined the Inland Revenue... No, he, he went to an office in London as an office boy, learnt to lick stamps, taught me how to put on lots of stamps on envelopes very quickly. You lick a lot at the same time, and then peel them off one by one. And, he, as I say, he, he then took his exams and joined the Inland Revenue and became a surveyor. So he learnt surveying, like, making a, a map of Wimbledon Common with a plane table, as I saw later, and... And then, then came the war, which must have been a great interruption, and he was a, he was in the war most of the war. He volunteered in, I think it was 1915. And then came back as I said and married my mother. Was then in the Inland Revenue as a rating surveyor.

Mm.

[08:56]

And after a while, I think especially after he had two boys to look after, he set up his own office, set up his own business, in Brighton, and quite soon he became a well-known rating surveyor in Brighton, and in the late Twenties there was an Act passed, it was the, the Landlord and Tenant Act I think, and the rates of premises were based on their rateable value, and a surveyor had to go round to all the houses and say how much they were worth, which was the rent at which they might reasonably be expected to let, that was the phrase. And he was given the job, rather surprisingly, of
valuing the whole of Brighton by the Corporation for the sum of £5,000, which he accepted willingly. And, set up an office to do the valuation, he needed staff of course, who needed to go round and look at every property. Finished the valuation, on time I think. And then of course came the appeals. And so he became accustomed to appearing in court to, as an expert witness on appeals. And he, he then won all the appeals, which was a, a great feat, and made his reputation. He was called Hundred Per Cent Nye. So, that was a great success. And he was getting on in the world, bought himself a better car and, that sort of thing.

[10:52]

He, he was the... You see he was toff of his family in his, in his early days when he was living in London with his family. He was the one who used to come in through the front door; his brothers came in through the back door. And he wore a top hat, whereas they wore bowler hats.

Is this at a time that he was trying to better himself through...?

It was a time when he was trying to better himself, yes.

Yes.

He knew what it took, yes. [laughs]

Do you have any sense of where that sort of ambition came from, in relation to the rest of his family?

I really don’t know. I mean he was a really religious man, coming from his father, who was the minister. And, he was a member of this Catholic Apostolic Church, that, that was a great driving force in his life. And, we were brought up in that, in that Church. We had to go to church on, every Sunday, and sometimes even twice on Sundays. He had even more religious brothers, he had, my Uncle Arthur I remember well showing me, well, pictures of the Creation. He had more or less working drawings of how it all worked. He was very, very much immersed in that sort of fundamentalism.
What did the pictures show, do you remember the detail of them?

Well they were, they were pictures that I’ve seen since, and I can’t remember the artist, but I, I think Gustave Doré had something to do with it probably. But, the Tower of Babel, and, and the Flood, which he believed in implicitly. Those sort of, improving Victorian books. On Sundays of course you were not supposed to do anything much except look at improving books.

And you were expected to do so?

That’s right, yes. Yes.

[13:02]

Do you remember your feelings about this religious instruction and observance as a child?

Well, I think my predominant feeling was, they were all so old, they all had beards, and, it wasn’t for me. Because I didn’t understand anything about it really. But the, the whole thing started in, I think about 1850 or thereabouts. They were called the Irvingites. There’s a chap called Irving who started it all. And they were self-appointed, twelve apostles, and the apostles appointed people under them. There was a whole hierarchy. I think the apostles appointed angels under them, and then the angels appointed priests, and the priests appointed deacons, and the deacons appointed under-deacons, and the under-deacons appointed doorkeepers. So there was a hierarchy. And, when the last apostle died, in I think about 1910, this was the story in the family, well the church died from the top down, because no more angels could be appointed and then no more priests and so on. So everybody was very very old in that church, and it didn’t appeal to a young boy.

I see, yes. It seemed to have sort of failure built into it, didn’t it, a lot of...

Abs... Oh it didn’t... No, not failure, success, because, they believed in the Second Coming, and it was going to happen any day. So it didn’t...
So it didn’t matter that it should...

They weren’t fazed at all by that. They really believed it you see. And, I never saw any disappointment on their faces, but my Uncle Arthur would be, was certain that it would all, the Second Coming would, would resolve everything.

[14:58]

What was your mum’s essential religious engagement?

She was, she was brought up as an Anglican, and, every now and again we would go to Mamie’s church. I called my mother Mamie. And we would go to Mamie’s church in the evening. So, my religious observance was split between the two churches.

And how did that work out between your parents, was that...?

Oh it was no source of friction I think.

Mm.

My father was, in spite of his membership of that church, he was a surprisingly tolerant man, and he became tolerant, more and more tolerant as he got older. I mean in some respects he was extremely unbending, but in many, in many respects he was, he was a very tolerant man. I remember for example, we used to have maids who came in, many of them Irish, and we had a lot to do with them as children, and one of them was an Austrian maid, and her fiancé was an Austrian policeman, and she wanted to bring her Austrian policeman to visit her in Hove. And the question was, should he be allowed to visit her in her room? And my father was absolutely dead against this, but my mother would say, ‘Look dear, don’t you think perhaps they would get on better in... Don’t you think we might do it just this once?’ It was that sort of relation; he gave way.

So that would be an example of his tolerant...?
Tolerance, yes. Yes.

*And, in some respects he was unbending, do you remember the sorts of things that he wouldn’t have given way on?*

Well my cousin Constance, who was a sort of Twenties flapper, a marvellous girl, she, she got married to a divorced man, and she was a frequent visitor to us, was one of my big, you might say educational influences, because she was about, ten was it, or, years older than I was, and she was, she told me a lot about the world. But... Her world. Cocktails for example. [laughs] But, she married this divorced man, and my father was adamant that he couldn’t possibly have her to stay with her, with her husband, because, what would happen if his earlier wife came in and said, ‘He is my husband’? Unanswerable to him, because marriage was indissoluble.

*Mm.*

So, that was a, a real sticking point.

[18:07] Thank you. Could you just tell me a little bit about, I’m skipping back a bit, your maternal grandparents invented the, the bag that allowed the plate...

No that was my paternal...

*Sorry, paternal grandparents. Yes.*

Paternal.

*Developed the, the bag which allowed plates to be changed in the field without sort of overexposing them.*

Correct.
What was their photographic interest in the field? What were they photographing in the field that led them to innovate in this way?

I never saw any of my grandfather’s pictures. I saw a lot of my father’s quarter-plate pictures. In fact I inherited his quarter-plate camera, and I took pictures with it, with a rising front and a swing front. And, you know this device is to stop the verticals converging when you took the camera up. You could do everything with these cameras. And, a beautiful camera, I’ve got it upstairs. My grandfather you asked about, I never saw any of his pictures. He did invent a device for inverting the image which you saw on the focusing screen. You put your head under the black cloth as you know, and of course the picture you see is, is upside-down. But you look through a Beresford inverter, and then you’re OK, you see it the right way up. It’s what I learnt later they call a dove prism. You look through a prism that shape, this way, and the image turns upside-down. I suspect, I saw one of them in a brass tube, and I suspect it had lenses in as well, but I’m not sure about that.

So they invented the bag and the inverter?

And, and the inverter. Yes.

What was your father interested in photographing then, when he was doing so?

Oh, what did he photograph? Architectural things, cathedrals of course. [pause] I really don’t think I can... Oh, some pictures of the family, at Williton House in London, in Battersea, where the whole family lived. Pictures of his sisters chasing a cat, and falling off a bicycle or riding a bicycle. A few sort of homely pictures like that I recall.

[20:49]

Your brother, when was he born?

He was eighteen months older than I was. He was born in 1921, 16th of September to be precise. And he was obviously a big influence on my life. He was the older brother. Better at everything. He was a great sportsman. He died only, during this
last year, and, he was tremendous at all sports, cricket especially, tennis. He beat me at everything. Beat me at... I think I once beat him at ping-pong, but that was only once. But we had great fun together, and we used to play tennis on... We had a court, a hard court, at our home, our second home in Hove, which my father designed and had built, and, it had, as part of the design, a hard tennis court. And my brother and I used to look after this hard tennis court jealously, and, it needed raking and rolling and the lines brushing, all kinds of maintenance. It was called en tous cas, do you know the...?

No.

It was a semi-loose surface, but it needed enormous maintenance, which we had to give it. And we’d spend all the morning preparing this court, and then we’d play in the afternoon.

*And then had to maintain it the following morning?*

Well whenever...

*Did it need to be repaired sort of, every day?*

Not every day, but...

*After every...*

Before we played probably, yes. Because it had holes in it. [laughs]

[22:30]

*Could you then, the first house in Hove, would you remember, were you of an age where you would remember that in detail?*

Oh yes.
Could you sort of, imagine yourself at the front door, if you like, and take us on a tour of the first...?

The house?

Yup.

Well let me start in the street. It was a, it was called 11 Wilbury Villas, in Hove. You know Hove always thought of itself as a cut above Brighton. And, you came, it was a semi-detached house, with a name plaque on it saying ‘Sherwood’, although it was 11 Wilbury Villas, ‘Sherwood’ it said. And you walked up a, it’s like lots of the houses in Bristol of, I would think, sort of Edwardian time. You walked up a side passage, front door was on your left, and then you walked through a little further and the back door was on your left. And there was a garden at the back. You came in through the front door, and, first room on the left was the drawing room; second room on the left was the dining room. So it was obviously two down. And, on the right you walked through a little place where the electricity meters were, into the kitchen. And then, the other room on the right was the so-called morning room, which was really our playroom, and where we had breakfast. That was the ground floor. Do you want me to go upstairs?

Yes please. Yes, and then I’ll ask you...

Well, you go upstairs, straight from the hall, and then upstairs you find, you find a bathroom and a loo, and you find my, you find the spare bedroom, and my parents’ room, the maid’s bedroom, and the bedroom of us boys. That was the, that’s the layout.

Do you remember...

Oh and a box room.
Do you remember anything about the house was sort of, you may well not have decorated or made particularly your family’s in terms of its, you know, the pictures, the ornaments, the decoration, the style?

Well there were masses of pictures. There were, these pictures you see are a very small remnant of what we had there. There were so many that the... It was very high ceilinged. There were two rows, one above the other, of oil paintings. So, that was a very dominant feature.

And do you remember anything in the house that you remember as thinking was modern or, you know, the latest sort of gadget or object?

Well we had, we had a wireless, which was built by my Uncle Fred, and you... Actually, at my grandfather’s house they had a crystal set, and you listened very hard through the earpiece and you could hear music, which was fantastic. At the home in Hove that you were asking about we had a wireless set built by my Uncle Fred with a loudspeaker which my mother thought was terrific, was actually a, I think they called it a moving iron loudspeaker, was actually pretty bad I think. But, I remember listening to Oxford and Cambridge Boat Races through it for example. And then, that was worked with accumulators, and with a high tension dry battery. And then my Uncle Fred came down from London specially to replace the high tension battery with a mains so-called transformer. For some reason this transformer had lots of light bulbs in it. I don’t understand why, but it was one of the, a very very primitive way of running something from the mains that previously had run on an HT battery.

And if it went wrong, who attempted to repair it in the house?

It never did go wrong. But I inherited it afterwards, and I painstakingly drew a circuit diagram for it. I never understood how it worked, but it had coils that you plugged in for the different wavebands. They were yellow coils about that size. You changed the coils if you wanted to go from medium wave to long wave.

When were you drawing this circuit diagram of it, at what age?
Oh I should think, fourteen, something like that.

[27:28]

*Mm. And when did you move from this first house in Hove?*

In 1936. Yes, it was 1936.

*So you were there quite a long time, yes.*

Yes, so I was, I was like, thirteen when we moved. And I went to school, walked to school from the old house and then came back from school to the new house. I remember that quite, quite well.

[27:55]

*Could you tell me about time spent with your, your mother as a child? So, significant things that you did together.*

That’s hard. She used to take us out. But she was, she was really an invalid. She had terrible gastric trouble. And a presence which was very important to us was the lady, Mrs Pelling, who used to be, used to come in and take us out. So we didn’t go out with my mother a great deal. She played tennis with a lady who cheated, [laughs] and, very scathing about that. But, otherwise, she wouldn’t strike as an active, physically active person, and I think really these two boisterous boys were a bit much for her. So, I remember her as a very kind, maternal presence, but I don’t think I can put my finger on any particular episode. Oh we went on holiday of course, summer holiday of course together.

*Where did you go on holidays as a child?*

We went to Sandown in the Isle of Wight. We went to Cromer in Norfolk. Generally sort of seaside holidays. And, and then later on my father drove us on a grand tour of England and Scotland, and we, we visited mostly cathedrals in different places. And then stayed in pretty crummy places overnight, and every now and again as a kind of treat we stayed in a luxury hotel. It was rather nice.
And would you remember sort of, sedentary things that you would do with your mother, like reading with her, or talking, just talking?

Yes, yes she, she certainly read to us, and we read with her. Oh yes, I remember the books reasonably well.

Oh good. What were they?

Yes. What were they?

Yes.

Oh. There was something called Chatterbox, Lottie of London Bridge. The Children of the New Forest. And of course things like Kidnapped and Treasure Island. Those sort of, mostly classic books. Of course there were nursery books as well, like, the one about a giant who was... You know when my father’s effects were being, after he died, been distributed in the family, we all had to say what we would like, and my brother said, ‘There’s only one thing I want, and it’s that book about the giant.’ It was a very thrilling poem, illustrated, and I remember the lines, ‘And if his chair be six feet high, what size must be his apple pie?’

And so this was your mother reading to you at what sort of age, this, this...?

Oh I, how can you tell?

Yes.

I suppose, six perhaps or... I don’t remember. I mean we were, we read ourselves fairly early on I suppose. I didn’t say that the... Maybe you were going to ask me, but the first school I went to was a kindergarten school, and it was in the same road, and in an identical house to ours. So it was rather strange, just walking up the road, going
in through a similar front door. And of course the layout was identical, but instead of being chairs and tables, it was desks.

*Mm.* *Was that a private...?*

It was a private kindergarten.

*Mm.*

Yes.

Yes, *I was going to ask you about your various schools. Yes, we’ll come...*

Yes, I expected you would.

[32:20]

*Thank you. Would you be able to say something of the kinds of conduct or the kinds of behaviour that your mother attempted to encourage in you, or discourage in you?*

My goodness, this is... [laughs] Well I mean, honesty was the number one virtue, and, I mean, the truth was the thing. I, I’m not sure that the word ‘love’ was ever really mentioned. You know, nowadays it’s, it’s so commonplace, one tends to say, ‘I love you, I love you.’ But it wasn’t that sort of relationship. But it was implicit. [pause] You said what sort of values did she try to inculcate.

Yes.

Hard to say really. I think, goodness would be what she would say. [pause] Really, it’s hard to put it into words.

*Mm.*

I mean she was thoroughly, a good influence, a lovely lady.
Do you remember anything that she told you off about, you know, that stands out in your memory? That might be a way...

Tell me off about. [laughs] No, she wasn’t a telling-off person. No, I mean I would go to, I would say I couldn’t go to school because I had a tummy ache, and she would rub my tummy and send me off to school and I would be fine.

[34:07] Mhm. And, I wonder whether you can do something similar with your father, tell me about sort of, significant time spent with your father as a child.

Yes. Well he, he went on holidays with us, and, I guess probably we saw more of him on the holidays, as you would. He, he was a very, as I was saying before, well I said strict, but, you know, I don’t think that’s quite the right word. Very, very keen on his values, and stuck to them, and tried to inculcate them in us. We, at the church there were boys called acolytes, who used to arrange the things on the altar and that sort of thing, and, Peter and I, my mother was called Peter, Peter and I were asked to become acolytes. And we refused, point blank. And he didn’t press the point. My poor cousin Ian was not so fortunate, and he had to be, we watched him every Sunday doing his stuff, and he tells me now, he used to envy us no end because we had managed to refuse to be an acolyte. We went to catechism you see every, every, not every Sunday but for the three or four Sundays before every feast day, and at the end of the catechism we were given, end of the sessions we were given a certificate which allowed us to take Communion. You handed the certificate to the sub-deacon who was in the aisle and he let you go and have, take Communion. My father would be very keen on us doing that, and would actually forego his afternoon nap on Sunday afternoons in order to take us down by tram to a stop where we, from which we could walk to the church, which happened to be through the worst slums of Brighton. So we used to see the bad side of Brighton on Sundays, and you saw these ex-servicemen walking around with wooden legs, and, generally it was, it was a post First World War scene, very slummy.

What did your father say of that to you, of the sorts of things you were seeing through the window?
He was a very patriotic man, but I don’t think he said much about poverty actually. It was called slum clearance in those days, and he was quite interested as a surveyor in the planning aspects of slum clearance. Indeed he must have been involved in it. But he didn’t, he didn’t express moral views about, about that sort of thing.

[37:29]

*What was the extent more generally of your parents’ political engagement?*

They were staunch Conservatives. They were members of something called the Navy League, which supported the Navy, and my mother would hold sewing parties for, in aid of the Navy League. And the ladies would, were supposed to come and sew. In fact they didn’t, they just had a tea party, and took their materials back home and sewed them at home. And they went to Navy League dinners. They were keen on going to official dinners. The Royal Surveyors’ Institution dinner was a big event for them, and, or the Mayor’s dinner or whatever it was, in, of course they were held in the big hotels in Brighton.

*And, how was this Conservatism expressed in the sorts of things that they said about the world or, about, about society or about...?*

We never, we never discussed politics really. I don’t remember them discussing very much the local MP or anything like that, or what was going on in Parliament. Of course, I don’t know what period you are speaking of, but, of course the, I mean, the late Twenties was the Depression, and, there were all kinds of... Oh, the General Strike, yes, I think, when was the General Strike? 1926 wasn’t it?

*Mm.*

I think I vaguely remember the General Strike. But there was no, no opinion expressed, I was only three, about whether it was a good thing or a bad thing or whatever.
And what was your, when you were travelling on the tram through these slums, can you remember your own reaction or impression?

Well just that everything was so grubby. These second-hand shops with dusty windows, and behind them were models of ships, and some of them I rather liked.

You wanted to stop and get out. And... I see. And, apart from this Communion, other things done with your father as a child?

[pause] Hard to remember actually. We went on a holiday to Norway, where he very uncharacteristically went swimming. And I remember we went, we went bathing together. But there was no sort of, tussling with the kids as the modern fathers are inclined to do, nothing like that at all. Very little physical contact actually between me and my parents, looking back on it. It was a different age you know. It was, we were half Victorians.

Yes, actually, in terms of the history of childhood, I think that, touching and loving and kissing and all that thing was almost discouraged wasn’t it, in the sort of, theory about how to raise children almost at the time.

I didn’t know that, I didn’t know it was discouraged. I think it was simply, just not done particularly.

Mm.

There was a great deal about what was and wasn’t done. My father was an absolute stickler for speech, and, this was after his elocution lessons no doubt, but, any lapse of grammar was, was pounced on. I learnt quite a lot about what to say and what not to say. And, he as an expert witness in court would enthusiastically relate, when he was brought up by a judge for using a word in a slightly wrong sense, I remember the words envisage, and, what would be a sort of half-synonym for it? Visualise. Yes, envisage and visualise. And the judge telling him which word he should be using. I forget which it was indeed now. But that was the kind of distinction that he would, he
would draw. And we learnt an awful lot kind of intellectually from that kind of corrective process.

[42:05]

Mm. Thank you. Now, were there any local landscapes? We might talk about holiday places separately, but were there any local outdoor places or landscapes that were important to you as a child?

You are going, you are going into detail. Yes, there were indeed. There was a great moment when I got a bicycle, and suddenly the whole area of Sussex near Hove and Brighton became open to me, that was, that was a revelation. We were in the house that we moved into in 1936. We were not too far from the South Downs, in fact it was walking distance to walk on to the great open spaces of the South Downs. And we walked frequently to the Devil’s Dyke, which was the local tourist attraction. The Downs are beautiful landscapes. It was, I think it’s now a part of a national park. But, for us, there were golf courses there which we went to. We went to a golf course there for the first time, I think it was called the Waterhill golf course, and we carried each a putter and a driver, and, went along to the clubhouse and asked to pay the green fee. And the chap in charge said, ‘Well you don’t look much like golfers, do you?’ [laughs] We were very, very shamefaced with our just two clubs.

*When you say we, is it you and your brother?*

My, me and my brother, yes. He was the, he was always the leading force in enterprising new sports. He got a whole golf bag full of clubs after that. I didn’t.

*Were you allowed to play on that occasion with your two clubs?*

Oh yes. Oh yes, we paid our green fee, and they were I think probably glad to get it.

*And, other, other outdoor places or other sights within the South Downs that were significant that you visited with your brother?*
Yes. Yes. Well, that ridge of the South Downs behind Brighton, going to the old windmill, and... You could look down you see onto the villages of the Weald, you could look down on Saddlescombe, and, what’s the other place called? Little, little, almost toy farms just below you, I remember them vividly, they were set out, planned, so would just see where the cowshed was and the farmhouse, just as you would in playing with toy farms.

*Mm.*

Those are my recollections.

*Did the cycle allow you to go further?*

Oh yes. Yes, we went quite a long way I think, to Ditchling and, Steyning. Those places used to get flooded in bad weather. My brother, always more enterprising, got a speedometer for his bicycle. He, he wanted drop handlebars, full drop handlebars, and my mother wouldn’t let him, a racing bike. But they compromised on quarter-drop, so he attached the speedometer to his quarter-drop handlebars. And there was a very steep hill down behind the, on the Downs, and, of course he wanted to get the speedometer needle to go to the end of the scale at forty miles an hour. But the trouble was that at the bottom of the hill there was a sharp turn and it went under a railway tunnel, bridge, and there was traffic sometimes coming in the other direction. So I was posted at the bottom of the hill to warn him if there was any traffic coming round the corner. And he had more than one attempt at it and got it up to forty, and there was a, maybe a myth, that you can get into a speed wobble as they called it if you went too fast, and he didn’t get into a speed wobble, he did get into, did get forty. And I think that little episode illustrates my relationship with my brother.

*In terms of sort of being, almost a sidekick or a support.*

I was, I was a supporter, yes.

[46:43]
Yes. Thank you. I think we ought to consider your education now. And, would you be able to tell me anything of the teaching that you remember at the, the first educational establishment, the kindergarten on the same road as your first house?

Yes. Yes, there was, I went there and was taught by a Miss Wybrow, who ruled very neat lines across a sheet of paper, and I was taught how to write beautifully, between the lines, on the lines. The... Yes it was at that school we learnt history, and we learnt dates, we learnt 1066, and 1087, and so on. I don’t think we got much further than the Normans. And, and there was a lady who came in to teach French, and we had to say ‘Bonjour Madame’ as she came in. She was clad in black. And, I’ve forgotten her name now, but, she was a formidable figure. And, who else would there be? Well, we didn’t see much of the headmistress. She was a more remote figure. That’s the sort of thing.

And, the next school, after the kindergarten, where was that?

That was a school called Holland House, Holland House School in Hove.

At what ages were you at Holland House?

I think, probably I went there at the age of perhaps seven.

Would you remember at Holland House the teaching of science of any kind?

Absolutely blank, total blank.

The teaching of nature study?

No.

Geography?

Yes, but, capes and bays. Very much capes and bays.
Not, no field work?

Oh, absolutely not. Geography was a different subject in those days.

Mathematics at Holland House?

[pause] Mathematics. There was a chap called Mr Brookes who we called Brooky, and Brooky was a rather splendid man. He was an oldish man, and he wrote with a pen with a very special nib. And we used to say, ‘Brooky knows everything.’ He even would know who invented chimneypots. [pause] I’m trying to think of other... Oh yes. The very first lesson, I remember vividly the first lesson was dictation, and the very first words of the essay were, ‘The Spaniards’. I couldn’t spell Spaniards. But that was the sort of level. And, reading and writing was the, was a major thing. And, it was actually, Holland House was a very bad school. The headmaster was a sadist. He was called Mr Chubb. And eventually the school went bankrupt. There were two, two partners, there was a Mr Chubb who was, as I say, a sadist, and a man called Parks-Davies, who was a very different kettle of fish, and, was a very good schoolmaster I think, but was of course being the junior partner, thoroughly dominated by Mr Chubb.

[50:36]

One day at home, we were in bed, and Mr Chubb came to visit my father. And we knew that there was some sort of confabulation going on in the drawing room, and what it was, was Mr Chubb telling my father that the school had run out of money and it couldn’t open the following morning, presumably to pay the masters, unless he had an immediate source of funds. And my father foolishly stumped up some money on the security of the school playing field. It turned out later that the school playing field was already mortgaged, as they used to say, up to the hilt. And, so my father just lost his cash, and the school shortly afterwards went bankrupt. The number of boys was getting smaller and smaller and it became quite, obviously, not a going concern. And then a curious thing happened. Well not curious, but... There was another school in Hove called Claremont, and Claremont moved into the premises previously occupied by Holland House. So I was in the strange position of knowing the, a new school, but in premises that I knew all about. So I had sort of one up on the incoming people from Claremont, who didn’t know all the hiding places in the bushes.
Claremont was a very good school I think, run by a very keen headmaster who was the, in the second cricket eleven for Sussex, O’Byrne was his name, Mr O’Byrne. And he, he really did splendidly by us I think, as, as we got on in the school. By the way, you were asking about geography. Let me go back to the geography in, at Claremont, because that I do remember. It was taught, I had been taught geography by a Mr Ross, who was all capes and bays, but in comes this new master at Claremont who we called Iggy, I don’t know what his real name was. And he had a, a globe on which you could write with chalk. A bit like a huge, it had little indentations in it, a bit like a golf ball perhaps, but, you could chalk things on it. And, he could chalk things like the trade winds, big arrows going where the trade winds went. I found that fascinating.

*Mm.*

And there was Mr Knowles, who taught us to draw with perspective, and taught, he drew a village church in perspective and we all did that. And afterwards I drew masses of village churches in perspective. It was, it was good, but there was, you asked about nature study, nonexistent.

*Any outside education at all?*

Outside education?

*I mean, any sort of going into the field in order to learn. No?*

No no. Oh, Parks-Davies, in the other school, had holiday expeditions, which were very very valuable. He took us to see the Mauritania in Southampton, he took us to see Croydon Airfield, where we went inside one of the old airliners with wicker chairs inside. A number of, number of expeditions like that from Holland House. Claremont, not so I think. But Claremont nevertheless was I think a very good school, and later on when my brother and I seemed to prosper there, we were I guess ahead of all the others, and they created new, a special new form for us, so we got more or less one-to-one or one-to-two teaching. And, then I think, I kind of,
exhausted their resources in mathematics, so instead of giving up, they sent me out to an outside tutor called Miss Anscombe, who taught me calculus and how to find the maxima and minima of graphs.

[55:32]
*Can you remember how she went about teaching you, and where?*

In her house.

*In her house.*

In her house. And, I think just by one-to-one teaching. And she pointed proudly to her son and said, ‘He’s doing conic sections.’ I didn’t know what they were, but it was clearly something very advanced. And it was quite interesting that, later, much much much later on, I went to, to Yale, and, kitty-corner as we said, the house, you know, across the road, and then across the crossroad, was living the son who had been conic sections.

*Gosh.*

So I just met him purely by accident later I life.

*Mm.*

But, she was, Miss Anscombe was, just taught me things in mathematics which were way ahead of my age.

*As well as being good at it, did you like it?*

Oh I loved it.

*Can you say what, can you say, as a child, then, putting yourself in your shoes then, what you liked about it?*
I liked geometry. And I know I drew a big distinction in geometry between the theorems that we had to prove which, where the diagram was already drawn, and the ones where you had to draw a line of construction in order to make the proof go. And I thought the drawing of the line of construction, well it was a creative feat. It was real mathematics. Not just taking a triangle and then trying to prove something about it, but, actually drawing the bisector and proving something else. Do you know what I mean?

I'm not... I can just about visualise it, but could you sort of, give us... Is there a way of giving a slightly more concrete example so we can see what you mean about the, the sort of, the line of fit or the construction, the creative part of it? Assume the level of mathematical knowledge of, very low.

Well the standard theorem was about, we had something called Euclid, and, the standard theorems involve a diagram, and you argue about angles, and so-called congruent triangles, triangles which are like, in the length of their sides, or just in their angles or something like that. And you, and you proved things about them. But there were other proofs for which you had to prove something about, say a triangle, which required you drawing what they called a line of construction, which was drawing a line which was not already on the diagram. And you had to invent it. It had to come out from your creativity. And I remember feeling that that was real mathematics.

I see, yes. Almost like the sort of dotted line that you might see within a shape, that some, somebody could...

Indeed.

And, it was up to you where you put that line.

Indeed.

And then had to do the maths in relation to it.

And then had to do... Exactly, yes.
I see.

You know, in so-called Euclid, it’s a very fixed form. You have axioms, proposition, theorem, construction, that’s what I was talking about, proof, and at the end you wrote ‘QED’.

Mm.

And, mathematicians nowadays, even when they give lectures, are inclined to write ‘theorem’ and underline it on the blackboard, and then comes the, comes, what they’re doing. It’s a, mathematics can be a very formal affair, and some, I think most mathematicians nowadays still stick to this old axiomatic formulae, formula. I don’t... Whether it was invented by Euclid, I don’t know, it probably was actually.

Do you know, or have any sense as a child, why you liked maths rather than, say, languages, or, English literature, why, why maths might have appealed to you then?

I think because I found it easier. I found languages, a bit tedious. And, of course we didn’t, we didn’t go abroad and actually speak them, so they weren’t any use. So I just found it boring. And I wasn’t much good at them. But later I was very, I guess, well drilled in, for example, French pronunciation. A great emphasis on getting it right. I think I quite liked that.

And, the teaching of science at Claremont, which had moved into...?

Nonexistent. In fact, I remember well going to my public school, which I have no doubt we will come to, and revisiting my prep school, Claremont, and saying in a very tentative way, ‘Would you, would you consider teaching any science?’ It was a totally revolutionary idea. And I realised it was at the time very very presumptuous of me to suggest such a drastic change in a curriculum. No no, science didn’t exist in prep schools.
At the time then of not being taught science at school, to what extent were you engaging with science in other ways, through books or popular publications or museums or anything like that, or radio programmes, was that...?

Oh I was furiously building with Meccano. And, I had these instruction books, and there was a boy on the cover making the Forth Bridge, and I decided I wanted to be an engineer and build the Forth Bridge. [laughs] And, Meccano sets came in numbers. You started with No. 1 I suppose, I think I started with No. 2. Then you got No. 2A, which built you up to a 3. And then 3A built you up to a 4. And I got as far as 6, and I desperately wanted a 6A, because then I’d be able to build a motorcar and a Slot Machine, which worked. And, I never got a 6A, which would have brought me up to a 7. If you bought a 7 in the shop, the price was 100 shillings, which £5, and that was just out of my, out of my reach. So I always regretted never getting a 7.

So you were sort of stuck at 6.

I was stuck at 6. I got a few extras actually, like motorcar tyres, in order to build one or two of the No. 7 things. But that was the sort of thing that I was keen on. And, I suppose it was probably a little later than that, but I, I was given a number of old mahogany cameras by my Aunt Edith, which she had inherited from my grandmother and grandfather, and I took no less than three of them and turned it into an enlarger. I used one as the main projective lens, then I used another one as the, to hold the condenser lens behind it, and a third one to prop the whole thing up and use the slides to tilt it up and down. So I did kind of make things.

Where did you get the understanding of lenses and projection in order to do that?

I didn’t understand lenses. The folklore about lenses was, the lens collects the light, and then focuses it. I had some image of the lens somehow gathering the light in. Of course it’s rubbish, but that’s what they said. And I was given a, I won a box camera at my school sports. Unfortunately the lens was a little bit wobbly and it put dark lines down every picture. But, that was a kind of optical interest. And then I wanted
to get a better one, and my mother took me down, and, to Boots, and we talked to the assistant there, and he recommended an anastigmatic lens. And we said, ‘What’s an anastigmatic lens? Why should we pay more for an anastigmatic lens?’ And he drew a picture of a lens, and he showed it at a narrow angle, like that, and a wide angle like that. And he said, ‘You see, an anastigmatic lens gets more in.’ Which of course is rubbish, but, it was enough for the time. [laughs]

I was a big stamp collector, my collection’s just up on the shelf there, and, we bought Stanley Gibbons’ stamp catalogue, which had all the stamps in. And you could value your collection. Which I did. [laughs] But... What was your question? I’ve...

No, this is, this was, you’ve gone into an area that I wanted to explore.

Good.

Well, what I originally asked was, given the fact that, apart... I guess you’re going to get to science teaching at grammar school, but before that you weren’t formally taught science. So I was interested in your exploration of things vaguely scientific yourself.

Yes.

And, you’ve gone into that through the Meccano and the use of the photographic equipment and playing around that.

Yes.

Is there anything else that you played with, you know, perhaps with your brother or just on your own as a, as a child outside of school?

Yes. Yes. My brother was given a chemistry set, and I was given an electrical set. He became a chemist and I became a physicist. I don’t, I think somehow my mother must have known, or perhaps Peter expressed a, a wish to have a chemistry set. We
played with the chemistry set together. And, we did the most alarming things. We would go to the chemist, and in those days you could just go in and ask for a bottle of concentrated sulphuric acid, or hydrochloric acid or nitric acid. And we didn’t know that the acids attacked the corks; we just put them in medicine bottles as they were called in those days with ribbed sides, and, I suppose we had some kind of instruction book as to what to mix together. But we mixed sodium chlorate and, and things, and they fizzed up, and, we gave a, we gave a sort of conjuring show where we turned water into wine by changing, dropping something into the, some chemical reaction, and...

[01:07:55]

Conjuring was, conjuring was quite a thing I was interested in, and my father, perhaps interestingly, had a, a book called Sports and Pastimes, which was a collection of articles that appeared in the Boys’ Own paper, but he had it all bound together. It was a splendid book. And it had not only sports and pastimes but the pastimes involved things like conjuring, and even making experiments, like, an experiment on the expansion of metal. And it had a rod with two... just resting, and, you heated it with a candle, and one end was attached to a lever, and when you heated it enough you were supposed to see the lever move, showing the expansion of the rod. Well the lever was so crude it couldn’t possibly have picked up the expansion, but I remember making that thing and being very disappointed when it didn’t work. Likewise, I bought a, a so-called electric motor at Marks & Spencer’s, which was a thing you made yourself, and the diagram showed a, I made the thing that went round, and then it showed a coil wrapped on a sort of arching piece of metal. And the coil it showed was just a few turns of wire. And I followed that religiously, and I made just about four turns of wire, cotton-covered wire. And of course the thing didn’t work. Because really, what you needed was hundreds of turns. But the diagram just showed four, and I put four in. And I took it back to Marks & Spencer’s and complained that it didn’t work. And they took it back.

[01:09:58]

Was there any sort of, were there any books on science that you were reading? Apart from this Boys’ Own version.

No, that was it.
Mm.

That was it. Except for, except for some kind of, oh yes, children’s encyclopaedia, which had a diagram, picture, of water molecules with oxygen and hydrogen. I thought that was quite fascinating too. But didn’t get any further than that.

Do you remember why you thought it was fascinating?

No, it just seemed a neat idea, all those little things inside the water.

Was this Arthur Mee’s Children’s... Arthur Mee’s?

No. No, we knew, Arthur Mee’s Wonderful World, that was one of our children’s books. Yes.

But the children’s encyclopaedia wasn’t an Arthur Mee’s?

The children’s encyclopaedia was something different I think.

Ah.

Yes.

And what else did you do with the chemistry set?

What else did we do? Well we tried to make gunpowder, not very successfully I think. What else did we do? We succeeded in making an awful mess, I remember that. We did it in the attic of our second house, led by my brother. Potassium permanganate played a big part in it, and indicator solutions I think. And we, we collected gases, we made hydrogen and, collecting gases over water. And, I think made a, made a bang with them. So we learnt about how to collect gases certainly. That was all very much home, amateur stuff, self-taught. We, we bought books on
these things, like tennis for example, we bought a book on it, how to play tennis. We were great ones for buying the manual.

[01:12:06]  

*Given that your mother bought you the electrical set, and bought your brother the chemistry set, what was the view of her and perhaps of both your parents of science and its value or otherwise as a...?*

They never discussed it. They never discussed it. I think they were happy to indulge our keenness, but they never expressed any interest in the experiments themselves or suggested doing things. There was nothing like that. They were, they were just happy for us to do our own thing.

*And what did you do with your electrical kit, or you do with your brother perhaps at...?*

Well, it was a good electrical kit, looking back on it. I, I stroked a compass needle with a magnet and magnetised it, and then it pointed North, which was quite nice. That was one thing. Oh, there was a, there was a, a Leclanché cell. I don’t know whether you know about Leclanché cells. Well, electric bells in those days, in houses, were run from batteries, and they needed a battery which would last for a long time with only very intermittent use.  [mic noises]  And there was a thing called a Leclanché, Leclanche or Leclanché cell. I don’t know how it worked, but it had a carbon electrode down the middle which you immersed in the appropriate electrolyte, and that, there was equipment for making that, which was fun. What else was there?  [pause]  Oh it was quite a big set actually.  [pause]  Oh there were lots of things for doing static electricity, there was an ebonite rod, which you rubbed on your sleeve. I think it was ebonite on silk, and, ebonite on wool, which were the most efficient ways of generating static electricity. And then you passed it from, you electrified something else and you, and you had a, a pith-ball on a very fine thread, two pith-balls indeed, and you could make them repel one another. And, you learnt about static electricity quite well. That was a good, that was a good set.

[01:14:39]
I don’t... you may not be able to remember, but at that time... You said that your brother became a chemist and you became a physicist. At that time did you have any sense of what you wanted to do in the future, or was this just playing with something?

No, I wanted to be an engineer. Because of the Meccano.

Because of the Meccano. Yes.

Structural engineer.

And your brother, did he have ambitions about the future at that time that he spoke about?

He didn’t speak of them. But he had obvious interest in chemistry. Which I didn’t actually share altogether.

Why did you not like it so much do you think?

Well partly because my, at school, we’re coming to my next school, but, we had to analyse things, and I couldn’t smell very well, and it was one of the important things that you had to do. And I wasn’t very good at that, and... Otherwise, I can’t... Only that, just pouring things in and out of test tubes was not altogether my idea of fun. I enjoyed watching some of the chemistry demonstrations. There’s a marvellous one... This is going on to my other school.

[01:15:52]
Yes, let’s do that, yes. The teaching of science at your grammar school.

The teaching of science... Yes, well, it was, the spectacular thing was collecting a gas over water. I don’t know what gas it was, but as the gas came to the surface, the bubble broke the surface, it ignited, and the result was a beautiful smoke ring going up into the air. That was a lovely experiment. No, that was quite late on actually. I went to a public school, I went to Stowe.
And, what age did you go here? This would have been, your teens.

I went to Stowe in, when I was thirteen I think.

And this is Stowe grammar school?

No, public school.

I see.

As a boarder or as a day pupil?

As a boarder. It was entirely boarding.

Right.

My prep school had boarders but I, I didn’t board. My brother, who was older than I was, went to Charterhouse, and, he may have got... and he got a scholarship to Charterhouse. Anyway, he went to Charterhouse. Which is, you may know, a public school near Godalming, old-established, hence the name. Used to be in London in the, in the old monastery there. And, it was kind of expected that I would probably go to Charterhouse, and so I took the scholarship examination to Charterhouse, and failed to get a scholarship. And my headmaster, this good headmaster, O’Byrne, said, ‘Well there’s this rather new public school called Stowe, it’s only been going since 1923, but it seems to be a good school. Why don’t you go in for that?’ So I went in for the Stowe scholarship, actually almost simultaneously with the Charterhouse one, and I didn’t get the Charterhouse one but I did get the Stowe one. And I was given the choice of which I wanted to go to, Charterhouse or Stowe, and because the fees at Stowe were slightly more than they were at Charterhouse it would have been financially the same for my parents. And, so I thought, well, I’ll go to a place where they’ve given me a scholarship, because, maybe I’ll be, do better there. So, so I said I’ll go to Stowe. I had visited it for the scholarship examination, and didn’t have all that high an opinion of it, because, it seemed to me to be a mixture of an ultra-modern style of architecture which was the art school, where I think we must have sat the
exam, and eighteenth-century buildings. It was only when I got there that I realised what an absolutely splendid place it was. Have you ever been there?

No.

It’s now National Trust. And it’s the finest landscaped garden in the country. Absolutely superb place. And we had the run of it. Amazing.

And how did you feel about not being at the same school as your brother?

I think it was probably a good idea that we were separated actually. Because we were somewhat rivals, and, I was quite happy with that.

And so...

We met in the holidays, and got on very well together.

[01:19:48]And so, what do you remember of the teaching of, let’s take the sciences in turn, of physics at Stowe?

At Stowe. Well they took it seriously. You had to make a choice. You couldn’t do biology and physics and chemistry. So I, I chose physics and chemistry. I think probably because, physics was a bit more like mathematics. And, they had labs. Being an isolated place, about, was it five miles or so from Buckingham, there was no mains gas, so we didn’t have regular Bunsen burners; we had special petrol gas burners, which were slightly different. So I had never seen a Bunsen burner until I left school. And, I think, you’re not allowed to have Bunsen burners in schools now because of health and safety.

I doubt they are, yes.

Yes, something like that. And anyway, at Stowe, we were taught, for example, lenses, and we had knitting needles as objects, and we had to line up another knitting needle
with the virtual image in the space, and, measure the focal length. And I remember
getting it very mixed up. And I remember a diagram I drew which was all technically
correct, but so, [laughs] had all the symbols all sort of, in the wrong places, so that it
actually muddled my physics master so that he got thoroughly infuriated with it. But I
knew it was actually right. [laughs]

[01:21:48]
It was, the teaching was... Well, I remember the first experiment we did was with a
copper calorimeter. Now a copper calorimeter is normally insulated with cotton wool,
inside another one. And, if you take away the cotton wool and you don’t have it
insulated, of course it’s not going to work at all well. But we didn’t have the cotton
wool because we would have made too much mess with the water if we’d had the
cotton wool. Nevertheless, we did the experiment, and we got an answer out, which
was the specific heat of copper. I think you poured in, I think you probably weighed
the copper calorimeter on a balance, and then you poured in hot water at a certain
temperature which you measured, and, and then you measured the temperature after it
had equilibrated with the, with the copper outside, and then you worked out what the
specific heat of copper was. It was .1 in some units or other, I don’t remember now,
but I remember it was .1. [laughs] Why that sort of, banal experiment should have
appealed to me, I have no idea. It was very boring, but it did seem to appeal to me.
So that was my kind of introduction to physics.

[01:23:12]
And were there any other striking lessons or images or ideas in physics at that school
that...?

Oh yes, we then had, later we had a, a very good physics master appointed, William
Llowarch, Welshman. And he, he had written, he wrote a book on the ripple tank.
And he was very keen on us pupils actually seeing with our own eyes the
phenomenon that he was talking about. He, he had the technician build a wind tunnel,
and we blew smoke through it, and we saw the smoke going round an aerofoil and
what happened when the, when the, when it stalled and all the vortices went wrong.
He, he bought an oscilloscope for the school, which of course you can do lots of
things with, and see waveforms. He was a very progressive chap. We, we couldn’t
do... A lot of the experiments depended on AC of course, and the school had
generators which produced DC. So under each workbench there was this great box about this big which converted the CD to AC, and then you could do the AC experiments. But the labs were quite, I would say well equipped by the standards of those days. We had a lot of experiments on prisms, on spectro... on spectro... What do they call them? Spectroscopes I guess. And, he showed us, which was fascinating, what happens if you have a glass tube with a gas in it, and you have electrodes at either end, and you pass electricity through the gas. And of course it doesn’t pass at first, but, as you evacuate the tube with a pump, gradually you get a more and more tenuous gas, and you see the most beautiful discharge taking place inside the tube, like a strip light but much more interesting and detailed. You see coloured bands. They’re called Crookes bands. And then as you go on evacuating the tube, it gets more and more interesting and more and more detail. Finally there’s nothing left at all because the vacuum is so good that no electricity can pass. But, that kind of experiment, which I’ve never seen anywhere else, was shown to us at school. I think that’s marvellous.

Yes, it seems very, it seems very advanced in terms of technology and set-up and, you know...

[01:26:15]

Well, you see, that sort of experiment was fashionable in the early years of the century, late Victorian times. Crookes was the big man. And, it’s just not fashionable to teach students about that sort of thing now. The interest has passed on to other fields. Science, you know, very fashionable, it changes. What’s considered interesting changes.

And the oscilloscope, could you describe that and how you used it?

Well we were, we were told how it worked, which was, that you had a, a cathode, an emitter, which sent out electrons, and they went towards an anode, which was positively charged, and then the saying was, that when they get there, the joke’s on the beam, because there’s a hole in the anode, so the beam goes through the hole. And that makes the, a narrow beam. And when you’ve got a narrow beam, then you’ve got the possibility of deflecting it with two oppositely charged metal plates,
which is the, makes an electric field, and, and moves the beam up and down. Or, if you, if you prefer, you can put a magnetic field there, which also makes the beam change its direction. And it then hits this fluorescent screen, which is what you’re looking at, and you see this spot move up and down, or side to side. And then if you’re clever you can make it scan backwards and forwards; if you’re still cleverer, you can make it scan up and down as well. And then you’re halfway to television.

It’s, it was a very useful piece of equipment to buy for a school. This was in about 1938, when we didn’t have television of course.

[01:28:33]

Thank you. And, have we, are there other memories of the teaching of physics that stand out, or should we move on to another science do you think?

Well, years later, I was interested in optics, and I had, this was in Bristol, and I had some students, and the idea was that, when you look at a ripple tank, do you know what a ripple tank is?

No, could you describe?

It’s a, it’s a, it’s a shallow dish with water in it, and you make waves in the water by vibrating something. And you look at these waves, and you do, you make them do various things like being reflected from the side of the tank. And you can see then what happens when you’ve got two lots of waves going in different directions. It’s called interference of waves. And it’s a, it’s a very good tool for teaching about waves. Now, my physics teacher, Llowarch, wrote a book on the ripple tank, and while I was at Bristol, comparatively recently, we realised that when you looked at these waves, you were looking at them with a lamp to illuminate them, and you weren’t actually seeing the surface of the water at all. What you were seeing was the pattern of, of light on the bottom of the tank, which was focused by the curved ripples. And we worked out how this, how this happened, and what it really was all about. So the students, I think we must have written a paper which was published, and I sent this paper to Mr Llowarch, we called him Fluffy Llowarch, who by then had long since retired, he lived in Bicester. And I sent this paper to him, because he had written a book on the ripple tank, and I also sent a few reprints of my own, my recent optics
work. And in my letter I must have said, ‘Do you remember me? I was in your class.’ And he wrote back immediately and said, ‘How could I forget you?’ [laughs] And the reason was perfectly clear, because I was the one in the class who was always asking the questions. And, I remember particularly, he was teaching about interference of light that you get when you have two parallel plates and the light beam strikes one and then it strikes the other one, and then the two reflected beams interfere, and the standard diagram in the textbooks shows a ray bouncing off the first plate and bouncing off the second plate. And I said, ‘Please Sir, but what about all the other rays?’ Not just this one you see. It was a question he had never been asked. But, all, all credit to him, he didn’t, he didn’t just pass it off, but, really thought about it and tried to explain why. And I think that was the sort of reason why he didn’t forget me.

So he tried to explain, well, presumably the diagram is away of simplifying the...

Well it only shows one ray.

Yes.

And of course, you’ve got to show where all the others go to.

Mm.

They might have mucked the whole thing up. I now know quite a lot about that, that system. But, if you look at the ordinary school physics textbooks, they’re liable just to ignore all the, what they would call complications like that, which are really what you have to think about.

Mm.

So, I think that’s why he didn’t forget me, and I was, I was the one who was always asking the, the questions in the physics course.
What did he think of the paper produced by the students on the ripple tank that you sent?

He didn’t say.

Oh.

He didn’t say. But I think it would bring to his attention lots of things he didn’t know. Because it depends, as, probably we’re going to talk about this later, but, it depends on what’s called catastrophe theory, which is quite a new, a new subject. He wouldn’t have known anything about that, in fact it wasn’t invented until, well, thirty years, forty years after these lessons.

I suspect he would have been very flattered that you remembered the ripple tank.

Well I don’t know, but...

[01:33:12]

And the teaching of chemistry at Stowe?

Teaching of chemistry? Yes. Well it was taught, and I, I approached it with interest, but nothing like the interest that I had in physics and mathematics. My real interest was mathematics. But the chemistry consisted of us doing practical things, and the master also demonstrating probably the more dangerous things, down on the, on the bench. We once very daringly took a photograph of him during the class. I suppose nowadays with the mobile phone that would be commonplace but in those days to have a little miniature camera and take a surreptitious photograph of your master was, [laughs] was considered very reprehensible.

Can you remember why you did so?

Why we did? Oh just for fun really. Yah. I think the masers had their eccentricities and we liked to caricature them. Poor chaps.
You said down on the benches. Was it a sort of ranged seating?

It was, the seats were raked. And you looked down on a, on a bench on which the experiments were done. These classrooms were all built in the old, well, outhouses of the, of Stowe School. I really must, perhaps at lunchtime I can show you the pictures of Stowe, it’s an absolutely splendid place.

[01:34:51]

Mm. And the teaching of mathematics?

Teaching of mathematics. There were two prominent mathematics masters there, Mr Archer, we called him Freddy, Freddy Archer, and, another one called Walker, Raymond Walker. Archer was the older man, and he, he knew about vectors, and he taught us, he taught us in a fairly modern way I think then about vectors. We had reasonable textbooks. But you know, the textbooks in those days were all so much duller than they are now. The production was awful really, if you look at them they’re, they’re horrible, horribly printed. But, I think for their day they were OK. There were textbooks by a man called Durell. We imagined Durell’s home as containing nothing but bathtubs with taps going in and filling at a certain rate and overflowing and, [laughs] that’s what all the questions were about. But, the teaching of mathematics I think was pretty good. They were... I was in the form where the so-called scholarship boys were, and, all the subjects were taught in the same room. And I vividly remember how, with the classics master taking the lesson, I was very much the sort of, at the bottom of the class, and, could never translate the wretched Latin. But as soon as the mathematics master came in, the pecking order sort of, subtly changed, it was a social phenomenon, and I was top of the class, and the other mere classicists were at the bottom. It was lovely, the inversion. [laughs] And, and I think there was a good deal of one-to-one teaching went on, because I would go up to the master’s desk and be taught one-to-one up there. And then later on in the higher forms it really was, not exactly one-to-one teaching but one to two or three, I think two actually most of the time, by either Freddy Archer or Raymond Walker.

[01:37:40]

And the time came then to set my sights on Cambridge, and, what college should it be? Well Raymond Walker had recently graduated from King’s, and he suggested I
should apply to King’s. So I did. And I took the scholarship exam to King’s, and of course was delighted to get a major scholarship there. And, I suppose that really was quite an achievement. And then I had two terms to spend before I could go to Cambridge, because you took the scholarship exam in December and then you entered the following October. So there were two whole terms. And they said, ‘Well what do you want to do?’ Well, I could have spent time in my father’s office. I thought I’d rather stay at school. And, ‘What would you like to learn?’ So I said, I’d like to learn shorthand, because I thought that would help me with the lectures at Cambridge. And, I’d like to learn about economics, which I didn’t know anything about. And of course I’d like to learn some more mathematics. So mostly, what I remember is learning the mathematics.

[01:39:06]

And Raymond Walker, he was sitting in a chair like this, and I was sitting on a chair by his side, and he, he took us to Eddington’s book on relativity. And he turned over the first chapter, which was about special relativity, and we went straight into the general theory of relatively, which was all about tensor calculus. I loved it. Of course I didn’t understand the concept, but I loved the manipulation of the symbols, and all those suffixes and superscripts, and covariant differentiation, and contravariant differentiation. And for some reason, it’s rather like doing crossword puzzles, I just liked it. And it was a very, when I look back on it, it was really an astonishing thing to do to a schoolboy. But it’s had a great influence.

Why do you, what about you as a, a person at that time do you think led to you liking the, the fine manipulation of these sort of terms in the equation and these symbols?

Who can say? I don’t think I can, I don’t think I can put my finger on any kind of... I think I was fairly systematic. And I was very assiduous, I did all the examples that were set me, and more. And, actually it was quite tough preparing for the scholarship exam, because, I worked very much on my own, just going through example after example after example. And... [pause] You see the Cambridge scholarship exam was, was not, wasn’t like the Higher School Certificate, which was a very cut and dried thing with a syllabus. It was more or less a free-for-all and you didn’t know what questions you were going to be asked. And they had general papers which might have covered almost anything. So, it was a bit of, I suppose it was perhaps a
matter of luck too as to what kind of questions you got. But King’s set the usual sort of, questions. I remember one, one year, not my year, the English essay was, ‘What determines the market price of a Rubens painting?’

_Gosh._

And, I know why the question was set like that, it was because King’s had just acquired a Rubens altar piece which they had put in their famous chapel, and the, one of the fellows, Michael Jaffé, was head of the Fitzwilliam Museum, and had obviously been asked to suggest a question, and that was on the top of his mind. So he, that was the question. And we all had to, they all, not me, that year, but in that year they all had to answer. I honestly can’t remember what the subject was in my year, but that was the kind of thing.

*But were you applying to study a particular subject?*

Yes.

*You were applying to study maths, or, or a Tripos?*

I, I did the mathematics and the physics papers.

*But the entrance exam would cover all sorts of areas?*

No, I had taken the so-called School Certificate and the Higher School Certificate already. As I said, these were much more formal things with syllabuses. And, in Higher School Certificate, I got... in School Certificate I did very well indeed, and in Higher School Certificate I did, you had to do supplementary subjects, and I did German, and I think chemistry too. And that was all right. Although I wasn’t very good at the chemistry I think, but I nevertheless did well enough. And, much was my joy when I got a scholarship in mathematics and physics. Could have been in either, or both, and it was in both. And, and went to King’s.
When did you join King’s?

1941.

Just before we get there, could you tell me about any significant friendships at the private school, and the sorts of things that you got up to when you weren’t being taught?

Yes. Yes, well it’s quite interesting actually. I, I can almost say I made no friendships at Stowe, and I made lots of friendships at Cambridge. I cannot understand why. But I was, I was sort backward in relating to people, and wasn’t good at making friendships with boys. I did, of course I did have some, but, nothing very significant. I’m trying to think now. But you see, they don’t come to the top of my mind, the friendships I made at Stowe. Masses of acquaintances of course, but no bosom friends.

It seems surprising given that you had been used to playing with your brother and, you know...

I suppose it was really. But I was, I really was very backward socially. I still am in a way. But, that’s just a personal characteristic, which I regret. But, but it wasn’t like that at Cambridge. I think it was more, at Cambridge there were people more to my way of thinking, and, I took to it. And it was quite, quite different. It was a revelation to me.

By your way of thinking, what do you mean?

I suppose they were more scholarly academically.

So what would, leaving aside the fact that you can’t remember making particularly close friends at Stowe, what sorts of things would you do when you weren’t in lessons? In other words when you were in your boarding houses.
I would go, I would go to the art school, and, I enjoyed art. There was a very good art master there, Robin Watt, who had been a portrait painter Canada, and during the Depression, nobody could pay for portraits, so he had come over to England, and he set up this art school at Stowe with his wife, Dodie. And, Dodie was a sort of mother for the boys, and they were forever sitting with her in their inner sanctum on the sofa. I often wonder what they talked about. I realised later they were talking about themselves, of course. [laughs] But, Robin Watt was very, was a good painter, and also a very good draughtsman, and he sat me down and taught me how to draw pencil drawings of people. And, I enjoyed pencil drawing. Wasn’t terribly good at it, but, I think I, I got better. And I gradually got the hang of it from...

*Is it something you continued to do beyond Stowe?*

I did it at Cambridge, but I really didn’t do it afterwards. I did paint, I painted afterwards, I’ve got paintings here which you can have a look at if you like. But, that was, that was a, a legacy of the art teaching at Stowe. And if you ask me what I did in my spare time, well, there was that, but remember it was also wartime, and there was the Home Guard. And, we spent a lot of our spare time being taught to be signallers and laying down wires and speaking to each other over field telephones, and going out on dawn and dusk patrols to watch for Germans dropping from the sky on parachutes, disguised as nuns of course. [laughs] But, I mean it was really, kind of serious stuff. We had live ammunition and, we, we took it seriously. Because it was the time when we could have been invaded any moment you see.

[01:47:55]

*Mm. What do you remember of, more generally of the sights and sounds of war?*

Oh war?

*Mm.*

Well I was...

*Part of which you would have been at Stowe, part of which at Cambridge, but...*
That’s indeed right. Well there was, there was the Abyssinian war, first war, which I followed with a lot of interest. And... And then, then the, when I was at school, the First World War, the Second World War broke out, and, I enthusiastically put flags into the Daily Telegraph map of the, of Europe, and we had British flags and Nazi flags and French flags, and we watched the... And then came 1940, and all the flags moved, and we watched them with enormous attention of course because, it was very menacing. And the call came out to join the... I was in the, I was in the Boy Scouts at first, and then, then I moved to the OTC, Officers’ Training Corps, because I thought that would be a bit more sensible. And then came the possibility of invasion, and they formed the so-called LDV, Local Defence Volunteers, which I joined. I have my card somewhere. And, quite soon Churchill turned that into the Home Guard, and we wore armbands, because there were no uniforms, but we had our, we had our OTC uniforms, and we wore the armbands on those.

At what age then were you wearing that uniform and carrying live ammunition and looking out for Germans dropping?

I suppose, eighteen I suppose. It was in 1940, I was, would be seventeen wouldn’t I? Yes, seventeen. I mean we, the war broke out when I was sixteen.

[01:50:17]

And did you, what did you see of bombings or of...?

Bombings. Not a great deal. In order to get to school, I had to travel through London, and, had to go to Liverpool Street Station from Victoria and went through all the bombed City, which of course made a big impression. And, I was caught in London on one occasion during an air raid, but nothing serious. And at Stowe, we were isolated in the countryside, so, weren’t likely to be bombed, but, at the time of Munich in ’38, we enthusiastically dug trenches as bomb shelters. And, they weren’t used. And then in 1939 we even more enthusiastically dug trenches as bomb shelters. They were only used on one occasion actually, when, I was in my dormitory and a German plane came over and dropped a stick of bombs. And, you heard them going off one by one. And of course at the different houses, the housemasters had different
reactions. In one, the house next to mine, he sent them all out to the bomb shelters, which were of course muddy, horrible places with just, they were just trenches with timber over the top. And, I think it was the only time they were ever used. But the bombs fortunately didn’t drop on the school buildings but they dropped in the playing field, just in front of the school, and we, we were sent out to pick up all the bits, because they were going to get, they were going to ruin the lawnmower. [laughs] So, that was that, that was that bombing. I mean it wasn’t, there were no casualties. But, during the night I vividly recall one of the more adventurous boys going out to see what had happened, and came back with alarming stories, which weren’t true.

[01:52:38]
We were bombed in Cambridge. That did do damage, it, it bombed houses and the Union building, and I remember that night quite well. At Cambridge we did what was called fire watching, which meant going on the roof of King’s College chapel on a rota system and looking out to see if any fires had broken out. Because the incendiary bombs were a very frequent occurrence. At school we had lots of incendiary bombs, bits of incendiary bombs lying around. I suppose they, we didn’t know they’d been dropped I suppose, but the boys all collected them, and the headmaster was worried that there might be still some unburned material in them. And we were told to throw them into his garden as a precaution.

*What did they look like, these incendiary bombs?*

They were just metal cones, like that, with fins on, and they were just the remains of something that had already burnt. About that size.

[01:53:44]
*I was interested in the Daily Telegraph map with the flags. Was that something that the Daily Telegraph provided?*

Yes, it was a kind of supplement, you know, just as the newspapers now provide every now and again a, a pull-out thing. It was a big wall map.

*And they gave you the flags as well?*
Probably, yes. And the Maginot Line was marked very prominently on it, which was supposed to be the impregnable line that would stop Germany invading France. And indeed it did, but of course it stopped at Belgium, and it was quite an obvious way, thing to do, just to go through Belgium.

[01:54:23]

*Mm. And, could you tell me more about the training that you received with the Officer Training Corps which became the Home Guard, in terms of sort of, civil defence methods?*

Well it was drilling of course, we had a staff sergeant who had been in the Guards, and he taught the recruits to march at double speed. There was something about being trained to march at double speed which made it easier then to do the proper things at normal speed. And, he was, he was a real stickler. There was another sergeant-major who was a much more gentle character who had actually fought in the First World War, who taught us about bullets. And I remember him telling us about the bullets, how they were, they were made of lead, he said, and then they had a coating of cupronickel, for humane purposes. We daren’t ask him what that meant. [laughs] Still don’t know why it was. But, he, we had these sort of lectures about, about weapons. We learnt all the parts of a rifle. You know that poem, *Naming of Parts*, ‘Today is naming of parts,’ and it means, learning all about your piling swivel and your butt and your trigger, and the bayonet socket and all that. So we learnt about, we had rifles and we learnt all about them. And we drilled with the rifles, shoulder arms, order arms, that, all that stuff. And we drilled. And we drilled. And every now and again we had a so-called field day, when we went around all these extensive grounds pretending to be soldiers, doing things. And, I remember, I remember a chap for example who was a rugger player, and he, in one of these situations, decided to have his platoon charge, and he shouted, ‘Charge!’ And off they ran, and charged. And of course, these are the very chaps who got killed very early on, and the... Many many of my contemporaries at Stowe were casualties of the war. There’s a war memorial on the, with all the names, on the wall of the chapel, and I think I knew, well about every, literally every fifth person on that, on that list. Because they were exactly of an age to be in the war. You see the school had started in 1923, and the early, well, my
contemporaries were the ones who went into the war and got killed. Partly because they were such sort of keen rugger players and...

*Mm. How did that affect their...

...brave people.

[01:57:34]

*How did that affect their, their call-up or the likelihood that they would be early casualties?*

Well, I mean... Well I mean there was, there was a call-up, and, I was liable for call-up. There were so-called reserved occupations, who were not called up. I as a scientist... We’re at Cambridge now. I as a scientist was interviewed by C P Snow, you may remember the name, who was the director of the Central Register, and the idea was that, this time round they were going to make better use of scientists than they had done in World War I. In World War I typically they had problems, for example with, say, ballistics, and they said, ‘Well, we must solve this problem, but the chap who knows all about ballistics is Littlewood, and he, he is out in Malaysia.’ And, they got their scientists all out in, in the field, and with the troops called up, and they weren’t using them. And this time they thought they’d do it better. So they had the Central Register which was a compilation of all the expertise there was in science. And, at Cambridge, arts people were allowed one year and then they were called up, and, with the chance of coming back after the war, and finishing their degrees. Scientists got two years, because the expectation was that they would be able to use their science in the war, particularly as radar officers. I fully expected to be either a radar officer on a naval ship or possibly in the Middle East, in the newly-formed REME, Royal Engineers Mechanical, engineers, something, I’ve forgotten. But, that was the kind of thing that was in prospect. And shall I tell you how I was not called up? Well, it’s a slightly perhaps involved story, but, the Tripos examinations... Excuse me a moment.

[End of Track 1]
Now, just off the recording we’ve been talking a little bit about Stowe School, and you’ve showed me some interesting books, and perhaps you’d like to say a bit more about those.

Well, Stowe had a very charismatic headmaster, J F Roxburgh, one of the greatest headmasters of our generation, and, or my generation. And, he was a very, obviously a very big influence on me when I was at Stowe. He would have the, let me see, the... He would have the heads of the houses, there were eight houses and we had, each one had a boy who was head boy of the houses, have the heads of the houses to dinner, with great ceremony. He was, he collected glass, and he would intentionally lay on a very complicated place setting. For example there were huge glass vessels with water in them. And I didn’t know whether they were finger bowls or whether they were for drinking until, with a great flourish one evening he drank from one of these goblets, and I knew then that it was OK to drink from them. [laughs] And, the sixth formers, he would entertain in a different way. He would invite them for port on a Sunday evening, and we would sit around in his study and drink port and try and be intellectual. [laughs] So, he...

[01:46]

And he, he made a point of teaching every form in the school once a week. And he taught us, all kinds of things, but particularly I remember architecture. He would come round with this suitcase of books, throw them around the classroom, and, teach us about Gothic architecture, on which he was an expert. And I learnt then to appreciate the construction of a Gothic cathedral, and why they had the flying buttresses, and, all that, and how it developed and all that, and the different periods, and that sort of thing. So I can date a church reasonably accurately now. He was that sort of a headmaster. He would also teach Latin verse, which didn’t appeal to me but the way he taught it was so flamboyant that you couldn’t help being riveted by his simulated attempts at translation. [laughs] He, obviously he would go through the same routine with many classes, because I’ve heard this repeated, but, it all seemed perfectly authentic at the time, and he was an actor as well as a, a great teacher.

[03:08]
Anyway, coming to these books. Towards the end of my school career at Stowe, there was a mathematics prize to be competed for, and I won it, and, what you got was a book with the Stowe crest imprinted on its cover. So I went to the headmaster and he said, ‘You’ve got this prize. What book would you like my dear fellow?’ And I was pretty nonplussed, didn’t know what to, what to choose. I said, ‘I really can’t think.’ He said, ‘Well have a look around my shelves and see if you can see anything that you like.’ So I walked around his shelves. And there was a book which did take my fancy. It was, had a beautiful maroon leather cover, maroon. And, I said, ‘Well I rather like this one Sir.’ ‘Ah,’ he said, ‘that’s the one we gave to Queen Mary when she visited.’ And there were photographs in it, real photographs, stuck in it, at the, in the end pages, for Queen Mary’s visit. So I realised I’d really dropped a clanger and asked for a book which was only fit for royalty. But I... He said, ‘Perfectly all right my dear fellow. I’ll choose the photographs. We’ll choose the photographs together, they’re all mine.’ He used to go round the grounds with a plate camera and take pictures of the various temples and monuments, school buildings and so on. And, I was allowed to make a selection from his photographs, he showed me a great collection and I chose the ones that I thought would be best. And, during the school holidays he had them printed by the photographic firm in Buckingham, and we corresponded about them. And, finally appeared this most beautiful volume. Not bound in maroon you notice, but in blue, which was the school colour, so it’s not an exact replica of Queen Mary’s book but it’s, otherwise it’s... And the photographs are different, because, I chose them, but otherwise it’s, it’s identical. Well that’s the sort of man he was.

*Mm.*

You asked me about that book. The other book is the, is the catalogue of the great sale at Stowe of 1846, when all the contents of this great house were sold and dispersed.

[05:58]

*Thank you. Now, after Stowe, you, you went up to King’s College. And, you were about to explain at the end of the last session how it was that you were not called up.*

*Yes.*
Yes. Well it depends on a particular sequence of dates. As you probably know, the Tripos exams at Cambridge take place in June, and the terms begin in October. So, after doing my two years, which I was allowed as a scientist, I was fully expecting to be called up, along with my, the people who were contemporary with me at the time. And the usual jobs, I think I, maybe I said this, did I, were, were being a radar officer on a ship, or to do with the battle in the Middle East where scientists were needed. But then it was discovered that the call-up date was the calendar year. So, I would be called up in January. So this was June you see, after the Tripos, I was going to be called up in January. So they said, ‘Well look, if you want to, instead of finishing now, and hoping to come back after the war, I, why don’t you start the third year in October, and then take a special Tripos exam in December, after one term.’ Well of course this was a tremendous gamble because, you can’t take a Tripos a second time, and if I had got a bad degree at the end of December, that would be it. Whereas my colleagues would come back after the war and get proper degrees.

So, all the class of about, I suppose twenty-five, were given this choice, and only two of us opted to gamble for another term. My idea was that the war was in a pretty bad state. We had, Singapore had fallen, and the two battleships had been sunk, the Prince of Wales and the, was it the Renown? They were, it was a big, big point in the war when everything looked very black indeed. And I thought, well, after the war, that’s the German occupation. They haven’t a hope in hell of coming back and finishing a degree, they must be mad. So I thought, well, I’ll take what’s going. So I did. And, the other chap was Sondheimer, Ernest Sondheimer, who was much cleverer than I was, and just the two of us did this extra term, and took a Tripos exam in December, which the authorities were a bit put out by, because to set a whole Tripos exam for two students was a great deal of extra work for them. And in those days there was also a practical exam to set. It was a marathon, it was a whole day’s work in the labs, as part of this so-called practical exam. And, we went through all that. And at the end of it, Sondheimer got a first, and I got a 2:1. And, I was then expecting to be called up. But during the Christmas holidays I got a letter from Sir Lawrence Bragg, head of the Cavendish, saying that a job had appeared, or two jobs had appeared, actually in Cambridge, and that Sondheimer would have one and I would have the other. And the two members of staff involved were, were, a chap who had invented
the cavity magnetron, Randall, Randall and Boot were the two who were credited with
the, having invented the cavity magnetron, which you may know was the kind of,
most important bit of the radar systems. When the first cavity magnetron was taken
across the Atlantic it was described as the most valuable piece of cargo that had ever
crossed the Atlantic. So, Randall was, was the top chap, and the other one was
Orowan, who was a Hungarian refugee. And Randall being the senior was allowed
first pick. So naturally he chose the chap who got a first, he chose Sondheimer, who
was then taken into the Cavendish and given a soldering iron. He was a theoretical
chap, hopeless with a soldering iron. [laughs] And, Orowan got me. So I went to
Cambridge and joined Orowan’s little group, and at the same time there went with it a
little bit of demonstrating as they called it in the labs, for the teaching. So that’s how
I was not called up. And I wasn’t at all disappointed about it, in fact I was delighted.

[12:05]

Orowan, was he involved in the designing of the, the magnetron that...?

No. No, Orowan was quite a different lineage as you might say. He was a refugee
from Hungary, he was an engineer physicist who had been trained in Berlin, and was
working in the basement of the Cavendish on, well, when I first went up to Cambridge
to do this, I went into Bragg’s room and we went over the arrangements, and I said,
‘Could you give me some idea what the work might be Sir?’ Bragg said, ‘Shatter
phenomena.’ Shatter phenomena. I’ve never heard that word ever used again. What
it was, was to do with the cracking of ships, welded ships. The U-boats were sinking
ships at a great rate, and Kaiser in America, the Kaiser ship works, were churning out
ships at an equally great rate, and they were welding the plates together, it was a new
method of ship construction. And a large proportion of these ships were cracking in
mid-Atlantic. They... I’ve forgotten the percentage now. I think it was something
like, I may have got these figures quite wrong, but, twenty per cent were cracking,
and, three per cent perhaps were cracking so badly they sank. They would break in
two. And this was a serious problem for the whole survival of the country, because
all our supplies were coming over in these ships. So they wanted to know what
caus ed this brittle fracture. And, Orowan had his little lab down in the basement
which I visited, and I remember being very impressed by the fact that it didn’t have
the usual tiddly bits of equipment that I was used to doing experiments with, but it
had a whole gearbox down there with a gear handle on it and a miniature rolling mill. And great testing, metal testing machines, on quite a scale. And it was all quite unfamiliar to me. And Orowan would sit on one side of a desk and I would sit on the other side of the same desk, because there wasn’t much room. And there was an Austrian refugee there who sat at another desk, which he had to clear in order to do any experiment. And there was a Polish refugee, Captain Los, who had somehow escaped from Poland, who was doing... you spell his name l-o-s. And, he was doing experiments on a, with a testing machine, to try and understand brittle fracture. We were all trying to understand brittle fracture. Although, Hoff, the, the Austrian, was working on, on a miniature rolling mill, because, Orowan had an interest, he’d just published a paper on how to calculate the power needed with a rolling mill. You know you put these strips of metal in the rolling mill, it’s a huge business, and they need to know before they start what sort of power they had to put into the rolling mill. And, you need to calculate that, it’s not an easy thing to calculate, to roll it like a pastry, you know, to...

Into a sheet.

Into a sheet. And, Richard Thomas and Baldwins had that huge rolling mill in South Wales, and, Orowan had invented a way of, a kind of, quick way, elementary way of calculating the power needed. And in order to test his theory, he built a miniature rolling mill with a little, little sort of, needle, not a needle, a stud built into it. And as the stud went across the metal, it would measure the pressure. And, this was what the, this was this heavy apparatus down in the Cavendish, the basement of the Cavendish. That’s what it was about.

[16:46]

But I was set to work on this brittle fracture problem with the ships. And Orowan sent me to the library to look up magazines, they were magazines, not journals, sort of trade magazines, on various fractures that had occurred. And after a while I thought, well this isn’t the sort of science I, I thought, I thought physics was. And I said, ‘Why do you, why do you read these, these magazines which have anecdotal evidence of a particular fracture of a railway line somewhere?’ He said, in his very thick Hungarian accent, ‘It is a hobby.’ [laughs] I didn’t think it really was great as a, as a bit of science. But, he was a good scientist, was Orowan, and, he had good ideas. And one
of the things that I was introduced to at that time, and it became important later, was Nadai’s book on plasticity of metals, which was a mathematical theory of what happens when metals deform, which was, as you might say, parallel to the theory of elasticity, which was a highly developed mathematical theory. The theory of plasticity, which you need if you want to understand what happens, say, when you draw wire through a dye, or when you roll a sheet in a mill, or when you punch an indentation, a permanent indentation, as plasticity, into a metal, if you want to understand what’s going on there, you need the theory of plasticity, which was a rather, early stages of a developed mathematical theory. And there was only one book, and it was by this chap, Nadai. And, so Nadai’s book became very familiar. And incidentally, perhaps it’s relevant here, Nadai’s book had a little bit about much larger scale phenomena, like landslips. I hope I’m right in that. I think it’s in Nadai’s book. Anyway, the science of soil mechanics is somewhat similar to the science of, of plasticity of metals.

So, we went on working there, and, went on until the end of the war. We sent reports. It was the Ministry of Supply. We sent reports duly to Fort Halstead, which was the headquarters which was giving out the orders for what we did. The head of Fort Halstead was, or the particular part of it, was Professor Mott, Nevill Mott, whom I, was later, was indeed the Professor of Physics at Bristol, in peacetime. And, I remember going down to Fort Halstead and Mott said, ‘How’s the, well how’s the work going Nye?’ And I said, somewhat boldly, I said, ‘It’s at a standstill Sir.’ ‘Why is that?’ he said, alarmed. ‘Well,’ I said, ‘Orowan’s having to type the quarterly report.’ ‘Oh don’t bother about the quarterly report,’ he said, ‘get on with the work.’ So I went back to Orowan where he was sitting as usual at his typewriter. He had no secretaries, and, typing the report. And I said, ‘You don’t need to type the report. We don’t need to send one in.’ [laughs] He was delighted. So we had these quarterly reports, or we had had them.

And, then came, then came the end of the war in Germany, when things let up a little bit. And it wasn’t until I think, just before the end of the war with Japan when we finished our final report on this whole business, which you can find now published. I think it’s the first in my list of publications. It was a joint affair between Orowan, me and a fellow called Warren Cairns, who joined us later. So, actually what we did was
not the slightest use, although it did, it did become the background for a report that Orowan later wrote for *Reports on Progress in Physics*, which was a serious study of fracture in metals.

[21:55]

I went to a meeting towards the end of the war to discuss all this, and to my astonishment I found that there were groups all over the country who had been working on this same problem. But secrecy being what it was, we were not allowed to know that. That was the way things worked. And of course it was unpatriotic to ask, because you might be, sound as if you were a spy. So, so I never asked what else was going on in the country. I don’t suppose I would have been told.

[22:31]

But we had this meeting in the engineering lab at Cambridge on brittle fracture. Should I tell you what happened at it? Because I think it’s perhaps an interesting account of the way science works. There were a lot of different contributors. First was G I Taylor, Sir Geoffrey Taylor, a famous name in these contexts, and he had done some experiments with a big flywheel which had something jutting out of it which hit the metal with a great bang. And, he drew this tiny little, almost illegible diagram on the blackboard, and talked about, talked about it. And then came the metallurgists, and they said, ‘Well, the answer’s really quite simple. It’s the molybdenum in the steel. And if you take out the molybdenum it won’t be brittle. And then the ship’s architects made a contribution, and they said, ‘Well if you look you’ll see that the fractures are beginning at the hatch copings. So what you do is to make the hatch copings out of a higher grade of steel, which won’t crack.’ You see what was happening was that the cracks, which in an ordinary riveted ship stop after they’ve crossed a plate, or run into a rivet hole, n a welded ship there’s nothing to stop them, they just go on, and they can go on all round the ship, and crack the ship in half. So the ship’s architect said, ‘Stop the cracks at source, at the hatch copings.’ There were other contributors too who had other explanations. The metallurgists, the ship’s architects. Who else, who else would be involved? [pause] Well there were the shipbuilders of course, on the shipyards, and they said, ‘Well the trouble is, it’s American steel. British steel wouldn’t do that.’ The point is... Oh I know, the, the navigators said, ‘Well if you examine these ships that are fractured, you will find that the ships, they’ve all taken place in very cold weather, and so what you have to do is to send the ships on a more southerly course, not into the north of the Atlantic, and
then it’ll be warm enough that they won’t crack.’ And the point of the whole episode is of course that they were all right. Every explanation was correct, within the realm of the people who were doing the work. For example, Orowan’s explanation was, typically, ‘Well the, if the fracture strength is lower than three times the yield strength of the material, of course it will crack. To be more precise, 2.8 times.’

[25:49]
*What’s the yield strength and fracture strength in...?*

Well, if you take a piece of, a rod of copper, say, and you put it in a testing machine and stretch it until it breaks, first of all it’s elastic, which means that if you let go it’ll go back again, and then, after a certain point it becomes plastic, if you let go it won’t go all the way back again. And when that happens, that’s called the yield point, it’s yielded. It’s what happens if you take a piece of metal and bend it and it stays bent. Then it’s yielded. So the yield point is the stress at which that happens. And with certain materials, like these, if you go on after that, and go on pulling, eventually it fractures, and that’s the cleavage strength or the fracture strength. So the yield stress is below the fracture strength, usually. But, if you, in certain geometries, if you confine the metal so that it can’t yield, for example if you stop it contracting, there are various ways of doing this, then it will fracture before it yields. And that was the basis of Orowan’s hypothesis.

*Yes, because it would be better if it yielded and merely bent, rather than actually cracked in a ship.*

[27:13]
Indeed, for ships, yes. Now, this is getting perhaps slightly technical, but, it’s an example of Orowan’s very clever thinking, that, one of the situations you can envisage is a block of metal with a punch. Imagine a cylindrical punch being pushed into the top face of this block of metal. And at first of course it’ll go in elastically, and then eventually it will punch a hole in it. And one of the standard problems in plasticity was to calculate this theoretically, what’s happening when the punch is overcoming the plastic strength of the metal. That was in Nadai’s book. What Orowan said was, well, suppose we take this problem, which is well understood, and
we reverse all the stresses, and instead of having compression, we have tension. And instead of having a punch going into a single block, we have two blocks with a, joined by a thin piece between them, so we’ve got a deep crack all around the, the composite specimen, and we, instead of pushing, we pull. It’s exactly the same problem with all the stresses reversed. And, so he took this and applied it to the fracture strength, or, in that case the effect of the notch, the crack, on the metal. I mean that was a piece of creative thinking, which was typical of him.

What was his background? You said that he was...

Hungarian.

But, what had been his experience of the war before coming to Cambridge?

He came to England in about 1936, and, was one of the pioneers of metal physics as it was called. And he was one of the three people who jointly, and more or less simultaneously, dreamed up the idea of the dislocation in metals. The idea of a dislocation in a crystal structure was a, a theoretical idea. I could explain what it is to you. But it’s absolutely central in all material science now, and it was dreamed up as a theoretical idea simultaneously by G I Taylor, whom I mentioned before, Orowan, and Polanyi, all of them very clever people.

Could you, bearing in mind the, the level of understanding of me and of anyone else listening to the recording, could you give us a sense of the theory of dislocation, given that it seems to be central to this material, and I know that it’s probably then going to be central to ice as a material later.

Yes. Well, a crystal, and metals are made up of crystals, a crystal is a repeating structure. It’s, it’s a geometrical arrangement of atoms arranged in, in planes, intersecting planes, as we say, a lattice. And, if you imagine one of these planes suddenly ending in an edge, you’ve got something wrong, and through the middle of the crystal there’s running a line which is the edge of that missing layer. That is a dislocation. Now it’s just a defect, there’s something wrong with the perfect crystal.
Now the reason for its importance is, that if you now try to deform this piece of metal, this crystal, it doesn’t have to be a metal, then, what will happen is that this defect moves through the crystal, and as it moves through the crystal so the crystal is yielding. It’s rather like, if you take a carpet and you try and pull it across the floor, it’s going to take a lot of pulling. So what you do is to make a ruck in the carpet and then you move the ruck across the carpet until it reaches the other end, and then you’ve moved the carpet. Well that’s what metals do. They don’t go all at once, they do, they go by moving the ruck, the dislocation along the plane. And that was the essence of understanding plastic deformation of metals.

[32:16]

Thank you. And, why had he come to Britain in 1936?

He came as a refugee I think. He was, he was, I suppose, I don’t know but I suppose he was Jewish by birth, but, I don’t know, and I, I mean non-practising certainly. But...

Did he talk about his, that move?

He very seldom... No. I went... No, he had lots of relatives of course on the Continent, and I went to a conference with him in Paris where he was greeted by his long-lost relatives on the platform of Gare Saint-Lazare in Paris. And, he never talked about really that background. He talked about studying engineering in Berlin. He was as much an engineer as a physicist. He taught me to do, for example, engineering drawings with old-fashioned pens. Nowadays you do it on a computer of course, but in those days you had to draw out all the lines in Indian ink and photograph them and... Things were pretty primitive. You had to, if you sent a paper to a journal, you had to draw your diagrams on Bristol board, it was always Bristol board, with ink pens. And we weren’t set up for any kind of photographing, so we had to then set up a camera and take a picture of what we had drawn, and then reproduce from the negative the necessary number of copies. It was all pretty, pretty primitive.

Is that how your diagrams in early papers on ice floe in the early Fifties, how those were made, or were they...?
Oh yes, I drew them myself.

*On... And what is Bristol board?*

Bristol board was stuff made in Bristol, down at the board mills, which had a, a shiny side and a, and a matt side, and you drew on the shiny side. It was a, it was a particularly fine sort of cardboard. And Bristol was famous for making it, they’re down at, the board mills down at St Anne’s, where, produced this Bristol board.

[34:35]

*Is there anything about the materials used to draw these diagrams that influenced the kinds of things that could or couldn’t be shown by the diagrams, and therefore affected the kind of aesthetic of the diagram itself? So, is the look of these diagrams as they ended up being in the papers partly an outcome of the kind of technology you used to draw them?*

Well, it was difficult to draw them. For example, suppose you had a square with rounded corners. You learnt to draw the corners first, the little quadrants first, because then it was easy to line up the lines with the ends of the quadrants. Not the other way round, which would be very difficult. And when you smudged a line, as you inevitably did sometimes, you had to take a razor blade, a Gillette razor blade, break it in your fingers, so that you... it sounds very dangerous, but you did, and, get thereby a little scraper, pointed scraper, and then very carefully shave the edge of the line to make it so that it was not jagged. You always had to clean up your drawing after you’ve finished it in that way. It was, it sounds very primitive, and I remember thinking at the time, when we had to fill in all the equations by hand, in all the necessary copies, which was a very tedious business, I remember thinking how marvellous it would be to have a machine which actually took one fair copy and then reproduced it. My thesis at Cambridge had to be produced in four copies, and, I remember some of the equations, writing, they were long things and I had to write them out four times. It took a long time to do that sort of thing, you had to allow for that.
Mm.

[36:35]
As for the appearance of the drawings, I don’t think it made much difference. I mean you had these pins with adjustable widths, and you turned a little screw thing to adjust the width, and you got the line, the width you wanted, and then you filled the pen with a dropper from the Indian ink. And, of course there was the meniscus at the end, and you had to break through the meniscus to make the thing draw, and the trick was to do it, lay it down slightly sideways and then bring it upright. And that breaks the meniscus and makes the line. There were little tricks like that.

Otherwise you might end up with a sort of, spot, if you...

Well the ink wouldn’t come out.

Ah. OK. I see. Mm. And how did you add the sort of, labels and figures and little, signs to, on lines?

How did you do it? If it was going into a, a journal, you did those in pencil, and then the art, what’s he called, not art editor, but the artist at the journal, would do his characteristic lettering for the journal. If you were doing something which was, say, for a, an open day at the lab, you would use, you would use a stencil, and, out would come these rather crude letters with no seraphs, because you can’t make seraphs with a stencil. That’s why the letters look a bit crude from a stencil.

Thank you. And...

Oh and later, later you went down to the shop and bought transparent sheets with lots of a’s on, and lots of b’s, and lots of c’s. And you, you rubbed the sheet with a blunt pencil or something and you transferred the letter on to the diagram, and then you got beautiful letters. But you ran out of 0’s, zero, you ran out of zeros, because, you were always putting zero point something on the graphs, and then you had to trot down to the shop and get some more zeros.
Where was the shop, do you remember?

In Park Row.

[38:55]

Thank you. You also had other individuals you said in the basement laboratory, Orowan’s basement laboratory. Did they talk to you about their experience of the war and how they came to be working on, you know, metal...

No.

No?

No, not at all.

Mm.

I mean, you didn’t, because of the secrecy. You didn’t ask about other people’s wars, because they weren’t supposed to talk about them.

Yes.

I mean if they said, ‘Well I, I was evacuated from Dunkirk’ or something like that, they might let slip some secret of how it worked. And, it was all, it was all considered thoroughly unpatriotic to be nosy about what other people’s wars were like.

I see. And, did they, they or Orowan or, or yourself, talk about the use of science in the war effort, in other words, did you talk about the value of what you were doing?

Well, I mean, I did have thoughts about being a conscientious objector, but I thought that, Hitler was so loathsome that really that didn’t come into it. But I think in the First World War I might have thought differently. Although the penalties for being a conscientious objector were, in that war were very serious; much less so in the Second. So I was, I was not worried about the, the moral implications of what I was
doing. I might have been if I had been working on poison gases or something lethal. I mean what was, what was what they called head of theoretical armaments, can you imagine, at Halstead, Fort Halstead, and, one of the things was, what happens when a tank is hit by a shell? You may not know, but what happens is that on the inside of the armour plating a piece breaks off and shoots across the tank and kills you. It doesn’t make a hole in it, but it, it makes a shockwave which then detaches a piece on the other side of the armour plating. Those sort of problems.

[41:16]

So what was he working on in relation to that for example?

Mott?

Yes. Preventing the bit coming off and...

Oh I don’t know what... I, I really don’t know, I wasn’t privy to that sort of research. I mean, G I Taylor was firing paraffin wax cylinders at metal, irresistible metal sheets, and watching the shockwaves going up the paraffin wax cylinders, and thereby arrived at the concept of what’s called a plastic wave, as distinct from an elastic wave. An elastic wave, as you will know, goes on and on and on. A plastic wave goes a certain distance and stops. But that’s the sort of physics you need to understand what happens in violent explosions.

And G I Taylor was working... Where was he working on this?

G I Taylor? He was in Cambridge.

So in the same laboratory?

No, G I Taylor was very much a, different. He, I don’t know, he was on his own. He worked in a different part of the Cavendish. He was, although he had invented dislocations, he... I’m trying to think what he was working on. If I, if I knew. But, he had this big wheel which hit things at great speed. I don’t think I know what he was working on specifically during the war.
There was a lady in the engineering lab where we did a lot of our testing equipment was, big machines, Mrs Tipper, who was a metallurgist, and her refrain as she passed our experiments was always, ‘It won’t work, it won’t work.’ And we got used to that, [laughs] but then didn’t take any notice. That was, that was what she was doing. But what G I Taylor was doing, I don’t know.

How many women were there in this laboratory at that time?

How many women? Well, Mrs Tipper was in the engineering laboratory, and was probably on her own as a woman. [pause] None when I first went there. Later, Audrey Douglas. They tended to be crystallographers. You know, crystallographers, crystallography for some reason attracted women. And... May have been the idea of crystals and jewellery and that sort of thing, but, precious stones. Anyway, they, many of them had done their, you might say, apprenticeship under J D Bernal, in London, Birkbeck College, London. He was a great influence in early crystallography, and, that’s why so many of them were communists, or anyway, very left-wing, because they had come under the spell of J D Bernal. And, people like Helen McGaw, Dorothy Crowfoot, who became, she was Chancellor of Bristol University, what’s her name? Oh. You must know her name. Dorothy, she was a crystallographer. Well known. Dorothy, Hodgkin.

Ah. Mm.

Dorothy Hodgkin. Married a, a scientist.

Were these all, these weren’t all at the laboratory at the same time as you, these women that you mention?

Well, you see there was, the war was on when I started in the lab, but then I kept on after the war. And after the war things were very different, and we moved upstairs from the basement and had a proper lab on the first floor of the Cavendish. And Orowan sat in the room later occupied by Perutz, and had lots of visitors. And I used
to sit on the other side of the desk from him. And he had masses of visitors, because he was a well-known figure in the metals, physics world, and steel people, steel, I was going to say magnates, but, not steel magnates, but, scientists, came to see him and consult him, and, he was generally a great focus. And I listened with my ears flapping to these conversations, and, that was very educational. I can explain perhaps later how that led to my introduction to glaciology.

[46:25]

Mm. Yes. Yes, I think, I'm going to have to pick up on some of these things later, but I think before we get too far ahead of ourselves, I ought to ask you about the teaching of science at Cambridge when you went to do your degree.

Oh yes.

Which we haven’t covered. And, you were being taught physics and mathematics?

I had a certain amount of choice, and I chose, rather than do the natural sciences Tripos, to read mathematics for my first year, and then, and then do physics in my second year. So I would then switch from the mathematics faculty I guess it was, to the science faculty. Now in the science faculty you had to do, if you went straight into the science faculty you had to do three sciences. And typically they would be, physics, chemistry and mathematics, which was fine by me except for the chemistry, and I didn’t want to do the chemistry but I thought I’d like to do a bit more mathematics. So I did mathematics in the first year in the maths faculty, and then switched faculties and went into the science faculty and did physics.

[47:48]

I skipped the first part of the mathematics Tripos because, actually the syllabus was, had already been covered in the scholarship exam. So I, I had a rather luxurious time. I went to the second year mathematics lectures without having to take the second year exam. I took then the, the first year exam which was a doddle, and, listened with interest to the second year maths lectures without having the responsibility of having to pass an exam on them. And I found some of them pretty, pretty above my head. I mean there was, there was a lecturer called Steeds, who was an eminent mathematician, but he started off with number theory, and he started off at the top
left-hand corner of the blackboard, writing that, \( n \) \( b \) number. And he went on from there. And after, I remember after several months, he reached the point of proving that one plus one equals two. I thought, well, so what, I thought I knew that. [laughs] I didn’t see that any proof was needed. But we had gone through all kinds of hoops to get there, which I simply didn’t comprehend. We drew Dedekind sections and, and, all kinds... and, existence theorems and... It was not my cup of tea at all.

[49:25]
The more down-to-earth mathematics I did find interesting. I went to Eddington’s lectures for example. Now I had been reading his science books, his popular science books, and I was full of the romance of the expanding universe, and, new pathways in science and all that stuff. And I thought, this is going to be tremendous. Eddington lectured on spherical geometry, which is about the dullest bit of formal mathematics you could get, but you need it for astronomy, for finding the positions of the stars, on a sphere you see rather than on a plane. Spherical trigonometry it’s called, it’s all about angles on a sphere. Great circles and little circles, small circles and things like that. Eddington, on the other hand, was sometimes called to give evening lectures to students, and there he really was, he really did talk about relativity and the expanding universe, and it was great fun. But he was a, he was a, a reasonably fluent speaker but nothing like as good as his books. And he was terrible when it came to answering questions, because, one student would ask him a question and he would think for a long time and then look up and say, ‘Yes I think so.’ Or something like that. I remember one student was, had the temerity to ask him, ‘Professor Eddington, could you tell me what a hyper sphere is?’ And he looked at him with incomprehension, as if anybody couldn’t know what a hyper sphere was. All he had to do was to say, ‘Well it’s such and such,’ and, and explain that it was an abstract concept deriving from a 3-sphere. But he, he didn’t do that. So, he was pretty miserable actually as a, as a answerer of questions. But he did give, he did give good evening lectures to students, which did inspire us.

[51:44]

*How did he appear and, what did he look like and...?*

Eddington?
Yes.

Well I’ve just read the recent book about him, which explains him in terms of his Quakerism, entirely in terms of his Quakerism. I had no idea at the time that he was a Quaker. It didn’t appear at all in his lectures or relations that I knew about. So he had this whole side of him which was driving him, which we knew nothing about.

Mm.

He was just another don, who wrote very good semi-popular books. I’ve just been looking at some of them, and they really are splendidly written.

And how did... You said that his teaching on spherical geometry was a bit of a contrast to these books.

Yes.

How, could you remember how those lectures were presented?

They were presented very formally, because they consisted of a series of formulae which need to be derived from first principles, which he did. And when you got the formula, what do you do with it? There were no sort of practical demonstrations or anything of that kind to catch your interest. It’s just a series of symbols. Later I learnt that such formulae are very useful in crystallography, where you deal with directions, and, the best introduction that I know is by F C Phillips in his book on crystallography, and that really gives you mnemonics for knowing which of these rather complicated formulae is relevant in a particular situation. But there was nothing helpful like that in Eddington’s lectures, they were just formulae.

And do you remember what in particular motivated or inspired you about things that he would say in his evening lectures which were of a different kind, the kinds of things then that seemed exciting and inspirational?

No, I don’t actually, no, there was nothing that wasn’t already in his books.
Yes, I see.

[53:52]
But you were asking about the teaching, and, I was called upon, as soon as I went back to Cambridge, up, as soon as I went to Cambridge with a job, I was called on to demonstrate in the practical classes. And the demonstrations were, well very old-fashioned. Cavendish was a very old-fashioned place, even then, and, Mr Bedford, who was in charge of the Part I laboratory, wore a winged collar, and, I suppose an ordinary tie, but, it was that kind of atmosphere. [laughs] And, the, the great figure became Dr Searle, Dr F C Searle. Have you ever heard that name?

Yes, but I'm not quite sure where.

He, he was a great teacher. And, if you go into a, into, or went into a school laboratory at that time, or any laboratory, you would find apparatus built by Pye in Cambridge, W G Pye, which would originally have been designed by Searle. Retort stands and clips and, three-legged things. He was very, he was very keen on kinematic, what we now call kinematic design, although he wouldn’t have known what you meant by it. But he knew the stools, you should sit on three legs, and not on four, if it wasn’t going to rock. That kind of design went right through all he did. And he, he ran an optics lab, and in due course, this was somewhat later, I was called on to help Searle run his optics course. Now, the reason he was there was that he had already retired at the age of sixty-five, and had gone on, he was probably in his seventies, and, he had been recalled because of the war, to kind of fill in. And, Searle then waded in with his, his optics class, where there were scores of experiments with lenses and things. And I was his helper. He was a very gruff figure. He, I remember him once saying to me, ‘Who is head of the Cavendish now?’ He was, he was a bearded old man. And I said, ‘Oh, Sir Lawrence Bragg Sir.’ ‘Ah yes,’ he said, ‘young Bragg, young Bragg, yes, pupil of mine. Don’t suppose he could find the focal length of a lens.’ [laughs]

[56:41]
Now, Searle’s lab consisted of about twenty different ways of finding the focal length of a lens. It was very old-fashioned, prisms and lenses. Nothing of what we used to
call modern optics there. But it was a great training in, you might say, classical ray optics. He did a few things too with waves, and, he designed an experiment which is most surprising really. Two, you take two glass plates, and you... transparent of course, and you put them at a slight angle to one another, making what’s called a wedge. And you then have a, a sodium lamp, which is a, a Bunsen burner with a lot of salt thrown into the flame, so it throws out a yellow light. And, then over here, a long way away from the plates, you put a ground glass screen, and if you put it in exactly the right position, you see a series of stripes on it, which are called interference fringes. Now most people would be amazed that you can find interference fringes like that as it were localised in space, from such a primitive, simple apparatus. Just a lamp, two plates of glass, and then far from them a frosted glass screen. Now he, Searle wrote a paper on this, and in typical style got me to read the proofs of it. So I had to understand it. He didn’t give me his original manuscript to check. I had to check all the formulae myself. And I said, ‘Looks all right Sir,’ you know. [laughs] And, much later I got students at Bristol to do this experiment, which they did, and then still later my colleague John Hannay, close colleague, I told him about it and he got interested in it, and produced what you might call a modern theory, which was different from Searle’s, in an interesting way. And we have recently been discussing it, and Searle’s paper is almost unreadable by modern standards. John Hannay’s stuff is always absolutely translucent, it’s beautiful, and, really sees through it all, sees the essence of it, which Searle’s stuff didn’t. But, that’s an interesting series of developments I, I think. I first saw this experiment at Cambridge, had to read the proofs, and then told John Hannay, he takes the bit between his teeth, does it properly, and then we’d discuss it together.

_Mhm._

Yes.

[59:30]

_Ando said that Searle’s lab contained sort of, old-fashioned optics..._

Yes.
Nothing of what you would have called modern optics then...

Yes.

What were the modern optics that were absent from this?

Perhaps that’s a little unfair, because, really, modern optics as it was then called was, is wave optics. And, it was... There was a book on it by Wood, an American, which we used as a textbook, and it was, it was, it was largely about waves. And it was of developments which had taken place since Searle learnt his optics. Searle was a tremendous figure in very late Victorian times. He was the one who invented the way of measuring the standard ohm, you know the NPL is interested in standards, and you have to have standards for every unit, standards of voltage, standards of current, standards of resistance, which was the ohm. And Searle was responsible for the apparatus which measured the ohm as accurately as you could measure it. He was very well known and a great figure for that in his time. But dragging him out of retirement was, really dragging out something from the past. I think, it may.....

[End of Track 2]
OK, I wonder if you could continue to talk about the teaching staff at the time of your degree at Cambridge in physics and maths.

Yes. Well as you must know, the Cambridge system is, you have a supervisor in your own college, if possible, and then you go to lectures given by other lecturers around the university. My supervisor was a brilliant mathematician called Ingham, and, I found him very, a very good teacher, and enjoyed my mathematics supervision with him. He... I told this story at a funeral the other day, but, he had a pen, and he wrote everything by hand with this dip pen, and when presented with a problem, the first thing he would do would be to dip the pen in the ink, black ink always, with great confidence. And I think I learnt as much about the confidence with which he drove the pen into the inkwell as I did the mathematics, what it was about. [laughs] But he was a, he was a lovely man.

Then, as for lectures, the tendency was for the lecturers in the Cavendish to lecture on their own pet subjects. For example, Aston, who had invented the mass spectrograph, which we were talking about at lunch, he gave lectures on mass spec... mass... no, on isotopes, which he had discovered with his mass spectrograph. And, let’s see, Kemmer gave lectures on thermodynamics, because he was hot stuff on thermodynamics, which I never really got on to. Then, let’s see who else.

Well, I was leaving till last the most prominent and influential, which was Sir Lawrence Bragg, who gave lectures on optics, and he did demonstrations. And, one of the ways he did the demonstrations, he did as many as he could before the class, during the lectures, but at the end, he set up a whole lot of other demonstrations in one of the labs, we could just go round and look at things that needed a lot of setting up.

And I remember most of those demonstrations very vividly. I mean, there was one with a, with a, a grating, diffraction grating. Now if you have a diffraction grating, it produces spectra, and if it’s, say it’s a sodium light, you get first order, second order, third order spectrum. He, he had borrowed from, I think the Observatory, a grating for infrared spectroscopy, which has its lines much further apart. And... Or do I mean closer together? Anyway, never mind. The point is, that he used the infrared grating
with visible light, and the result was a series of spectra which were projected on the, around the room, the lecture theatre. And you could count them. And this grating was as they say, blazed, that’s to say, the lines, each made into a particular shape, this one was sort of jagged, sword tooth kind of shape. And one of the spectra, like the twelfth, was much much brighter than all the others, because that, the grating had been, as they say, blazed to pick out that particular order of the spectrum. Well that’s the kind of experiment, I’d never seen it done elsewhere, but that’s etched in my memory.

[04:15]

As opposed to the... Leaving aside the blazing, is it just a... What is the grate, just a piece of...

Grating?

...metal with slits in?

Well, it’s always drawn that way. Actually it’s, this was a reflection grating, and was made of a, a shiny metal called speculum I think, and on it had been imprinted a repeating pattern of lines. And each line was of the same profile as you might say, and the profile was chosen to bring out a particular order of the spectrum. Now that was a pretty impressive demonstration. He showed something called Talbot’s bands, which is a rather esoteric thing you do with a spectrometer, for which the explanation is really quite complicated. He showed us Talbot’s bands, which we read about in the books. He, he was a great one for vivid, everyday explanations of things. He would say it’s like... We were talking about dislocations. He says, ‘It’s like, when I do up my wife’s dress, do up the buttons at the back, if I miss one out, I have to go all the way to the top before it comes right.’ Well that was like the ruck in the carpet. He loved homely illustrations and analogies. That was, he... And I later learnt actually that he had learnt that sort of lecturing from C T R Wilson, of the Wilson cloud chamber, who was a whole generation earlier. So there was a kind of succession there. And when I gave my optics lectures at Bristol, I inherited a certain tradition of that kind.
Are you able to... These are very interesting. Are you able to remember other demonstrations and other homely metaphors that he used to illustrate?

Gosh. [pause] Well I remember when we were discussing the yielding of metals, which was another of his interests. He said, ‘Does it all happen just suddenly, or, is it like the Civil Service, where everybody’s waiting for everybody else?’ [laughs] That was his sort of approach.

[06:40]

And, to what extent did you know him as a, as a, come to know him as person, sort of individual?

Well really quite well. Because, I was doing the research I spoke about with Orowan, and, that hadn’t really quite come to an end. I was still writing my thesis. But Bragg had invented this so-called bubble model of a metal. Should I explain what that is?

Yes please.

Well, Bragg was a great gardener, and he used to mow his lawn with a motor mower, for which he had to mix the oil. I think, I’ve forgotten what you had to mix, but, you need to mix the oil. And, as he mixed the oil, he noticed little bubbles coming to the surface, and collecting together in a little cluster on the surface. And they were all the same size. So they were collecting together to make a, a regular structure, like a honeycomb. So when he came in to the lab on the Monday morning, he says to his assistant, Crowe, the reliable Crowe, ‘Just fit me up a tube to blow bubbles, and I want you to blow the bubbles, and we’ll come and see it.’ Well, Crowe was having no success whatever. He had got a glass tube with a nozzle on the end which was blowing out small bubbles, and they were all coming up to the surface, and they were all different sizes. And my supervisor, Orowan, happened to be passing along the corridor at that time and looked in, said, ‘What are you doing Crowe?’ ‘Well, the prof tells me I’ve got to blow bubbles.’ ‘Well,’ said Orowan, ‘why don’t you put a bend in the tube?’ So the tube came down, and instead of pointing downwards it was crooked so it was pointing, the end of it was pointing upwards. Because what was happening was, the bubbles were interfering with one another as they came out of the end. And
he had put a little crook in the tube and out came the bubbles and they were all the same size. And they gathered together on the surface in perfectly regular, as I said, honeycomb-like patterns. And then, every now and again there’d be something that went wrong, and you would get one like this, and then you would get the next one in a different orientation. And these were the grains of a metal, different crystallites as they said, of a metal. So it was a vivid model demonstration of the structure of a metal. And when you squeezed it, squeezed one of these clusters, lo and behold, you got dislocations. They were little, little sort of black things, little faults in the way the bubbles were packing together, which I described to you, and as you push harder, so the dislocation shot across. And that was the way this whole bubble raft deformed.

[10:40]

Is there any, is there a reason why a collection of bubbles on the surface of water should deform in the same way as metal? I can see that it was a, a nice way for him to be able to demonstrate how metal deformed in a lecture...

Yes.

...because, it’s easier to make some bubbles than it is to show a group of students the inside of a metal. But, is there a reason why that works, why bubbles on the surface of the water are metal?

Yes. Indeed there is. Well there has to be. Of course it’s two-dimensional, whereas a real crystal is three-dimensional. But if you just forget about that for the moment. The forces between the bubbles are very like the forces between the atoms of a crystal. You’ve got, you’ve got an attractive force, which is due to surface tension, which makes them tend to cluster together, and you’ve got a repulsive force, because they, when they come together you get a little flat between two bubbles which acts as a repulsion. And my successor, Loma, what’s his Christmas name? Gosh. I suppose it was, I suppose it was Dick. He was W. Anyway, Loma, W... We always talked to each other by surnames, you know, in those days. And so Loma took on the job of calculating what was the best size of bubble to use. Because, very small bubbles are very hard, they’re like ball bearings, and they won’t deform. And, larger bubbles are much more squishy, deform easily. And if you choose the bubble diameter to be
about 1.1 millimetres, or was it .9 millimetres, you just about match the atomic forces between copper atoms, which is what Loma did, and said, about a millimetre is the size you’ve got to go for to simulate what’s really happening in a metal. And it was extraordinarily successful. One, we made a movie out of it and it was, became very well known, and, in the end Bragg and I wrote a paper on it, I say Bragg and I, he wrote the paper, I did, I took the photographs, and, kind of assembled them. He, he wrote the paper actually one evening, and he said, ‘I always like to get the paper written in one go, because, otherwise, when you get to the end you don’t know how you began. [laughs] Well I mean, who, who would write a paper in one evening now? Incredible. He wrote it in tiny handwriting, and, came in with this manuscript. And, it was published after due amendments of course, with the photographs all interleaved with it, it was published in the *Proceedings of the Royal Society*, became quite a celebrated paper, and, Feynman in his book, volume one of his... volume one of his, the book’s just called *Physics* I think they are. Anyway, there’s a series of very well used textbooks by Feynman, used all over America and in England too. Feynman’s volume... is it volume one or volume two? I can look it up if you like, has this whole paper verbatim at the end. Which is something that I’ve never seen in any other textbook.

*What’s the title of that paper?*

The title is, ‘A Dynamical Model of a Crystal Structure’. Actually Paul, I’ve got a, I ran off yesterday a, a publication list of a, and some dates in case I need to refer to it. Now of course I can’t find it. But it doesn’t really matter, does it?

*No.*

[13:47]

OK. So, so this was, this was a great education to me, because, here I was in Orowan’s group, Orowan was my official supervisor, and here was Bragg, who had been doing these experiments kind of, out of hours, during the war, when he was supposed to be doing something else, and, was dying after the war to get going on some real physics. And one of the things he wanted to do was to develop this bubble model. So far, everything had been done in a little dish about that size, and, I’m
pointing to a dish about, ten centimetres in diameter. And, he had done, he had given us, I remember, demonstrations in the evening to student, student societies, with this thing, it was quite impressive, projected on to a screen. I remember being impressed with the, with the brassware that Crowe had made to hold various bits and pieces, because that could be done for the Cavendish professor, but not for your ordinary research student. So, Bragg wanted this done, and, he said to Orowan, ‘Have you got anybody who could help me?’ So Orowan said, ‘Well why don’t you have this Nye chap?’ So I found myself going up and working instead in the Cavendish professor’s research lab. Now when the... this was the new wing of the Cavendish at the time, not the one out in Madingley Road, but, built behind the old Cavendish, it’s called the Austin Wing. And, up on the third floor were the offices where professors had their desks, and Bragg ran the show from his office up there with his secretary in an adjoining room. And across the corridor from his room was the Cavendish professors’ research room, which the architect obviously thought was needed. It was a large, really large room, and here was I, the sole occupant of this very special room. And Crowe had already made a larger dish with a blackened bottom, and, with little feet which you could adjust in height to make everything level and so forth. And I was given the job of, well developing this thing. Which I accepted with great pleasure. There was a little well where you put the, the bubble maker in the little well which was deeper than the rest, and then the bubbles spread out over this much shallower layer of soap solution.

[16:29]

There weren’t... You see you couldn’t buy detergents then, across the counter, or indeed anywhere, they hadn’t been invented really. So Bragg, and this is typical, telephones the chief technician at the Royal Institution, and says, ‘How do you make bubbles?’ Well the Royal Institution has a long tradition of bubbles, ever since Boys worked there. C V Boys was the great man on soap films. Fascinating subject. And, so Bragg rings up his, rings up his technician and says, ‘How do we make a soap solution, make bubbles in?’ And out comes a recipe, a very elaborate recipe, involving very pure oleic acid, and, I’m no chemist but I followed the recipe exactly, and, that was the soap solution we used. Nowadays, if they ever do it again, or they do do it again, they use just detergent out of a bottle, a kitchen, kitchen detergent, works fine. But, then we made our own soap solution. And I did experiments, took lots of photographs, and perhaps interestingly, it’s not all that easy to do that, because,
the bubbles are very small, and you have these events taking place as you deform it, you have these dislocations dashing around, and sometimes two dislocations meet each other and do an interesting reaction. And I remember saying to Mick Loma, who was my, who was later research student there, saying, ‘There must be some sort of algebra to tell us what happens to these dislocations when they meet,’ you know, a plus and a minus becoming a, a nothing, or a plus and a plus become a, something else, and dashing off in another direction. I didn’t know what it was. But, some years later Charles Frank here invented the idea of the Burgers vector, which classifies the dislocation. It characterises what sort of a dislocation you have. And the idea just didn’t exist. But, if I had been smart enough I would have, I would have managed to do that, but I, I wasn’t used to thinking about taking circuits around things as Charles Frank learnt about. Because he taught electricity where you take circuits around things.

[18:55]
Anyway. I kick myself when I think of all the things I missed, but one perhaps interesting thing is that I took a photograph to Bragg which had some interesting things in, and in the corners of it was a sort of white mess, which I had ignored. And he pointed to it, he said, ‘What’s that?’ And I said, ‘Well that’s where the bubbles piled up on top of themselves. I didn’t want to photograph that bit.’ And then we got, we did that systematically, and we got a three-dimensional arrangement of bubbles, one layer on top of another. And, when you pack these bubbles together three-dimensionally, it’s rather like putting oranges on a stall, there are actually two different ways of doing it systematically. One’s called cubic close packing, and the other’s called hexagonal close packing. It all depends whether the layers, which are of three kinds, A B C, are packed A B C A B C or A B A B A B. Two different ways of stacking them together. And metals, according to whether they're hexagonal or cubic close packed, adopt one or other of these packing styles. So it was all there in the bubble model.

[20:20]
And then even more, or very frustrating to Bragg, I guess, he, he had the idea of what’s called a partial dislocation, which is actually half a dislocation, and it involves what are called stacking faults. When I said A B A B A B, suppose you make a mistake and say, A B A B A B and then A B C A B A B, which you can do. That’s called a stacking fault. It’s an error in the stacking. And those really occur in metals,
and if you have stacking faults, then, the dislocations find them as a problem, and they become what’s called partial dislocations. And Bragg was very excited when he thought of partial dislocations. He had made a little model with ping-pong balls glued together to make a hexagonal, only had two such layers of ping-pong balls, he used to put them one on top of the other, and then shift them relative to one another and say, ‘Look, it can go that way or it can go that way. And they’re different aren’t they?’ And, he thought of partial dislocations. And then I had to tell him that, Bill Shockley, whom by then I knew and knew about, in America, had actually already published, done a paper on it in a conference. So, Bragg had his paper all ready for *Nature*, and I had to go in and tell him that actually, it had already been done. But, but, and here’s the, here’s the aftermath. I had taken pictures of these three-dimensional arrangements, and only years later did I realise that I had actually taken a photograph of a partial dislocation, and there it is, in our published paper. And I hadn’t spotted it, he hadn’t spotted it, but there it is. So it had appeared in the bubble model really before anybody knew what they were looking for.

[22:25]

*How did you photograph the bubbles in such a way that you could see the arrangement of the bubbles either two-dimensionally or three-dimensionally?*

Well they were, they were very tiny, they were about a millimetre diameter as I said. And, the conventional way of doing it would have been to use a, well a thing called a miniature camera, a Leica or Contax camera, and take 35mm photographs. But, the resolution of 35mm photographs was not all that good, so I, remembering my grandmother’s quarter-plate cameras, was quite at home with using glass quarter-plates. And I got a quarter-plate camera, and took quarter-plates, which I still have, of these bubble arrangements. And the great thing is then, having taken it, you can then not be worried by the bubbles popping or moving; you can then examine it with a microscope or a magnifying glass and really analyse what’s there. And that was the trick.

*What about the process of applying stress to the bubbles, but also photographing, how did you manage to do that?*
Well, Bragg had already, or Crowe, I don’t know which one of them invented it, but, had already got two callipers, the kind the engineers use for measuring things, and, he had joined the tips with springs, ordinary coil springs, so you could, by moving the callipers, opening or closing them, you could change the pitch of the springs. So he was able to open them up or close them down to match the pitch of the springs with the spacing of the bubbles. And he put two of these in, and it became, they became sort of, grips for the floating bubbles. And then he could move them with respect to one another, parallel to them, not pushing but, but shearing. And, shearing then produced dislocations. And interestingly, the dislocations never started at the edges, as you would expect perhaps. But I was quite pleased with myself with explaining why that happened. If you, if you shear something like that, the biggest stress is not at the edge on the face, because that’s a free, actually a free edge, and can’t, can’t support a shear stress. But, greater stress is just a little way inside the specimen, and it’s there that dislocations appear in pairs, one goes off that way and the other one goes a very short distance back to the edge.

*Why can’t the edges support a shear stress? I didn’t understand.*

Well there’s a theorem which you can derive directly from Newton’s laws of motion, that, if you got a shear stress on one face, or, say, a cube, then it’s equal to the shear stress on the face right-angles to it. Otherwise, the cube would just spin round. They’ve got to balance each other. So, there can’t be any shear stress on the outer face, because it’s a free face, and therefore there can’t be any shear stress on the plane at right-angles to it, which is what we had to prove. QED.

[25:55]

*Thank you. And, could you tell me about any sort of social relations with Bragg at this time? You’ve given us a sense of your working relationship, I can picture you going into his office and telling him about the paper.*

Yes.

*Presumably he came to look at what you were doing with the bubbles work?*
Oh yes. He would, he would come in, typically, late in the afternoon, thoroughly frustrated, and he said to me, ‘You know, I spend the day dictating letters to Miss,’ I’ve forgotten her name, ‘and they come out typed beautifully, and I sign them. But I haven’t done a day’s work,’ he said. [laughs] And he wanted, he desperately wanted to do something hands on. And, so he would come along and discuss these, what to do next.

And, what did he, as a man, what did he look like, and, dress like?

He was a fatherly man, with a moustache. Very much of his period. Large, largish. A sort of, favourite uncle type.

[27:06]
And, this is now the end of the war, and, I can see that during wartime the interest in the way that metals behave is of obvious importance. You’ve already mentioned two things, the cracking of ships and the way that a piece of metal on the inside of a tank gets knocked off when the projectile hits the outside. So... And we can really appreciate that during the wartime, the way metal behaves is sort of a, at this time was a key question. Could you explain what Bragg wanted to do after the war in terms of continuing work on metals, in a sort of less applied wartime sense, in terms of, just, science?

Well, I think, Bragg was always interested in metals, from a pure scientific point of view. And it was at that time quite a, a going subject as to the ductility of metals, and why some metals were stronger than others, that was a purely academic question. Bragg wasn’t interested in, in applications of it, especially to real industrial or even military problems. I remember him saying during the war, ‘Don’t tell me any secrets, don’t tell me any secrets, because then I have to remember what’s secret and what isn’t.’ [laughs] So he didn’t really know wartime work intentionally. But, as for why he, what his interests were, well, the deformation of metals was merely the beginning of material science, it became called, first of all it was, well, dislocations I guess. Just to go back to dislocations. They were theoretical concepts invented by those three people I mentioned, Taylor, Orowan and Polanyi, in 1936. And during the early Forties it was quite reasonable for people working on metals to say to each other, ‘Do
you believe in dislocations?’  They were totally theoretical things.  And so, Bragg and I were very interested to see, they just were there in the bubble model, it was the first demonstration of the real existence of these things, but not in real metals, only in these artificial bubbles.  Then, not long after that Peter Hirsch with the electron microscope actually saw the shadows you might say, the traces of these things, in real metals.  And from then on there’s no question about whether the dislocations were purely phantoms, theoretical things, but they were real, they were real things.  And they explained the whole, a whole raft of problems in material science.  So it wasn’t a, it wasn’t driven by industrial needs, it was driven by curiosity, very largely.

And so, for instance, the paper that you published with Bragg which contained the photographs of the bubbles, what was the response to that from people who at that time didn’t believe in dislocations?  Do you remember what the academic response was to that paper?

Well I don’t think they were polarised really quite to that extent.  I’ve exaggerated that perhaps.  No, I think they thought it just really interesting.  And there was no rivalry there.

[30:46]
And, was there an even more social side to your relationship with Bragg, in terms of, you know, things done outside of labs and...?

Well, he, he was really an Edwardian, and at his big house in West Road they occasionally held dances in the evenings, and all the furniture was cleared away from the main drawing room and there were dances.  And he had daughters and, and they, they had these sort of, evening events.  And he did occasionally invite me to his house alone, and would show me with pride his various things he did.  For example, he loved drawing, and when the Biro, the first ballpoint pen, was invented, he loved it for drawing, because it didn’t smudge, and, like a pen and ink sketch normally would.  And he had shown me with some pride a frame he had made with cottons strung across it, horizontally and vertically, which he could hold up in front of a subject, typically a person’s face, and sketch the face.  He didn’t feel there was any, any stigma in using such an artificial device, in fact I think the, the Dutch interior painters
used even more graphic devices than that. But he, he loved sketching, and, he, as I say, he kindly invited me round to his house, and, I remember when I did my first calculation in glaciology, he invited me round to explain what I had done to him. Which...

*Perhaps you could...*

...was very nice of him.

[32:44]

*Well first of all, what sort of things was he drawing, with and without this cotton frame?*

He was drawing, mostly portraiture. But he could do, well, landscapes with houses and farmhouses and, country scenes. But he liked doing portraits.

*And did you meet his wife?*

Oh yes. Alice Bragg, she was, she was Mayor of Cambridge.

*And what did you sense of the relationship between them as a couple?*

Well, nothing very much, but, I’ll tell you, I was going on a train once from Cambridge to London, to attend a conference, a meeting in London, and Bragg was going to the same meeting. He was sitting opposite me in the compartment, and we were both doing the *Times* crossword puzzle. I was struggling with this puzzle, and I had done very little, and I looked across at him and I saw he hadn’t done much either. So I said to him, ‘It’s a bit difficult this morning isn’t it Sir?’ It was always Sir. And, he said, ‘Oh I don’t think so. I’ve done most of it.’ And I, I said, ‘But you haven’t... I can see it’s blank.’ He said, ‘Oh I always leave it blank. You see, I come down to breakfast and I do it in my head, so that when my wife comes down she can do it and fill it in.’ [laughs] You asked me about his relations with his wife. That’s all I can say.
Thank you. OK. And, since you mentioned it and it’s now in your mind with visits to his home, could you date and tell me about the visit where you explained your first calculation in glaciology and what that involved, that conversation involved? Or would you like to leave it till we move on?

Well I published it in 1951 I think. I could, I can verify that date. So it must have been before that. So I would think probably ’49, around that time. You see, after I had finished with the PhD research, which was, my PhD thesis consisted of some calculations I had done in theory of plasticity, and then, the main experiment that I did for my PhD, which was about the deformation of silver chloride, and the final chapter was on bubble model, just really reproducing the photographs and the explanation. So my thesis was pretty, a pretty bitty affair. I just called it ‘Plastic Deformation’, and bound them together with that word. And the middle bit, which was really the guts of it, was my PhD project, which was suggested by Orowan.

Could you, in that case, explain that key main experiment that you were doing on the, what was the material?

Silver chloride.

Silver chloride. So, perhaps we could start with where the experiments were done, if you could describe the room that you were working in.

Well this was on the first floor of the Cavendish. And, a firm called the Harshaw Chemical Company in America was producing rolled sheets of silver chloride for use as an infrared transparent window for various purposes. And, Orowan, ingenious as ever, took one of these sheets, which just looked vaguely opaque, and he heated it up in a furnace, knowing that silver chloride is a crystal and it would finish up as what we call a polycrystal, that is, made up of a lot of separate crystal grains. And, he thought, and he took one of these sheets in his fingers and he realised he could bend it around. It was plastic. Just like a metal. And it became called transparent metal. It meant that you could look inside as it... look inside a metal as it were, as most metals, all metals, are opaque. So, looking around for something to give to a new PhD
student, he said, ‘Have a look at this silver chloride.’ So I duly was given some of these sheets, and I duly put them in a furnace, and they came out, and, and I looked at them through what’s called a polarising microscope. It’s a standard tool of mineralogists. It’s a regular microscope with Polaroid, cross Polaroids in it.

[37:55]
Could you say what those are, what they look like and what they do?

What they do? Well, mineralogists use them to identify minerals, because, different transparent... I mean typically in mineralogy you take a rock, and you take a very thin slice of it, so thin that it’s transparent, and you put it under a microscope, and you look down it and you see the grains of metal, and the various, grains of crystal and the various inclusions. You then look at it between what we call crossed Nicols, that is, two sheets of Polaroid, as you have in sunglasses, which are oriented so that they’re, as we call it, say, crossed. The first one produces linearly polarised light, and the second one is crossed with it so that the light can’t pass through it. That’s a feature of polarised light. But if you now put a, this mineral slice between them, it lights up in the most beautiful colours, and as you rotate it between the two Polaroids as we call them, the colours change. And, there are certain orientations of it where it goes all totally black, other orientations where it goes mauve, or yellow, or green or whatever, and it’s a fairly fascinating business, and it’s the standard tool of mineralogists in identifying minerals. So that all, that technology existed. But what was new was to put a sheet of this silver chloride between them, and, what you saw was just a, you saw the crystal boundaries and that was about it. But I noticed, when you bent it, because the idea was to see about plastic deformation, when I bent it, suddenly all the grain boundaries lit up, they all became light. And, I had to figure out why that was. It wasn’t difficult. But, that was the first thing that I did with it. And then, then I stretched it to make it deform plastically, and of course, all these so-called dislocations, because we didn’t know whether to believe in them or not, crossed across the grains, and they left stresses, that is, internal stress inside the crystals.

[40:30]
Now, there’s a subject called photoelasticity, which was well developed at the time, where engineers put a block of transparent material between crossed Polaroids and they stress it, and typically an engineer makes a model of a, shall we say a hook or
some device, and looks at it, it’s made in Perspex, looks at it between crossed Nicols, and he sees where the bad places of stress are. They light up in a special way. And he can see where the stresses are, it’s a very well-established technique in engineering. So, the silver chloride showed what’s called photoelasticity, it showed this lighting up where the stresses were. But, the trick is that the, whereas the Perspex the engineers use is as we say isotropic, it’s the same in all directions, what I was looking at was a crystal, which is by no means the same in all directions. Admittedly the silver chloride crystals were cubic, and there was a sort of superstition among optics people that because they were cubic, they would also be isotropic. But I, who knew a little bit about tensors by that time, knew that that wasn’t so, and that there would be something different about cubic crystals from, say, Perspex. And I used this knowledge to interpret the patterns that I saw in the silver chloride. And I could see these stripes, the stripes running across, which I interpreted as collections of dislocations, all lined up in rows. And, so in a sense I was seeing, not dislocations but their effects, the stresses they caused. I wish I’d been able to see an individual dislocation by that means, but, it was too difficult. What I saw was the effect of a collection of dislocations.

*Why could you not see an individual one like you could with the...*

Well there was a paper by...

*...across the bubbles?*

There was a paper by Shockley I think which did say they could see a single dislocation. But you understand that this is on an atomic scale, these dislocations, and so any such stress pattern would be highly localised, and very hard to recognise.

*So you were seeing weaknesses generated in the material caused by a number of dislocations?*

Absolutely, by rows of them.

*Mm, I see.*
Yes.

Thank you. So that became the, the central part of your thesis.

Yes.

With the work on the bubbles the last part.

Correct.

[43:21]

Thank you. Do we have other people at Cambridge at this time that were significant? I note that you mentioned that you were interviewed at one point by C P Snow.

Yes.

Was he someone that you interacted with at...?

I never met him again.

Ah.

No. No, I mean obviously there was lots of work going on in the Cavendish. I was closest to the Crystallography Department, which was very active, and, Orowan’s group became bigger and bigger. I think he had about twelve research students at one time, and two of them had a special lathe, and they worked in a different room. And I was kind of curious about this, and, I said to Orowan, ‘What are they doing there?’ You weren’t really supposed to ask these sort of questions. And he said... I said, ‘What are they, what are they turning on the lathe?’ He said, [Hungarian accent] ‘It is an element.’ Well, it was uranium. And, these people were looking at the, at the strength of properties of uranium, which was highly secret at the time. But, that was going on in the Cavendish.
Why were they doing that, what would have...?

Because of the, they were making the atomic bomb in, at Los Alamos.

And, what was the link between that and measuring the stress properties of it on the lathe, what, what did it contribute to the bomb?

Well, you make a... I don’t know what they were doing, they were making cylinders on the lathe. But, typically you turn on a lathe, turn things into cylindrical shapes. They were probably making a, what we call a tensile specimen, or a compressive specimen, which you then put in a testing machine and you pull or push it. And you measure the strength. Which would be a crucial piece of knowledge for making the two hemispheres of a bomb. I conjecture this.

Who were those research students working on that in the separate room?

Who were they? They were, they weren’t literally research students, but they were sort of, war workers I guess. I’ve forgotten their names actually. I never, I didn’t know them personally very well.

And is that why they were in another room, because of the material?

Well it was slightly radioactive, yes.

Mm.

And the lathe would get radioactive. Yes.

[45:54]

Mm. Thank you. Could I ask you next to tell me about, I suppose extracurricular life at Cambridge at the time you were there? And perhaps we could start by you telling me where you lived, which we don’t know yet.
Well I went up to Cambridge of course right in the middle of the war, and, I went to King’s College, and normally freshmen were accommodated in college, but, for some reason I was accommodated in what was called ‘the hostel’, the King’s hostel, which was very near the college. It wasn’t actually literally in the college. So, I lived, it was just off St Edward’s Passage if you know, if you know Cambridge.

**OK. And did you share that accommodation?**

No, no it was, it was a modern room at that time with a fixed desk and a one-bar electric fire, and a separate bedroom.

**Do you remember what you put on the walls?**

I don’t think I put anything on the walls actually.

**Do you remember which, the possessions that you had there?**

Possessions? A toasting fork, which was essential, which you pushed between the bars of the electric fire and toasted things on the one bar, and which I touched inadvertently with the toasting fork and it melted in my hands. [laughs] That was the sort of essential possession. Lunch was a real difficulty, because, you shopped around hopelessly for something to eat, and I found a shop which sold Cornish pasties, and, existed on Cornish pasties for lunch. Of course we had dinner in college, and that was nice, although they were very restricted in the food. They had juggled hare, which was off the ration, so they relied a lot on, on juggled hare and that sort of thing. But, otherwise it was what they called poor man’s pie, which was a vegetable pie. And, you could… However, you could drink beer out of silver tankards, or, I suppose they were silver but, anyway, pewter, very nice tankards, embossed. And, all the trappings of luxury were around there, but none of the actual, you know, food ingredients. I remember I was sent a, a kind of packing list by the college, which clearly came from *Brideshead Revisited* times, and you had to bring up various items of equipment, like a soup tureen from home, because of course you were going to entertain in your rooms. But all that had just gone by the time I got there in the war. And my mother
was astute enough to know that I didn’t really need a soup tureen. So, it was, it was very much dominated by wartime conditions. What else did you ask about?

[49:10]
Thank you, that’s… Presumably, the members of, when you ate dinner in college, you had students, not all of them scientists, students from various disciplines?

Absolutely. Yes, I mean that was the great thing about a Cambridge college, you, you went in to hall, you wore your gown, short undergraduate gown, which we always thought practically speaking was there to protect you from the soup that the servants were bringing round in the big tureens. And, it was the tradition to sit on benches. They have chairs now, but you sat on benches along these long scrubbed tables, and you always sat on the first available seat on the bench. Otherwise it would be very antisocial. So you found yourself sitting next to who knows who. And as likely as not he would not be a scientist, he’d be a historian or a classicist or, whatever. So there was a tremendous mixing. And you got to know a lot of people that way.

[50:22]
This is obviously a long time before C P Snow’s writing about the two cultures, but, was there any sense of science and other subjects having different sorts of status within the university?

It wasn’t a question of status. I think it was a question of interest really. I don’t think either had a status. Because Cambridge after all was the home of, of natural philosophy, physics, and so, that certainly had great prestige. But then equally, it was famous for its, for the arts, English particularly, and, one of the marvellous features of Cambridge certainly at that time was, you could go to any lectures you liked. So, since I was at Cambridge quite a long time, I was able to go to many lectures outside the science faculty. There was for example Dadie Rylands, who lectured on English, he lectured on Shakespeare on the stage, and on the Victorian novelists, and I went to his lectures, which were really theatrical productions. Quite unlike scientific lectures. So I went to things like that. And, and I met of course the undergraduates who were studying other subjects, particularly a friend of mine, I made many friends I may say,
John Butler, who was a kind of genius. He became President of the Union, and had a room absolutely crammed with novels. He was, he had read everything. He was a genius. And, he was also something of a politician, and I remember him haranguing the Labour Club on Marxism and some undergraduate said, ‘And have you read *Das Kapital*?’ ‘Several times,’ says John. [laughs] And I asked him afterwards, I said, ‘John, have you really read *Das Kapital*?’ He said, ‘No of course not.’ [laughs]

[52:34]

_Could you tell me then about your other friends and also significant relationships at university?_

Well I, as I say, I had lots of friends. And, some of the friendships have lasted a long time. We have a, an annual reunion of some special friends who go to each other’s houses and have lunch once a year, and this is the first year for some ten, twenty years that it’s not happening actually, to my, to my dismay, because we’re all getting so old.

_Mm._

But, some of the friendships stuck. I was particularly fortunate because I was at Cambridge you see for, from 1941 until 1951, that was ten years, whereas most people were at Cambridge for, for three years if they were undergraduates, and at the most six years if they had then gone on to do research. So I was really especially favoured and was able to stay on and do all the right Cambridge things and get thoroughly entrenched there.

_Could you tell me then about sort of significant relationships? I’m probably thinking about girlfriends and this sort of thing._

Well, there were, in King’s, I mean, we had very good relationships with the dons there, we were all on Christian name terms, which was very unusual for those days. Provost Shepppard at King’s was a real eccentric. He would go round and he’d accost somebody in the Court and say, ‘I remember your name but I can’t remember your face.’ [laughs] And, we had very good relations with the dons at college, and there were, there were college societies where we met them. I was particularly involved
with something called the Ten Club, which, it was a play reading society, and, of course there were many aspiring actors, and some of them really became well-known names around, and they thoroughly enjoyed reading the plays, and I was given these little bitty parts. But, I think I was regarded as what they called a civilised scientist, which was a, you were asking me the relations between the different cultures, well that was the way scientists were regarded. You know, it was rather unusual to find a civilised scientist as they called them.

[55:17]

As for girlfriends, well there were dances, and, I, I remember going... I joined all three of the main political clubs, the Labour Club, the Liberal Club and the Conservative Club, not because I had any particular, obviously political allegiances, but, because I wanted to find out what they said there. And they all held dances. I was... I think most undergraduates came up to Cambridge, unfathomably to me, already kind of belonging to a political party. I knew nothing whatever about political parties, so I wanted to find out what they said. And I duly went to their meetings and listened to what those speakers said, and we had a lot of very good speakers, very charismatic speakers of the time, politicians from London who loved coming down and showing off to the undergraduates. And, we had, we had people like Kingsley Martin who edited the *New Statesman*, and, Herbert Morrison, and, you know, the well-known politicians of the time.

[56:35]

You were asking particularly about girlfriends. I, there was a mathematician called Audrey in Newnham, and I became friends with her. But I had no experience with girls at school, being a boys’ school and thoroughly unisex, and I was totally ignorant. And I had this friendship with Audrey, and it was, I remember having my eyes opened really for the first time about a real friendship. I had often wondered when I saw people together what they talked about, what did they have to talk about? And I realised, they were talking about themselves. That was... I don’t... It sounds banal, but, that really was quite a, a revelation to me. And, Audrey has remained a friend ever since. I also used to play tennis with quite a lot of people, and, including girls, and, one or two of the friendships remained for quite a time actually. There were two girls called Wrong, Elizabeth Wrong and Imogen Wrong, they were daughters of a master of an Oxford college, and there was an occasion when I was asked to a party and they said, ‘We are short of girls. Can you do anything about it?’ So I said to the
Wrong sisters, ‘Do you want to come to a party?’ And Imogen said, ‘I always say yes to parties, because who knows, you may meet your life partner there.’ [laughs] A typically Cambridge thing to say. And, they duly went to the party, and they both got engaged to other guests at the party. Imogen Wrong became married... Have I got this the right way round? Yes. Let’s get it, I must get this the right way round. Peter Shore. Liz? Who did Liz marry? Anyway, they both got married to people... Oh yes, sorry. Imogen Wrong got married to Brian Rose, who was an economist, and finished up in Washington DC. And, Liz Wrong married Peter Shore, who became, was he Home Secretary? I think he was.

Mm.

And, so, I, I was pretty proud of that bit of matchmaking. But that was the Cambridge scene.

[59:33]

*Were you involved in any other clubs and societies other than the, the Ten Club that you’ve mentioned and the political groups?*

Yes. Well the Ten Club and the political clubs I went to because of the dances, and the Liberal Club was, the Liberal Club was best, because there the numbers were fewer and there was room for tables around the floor, which were much better than just sitting in chairs at the, at the Dorothy Café where all these dances were held. But there was a particular society called CUSIA, that stands for Cambridge University Society for International Affairs, and, it, I remember I joined it, and there was a committee there, and the committee was very remote, I didn’t have anything to do with running it. And then somehow, and I can’t imagine how this happened, I think as I graduated, about my third or fourth year, I got on the committee of this body, and very shortly I was made Chairman of it. It was a large society, we had over 1,000 members. And the President, there was always a, a senior academic as President, was G M Trevelyan, the Master of Trinity. So I used to go and see Trevelyan and go over the list of speakers, proposed speakers, and he would look over and, if there were any friends of his, he would say, ‘Oh well, I’ll invite him to stay at the Lodge.’ And, otherwise he would, he would look at them and say, ‘Oh well I wouldn’t have had
him.’ And, so on. We got, we got lots of speakers. We had a political speaker every week; we then branched out and did a series of weekly meetings on the arts I think it was, and, and we even had a, a discussion group on Sundays, lunch, a discussion lunch on Sundays. And we had to organise all these things, and I was happily chairman of this whole caboodle, but got all the, I think I was good at delegating, [laughs] because, I didn’t do so much work, and, we had a treasurer and a secretary who did the real work, somebody to do the dogsbody work of the arrangements for the meetings, and, going to the printers with the posters. But it was all great fun. And I think I, I was quite an organiser at the time, must have been I suppose. I had been... I’d got a bit of experience by being head of my house at Stowe, where the head of the house had to organise all the teams and the games and generally be in charge of a lot of activities. And I think that greatly helped. But I, I went into this job with CUSIA with a will, and thoroughly enjoyed it.

[01:02:38]

Thank you. Within the sciences, could you just quantify really the, the sort of, ratio between men and women taking science at this time?

I don’t think I was very aware of the, of counting numbers. I think, in the mathematics lectures there would be perhaps, half a dozen women from Newnham and Girton among, in a class of perhaps thirty or forty. Those are terribly vague numbers, I don’t think...

That’s fine.

...they’re of much value actually. I think in physics there were probably many many fewer women than men. The women from Girton... from Newnham, were not allowed to wear trousers on the Cambridge side of the bridge over the Cam, that was the sort of atmosphere, had to wear skirts. And Girton and Newnham girls were not allowed to take proper degrees, they were what were called ‘recognised institutions’. Cambridge was terribly standoffish when it came to coeducation. They took the same exams as the men, often did better than the men, of course, but, they weren’t recognised in the way the men were. But numbers, numbers were about as I was
saying. There were a lot of, if you went into an arts department, in English, you would find many more women.

[01:04:31]
*That’s what I was going to ask. What was specifically male about science then at this time?*

Well of course, experimentally it was a hands-on thing, and, it was just not considered so ladylike to be doing science. I mean there’s a story about Searle. I witnessed some pretty bad bullying by Searle of women. But there’s one woman who couldn’t get the magnetism experiment to work at all, and she was sent out to take off her stays, which were upsetting the experiment.

*Take off her...?*

Stays, her corset stays. Steel stays. And, experiments were unsuited to women you see. That sort of thing would happen. They were considered not suitable for, for that kind of activity. I mean, Cambridge was a very conservative place. Do you know, may I give you a quotation? But this comes from way back, this comes from the time when the Cavendish was just being set up, and when it was considered unusual for undergraduates to do experiments of any kind, and a man called Todd Hunter wrote a well-known book, said, something like this: if a gentleman is unable to accept the word of his tutor, who is a man of integrity and probably in holy orders, then he is unlikely to benefit from the evidence of his own senses. Which was the point of course of practical work. But that was the kind of conservative view of mid-Victorian times, and it, it persisted in an attenuated way I think all the way through, right up to when I was at Cambridge. It wasn’t considered quite a ladylike thing to do.

[01:06:38]
*And would... You mentioned other instances of Searle and bullying female students.*

Yes.

*Do you remember the nature of those incidents?*
Well I remember one quite vividly. You looked through eyepieces with cross-wires on them, and you had to focus the cross-wires so that you could see them in focus clearly. And, Searle goes up to a girl who can’t focus her cross-wires. You had to keep one eye open to focus on infinity and look at the cross-wires with the other eye, so you didn’t suffer eye strain. And, he said, ‘How many cross-wires do you see?’ And she said, ‘Two.’ ‘Have another look.’ ‘I think there are two, Dr Searle,’ through her tears. ‘Have another look.’ She couldn’t answer. ‘Well there’s only one.’ He was a real bully, I mean it was horrible to see. And he would shout, ‘Nye!’ to me at the other end of the room, ‘Come and have a look at this.’ And I would pretend that I didn’t hear, and all the undergraduates would titter. And he’d say, ‘Nye, come over and have a look at this.’ And I would eventually come over and have a look through his eyepiece or whatever, and see what he had had a look at. But he was a very imperious man.

What would Sir Lawrence Bragg’s view of women in science be do you think at that time?

I’ve no idea. He had a favourite daughter called Patience, whom he was very close to, a lovely girl. And, I don’t think he made any, any kind of discrimination between the sexes, because as I said, many of the women were actually in crystallography, and he was a crystallographer, in fact he invented it. And, there were so many women crystallographers who were perfectly OK persona grata with him and everybody that, I don’t remember any kind of discrimination. But it was around that time that Kathleen Lonsdale, who was a crystallographer, became I think the first woman FRS. So you can see the, what’s happening in the change. I hope I’ve got that right, that she was the first woman FRS, but, you could check that.

Yes. And, what was the, what was the attitude of the undergraduates to these few women taking science? I’ve got a sense of the attitude of at least the university as a whole and of this particular lecturer, Searle, in particular, but, what would have been
the attitude of, you know, the other male undergraduates to the presence within the course of these women?

They were perfectly tolerant of them, in fact glad to have them there. I mean after all there was an enormous shortage of women among all those young men. And, I mean, there weren’t enough women for the dances. And, after the war, when the warriors returned wearing their duffle coats and, quite a different sort of chap, it was even, it was even worse, because, there really weren’t enough women for them. There was, as I say, a different sort of chap. They were, there was a very distinct difference. You see the, a lot of the people who were in Cambridge, the men, were there for some, probably medical reason, for not being fit for the Army or something of that sort. So they weren’t quite a representative cross-section. And then came the, the demobilisation with the war in their demob suits, and, they came back and, it was just a different lot of people.

I’ve been told, other people have said a slightly more worldly and, and wealthy group of people.

Who came back?

Mm.

I’m not sure they were more wealthy. They were far more experienced of course, I mean they after all had been out shooting people and being adults. Whereas the others were still sort of late adolescent. It was, it was a big difference. And the ones who came back were much less tolerant of authority, the proctors, whose job it was to enforce petty rules like wearing of gowns after dark, found their job much more difficult when the chaps returned from the war.

[01:12:01]

Yes. Thank you. Could you now explain the origin of your interest in ice, if you think that is the next...?
Right. Yes, I can. It’s really quite specific. I think the first thing is to say that one thing leads to another. They’re not detached. There’s a, there’s a transition. And, I think I told you that I sat at the same desk as my supervisor, he was on one side and I was on the other, it was a very wide desk. Or maybe it was two desks pushed together, I can’t remember. And, he was continually having visitors, and one of his visitors was a geographer called Vaughan Lewis, W V Lewis, geomorphologist. You may have heard his name.

Yes.

And, he came to Orowan because he had a particular problem with his cirque glaciers. Cirque glaciers were all the rage with geographers, they seemed to talk about nothing else. And, he would call it rotational slip. The little cirque glacier rotated as it, in its basin, and formed... and when it melted it formed a little lake, a tarn, there, which you could see all over the Lake District and North Wales. And, what he had done was to make a, I think it was a cardboard or a metal model with a sort of, I think the word would be a segment of a circle, a segment of a circle, representing the glacier seen from the side, and this was supposed, this could rotate on a, on a pivot, and you could put weights on this lever and see how much weight it took for this thing to, to move, or to make it move. So something that you could have very easily done on the back of an envelope, as I would have done, he had actually made a metal model of this with a string to pull the, put the weight on and that sort of thing. And he came to Orowan, realising that it was a mechanical problem, and Orowan was hot stuff on mechanical problems of all sorts. He had this training as an engineer. So I listened, as I did to most of the conversations of Orowan’s visitors, I listened to this one with interest, and heard Vaughan explaining what the problem was with cirque glaciers and how they, he had a theory he called rotational slip, where the gravity tended to pull the glacier down rotating it, and the glacier slipped on its rock bed, and as it slipped, it kind of ground out the circular shape that resulted in the corrie that was left. Of course, it was a question of whether the glacier formed the corrie like that, or whether the corrie was like that in the first place, and the glacier simply slipped down it. I mean, it was a question of cause and effect. But he had the idea anyway that what was going on was that the glacier went down, and it had as it were rock tools in its, in its lower edge, which ground away at the rock and made it into this nice circular basin. And that was
his great interest. And he wanted Orowan to kind of, look at the mechanics of this. So that was the first time I learnt about glaciological problems.

[01:16:02]

And then later, not very much later I think, there was a famous meeting in London joining together the, I think it was Institute of Mechanical Engineers, I think some geographers. Anyway, it was a meeting of the minds, concerned with mechanical problems to do with glaciers. And it became, to my mind, the beginning of modern glaciology. And, Orowan typically thought about it and then went to really first principles. He said, ‘Well what would happen if you had a, a slope, sloping plane, and you put a block of ice on it, what would happen?’ Well, depends on the properties of the ice. And, Orowan knew all about plasticity, and he said, ‘Well, as you tilt the plane, there will come a point when the plastic resistance of the lowest layer of the block was overcome, and the block would slide plastically down the plane.’ He didn’t say friction, as you could have said, but, he said plasticity of the, of the bottom layer. And the plasticity of the bottom layer depended on the strength of ice. So what was the strength of ice? Well, I think we knew enough about the strength of ice at that time to make a sort of ballpark guess, that it was about one bar, a bar is about an atmosphere, and, this is not a pressure but this is a, a shear stress, a sliding stress. And, so it’s, it’s one, it’s one atmosphere. And if you work out what sort of slope you can have for the ice to give way at the bottom, you find that the thickness of the block has to be, I think it’s about 300 metres. And that’s about the thickness of glaciers. So Orowan said, ‘Well that’s why glaciers in the Alps are about 300 metres thick.

Because that is the depth at which they begin to move?

That’s the, that’s the strength at that depth. The stress at the bottom, the shear stress at the bottom, is enough to overcome the strength of the ice, which otherwise is frozen to the rock. That’s what’s keeping it there. And if you can stress it enough, it’ll give way, and to stress it enough you have to make the block thick enough. And if it’s as thick as 300 metres, it’ll just about give way.

How did he calculate that, what did he do practically to work that out?
Well, it’s just a back of an envelope calculation. It’s, it’s density times g, gravitational g, rho g times the thickness of the ice block times sine of the angle of the slope. Rho g h sin(θ) became a famous formula. And you put rho g h sin(θ) equal to the strength of the ice, and out pops a thickness of about, as I say, 300 metres. So that was his model number one. Explained about glaciers, to his satisfaction.

[01:19:35]

And then, then he said, ‘Well what would happen if you had a, a very tall column of ice just sitting there? Obviously if it’s tall enough, it’s going to start squeezing out at the bottom, it’s going to give way at the bottom. How high has it got to be to squeeze out at the bottom? And you work that out, and you say, well the pressure at the bottom is rho g h density little g times the height, like a barometric pressure, rho g h, but it’s the density of ice, and you put in that equal to the strength of ice, and you come up with about ten metres, ten or twenty metres, something of that sort. And, so that’s, that’s how high you can make a column of ice. And, in the discussion that followed, my friend Bill Ward, who was at the Building Research Station, Watford, said... who is a glaciologist, said, ‘But that’s about the depth of crevasses.’ And that’s why crevasses are about twenty metres deep, rather than infinitely deep. The law of the time was that crevasses were bottomless, they could be any, any depth, and it was terribly dangerous. And, so, that explained the depth of crevasses. So then Orowan went on and said, ‘Well, of course, if it wasn’t a tall, slender column, it wouldn’t give way like that. Suppose it was a, a great cake of ice, like the Greenland ice cap, a great cake of ice, well that’s much thicker than ten metres, and that must be somehow giving way. So he did an ingenious calculation on that in which he assumed that the pressure would start at zero at the top, atmospheric actually, and then get bigger and bigger and bigger as you went down to the bottom. And this pressure of course acts not only downwards but also sideways. So, if you think of one half of Greenland ice cap, it’s been pushed from the side by the pressure in the middle. And, that pressure’s got to overcome the resistance of the sliding all the way to the coast. It is a long way. Hundreds of miles. So you just calculate the equilibrium between that outward pressure and the resistance of the bottom which is of course to do with the strength of the ice, it’s all the same idea. And what you come up with is about 3,000 metres, which is the depth of the Greenland ice cap. Now this is typical Orowan thinking, totally new at the time. Geographers have never even dreamed of doing any such numerical calculations, but the numbers were coming out about right.
Mm. How was he able to verify that the numbers were right, in terms of contemporary knowledge of the depth of these things?

Well, there wasn’t much contemporary knowledge actually. There was seismic work on the alpine glaciers, so they knew something about the depth. And anyway, you can just extrapolate from the sides of the valley and see how deep it ought to be in the middle. In Greenland, I think the Wagner expedition had done some seismic work right in the middle, and had come up with about 3,000 metres. And anyway, the summit is about 3,000 metres above sea level, which is not unreasonable for, that it’s 3,000 metres thick. So, it all seemed to be coming out about right.

So here am I sitting on the other side of the desk where all this is going on. And I go to the meeting and...

Sorry, the meeting of, in London of the...

Meeting in London of the three groups, the, I think it was the Glaciological Society, the Institute of Mechanical Engineers, and maybe another, maybe there was another. I think there were three groups.

Institute of British Geographers do you think?

It could have been. You can look it up.

Yes. What date was that do you think?

What date was it? Yes, now that’s a good question. It must have been about 1946 or 7 I would think.

And had you had any contact with the Glaciological Society before?

No, not at all.
No. OK.

No. No, it's just that I went to the meeting, and I was asked by Nature magazine to write an account of it for the magazine. And why I was asked by Nature, I can't, not sure that I can remember, but I, I contributed to Nature, and, I had also actually, maybe, ah maybe it was after that, I had applied for a job at Nature, but, anyway, they knew me. And I was asked to write an account of this meeting. So, in order to write the account, I had to understand these problems. And, sort of, what shall I say, ambitious, [laughs] over-ambitious at the time, I thought I could do better than these models. I thought I could improve them. Because I had read Nadai’s book, and I knew about plasticity. And in Nadai’s book there’s a lovely picture of what happens if you take a block of plastic material like metal and press it between plates, and squeeze it out sideways. And I had a, I suppose it was a kind of inspiration, because, really I took my cue entirely from Orowan, I said, well what happens if you cut the thing in half and switch on gravity, and put it on a slope? So, cut it in half, switch on gravity, which doesn’t apply in small cases, and put it on the slope. Now you’ve got a slab on a slope.

Why cut it in half?

Well because I wanted the top surface to have no stress on it, because it was just a free surface. And, in the experiment where you press the two, press the thing together, it’s got a compressive stress, obviously along the middle plane. But if you cut that away, you’ve got a surface with stress on it. But if you, if you also add gravity and tilt the plane, you will find that the same mathematical solution applies. And, this was a really, I might say a typical sort of Orowan trick, to turn one model, which was well understood, into experiencing something entirely different. So, I was pretty pleased with this, this thing, which I discovered was actually a model used in soil mechanics for the slip of soil, earth, down slopes. So it wasn’t original really. But, in soil mechanics, you really, in those days anyway, you really stopped as soon as the material failed, you’d explain the landslip, and that was it. Whereas this was what happens afterwards, how does it deform afterwards, which was a very different kind of thing. It’s all based on what they call slip lines, and intersecting systems of slip
lines which are theoretical ideas and mathematical ideas. And, and I applied these to the Greenland ice cap.

[01:27:36]

*How did you do that, by going and measuring, or...?*

No no, theoretically, I just thought, this is a better model, well, not to the Greenland ice... well especially to the Greenland ice cap, but, in the first place to glaciers. I thought this was a better model than Orowan’s block, which was just yielding at the bottom.

*Mm.*

I said, suppose it’s yielding all the way through, in this particular way, which is appropriate actually to a very thick ice cap like Greenland. And, I wrote this up, and was very pleased with it and typed it out very carefully and, and, and as I say, I remember Bragg saying, ‘Could you come to my house and explain what you’ve done?’ That was really the first thing I did in glaciology.

[01:28:28]

And then I was, in my thesis, I had attacked that, a problem like that, but I had taken into account what they call strain hardening. Now in standard plasticity, when something yields, it yields, it goes, but, in reality if you take, say, a piece of copper, copper rod, and you pull it, it doesn’t yield and then immediately go plastic and yield forever. It, it goes, as you know, it goes slowly, and it gets actually stronger, hardens, as the strain gets more. It’s called strain hardening. Now strain hardening is completely absent from a standard theory of plasticity, and it’s extremely hard to incorporate strain hardening in that theory. And I tried to do that in my thesis, totally unsuccessfully, but I tried to do it.

[01:29:32]

And, coming now to the glacier case, it was a natural idea, having thought about this problem a lot, of this block compressed, to think about strain hardening, which in the glacier case is the so-called creep law of ice. Well, what it means is, that if you take a piece of ice and you start squashing it, then, at small stresses it’s just elastic, bounces back. If you put the force on more strongly, the ice begins to, as they say, creep,
that’s to say, deforms at a certain speed. Not quite as simple as that, but it, first of all it deforms very fast indeed, and then settles down to what’s called the viscous creep stage. And this viscous creep stage we thought, Orowan and I, thought must be the one we want for glaciers. That must be what glaciers are doing, they’re creeping, they’re like metals, but they’re creeping, because they’re near their melting point.

Are they, are they creeping rather than flowing because a kind of hardening is happening as they move then, is that the relationship with the...?

Well, I guess the analogy with hardening is a bit misleading. We’re talking now about, about creep, and, there are certain, there are different stages of creep. If you put on the force suddenly, you get at the beginning a very fast creep called transient creep, and then you go into a, as you keep the force on, it settles down into creeping at a rather steady rate. If you keep it on for a very long time, it finally speeds up again, which is called tertiary creep. And we thought that the middle stage, the viscous creep, was the appropriate one for glaciers. It’s, it’s what’s called non-linear viscous creep.

[01:31:51]
Is this... In for example the first paper on glaciology of yours that I’ve read, you’ve got a graph which has got lines A, B and C, and, I think you say that, ice is not, as it had been assumed, like a viscous liquid, because the relationship between the amount of stress on it and how it behaves isn’t a straight line, proportional, but instead it’s a curve, which was B in this particular paper which sort of goes up and flattens. And I think there was a link between that and some work by John Glen, which, and I don’t know how that fits in to the picture, but is that, is the, is the bit of the curve as it flattens what you’re talking about there that...

No, you’ve described it absolutely accurately. The curve that’s being plotted is the rate of deformation against the applied stress or force. And, it’s not linear as you said, it’s not a straight line, as it would be with, say, treacle, but it is a curved line. And it’s a curved line which is so strongly curved that it’s even approaching I think what you described as curve C or something, which was, which is, it does nothing up to certain point, and then suddenly gives way. Which is an extreme example.
Mm.

That’s perfect plasticity. Well ice is not perfectly plastic, it’s not quite there. It’s something intermediate between a treacle, viscous liquid, and a perfectly plastic material which has the sudden giving way, sudden yielding. So, we said, ice is a quasi viscous material.

[01:33:49]

Now, you’re right to mention John Glen, because, he then did experiments in the Cavendish to, not just to hypothesise what this curve might be, but what it actually was for ice. He did the experiments in a fridge, with real ice.

So in terms of the timescale of this, you’ve gone to the... Vaughan Lewis has come to the desk and spoken with Orowan, and then he’s done some calculations on paper, and then you’ve gone to the meeting, which we’ll talk about a bit in a minute as well...

Yes.

Been asked to write a paper on it, but then felt you wanted to do your won research on this rather than just... or, you felt that in order to be able to document what they’re talking about, you needed to work on it yourself.

Yes.

And you have worked on it yourself, and then, how does John Glen fit into the story, how, how does he appear doing the experiments in the...?

He appears as Perutz’s research student in the Cavendish. Perutz, who is a, a mountaineer, was I should say, was keen to explain the motion of glaciers. And he is also a physicist of course. And he realised that a good thing to do would be to measure how fast the ice is moving, not just at the top, but all the way through. And how to get at that. So he did his famous pipe experiment. He said, the thing to do is to drill a hole right the way from the top of the glacier to the bottom, and put a pipe in it. Initially the pipe would be, you might say, vertical, or straight, and then come back
after, say, a year, and the pipe would be curved. And it would be, it would have moved further at the top than it would at the bottom, in fact it might even have stayed fixed at the bottom, and then be in a curve from the bottom to the top. And in order to find out what that curve was, which would tell him the velocity of the ice at different depths, he had an inclinometer device, he simply measured the slope, the inclination of the pipe at each depth. And then by, as we say, integrating these different tilts, slopes, of the pipe, he could work out what the actual shape of the pipe was, and having worked out the shape of the pipe, the idea was that the pipe was being carried forward by the ice, and not moving relative to it; he would then know what the distribution of velocity was with depth.

Had he, had he done that experiment by the time that Vaughan Lewis came to see Orowan, or...?

[01:36:43]
No I don’t think so. The experiment was I think from ’48 to ’50, but again, I can verify those dates. And, it was carried out on the Jungfraufirn in Switzerland. And, both Glen and I were, knew all about this, and at some point during, must have been during that period I guess, John Glen, who was I think slightly younger than I am, came as a research student to the Cavendish, was allotted to Perutz, and Perutz said, ‘What you’ve got to do is measure the creep of ice.’ For real. And so John set up a, a fridge with a, a window in the door, so he could see what was going on inside, and, had a kind of telescope I think looking through the window, and watched this ice as it gradually got... I think he did the experiment in compression, took just a cylinder of ice and squashed it. And, did the usual creep experiment which was pioneered by Andrade back in 1910, done, he did lots of experiments on metals, that was a standard creep experiment, but John did it on ice, which is very unusual, and he did it in compression, which was probably also a bit unusual. And, he produced then a graph showing how the speed of deformation related to the force you put on it. And this... And he found out what’s ever since been called the Glen law. This is called the Glen flow law, which is, as you can now guess, a non-linear law, not like a... If he’d been compressing treacle, he would have got a linear law, but he was compressing ice. He didn’t get this sudden yielding, but he got a, this more gentle motion which was
nevertheless non-linear, highly non-linear, in fact, in the jargon, we call it a third power law.

*Because it’s closer to being plastic than it is to being liquid in the...?*

Yes. Being liquid is a first power law, one, and being plastic is an infinite power law, being infinity, as the power of... Ice as we now know is about three or four. There’s, there’s a lot of, a lot of talk about whether it’s three or four or what you should be measuring, but, broadly speaking the Glen law has stood the test of time.

*And, what do you remember of that period when he was, I suppose looking through the, the telescope through the window of the, of the fridge? What do you remember in terms of your interaction with him and the things?*

Well, I was interested of course, and it was close to what we were doing with metals. And it was of a, he bridged a gap here between the glaciers which I was beginning to get interested in, and what I had been doing before, which was the strength of metals. And Glen was the chap who kind of bridged the two. And...

[01:39:58]

*Why did Vaughan Lewis not go and see Max Perutz about his glacier problem?*

Why did he not see Max Perutz? Possibly because he didn’t know him. But, Perutz was really wrapped up of course in his crystallographic problems, and Orowan was probably known as a man who was interested in almost everything. I don’t know whether that’s the reason. But, you know, Perutz was, was absolutely dedicated to working out the structure of haemoglobin, and we always said, ‘You’re doing something which is, you know, impossible. You’re, you’re really running into a brick wall here. Because how do you work it out? It’s too complicated, the molecules are too big.’ And of course we were quite right. But we didn’t know that computers were going to be invented, and, nor did Perutz, but he had enough sort of drive to go on nevertheless. And if it hadn’t been for computers, I suppose he wouldn’t have solved his problem. So he was, he was triumphant. And of course Bragg was tickled pink because, Bragg had invented X-ray crystal analysis, and had applied it not only to
rock salt which was the primitive thing, but all the minerals, and got out all the structure of the minerals. And to apply it to something organic was an absolute triumph. It meant crystallography belonged in the life sciences as well as in crystallography.

And what was Perutz’s reaction when you said, you and others said that it was impossible, that you wouldn’t be able to determine the structure of such a complicated organic substance?

Well, I think... I, I don’t know, I think he just went on. Probably didn’t... He was a very polite man, he wouldn’t have, he wouldn’t have engaged in argument about it. It was, you know, just something one says to colleagues who are doing, doing work and you make a comment about their work. There was nothing kind of offensive about it, it was simply a statement of fact.

Where was the sort of social space in the Cavendish laboratory, in other words, was there an area where you had coffee or...?

Oh yes, absolutely, it was very important. J J Thomson had left money to the Cavendish specifically so that they could have tea in the afternoon together, tea and buns, or rolls, and it was quite a religious ceremony to come along to tea in the Cavendish, strictly at four o’clock, I think it was four o’clock, every day. And there we all sat round in a rather formal way around the edges of a large room, and, we chatted. And then later on, later on we, when I was more concerned with the, more with the crystallographers, we had coffee in the morning and I think tea in the afternoon in the old anatomy school, which was all in the same complex of buildings behind the old Cavendish, and there we sat round a table and had general conversation.

Who was there at that time?
Oh people like, Audrey Douglas, Helen McGaw, possibly Wooster. Who else? The crystallographers. Not, not Taylor, he took over later. Oh yes, Dennis Riley, charming fellow, who was working on coal, crystallography of coal. Who else would there be? Those are the, those are the names I remember.

*Max Perutz?*

No. No, Max was, was a bit of loner. Max worked at the Molteno Institute, which was separate building in Cambridge. Because, really, you see Max wasn’t just a straight down the middle physicist at all; he was a, he was a chemist, and, with one foot in, in physics because of X-ray crystallography. But he did his work and got his specimens and all that at the Molteno Institute, which was funded quite separately. And it was only later that he moved into the Cavendish, really after he had had a bit of success. I think he was a, you know, a bit of an outsider, and under, well not, I won’t say a suspicion, but, not, not considered necessarily a straight down the middle sort of, physics person who would give physics lectures. He wasn’t that sort of a man.

*Mm. Why would he not have been seen as someone who would give physics lectures? Because...*

I think his knowledge of, probably, fundamental physics, was, was just not, not... I’m absolutely guessing here, probably not up to it, at that time.

*Mm.*

I mean, who knows what Perutz might or might not have known? Not for me to, to suggest, but, he just wasn’t professionally, I think, again this is only a, a hunch... I don’t, I don’t know what his training was, but he was as much a chemist as he was a physicist I think.

[01:45:44]
*And, this meeting in 1946, ’47, the... which included, in London, glaciologists and mechanical engineers, and possibly geographers we think, can you remember people who were at that meeting?*
Not very well. I know Bill Ward was there, but I don’t think I can... Oh yes, I
suspect that Hollingworth, who was the professor of geology at, probably University
College London. I suspect he was there. I expect Jean Clark was there. Again I can’t
swear to it, but these are the sort of people who would have attended.

[01:46:26]

*What then is the next step in the development of your work in this area? We’ve got,
we’ve got John Glen in the department doing work on the compression.*

That’s right.

*Orowan is interested; you’re working on it in terms of the theory of plasticity and
flow.*

Yes.

*Then, then what happens next, in terms of this?*

What happens next? Well knowing about the Glen flow law, there are a number of
quite elementary things you can calculate about the motion of glaciers. I thought they
were pretty trivial, and so I didn’t even bother to publish them. I, I wanted to
introduce the Glen flow law into the calculation that I had made about the slip lines
and the block of ice sliding down the slope. And, I managed to do that actually, but,
the route by which I did it was, was perhaps a bit indirect. I had actually for my thesis
done real experiments on compressing blocks, blocks of lead between plates in the
engineering lab, and I had inscribed by a certain means inside the lead block, a grid of
lines. Then squashed it between the compression plates, taken it apart later, and
looked at this grid, which, which showed a lovely pattern of distortion, really
beautiful. And, that of course was, lead was a real material, and it wasn’t perfectly
plastic. Whereas the pictures in Nadai’s book were of a perfectly plastic material.
And I took, I didn’t know how to go at that time from this grid on the lead to a piece
of paper with a, with a, with the picture on it, but I found a machine at, in the
engineering lab, which magnified it up, and projected it onto a screen. And it did that,
and I traced it then on the screen. So I did a kind of enlargement of it. And then, took it back to my lab, and Orowan said, ‘If you’re going to put this, publish this or put it in your thesis, you’ve got to draw it properly.’ So I had to painstakingly go over these pencil lines with proper drawing pens, they were curves of course. You used, I don’t know whether you know them, you use a, a very complicated curved shape which has every conceivable kind of bend on it, and you move it around until you get the right curve to fit the curve you want to...

_No, rather, like a stencil that you put the nib of the pen through, is it?_

Well you put it on the side of it. It’s like a ruler, curved ruler. And you had to draw curved lines with this thing, and you... little arcs, of just the right curvature. So I had to take this long piece of tracing paper and...

_About a metre and, about metre long?_

Yes. Oh yes, a couple of metres long probably. And, and then, transcribe the pencil lines into ink lines. And they’re in my thesis. And I published it later. But, that’s what you had to do. So I had this thing, and, I looked at it, and, actually I think it was Orowan who, who noticed that if you went along the centre line, you had the same sort of pattern at each point, that’s to say, the pattern didn’t change, along the centre line. There was a region, two regions on either side of the central bit, where there was a pattern of distortion which didn’t change. And I thought, well, that’s interesting. So, I’ll try and work this out and apply it to a glacier, and, it worked. It worked. I did my same trick of turning the block into a, one half of a glacier, and, and it, and it produced a mathematical solution which satisfied all the right equations. And just one thing wrong with it, and that was, that it produced a slip on the bed of the glacier which was different at different points. That’s to say, the glacier was yielding or sliding on its rough bed, but although it had the same stress on it at every point, in some places it was sliding faster than others. And this was a real puzzle because, if you introduced what’s called a slip law artificially and said, slip is something like friction, it, put on a certain shear stress, you get a certain amount of velocity of slip, it just wouldn’t work. So I was really distressed about this, and I got the whole paper written, and I realised this was a fatal objection to the paper. And, so, instead of
publishing that stuff, I went right back to the original perfect plasticity solution where this difficulty doesn’t arise, but it’s a much cruder kind of approach. And that’s the paper I published, in 19... about 1952.

And, I think at about the same time I was doing these rough and very simple calculations arising from Glen’s law on glaciers, which I didn’t think worth publishing, they were so obvious. And then I was asked to write a paper for the *Journal of Glaciology*, by Gerald Seligman, who was the founder of the society, and editor of the journal. So I, I incorporated both these pieces of work into a paper, the, what I thought then were trivial calculations, mentioned the perfect plasticity stuff I suppose, and then went on to, to apply this to the patterns of crevasses that you get on glacier surfaces, very well known to climbers. You get transverse crevasses, you get curved crevasses at the edges, which curve in a particular way, and you would explain all these in very very simple terms, once you knew what was going on. And, I put all this in this paper. And, I didn’t think very much of it, but it’s become a rather well read paper.

Do you know how you came to be invited to write it by Gerald Seligman?

Well, Gerald Seligman was keen to promote glaciology, and he had been on an expedition in the Alps with Perutz, and they had looked at the, how grains of ice grew as they went on down the glacier. And I remember Seligman, who was really an amateur scientist, he was mainly a skier and a connoisseur of snow. I remember him saying, as a grand conclusion, that the chief factor in the growth of these grains is time. Well, I was a kind of, [laughs] philosophical chap, thought, well the growth of anything is due to time. But it seemed to him to be a sufficient explanation. The older they were, the bigger they were. And, that was the outcome, it seemed, so it seemed. I’m being very critical of that explanation of all that stuff.

But Seligman was above all an enthusiast, and he invented the Glaciological Society. It started off by being called the British Glaciological Society; Seligman was a patriot and he liked the idea of it being the British Glaciological Society. And when
eventually the time came when people in America were publishing in the journal, it became time to call it the International Glaciological Society, he was, not too pleased.

*Do you remember what he said about that, that change?*

Well I do remember that he was involved in the discussions before Perutz’s pipe experiment, and, the experts were all gathered together, there was G I Taylor who I mentioned, there was Bullard, who was the great geophysicist, Sir Edward Bullard, there was Perutz, and there was Seligman, and there was Orowan I think. And they were all discussing the problems of putting this pipe down in the glacier. And, Bullard said... No, no Seligman said, ‘Well I think it should be a British affair.’ And Bullard said, ‘Oh I rather like an international crowd.’ [laughs] And that was a, a comment on, on Seligman’s attitude. He wanted to keep the thing British.

[01:55:55]

*When was this group of people discussing how to put the pipe in the glacier?*

Must have been in the late Forties of course. And...

*Where?*

Where? I don’t know. But I know that one of the problems was, would it go down in a kind of corkscrew? Why, why would it go down straight? Will it be unstable and go in, go in the wrong direction? Those sort of things were discussed. How were you going to make it, how were you going to melt the hole down, hole. And, there’s really a very, a very dramatic story about the whole thing. Have we got time to go into this?

*Yes.*

Well, Perutz was to melt this pipe down, with, by having a heater on the end of it. And then, they were to lower an inclinometer down the pipe, and being a steel pipe, they couldn’t use magnetism for the inclinometer, they had to use gyroscopes I think it was to keep the inclination, to keep the, well, keep an orientation. And they had a chap called Broad who was an instrumentalist who did a very elaborate and rather
beautiful design for this inclinometer which he made. So, the time came for Perutz to
go out to Switzerland and put his pipe in. By the way, his, Seligman had a brother I
think. Seligman was a very rich man, and I think his brother had a, a metals factory,
and I think they probably got the pipe from the brother, I think it’s Richard Seligman.
I may have got that name wrong, and I may have got that detail wrong. Anyway,
there they were, on the channel crossing, with the heater that they had designed for
heating the end of the pipe. And such was the drama that developed, that Perutz,
when he came to deliver his report to the Royal Society who were financing it,
thought that the proper, the only reasonable medium was by way of a play, a drama,
which you could find actually in the Royal Society Library as the report on his
expedition. And it’s, one of the scenes starts something like this. They’re in a train
on the way from Calais to Paris, and the young Perutz daughter says, ‘Daddy, was
there an aeroplane in the package, in the parcel?’ ‘No dear, of course not.’ ‘Well
where is the parcel?’ The parcel had been on the luggage rack, and some rather
helpful people had been helping to rearrange the luggage, and it had fallen out of the
window. So, Mrs Perutz, Ann Perutz, watched, looked, saw the station names
whizzing by the train, tried to write them down, to know where they were. And when
the train got to Paris, Mrs Perutz organised parties of Boy Scouts to go up and down
the line in the region where the parcel had disappeared, to try and find the precious
heater. They never found it. So Perutz went on to Switzerland where the experiment
was to be, on the Jungfraufirn, which is up towards the Jungfrau mountain. You go
up on a mountain railway, inside the mountain, and come out at the research station at
the Jungfrau, at the top station. And, he arrived in Switzerland without his heater.
[01:59:49]
So being an enterprising chap, as Perutz certainly is, he went round to try and buy
one, although this one had been elaborately designed, made in Cambridge. And he
found that in Switzerland, in beer barrels, when you want to bore a hole for the bung,
you use an electric sort of poker, which bores the hole through the barrel. So he
bought a collection of these electric pokers, and lashed them all together, and that was
the heater that made the hole go down. So he got there with his pipe and his heater,
and the pipe went down, and they had all kinds of problems. They were accompanied
by a man who had climbed Everest, Roch, what was his name? André, André Roch,
who was a great mountaineer chap, lovely chap, and, he helped them. And the rig
was struck by lightning. Every possible disaster. But eventually they got the hole
down to the bottom, and then they measured it with Broad’s inclinometer.
[02:01:11]
I went out there, I guess the following year. Yes, I think it must have been the
following year. And, the reason I went there was that, Orowan and Perutz and
Professor Hollingworth had previously got a grant from the Royal Society to go and
tramp over the glaciers of Switzerland, to, to really familiarise themselves with the
real stuff, actually to set foot on a glacier, whereas Orowan had never been near one.
And it was considered a good idea to actually make contact with the physical
substance that you were doing your calculations on. So, they had all been out there
and done a tour of the glaciers, and then they got a grant for the following year. And
for some reason they couldn’t use it or they were doing something else, and, the
money was there, and so it was given to John Glen and me to do a similar trip in the
Alps. It was really very generous. And so, we went out there, never having seen a
 glacier in our lives. Not quite true, I had seen one in my teens. But, John and I
arrived and were expected to tramp over the glaciers. Well, you need a little bit of
introduction, and Perutz was out in the Alps, which he loved, and offered to kind of
initiate us in the various ways, what you do on glaciers. You wear crampons and you
use ice axes and, you look at these, these boulders perched on pillars, and he told us
all about the sights and scenes of a glacier, which he knew only too well. He loved...
He did his glaciology very much as an offshoot of his mountaineering. In fact he, he
used to say, frankly he only glaciology in order to allow him to go mountaineering at
somebody else’s expense. So, he was first and foremost a mountaineer. And he, we
were lucky to be with him for the first few days as we tramped over the glaciers. And
we, John and I, looked at lots of glaciers, and, we were looking for, I had predicted
that there would be a special sort of slippage in the upper reaches of glaciers, which
went in the reverse direction to what geomorphologists usually thought of, instead of
slipping that way, slipped down that way. And John and I went all over the glaciers
looking for these, these little special shear zones, and we only found about one, of
which I zealously took a photograph, and put it in the paper.
[02:03:54]
Anyway, John Glen and I finished up on the Jungfraujoch to help Perutz with his pipe
experiment. I remember I was desperately sick from altitude sickness, because you go
up in the train and you get to about 10,000 feet in no time, and I went straight out on
the glacier, and had to haul in this very heavy electric cable which went from the research station down to where the pipe experiment was. It was a cable about that thick. I’m pointing to something a few inches thick, a very heavy cable, and, with all the electricity they needed from the research station. So it was quite a job to haul it back up into the research station. And as I say, I was very sick. But I did see what was going on there. And, there they all were, André Roch and Perutz, busily putting their inclinometer down the pipe and measuring it all.

[02:04:56]

And, that was, turned out to be the second year of measurements. You had to have at least two years. And then there was a third year of measurements, and the, I think there was something wrong with the, with the first year’s measurements, so really the measurements that mattered were the second and third year. And it was those I think that I quoted in my paper, trying to explain the shape of the pipe. It was all a bit, a bit unfortunate actually, because, Perutz had already explained the shape of the pipe on a very simple hypothesis that the glacier was simply shearing at different rates at different depths. And, he published that. And, I thought I knew better. And I had my, my solution, mathematical solution, which allowed for the fact that the glacier was also compressing or extending sideways. And when you allowed for that, you got a different answer. Which didn’t explain the pipe experiments anything like so well. So I was a bit embarrassed about this, and, it was much, the shape of the pipe, instead of being a, a sort of parabola, was much more like an ellipse, because that’s what was in Nadai’s book, about plasticity, it was an ellipse. It was actually halfway between the two, in my calculations, and unfortunately didn’t explain Perutz’s measurements quite so well as Perutz’s first primitive you might say, more elementary, explanation. So it was rather sad. I don’t know what the final explanation was, but that’s what happened.

[02:06:47]

So the, so the idea of, your idea of the ice moving down plastically and being, not just affected by gravity wanting to move the thing downhill, but, of actually squashing into itself and deforming as it goes down.

Precisely.
That didn’t explain the shape bent into the pipe as well as a simple law of the different layers of the glacier moving at different rates, or explained it better. What was... I wonder...

Yes, I think you’ve put it absolutely correctly. The, the simple idea, as you said, did explain the observations better than the more sophisticated idea that I claimed ought to be applying.

Did that make you doubt your own explanation?

No, not at all. No. I...

You doubted the pipe, rather than your...?

I think I probably doubted the experiment more than the, the theory, yes. Interesting isn’t it? [laughs] Yes, I don’t know. But I suppose so. Yes, I was going to say something else about that, that experiment, which has escaped me just for the moment. Could you switch it off a second?

[End of Track 3]
I wanted to tell you about the so-called theory of extrusion flow. It was somewhat connected with the pipe experiment, Perutz’s pipe experiment, because, it was quite a vogueish idea at the time, that the ice at the bottom of a glacier was more plastic, squishier, than the ice that you saw on the surface, because, they said, of the pressure. And of course we have, we don’t have any direct experience of what happens when ice is under high pressure. And so, the idea was that it squashed out like toothpaste from the bottom of the glacier. The word was extrusion flow. So, the ice was squashed outwards, faster at the bottom than the top. There was a layer at the bottom which was moving faster. And this explained, so they said, the measurements at the Konkordiaplatz, which is just below the Jungfraufirn, where a lot of glaciers come together and they measure the speed of the different glaciers. Couldn’t explain how the inflow balanced with the outflow, and came to the conclusion that it must be moving much faster at the bottom than the top. And this was called extrusion theory. And a lot of people believed in it.

Now, I didn’t believe in it at all, and nor I think did, say, Orowan or Glen or maybe Perutz. Anyway, part of the interest of Perutz’s experiment was that it did not show extrusion flow, the bottom of the pipe did not flow much faster than the top. And there was a chap in America called Joel E Fisher, who wrote in a, wrote into the Journal of Glaciology, saying that, of course, extrusion flow is what happens. And, now his stuff is all crazy.

So I did a little calculation which I then sent as a reply to Fisher, where I imagined that the Greenland ice sheet was doing its, doing its extrusion flow stuff. And I proved to my own satisfaction that if it were so, the ice cap would be moving after 100 years, say, which is small in comparison with the life of the ice cap, in moving at the velocity of light. [laughs] And, I think that flattened forever the theory of extrusion flow. Joel E Fisher said, ‘Well of course it can’t apply if it’s going with the velocity of light.’ Even he had to admit that, but, it was a fairly dramatic application of really Orowan type ideas, to say that the, if there was nothing to balance the outward pressure, and no resistance from the bottom, then the thing would just
exhilarate. And I just calculated what the exhilaration was, and kept it going for 100 years, and it got pretty fast.

[03:21]

*What then was the, the next stage after this Royal Society funded field trip to the glaciers at which you observed the pipe experiment?*

Yes. Well, I finished... I was... Perhaps I should interpolate that I was appointed Demonstrator in the crystallographic lab at Cambridge. It was called the Department of Mineralogy and Petrology. Shall I tell you about that? Well, I knew nothing whatever about mineralogy and petrology, it was one of the subjects that you could study in your first year of the Natural Sciences Tripos, but I hadn’t done that. And that was what they did, they taught the students mineralogy and petrology. I had however used a polarising microscope, and Dennis Riley, I think I mentioned him before, doing the research on coal, I think it was he who got together with Orowan and decided that I might be a candidate for this job. Well I was applying like mad for various jobs, and it wasn’t easy to find them at all. In fact at one time I think I was appointed to the Guy’s Hospital medical school in London, to teach nurses how to read clinical thermometers, and, I was pretty desperate for a job and thought, if, if I can’t get a job here, I’ll go to America. Because there aren’t any jobs. And around this time, I think Orowan and Riley were putting their heads together, and saying, ‘Well maybe he could go for this job in the Mineralogy Department.’

[05:06]

Now the Mineralogy Department was a split department. It had the old style mineralogists who looked through microscopes at minerals, through the thin sections as I was describing, and it had X-ray crystallography, which was this new-fangled stuff which only went on in the Cavendish. So you had researchers in the department doing their experimental research in the Cavendish, and you had the old style mineralogists looking for their microscopes in the Mineralogy Department. And I was appointed as a demonstrator, knowing nothing whatever about mineralogy. So I read the, read F C Phillips’ book on crystallography, which was totally new to me, all about symmetry, which I liked, and I carefully worked through all the first-year demonstrations in the department, which were very formalised. [background noises] They had a pattern to them which... Shall we, shall we just.....
You were working for the first-year demonstrations for the new teaching post.

Indeed. And, I worked through them and learnt enough to do the demonstrations in the department. And as I was going to say, it was a very, very formalised department. They had a very set lot of exercises that the students did, quite unlike the Cavendish, which was a free for all. And the apparatus was cut and dried, and all the students did exactly the same experiments, and everybody in the department used the same notation, this is quite important. Crystallography is full of notation, uses the same notation which was uniform throughout the department, through all the supervisors. And I, I remember going to see the head of department who was called Professor Tilley, a very eminent mineralogist, and, it was quite unlike seeing Bragg. Tilley had a very cluttered office, and I remember him taking a microscope off the seat of a chair so that I could sit down, and said, ‘How’s your morphology?’ I didn’t know what the word meant. [laughs] So I think I said, ‘A bit rusty Sir,’ or something like that. And, ‘Oh well never mind.’ And, I mean morphology’s about the shapes of crystals and their facets and so on, which was, this was before I had read the books. And, he was quite happy, or, I don’t know whether he was really happy, but, content, to accept me. Although I was quite outside his field of interest, I was a physicist in a, in a mineralogy department.

And after, I think the second year, I gave some lectures on X-ray crystallography which I by then knew enough about because I had been taking X-ray pictures of metals in the Cavendish and knew a bit about, about that, I was fairly happy about that, and it’s also a nice geometrical subject which appeals to me, and I think I gave the lectures, you know, pretty well OK, although I was, I didn’t know all the gadgets that the mineralogists used. And that went fine.

And I also attended a series of lectures on what was called crystal physics by Wooster. Now, Wooster was a very difficult man, I don’t think anybody would deny that, and, he had, I guess he had studied under Bernal, and he was certainly a
communist. And he was invited by the People’s Republic of China to set up a crystal physics lab or a crystallography lab in Peking as it was then called. And so off he went one summer to go and do this. And somebody had to do the crystal physics lectures. So they chose me. ‘You will give the crystal physics lectures,’ I was told. Well I, I had been to Wooster’s lectures, and he had written a book, and I thought there were all kinds of things wrong with them, and in particular, that sort of crystal physics involves the mathematical theory of tensors. Well I loved tensors, ever since I was taught general relativity at school, and I had used them in my research on silver chloride. So I enjoyed this. And, and I, as I thought, kind of put right all the errors in Wooster’s teaching. And, being a, as I said, a united department, the other members of staff in the department wanted to know about these changes in the syllabus, because all their students would be doing different kinds of questions and they had to mark them and teach them. So there was a special meeting arranged when Nye would explain the new things in the crystal physics course. And I wrote on the blackboard, and I put down all these tensors and matrices and said, ‘I’m going to deal with it in this way and that way,’ and, and then, there will be gyromagnetism and I’ll teach about that, and... And who should come in but Wooster. Now Wooster had got as far as Prague, because there was no consulate in London for communist China, and he couldn’t get his visa in London, so he had gone to Prague which was the nearest available state, and sent us postcards saying he was staying at the People’s Palace and waiting for his visa. And he never got it. So he came back just as we were preparing for the following year’s lectures. And so there I was, explaining all the sins of Wooster’s book to an audience which then, into which then came Wooster, like Banquo’s ghost, you know. It was a horrible situation. And then we came to the questions at the end, and were there any questions? Yes, well Wooster immediately weighed in and said, ‘What are all those, all those things on the board? All these matrices Nye has been talking about. What do we want those for? And that’s no good anyway.’ And, I said, ‘Well, actually, all the symbols on the board are matrices.’ I think a word he wasn’t perhaps familiar with, I don’t know. No, he must have been. But anyway it was a, a different way of looking at things. And, so I was really very distressed by this whole episode. And I was called in by Tilley, the head of the department, who said in his usual sort of bruff way, ‘I hear you had a bit of a...’ I don’t know what he said, a bit of a, tussle perhaps, ‘with Wooster. Well, don’t bother about it, just carry on.’ [laughs] Which I thought was lovely.
So, the beginning of my interest in crystal physics. And at the end of my job at Cambridge as Demonstrator, it was a job with a definite term to it, three years, I... It was customary actually for the, many of the people in that job to write a book. F C Phillips had written a book about crystallography, and R C Evans had written a book about crystal chemistry, and my successor, Peter Gay, wrote a book about crystal optics, and, it was quite natural to write a book about crystal physics. So I thought I’d do that. And I left... They wanted some sort of legacy in the department, and so they, they Photostatted, that’s what you did in those days, they photographed my lecture notes, and printed them on photographic paper. So these scribbled notes were, were my legacy to the Mineralogy Department.

I then went to Bell Telephone Laboratories in New Jersey, and, Summit, New Jersey, and, there I worked with Shockley, who had just invented the transistor with two others there. I was about the only person in the group who was not working on transistors. Shockley had an interest in plastic deformation, in fact he was the author of the paper on partial dislocations that anticipated Bragg. And, so Shockley was in that world, and he was quite happy that I should work on a particular problem that he had suggested by an experiment that had been done at Bell Labs on aluminium, which actually didn’t come to anything, but which I, I did work on for a time, but obviously wasn’t going to work. And I did that. And then I said, what I’d really like to do is write this book on crystal physics. So, it was pushed around as a proposal, and all the members of the group were asked to comment on its appropriateness or otherwise. I remember Holden saying, ‘Were...’ He started off, ‘Were Wooster’s book a better book, then I would question the need for this one.’ And... But, and then he went on to, to say that perhaps it would be useful. And he was a, he was a good friend of mine at Bell’s, and, the book that arose is this one here, this one at the bottom, and, has been reprinted many times. And there’s a second edition here.

What was the link between the Department of Mineralogy and Bell’s?

Bell Labs?
Yes.

Well, Bell Labs was a very large organisation, and it was the only really large enough commercial organisation in America at that time to be able to afford to do what nowadays we would call blue skies research. Almost anything. There was a chap called Shannon for example who was measuring, who was talking about information. And he invented information theory. That was the sort of thing that Bell’s did. Shockley had visited us in Cambridge and said, ‘Well, we know all about, or we know a lot about conductors, and, we know a lot about insulators. We have to at Bell’s. But we know almost nothing about,’ what he called semiconductors. So, he thought, ‘We’d better study semiconductors.’ That was the attitude at Bell’s. And as a result of doing that, as I say, he invented the transistor. So they were very open-minded about the sort of things that could be done.

[17:17]

And how, how was it that you ended up being there rather than in Cambridge?

Well the job in Cambridge was strictly a three-year thing. So I had to do something. And, I thought it would be rather fun to go to America, because I had seen the movies and I wondered really what sort of a country it was. And, I found it was just like the movies. [laughs] But, it was curiosity really. And I went over strictly intending to come back after one year. When I arrived there I found that that was a very unusual thing to do. When you went to America you were so impressed with the place, you stayed there. You wouldn’t want to go back to Europe, it was sinking under the sea. Why would you want to do a stupid thing like that? But I said, ‘No, I think I belong there.’ And, I duly did come back after a year.

[18:10]

But, in that year, I started writing the book, did various bits of theoretical work, published a paper about the glaciology on the Jungfraujoch about how fast tunnels in ice will collapse in view of the pressure on them from the ice, and ice having certain flow properties, and that’s, that was a piece of theoretical work which applied to a number of different places. So I did that at Bell’s, sort of, kind of on the side. And I did a bit of work which at the time didn’t seem so important, but actually, if you look at citations, say, it turns out to be very much, well, cited. And what it was, was, this
idea of dislocations which were, as I was explaining, little defects which move around, and in the silver chloride experiments, I had seen what happens when you have a lot of them close to one another that you can’t even distinguish between them. And this led me to the idea of what I called a continuous distribution of dislocations. What it means is, that you start with N dislocations, each of a certain strength, and then you let N go to infinity, so you’ve got an infinite number of dislocations, but you let the strength of each one go to zero. So you’ve got an infinite number of defects, each of infinitesimal strength, so that you’ve got what we called a continuum. They’re no longer dots, but they’re smeared out throughout a space. And of course, mathematically that’s much easier to deal with than, than just a, a few isolated bad places.

[20:19]

Why is it easier to deal with that?

Well it’s because it’s a continuum, you can get some equations as to how it behaves, and they apply everywhere, not just to here or there. I mean, not just saying, this part of the crystal is elastic, but this bit of the crystal has something bad in it, which we don’t quite understand, and which is going to move under stress. Which is all very complicated. But if you say, they’re all smeared out in a way... You see, if you take... I’ll tell you what triggered it off. I had one of these bars of silver chloride, which as I said, you look at with a microscope between crossed Polaroids, and it lights up if it’s got a stress in it. Well, you take a bar of this stuff, and you bend it in your fingers. It goes into a curve. And course, one side is compressed and the other side is extended. And so it’s got lots of stress in it, but it remains bent. You now put it in a furnace, heat it up, and what’s going to happen? Well it doesn’t straighten out, that’s for sure; it just stays bent. But now, you look at it through the crossed polars and it shows just a uniform greyish colour. What’s happened is, that there are still dislocations in it, but they’ve moved themselves around into such a way that they tend to cancel each other out close together. So the thing is bent, it’s full of dislocations, and yet it looks like a continuum, it looks continuous. And, I thought that’s interesting. And, knowing how much it’s bent, you can say something about how many dislocations there are. Well, what’s the strength of each one? So just assume there’s a infinite number, but each one is of infinitesimal strength. And then you’ve got a, a way of
describing the curvature. So I related the curvature caused by dislocations to the dislocation strength, and I did this in three dimensions. So I developed a formula which described curvature in three dimensions, it’s curved this way and this way and this way, and, that’s a tensor, which I called a curvature tensor. It’s sort of vaguely related to Einstein’s curvature tensors but much more primitive, more naïve. And, and I also had another tensor which described the strength of the dislocations. And that was also a three-dimensional thing, like, it corresponded to a certain number of dislocations going in this direction, another lot going in this direction, another lot going in this direction. And lots going in all directions, all summed up in a tensor. All tensors you notice.

Mm.

[23:32]
And, the relaxation between the curvature and the dislocation density was something that I worked out at Bell’s, and I thought at the time, it was just sort of, interesting, and I published a paper on it, but it was later taken over by theoreticians who understand these things much better than I do, and turned into a thorough-going geometrical theory. It’s, the mathematicians call it, they call it... You see, an ordinary space which corresponds with a crystal which is not distorted at all, is Euclidian, it’s what we all understand as the three-dimensional world around us. If you bend it, the space becomes curved, and that’s the sort of thing that Einstein uses in his theory of general relativity. It’s curved space, which corresponds to gravity.

[24:31]
But going further than that, you can get a, you can get a, a space with torsion, and this is a special mathematical kind of space in which you can’t draw closed parallelograms. You can start off drawing a parallelogram here, draw a line like that, then you draw a line like that, then you’ve got to draw a line parallel to the first one, another line parallel to the second one. And you would expect it to join up at the end. But, you had to have a law, what you mean by parallel, and, in this particular space, when you’ve drawn such an infinitesimal parallelogram, it doesn’t join up. And such a space mathematicians call ‘a space with torsion’. Now that’s really all I know about it. But mathematicians love it, and they, this is a branch of mathematics which I don’t know anything about. But it was kind of taken then by mathematicians and who have
turned it into a, a subject that I don’t understand. But it, what I did was, the beginnings of it.

Mm.

[End of Track 4]
Could I ask you next about a field trip to a Norwegian glacier that appeared to have been organised by the Geography Department at Cambridge, but, of which you were a member?

Yes. I think this really links up with Vaughan Lewis’s visit to Orowan, which I mentioned. Vaughan Lewis was interested in cirque glaciers or corries, and he picked out a glacier in Norway called Vesl-Skautbreen, and, his idea of investigating how the glacier was eroding its bed was to go there, go to the bed, by digging a tunnel which would, it’s such a steeply sloping glacier, dig a horizontal tunnel into it and going and having a look and seeing what was going on at the bottom.

So, so far from going down vertically, as Perutz did with his pipe, he went in horizontally, into a very steeply sloping glacier. And, the question was, how to make the tunnel. Well just as all this was in the air for discussion, a person appears on his doorstep in the Geography Department called McCall. I’ve forgotten his...

John.

John McCall. John McCall. Who said, ‘Look, I want to be a research student here. I’ve sold my house in Alaska, I brought my family over, and, and please, I want to start doing research.’ [laughs] Well, you’re not usually confronted with that sort of research student. And so, Vaughn said, ‘Well what can you do?’ And he said, ‘Well I can...’ Vaughan said, ‘What I really want is a tunnel.’ ‘Easy,’ says McCall, ‘I know all about tunnels, mining and so forth. I’ll dig a tunnel for you at Skautbreen.’ So he did. And so, there we were with this team of Cambridge undergraduates armed with miners’ picks digging this tunnel dead straight, straight as a die, right in to the glacier, right to the bed. And they reached the bed, and there was a great big moraine, you know, the pile of rocks at the end of a glacier, a great big moraine at the end of this glacier, on which John McCall could put his theodolite and look straight down this tunnel. And every day he would go to the moraine, look through his theodolite, and look at a, what he called a holy cross [American pronunciation] which was a, a cross bar thing that the student would hold up, and he would direct them to move the sign
on the cross, either sideways or vertically, and that was to be the middle of the next
day’s work. And in this way he had made the tunnel absolutely straight. Now of
course, the tunnel was under pressure from the ice, and so it was going to contract.
And it did contract.

[03:19]
And, I guess John Glen and I went out to have a look at this tunnel. I can’t honestly
remember what year that would be, but I could look it up. And, we went and stayed
there for several days, went out and looked at the tunnel, and, John McCall had
finished it, and, I suppose he must have surveyed it very carefully, and so well did he
do it that there was time left over to dig another tunnel higher up the glacier, which he
did, and also got to the rock bed. So we were able actually to look at it. And all kinds
of geologists came along to the tunnel and peered at this rock face. I don’t know that
we learnt very much from it, but there it was, the contact between the ice and the rock.
So that was the tunnel.

[04:09]
In another description of this, there was an ice, a cold laboratory in the side wall of
the, of one of the tunnels, do you remember that, and what it was used for?

[pause] Perutz had a, I think, a sort of ice lab on the Jungfraufirn. I remember seeing
photographs of it, I never saw the actual place. But I think it involved Perutz sitting at
a bench, heavily padded, especially his feet, and, parkers an inch, you know, feet
thick, looking through his microscope at ice specimens. But I think that, that was on
the Jungfraufirn. Now I think Ray Adie also did some sort of work like this, but I
don’t actually recall a chamber in the Norwegian glacier. There may have been one,
but I don’t think I knew about it.

I think that someone said that, perhaps John Glen was looking at sections of ice in
this thing that...

Well if it existed, John would have looked at them, but, I don’t know. If you’re going
to interview John Glen, you must ask him.

[05:23]
And... OK. And, do you remember whether the geographers, the geologists, the engineers on this trip, approached the study of this glacier in different ways?

[pause] Well Vaughan Lewis certainly approached it from the point of view of a geomorphologist, and not from the point of view of an engineer. John McCall approached it from the point of view of a mining engineer. It was a job to be done. He did I think write a paper on it, but it wasn’t, you couldn’t, I don’t think you could detect a sort of, attitude towards glaciology in it. I really don’t know how to answer your question.

[06:17] Mm. Well I, in the, there’s a little sort of, I think it might be a Royal Geographical Society publication on this tunnel work, obviously published from the point of view of geographers mainly, but in the preface Vaughan Lewis says something like, ‘This trip was remarkable for the fact that it was very interdisciplinary and that we had geologists and geographers and geophysicists and so on all working together.’ But I wondered how those different kinds of scientists sort of marked themselves out at the time by what they were doing there.

Yes. Are you sure you’re talking about the tunnel into Vesl-Skautbreen and not the tunnel into Austerdalsbreen, which was an entirely different mater?

I’m...

There are two tunnels.

Did you, did you go to both of these?

Yes.

Ah.

Do you think it might be Austerdalsbreen?
Could be, yes, yes.

Well, Austerdalsbreen is a very different story. There, my friend Bill Ward picked out a glacier which was remarkable because it showed so beautifully the so-called Forbes bands. They're rather like tree rings that you see on a glacier, you see them on the Mer de Glace spectacularly in France, near Chamonix. And, this glacier, Austerdalsbreen, had these beautiful bands on it, and it had two superb icefalls coming in. And at the feet of the icefalls the glacier went into a series of waves, and these were well known and they were called pressure waves. And the idea was that the glacier was pushing down on the, the icefall ice was pushing down on the other ice, and rumpling it up into these rumples. And, Vaughan Lewis organised yearly expeditions to Austerdalsbreen, which was I think primarily really to educate the students, the geography students. And his idea of doing this was to take all the gear, the theodolites, plane tables, levelling staffs, and go out there and just use them. There was no real plan to it. But I was invited to go and have a look. [08:46]

So, I went out in 1955 I think it was, and, he had got this idea also having been successful on Vesl, the small cirque glacier, Vesl-Skautbreen, to do the same thing at the foot of the icefall on Austerdalsbreen. Now this icefall is about 1,000 metres high, a very spectacular thing. And, the idea of digging a horizontal tunnel into the bottom of it was really rather ambitious. And so, the Cambridge students were assembled with their miners’ picks, and, in they went to dig the tunnel. There was a, what we called the tunnel camp, which was actually on the glacier, in a moraine there. The people in Cambridge, back in Cambridge, were a bit worried about the dangers, because of, in a moraine the ice is melting around in the summer, and the boulders shift, you know, huge boulders sitting on little tables, and there they were camped among them. But it was OK, it didn’t, they didn’t have any casualties of that sort. And the tunnellers worked with a will, and Bill Ward was there, and the idea was that the tunnel was going to collapse under its own weight, and John Glen undertook to measure this very very carefully, how fast it was contracting. And Bill Ward undertook to lay a horizontal pipe along the tunnel, so that when it finally contracted, they would have a marker in the tunnel and they could push an inclinometer through it which would measure its inclination and therefore its shape. And it all sounded very, very reasonable. And we went, we went there and made all these measurements.
I, I was interested in these bulges that happened at the bottom, which they called pressure waves. I couldn’t see how they could possibly be due to pressure, but that was the prevailing wisdom. And I remember going over... You said, what were the interactions with the different disciplines? I remember tramping over these with Professor Hollingworth, the geologist from London, and I said... He said, ‘Oh well, there’s a push coming from over there.’ Well I knew perfectly well that stresses acted in opposite directions. You had a push coming from there, you had another push coming this way, and it was called a compressive stress. Hollingworth just said, ‘Well there’s a push coming from over there, that’s making these waves.’ And I said, ‘Well if you put a, if you drill a stake in here, and you put another one there, and measure the distance between them, come back next year and see what it’s, how it’s changed, you could test your theory.’ ‘Yes,’ he said, ‘but hardly worth it is it?’ That was the geological approach. Which was not unreasonable, because, geologists deal with rocks, which deform extremely slowly. You can’t do with them what you can do with ice, which is after all just another sort of rock, but it deforms faster. So, I was all for putting in stakes and measuring them, and seeing how the ice was deforming.

And... How did it happen? Vaughan Lewis organised these expeditions and he assembled his geography students, geomorphologists, Cuchlaine King, whom we mentioned, and, a nice man called Sharp, who was on the staff of the Geology Department, and then there was another man back home in Cambridge who transcribed all the theodolite readings and worked them out, which is difficult to do on a glacier, because you need seven-figure logs. And... Jackson was his name. Got very little credit for it. But he worked it all out back at Cambridge. And I was there trying to decide what it all amounted to. And I remember in 1955 going around with a prismatic compass on the glacier surface and measuring, making a little map of the, of these bulges and how they curved. And then thinking about it.

And I came back in, probably ’56, and by that time Vaughan Lewis had had a nervous breakdown, and, just not doing anything about the expedition. And I was there all fired up, and, he was in no state to organise things. So, I with the help of Bill Ward tried to work out what to do. And I worked out a scientific plan, and Bill Ward did a lot of the organisation with the students, about their travel and their various grants, and so on. It was very informal. And we had, we an enormous number of students,
we were, we were there for, well over a month, I think about, maybe as much as two months. And I think, there were maybe fifty students involved over that period, not all at once. So it was quite an organisational feat. And Bill, Bill turned out to be the field leader, and I turned out to be the scientific leader. Nobody appointed me, there was no committee or anything, I just, it just happened like that. And I just proposed this, and, I was never, there was never a meeting to say, ‘Yes, this is what we will do.’ But I was the only one who was, well, organised enough really to say where to put these stakes in. Bill told me how to drill the holes, and how to make a hole in ice, which was, I watched them doing it the year before, and it was terribly tedious to make a hole in the ice and put a stake in. And the reason was simply that the drill bits were too blunt. Bill knew how to sharpen a drill, he was that sort of an engineer. And, you did it with a brace and auger you know. And, his, his drills just went, very much through, like cheese, went through the ice, and we made the holes in no time. And all the students were busy drilling these holes and in went the stakes and I invented a way of marking the stakes by going to the suspension bridge and using a, a machine, Clifton suspension bridge, and using a machine there for embossing little strips of metal with numbers. And we pinned these onto the stakes, and we measured, we put the stakes in a line, all down these, longitudinal line, across all these ripples. And I expected, if they were right about the pressure waves, to see the stakes getting closer together where the bumps were and further apart where the troughs were. And I looked at the measurements, and they showed no such thing, there was not a sign of any connection whatever with these bumps. So, I had a line of, I think about twenty stakes down the middle of the glacier, and then I had special places where there was a square of stakes, and I could use my tensors to work out from the motion of the stakes what the distortion was laterally as well as longitudinally.

[16:39]

So there was Glen measuring the deformation of the tunnel, and there was I working out the deformation of the rest of the glacier. It was a very good combination actually, because Glen’s stuff was done really meticulously, and indeed I was, although I had laid out the stake pattern and made the first measurements, I didn’t stay out the whole time there, and laid out a kind of, plan of campaign, and Glen duly followed my plan and made absolutely meticulous experiments, observations on these stakes. We had to measure not only the distance between them, but how tilted they were, and how much melting there had been. Because, we had no idea at all on what
sort of accuracy was needed, really no idea. We didn’t know how fast it was moving, or anything like that. So we just had to do everything as accurately as we could. We just didn’t know what kind of dimensions, what magnitude we were measuring. Because it took a long time of course so it wasn’t, you couldn’t afford to do pilot experiments and that kind of thing.

[17:54]
So there it was. And then I brought all these measurements back to Bristol, and suddenly it dawned on me as I was working on the results that the bulges were nothing to do with the deformation of the glacier; they were all to do with the melting of the ice as it came down the icefall. Which was an amazing thing really, because, it was a very... We never thought of it standing on the glacier, we never thought that ice could go from the top to the bottom of the icefall in the order of one year. I mean that’s an enormous speed, considering, compared with, say, a few, well a few hundred feet on the main glacier surface. It was beyond the imagination of any of us. But actually that’s what happens.

[18:46]
And could I explain to you how the bulges are formed?

*Mm.*

Well, I have to demonstrate with my hands, which the listeners can’t see. But I’m holding my hands out at a certain distance apart, and they represent the distance, they represent a block of ice at the top of the icefall. The front hand comes towards the top of the icefall, and it starts going much faster. So the whole piece of ice is stretching. And it goes on stretching, and then eventually at the bottom the back hand catches up with the first hand. So that it, while it’s in the icefall it’s enormously stretched. And that’s why the icefall is full of crevasses, seracs. It’s a very jumbled mess.

[19:30]
Now, there are two kinds of pieces of ice that go down. One piece of ice is in the middle of the icefall in the summer, when it’s melting all the time, and the other piece of ice is kind of more lucky, and it’s in the middle of the icefall in the middle of winter when it’s not melting at all. So the bit of ice in the summer is getting melted, and the bit of ice which is fortunate enough to be in the icefall during the winter doesn’t melt at all. So when you get to the bottom of the glacier, the lucky ones are
the bumps, the high bits which haven’t been melted, and the troughs are the bits that have been in the icefall during the summer. Now, how did we know that the ice could go down the icefall in as short a distance as one year? I think it’s slightly over a year actually. Well, we used, I used a box, not a box camera, an ordinary bellows camera, and set it up on a rock outside the tent and took a picture of the icefall. We were camped at a reasonable distance from it, I don’t know, a quarter of a mile perhaps, even half a mile. And, took a picture in the morning and another one in the evening, and then another the following morning, and the following evening. And when I left, gave strict instructions that these pictures were to be taken from this exact spot on this rock every morning and every evening. And finished up with, I don’t know how many, about, as many as 100. It was about, a few months, I suppose, maybe, something less than 100 pictures. Which I made into a movie. And, you could see the ice tumbling down the icefall. But more importantly, by making measurements on the negatives, I could work out what the speed of the ice was in the icefall. And of course it was vast, far more than the few feet a day, three feet a day at the bottom, which we could measure, but it was, I don’t know, hundreds of feet a day in the middle of the icefall. Quite enough to make it come down the icefall in, of the order of one year. Which then became the keystone of the theory about how these waves were formed by melting and not by pressure.

Yes.

[21:59]
So, I then had this idea suddenly, and went out on the ship the following year with John Glen and Bill Ward, and I remember on the ship going over from Newcastle to Bergen I had a little conference with them and explained this idea, and said, ‘What we have to do is not what we were planning to do at all. What we have to do is reconstruct the stake positions as we had them last year.’ And of course the ice had moved on, so, we had to go back to the exact place in space. There was no GPS in those days, you went to the actual spot and drilled another hole and put a stake in, and watched the line of stakes for a second time. And, was it going to be the same movement as it was the previous year? And such questions. And thereby verified that this theory was, did explain the, the whole idea of the crevasses, of the waves.
So I published this idea, and it’s been, it’s held, I think it’s still accepted now. And, a clever fellow at University of Washington, a good friend, realised that mathematically what was going on, I had written it down in the form of integrals, which I had interpreted, and, he recognised that what I was doing was what in mathematics is called convolution. Now, I at that time didn’t know anything about convolution. Nowadays it’s just bread and butter to any student, you speak about a convolution, he’ll know immediately, a physics student, he’ll know immediately what you mean. I didn’t know about that. And, so in this paper, although I was talking about convolutions, I didn’t know I was talking about convolutions, and the way the waves are explained now is properly by convolving the velocity function with the, with the, you might call it the climate function, the seasonal summer-winter oscillation. So that’s, that was the Austerdalsbreen main result so far as I was concerned. So really, looking back on it, and being very wise after the event, there was no need to go there at all, you can just dream it up at home. But it needed the stimulus of being there to do it.

Now interestingly, the tunnel did indeed collapse, and, what happened? The pipe that Bill Ward was going to put in the tunnel turned out to arrive late, so he wasn’t able to put it in the tunnel. So, rather than do nothing at all, he put a piece of telephone cabling in along the tunnel, just to mark its position. And went out again to see what had happened to it. Totally invisible. There had been an avalanche during the winter which had totally buried the whole lot, and no sign of the tunnel entrance or anything. And it was only years later when we went out and we found this piece of wire which had been in the tunnel and had been compressed by the tunnel. And, imagine what happens to a piece of insulated, tough cable when it’s required to compress along its length, which it’s not accustomed to do. Well what happens is, that the insulation splits and gets concertinaed up in sections, so there was a series of concertinas of insulation all along the wire. It’s kind of interesting.
[25:55]
But, the, the tunnel just, just disappeared, and was never seen again. And a ladder, an aluminium ladder had been put in it. The ladder was found later in an absolutely mangled state, completely mangled up. And when we revisited the glacier as kind of tourists not so long ago, there were pieces of the pipe down towards the end of the glacier, which were seized on by the people as being relics of our experiment.

*Gosh.*

Actually that wasn’t the original pipe, that was a different one. But they moved down the glacier.

[26:45]
*And so, the 1956 trip when Vaughan Lewis had dropped out of it because he’d taken ill...*

Yes.

*Who were the students who were going, were they still students from geography?*

They were mostly students from the Cambridge Geography Department. But I did, since I was at Bristol, I did recruit one or two of our physicists.

*And 1955 then, was the first time you went to this...?*

I think so, I think it must have been ’55, yes.

*Yes. And, do you remember the presence there of Dick Grove and Jean... well, Dick Grove and Jean Clark at that time?*

Jean Clark. Yes, they weren’t married then. I’m not sure they actually went on Austerdalsbreen, because their stamping ground was the Josterdals... was the, sorry, Jotenheimen, where the cirque glaciers were, and they had been very much in the
picture with the earlier tunnel into the little cirque glacier. But they didn’t have anything much to do with Austerdalsbreen, as far as I remember.

[27:50]

*Mm.* What do you remember of them in terms of the cirque glacier tunnel?

Cirque glacier? Oh they were very friendly and, they, they helped me. I borrowed windproof trousers from Jean Clark, and, they, they slid around on the snow in a very spectacular way. I think Dick Grove was much more interested, wasn’t he, in tropical Africa than in glaciers, but Jean Clark as she then was, was a thorough-going glaciologist and she went for this rotational slip business, and the, and the slip bands as they were called, which could be seen in section in another glacier in the Jotenheimen, which was bordered by a lake. So you could really see a longitudinal section of this glacier, and there were these beautiful curved bands, just like in the textbooks. Lovely. That was her stamping ground.

[29:04]

*Mm.* Thank you. Could you comment on the, I suppose the, the relations between the Departments of Geography and Geology and Geophysics and Physics at Cambridge at this time, in terms of, in particular the status of geography at that time?

That’s very hard, because, of course I was not in the higher echelons, going to the committee meetings which were discussing all these things. So it was all very much second-hand. The Mineralogy Department, as I explained, was split between people who adhered to the Cavendish where they did their X-ray experiments and the old-fashioned morphologists who looked through microscopes. The Geography Department, as I think we were saying informally, not on the recording, was composed of people who were doing research, but they were, because they were at the research stage, they were really doing other specialisms, and I mean specialised things like meteorology or geomorphology, which were established subjects in their own right. And somehow the geographers wanted to do everything, and they, I think they had a struggle to get geography established as an undergraduate discipline at Cambridge, and having had the struggle to get it established as an undergraduate discipline, they then had the further struggle of having it established as a graduate
research student activity. So that was the kind of place it was, that was its status. It had a connection with geodesy and geophysics, because, geodesy was to do with very accurate surveying, and using theodolites, I mean, really accurate theodolites, which was another, in a sense, another thing geographers wanted to do. But, it was being done in a very professional way at the Department of Geodesy and Geophysics. Geophysics in those days didn’t mean, I, I could be corrected on this, but I don’t think it meant very much more than the sort of seismic surveys that oil companies do to examine strata near the surface. There was of course Jeffreys’ book on the Earth, which was, a lot of it to do with seismology. And they were concerned with that. But the, the whole idea of studying the whole Earth as a geophysical object was in its infancy.

*Mm. Yes, thank you.*

[end of session]
OK, well this is the start of the second session, and I understand you have some further comments on session one that you’d like to add.

Well, the first point that occurred to me was in connection with the tunnel on Austerdalsbreen, which as you may recall was the idea of Vaughan Lewis, who had already had this tunnel in the small cirque glacier, Vesl-Skautbreen, and dug by John McCall. I’ve got some pictures of that actually I can show you. Now that one went of course horizontally, and, being a small cirque glacier, the glacier didn’t move very fast. It both moved forward and rotated, which was the whole idea of Vaughan Lewis’s work. But the forward motion each day would be measured in, I guess centimetres. Then he had the idea of digging this more ambitious tunnel into the foot of a 3,000-metre icefall on Austerdalsbreen, slightly to the west of the Jotunheimen where the other one was. Actually that glacier had been picked out by Bill Ward as suitable for such a project. So they, as I explained I think last time, they dug a tunnel in horizontally at the foot of the icefall. But it was very different from Vesl-Skautbreen, because, there the ice was moving, I, I speak from memory, but, at about one metre a day, which is of course vastly larger than the smaller cirque glacier. Now we all should have realised what that implied, but we didn’t. And we didn’t know it was, none of us knew then how fast it was moving because, of course you take the readings with the theodolites and they’re all worked out back at Cambridge, and you don’t know what the answer is until you get back home. So, we really had no idea how, actually how fast it was moving. But as I explained I think, it was moving fast enough for the ice to go to the icefall in something like one year of that timescale. And because of course the ice is levelling out as it gets to the bottom, it’s all rotating, so the horizontal tunnel that they were digging was rotating, and if you think about it, it’s rotating in the bad direction, it’s getting steeper and steeper inwards.

Mm.

So water, which of course is there all the time, melting all the time, is gradually accumulating at the end of the tunnel. So the tunnellers found themselves working in a big puddle, and so they tried to make the floor behind them more horizontal to get
the water out. And Bill Ward had this pipe that he was going to leave in the tunnel to mark its position, and he used it as a siphon. So there was a tremendous moment when much of the water was siphoned out from where it was. But still of course it went on rotating, and finally it had to be abandoned, and that was what went wrong with that experiment. It was, it never reached the rock wall behind, and it never would have done without tunnelling very very much faster than they were going. And, so that was something we should have foreseen, but none of us realised how fast the ice would be moving. So that’s what scuppered that, that particular aspect of the experiment. Not that it didn’t yield results, as you know, but, that’s what happened.

[03:56]

_I see, thank you. And is this the icefall which you filmed using..._

Sorry, the icefall which...

_Which you took photographs of..._

Yes.

...and then collected them together into a film showing that rate of movement.

Precisely. But until the photographs were developed...

_Yes, of course._

...and put together, we had no idea.

_Mm._

Yes.

_Do you still have that film some...?_

Yes I’ve got it in the lab.
Ah, I see.

Yes. Yes. It was shown, we had this kind of reunion, not reunion but, commemorative meeting in Norway only a few years ago, and I showed it at that meeting.

Ah. And what does it play on? Have you sort of converted it into...?

It’s, 16mm, as I recall.

Right. Mm.

I have a, I fetched from the lab yesterday a reprint to give you about that meeting that we had.

Ah, great.

And which is a kind of, a history of what happened on Austerdalsbreen.

Lovely, thank you.

For your interest.

Mm.

[04:58]
So that was the first point. Now the second point refers to quite a different thing, that is, my book on, Physical Properties of Crystals, and I think I may have told you the story of how I was somewhat critical of Dr Wooster’s way of presenting the crystal physics course, and how I was insistent on changing the notation. It seemed a trivial matter in a way, and in fact it was trivial, but, it was crucial to the members of the department, and, it all depended on the order in which two suffixes appeared, i and j,
whether it was \(ij\) or \(ji\). And I have a copy of the book that we had out just a moment ago.

*Underneath that sheet of paper I think.*

And after, this is four years later, and after I had written it, the Oxford University Press sent me what they called an advanced copy. And on page seven you will see a discussion here of the order of the suffixes. And on the crucial equation where the, it’s explained, very carefully explained why the suffixes have to be in a particular order, they made a misprint, which had not appeared in the previous proofs, but had been done at Oxford University Press.

*Ah.*

Now, it’s a most extraordinary coincidence, I can’t believe that there was a kind of spy in the Oxford University Press. I think it was purely accidental, but it just happened on that crucial point that they switched them round again. I think there’s probably a footnote saying that that’s not why they have to be the way they are.

*Mm.*

But they immediately, as I wrote in the front of the book there, under the flyleaf there, a revise was later printed.

*Mm.*

So that’s a unique copy.

*Ah. Yes, I mean, it’s difficult to imagine that, that Wooster might have had a, a contact at Oxford University Press and...*

Well indeed. Indeed it’s quite inexplicable, it’s just the most extraordinary coincidence that you could think of. And, I mean the book is not full of errors as you
could imagine with Oxford University Press. I mean we later obviously discovered errors that were put right, but that was a, an extraordinary thing to happen.

That it should be the controversial one...

Absolutely, yes.

Mm. Because you mentioned that Wooster came back early, having failed to get his visa, just at the point you were sort of pointing out the revisions you were making to his course.

Exactly, exactly. Yes. Yes, so...

So this, this could be his sort of, small act of revenge if he, if he had any contact at OUP, but probably not, probably just an amazing coincidence.

Not only probably not, almost certainly not.

Yes. [laughs]

[laughs] I’m not... I, I think it, I think it was just pure coincidence.

Gosh. Thank you.

Thank you very much. Those were the two points that occurred to me that I wanted to make about the last time we met.

[08:22]

Wonderful, thank you. And I’ve got some follow-up questions from last time as well. The first one is, I wondered whether you could tell me anything more about a person that you mentioned who was the Cavendish lab when you were there, and it was a Mrs Tipper who used to walk past you when you were working on an experiment and saying, and said it won’t work. Now, this I think is Constance Tipper, who later became Constance Elam [pronounces erlam], as far as I can tell. And, I wondered
whether you knew anything more about her or had any other descriptions or stories connected with her.

I think it was Elam, e-l-a-m.

Thank you.

No, she worked in the engineering laboratory and we, we saw her from time to time, but it was a purely incidental contact. She didn’t, she didn’t follow what we were doing in any detail. But it was simply that she was interested in, I suppose, creep of metals and general behaviour, general deformation behaviour from a metallurgical point of view, and we were approaching the same topics from a more physicist’s point of view.

Mm.

But, there was no more contact than that.

Mm. And, you mentioned last time that it would have been unusual, that women in crystallography at this time were starting to happen, but that it would have been unusual for, it was unusual to have women in an engineering department. Do you have any sense of how she came to be working in the Engineering Department?

I think it was unusual to have women in science at all, and probably even more unusual to have them in an engineering department, full of big machinery. I have no idea how she came to be there.

[10:20]

Mm. Thank you. Also, at the end of track 1, we’d just got to the point where you were meeting C P Snow, and then that track ended and then when we started track 2 we had sort of, skipped over that a little bit. I wonder whether you could describe the meeting that you had with C P Snow where he talked about the war effort.
He happened to be chairman of, what was it called, the Central Register. The Central Register was a compilation by the Government of the available scientists within the country, as a kind of, I think, as a kind of reaction against what had happened in World War I, when they found that the scientists they wanted had been called up and were in places where they couldn’t do science, and when they were wanted for a particular problem that turned up. And they decided that that shouldn’t happen again in World War II, so they... and they wanted to use their scientific talent. So they set up the Central Register. C P Snow was the, was in charge of it, and, he was obviously very much hands-on, and he did the interviews with, certainly with the Cambridge lot, and I was one of the Cambridge lot. So, we had a brief chat on what I could do and what I couldn’t do, and what I liked and what I didn’t like I think. And presumably that was the basis of some sort of entry in the register.

_Mm._ What do you remember of him as a, as a person, as a...?

Simply as an interviewer.

_Mm,_ yes.

Asking the usual sort of questions that interviewers ask.

[12:10]

Thank you. Now, a couple of times in the interview last time you, you mentioned, this is before you were at university, at the age that you were before you were at university, you mentioned looking at people talking together and wondering what they were talking about. And then, later in life realising that they were talking about themselves. And the first time that you talk about this was, the art school at Stowe, Robin Watt’s wife Dodie, and the boys used to go and talk to her, and you remember, you remember drawing and looking at them talking and wondering what they were talking about. And I thought this was interesting in terms of your sort of, memory of yourself at that age. And I wonder whether you could say more about your sense of yourself then as being slightly backwards socially, _I think you said, another point. I wonder whether you could expand on, on this, this, the fact that you couldn’t understand, you know, these kind of, sort of casual social relations._
Yes. Well I guess I was a lonely child, and, didn’t know how you made friendships with people. I mean occasionally it happened, but, I wasn’t very much self-aware in that way. And, it was a kind of, disability if you like, which dogged me all the way through my school career, and which dropped away when I went to Cambridge. In fact I remember fairly distinctly the sort of period, brief period in which it did drop away, and I, it was a revelation to me. Strange, it was a strange thing to happen, but it did.

[14:13]

What changed in that brief period where...?

It was probably a girlfriend actually, my first girlfriend I think.

Yes, I, I’ve made a... Could you...

Audrey was her name.

Mm. Could you tell me about the, about meeting Audrey, and about that relationship?

She was reading mathematics in the same year as I was. And, you know, it was difficult to meet girls as a raw undergraduate, and, I think we went, we certainly went to the same lectures. And I remember asking her to come and go to a meeting of the Student Mathematical Society with me, and she came up before the meeting and we had the usual Cambridge coffee, always had coffee, or cocoa. I think coffee wasn’t available, I think it was cocoa we had probably. And, and talked, and went to the meeting. And I remember being, sort of thinking beforehand, what on earth are we going to talk about? And rather consciously sort of setting up a, [laughs] an agenda, to get me through this social situation. But, I became, I became on easy terms with her, and I think it was that which probably helped me more than anything else.

Do you remember what you put into your agenda?
I think it was probably hobbies. Are you interested in photography? That kind of thing.

*Mm.*

I can’t remember anything else.

*And, and through getting to know her, you found that, it was possible to talk about yourself, is that what...?*

Probably, yes. Yes, I think so. Yes. I, I mean, I knew her for a very long time, I still keep up a correspondence with her. And, so, it lasted a long time.

*And, could you say more about the sorts of things that you did with each other after that initial...?*

Well we, at Cambridge what you did was ask people to tea in your rooms, and you went for walks with them. You went on what was called the Grantchester Grind, which was the path to Grantchester, and maybe you had tea at the other end. You went to meetings of societies in the evening, and you invited the girl back to your room afterwards for cocoa. [laughs] That was the kind of thing you did.

*And do you remember what you tended to talk about, or...?*

I guess ourselves, as I was saying. What did we talk about? She, she had a great difficulty in her final year with her mathematics Tripos, and, I think needed some kind of friendly support, and by that time I was much more feeling in command of myself, and, I think I helped her and she certainly helped me. She married a man who was much older than she was. He was, he was, I don’t think bursar, but, anyway a financial wizard in Selwyn College, a don. And, he was about forty-six I suppose and she was twenty-two or something like that. And I thoroughly disapproved of this of course. [laughs] But, I later met him in curious circumstances. When I went to America we were on the same ship, the Mauritania, going from Southampton to New York. He was travelling first class, of course, I was travelling in a lower class, not, I
think I, I think I was in what they called tourist class, I can’t remember quite. But he invited me to dinner in first class, and, I remember very well the dinner we had. He ordered a steak. Now this was a time when you didn’t have sort of, that sort of food. And, he ordered a specially thick steak, you could get those sort of things on the Mauritania. And, talked about how he went to New York and how he managed to avoid the Customs by getting into the pilot boat with J G Crowther, and, he was an important chap, obviously. And then, I remember him saying rather, rather interestingly, ‘I do hope I can make Audrey happy.’ And, I just remember that.

[19:40]

Mm. Thank you. The teaching of physics at Cambridge, I wonder if you could tell me more about. We talked mainly about Sir Lawrence Bragg’s lectures, but I wondered whether you remembered anything of Sir Harold Jeffreys and his lectures?

Yes, Harold Jeffreys lectured on probability, and he had written a book on probability. He... I don’t remember him as a particularly good teacher. He was obviously a very nice man. I just remember a few things about the course. One of his illustrations at the end was to do with how the Tripos papers were marked, and the distributions of the marks, and the classes. He did fairly elaborate statistics on these, and came to the conclusion that the examiners were looking not for a particular numerical mark, but looking for a gap in the marks. And that’s how they chose the class boundaries. And that was a result of his statistical analysis, which was satisfactory to him, but I mean, [laughs] that examiners do that, is something that I learnt from experience afterwards.

And, did he talk about the earth sciences at all in terms of...?

None, not in the lectures I went to. They were not on geophysics.

Yes. I see. And, could you tell me about his, his wife as a lecturer in the department?

Yes, Mrs Jeffreys. She lectured on quantum mechanics, and I, I got, I got very little out of the lectures. I mean they may have been good lectures for people who were better than I was, but I couldn’t, I couldn’t engage with them particularly.
Could you describe her?

Do you mean physically?

Mm, well, yes.

She was particularly ugly actually. [laughs] But... She... Otherwise, a pretty neutral sort of character I thought. I don’t know how Harold would have proposed to her.

And the only, the only female lecturer in the department at that time?

Probably... No there was, there was Mrs Houghton, Mrs Houghton was jointly in charge of the third year practical class. I don’t remember much about her actually. But I’m trying to think of any other women lecturers and I can’t. I don’t think there were any. There were, there were women crystallographers around, as I mentioned. Helen McGaw and Audrey Douglas. I shared a room with Audrey Douglas in the Mineralogy Department a little later, but she certainly wasn’t on the Cavendish staff when I was being an undergraduate.

[22:54]
Mm. Why do you, why do you think it is that crystallography was the particular kind of science that brought women in to the Cavendish for example, as opposed to any other branch? What is it about crystallography in particular?

I, it was probably partly personal influence from people like Kathleen Lonsdale. I say like Kathleen Lonsdale but she was perhaps unique. I think it was also pattern, and the geometrical cleanness. It wasn’t a dirty subject in any way. It was a careful, meticulous, rather well-drilled kind of subject. And I, I think that’s perhaps what attracted women.

Why would that cleanness and order have attracted women rather than men?

I think you must answer that question. I just think it’s, it’s, it goes with... You know, women are very good at, say, botanical drawing, careful, meticulous use of a pencil
drawing. That kind of meticulousness that I think women are, are better at than men. And that’s, some of that is what is needed in crystallography.

And, what then, in terms of the, the machinery or, or the grubbiness of other subjects, what made you feel that that sort of repelled women in a way, or, or put them off, or...?

You’re really asking leading questions aren’t you?

Yes, sorry.

[laughs] It’s purely conjecture on my part. I mean I think if you asked them, they might have said, ‘Well it’s, it’s rather nice and cleanly geometrical, isn’t it?’

Mm. Did they talk? I mean, you shared a room with Audrey.

Yes.

Did she talk about the fact that she was female in a, in a scientific world that was...?

Absolutely not.

No.

Absolutely not. No, there was no kind of recognition actually that they were any different.

So no self-consciousness of being female in a world that was male, generally?

If so, it was very much below the surface. It certainly was not overt.

Mm.
No, they were, they were treated, as far as far as I know, identically, and, sort of, horrible to say, but as honorary males.

[25:32]
Mm. And, you said that you suspect that Kathleen Lonsdale was both unique and influential. Could you say in what ways you think that she would have been influential?

I suppose because she was a pioneer, and would be a, a role model. She was, she was the first of her kind. I think she was the first woman FRS, as I mentioned before. You would have to, you would have to check that. I think so. And, well she was somebody who was clearly going to inspire people, and if she could do it, and you were a woman, why shouldn’t you do it?

Mm. Could you tell me about Audrey Douglas then just as a, as a person, as someone that you interacted with?

I remember her very little actually. She married a, a colleague, Sandy Douglas I think his name was, who was doing research either in the Cavendish or closely associated with it. And, I really can’t tell you much more about her.

[26:56]
Mm. Would you be able to tell me about the reliable Crowe, all you know about him as a person? He appears in the last recording as the assistant to Sir Lawrence Bragg. I picture him setting about making equipment, following instructions and that sort of thing, but, I don’t know anything about him as a person, and I wonder whether you did.

I know very little about him as a person. Did I say the story in the last interview we had about Coslett’s book on electro microscopy?

No.
I simply remember that, the Cavendish took delivery of their first electron microscope, I think it was made by EMI. And, Crowe had got the job of setting it up and making it work. And, while he was setting it up, Coslett, who was the academic who knew more about electron microscopes than anybody else and wrote the standard book, came to Cambridge. And I recall asking Crowe, or somebody else perhaps asking Crowe, ‘How are you getting on with Crowe with the...’ it was always surnames, ‘How are you getting on with Crowe with setting up that electron microscope?’ ‘Well I’m getting on a bit quicker now,’ he said, ‘especially now I’ve got Coslett to help me.’ That was Crowe. I didn’t take to him particularly.

[28:35]
I’ll tell you a little story about another technician in the Cavendish if I may. He was the man in charge, what was his name? Maybe you know. He was in charge of the stores, and in charge of... and generally in charge of the lab from a technician’s point of view. And I remember, the equipment was extremely primitive, and, I needed a piece of wire to join two terminals together. So I went down to the stores, and said, ‘Please, I want a piece of wire.’ ‘Ah, see what I can do,’ he said. And he went into the back of his stores, and he came out holding a piece of cotton-covered, rather screwed-up wire, and said, ‘Here, Rutherford used this. Let me have it back.’ [laughs] The general attitude in the Cavendish towards apparatus was actually set out in writing. When you went in to the third year lab you, you read that the apparatus you will be given is no better and no worse than the apparatus you will use when you do research. And, you then went in to a lab with rickety old tables, very much worn, and, what you learnt was that every instrument was inherently unreliable, and you mustn’t trust anything. You had to test for yourself that the instruments were working properly. It’s so completely different from now, when you take a black box and you read the digital output, and if it doesn’t work, you call in the engineer. I mean that’s totally against what was then the Cavendish spirit. They call it, they called it string and ceiling wax; indeed it was. And we made things for ourselves, we, we did our own glass-blowing. We were taught workshop... Well was I taught workshop practice at Cambridge? I must have been. Yes, because I did turn things on the lathe. It wasn’t very systematic but I did learn how to do it, and, not to file off the tops of screws and lose their slots, and, not to get my tie caught up in the, in the lathe as I leaned over it. And, take the chuck key out before you turn the lathe on. That sort of basic law. We learnt it all for ourselves. There was no health and safety.
Yes, if you, if you left the chuck key in, it sort of flies round and hits you in the face.

That’s right. Yes exactly.

So it was a sort of, closer relationship to the equipment that you were using than you would have now?

Oh yes. Yes.

And do you know anything of the sort of background of the assistants, Crowe and the man who was in charge of the stores, in terms of their, their sort of educational background or social class or...?

No, I don’t. I don’t. No, I, I’m afraid I... I wish I did, but I, I can’t answer that.

Does that mean then that they didn’t have coffee with you in the same...?

I think that’s true. I think they were off, maybe somewhere in a separate room.

Mm.

In the Cavendish I’m talking about.

Yes.

You see, J J Thomson had left money to buy free tea and buns for the whole of the Cavendish staff, and, so we all went into a big room and sat round the edges rather formally and had our J J Thomson tea and buns. The buns turned out to be rather hard, crusted rolls. This was, this was during the war, and, and some slightly after, when things were equally austere. I bet it’s better now.

[32:53]
You mentioned when talking about the lathe that you were concerned not to get your tie stuck in, which made me think about dress, and, I wonder whether you could tell me how the male and female scientists dressed at the Cavendish.

Well the, the standard male dress was a Harris tweed jacket with, probably with the elbows patched with leather. And possibly the cuffs as well. And a tie, certainly a tie. I think you would have been considered pretty scruffy without a tie. Of course you didn’t wear a gown, all the art students wore gowns to their lectures, but the scientists, and I... but not he mathematicians I think, were let off from wearing gowns. And the reason that they were let off from wearing gowns was simply because there were practical classes, I think. So that was the male dress. The female dress was skirts and blouses I suppose. Certainly not trousers. That would be considered a bit, that would be a bit, what, déclassé.

And, formal, a kind of formal skirt and blouse, rather than what might be worn outside of work? Was there an attempt at formality there?

I think they often wore dresses actually. [pause] Yes, dresses were the thing, dresses or, or skirts and some kind of, a top, you know.

And the technicians, what would they wear?

Technicians? Did they wear white coats? I’m trying to remember. I think they tended to wear white coats.

Mm.

That’s how you could tell they were technicians. Yes. Yes. Yes, we, we had, we had student lathes, and little workshops which we could, which we could use. And we didn’t have the proper tools, and I remember for example, sawing off the end of a brass rod, and of course you got a rough end to it, and then painstakingly filing down the edge and polishing the end. And, that took a long time. But on another occasion I happened to take such a rod into the main workshop and the technician there simply put it into a rapidly rotating buffer, and in about two seconds it came out beautifully.
polished. He could do in two seconds what I was supposed to be doing in about, ten minutes. But I do remember that I had to do experiments as a research student, I had to do, I did experiments as a research student, on silver chloride. And I wanted to make my own crystals of silver chloride. So, you couldn’t just take a bottle of silver chloride, I had to make my silver chloride. So, I, I wasn’t a chemist, so I looked up the chemical encyclopaedia and found the official way of making silver chloride, which was a fairly long process involving mixing two things together and then collecting the so-called flocculent material. And, then, it had to be put into glassware, and the glassware had to be connected by tubing, and the tubing had to go into rubber corks, and in order to get the tubing into rubber corks you had to have a cork borer. So you had to make a cork borer. Which means taking a brass tube, putting it in a lathe, and filing the end so that it makes a sharp end on the end of the brass tube, and then forcing this, or using the lathe, through a cork. And I began to think, this is an awful long way from the experiment that I want to do. And I remember saying to my friends, ‘You know, the Cavendish really isn’t equipped for experimental work.’ And here was I, a humble research student, saying this about the most famous laboratory in the world for experiments. But it was true, it wasn’t equipped for experimental work. I mean nowadays the attitude is entirely different.

It makes me wonder...

[pause in recording – interruption]

[37:47]
I was wondering therefore what the role of the technicians were, if you, you seem to be making everything, and, doing almost everything yourself. I know that Crowe was a technician assigned to Sir Lawrence Bragg in particular, so, does that imply that Sir Lawrence Bragg wasn’t making his own cork stoppers and things like that?

Correct. He would have got Crowe to do it. But you see, during the war Bragg wasn’t doing experiments.

Mm.
And, what technicians there were, were working for the, the academics. Not for the research students. I think that was a distinction. But there was a pretty good workshop actually which would do very good work if you, if you were privileged enough to be able to get them to do it.

*And who started the workshop?*

Oh, high class technical, metalworkers, largely.

*But as a research student, you weren’t, were you able to call upon their services, or would that have been...?*

Oh, my, via my supervisor. Yes, I mean I would design some piece of equipment and he would look at it and say, ‘Oh yes, this is fine, but it’s too elaborate. Let’s simplify it.’ And then he would then be able to authorise it being put through the workshop. I think that’s the way it worked.

[39:27]

*Mm. Now, last time you mentioned that when you were in the basement lab, working in Orowan’s section, you mentioned a couple of people working on a lathe using uranium, and you thought that this was almost certainly a project linked to the bomb. And I wondered whether, perhaps connected with those two or more generally, you know anything about Britain’s role, Britain, British scientists’ role in making and designing the bomb.*

Well actually that incident happened, not when I was working in the basement of the Cavendish but when I was, when we had moved upstairs onto the first floor. I am just conjecturing that, very early in the stages of Britain’s involvement, it was necessary to know what the mechanical properties of uranium were, in particular what would be its, what would be its behaviour when it was plastically deformed. And to do this you need specimens, typically cylindrical specimens, which you put in testing machines or, or creep testing machines, or indeed machines to measure its elasticity. All these crucial numbers were needed, obviously would need to be known. So, this is all guesswork on my part. Now, of course there was, there was total secrecy, and I
certainly didn’t, I never signed the Official Secrets Act and I wasn’t told any secrets so far as I know, and I had no idea that an atomic bomb was being made. But I learnt afterwards that the cyclotron in the Cavendish was being used to make...

*Thanks. [receiving drink]*

Excuse me. [pause] I learnt afterwards that the cyclotron was being used to make certain crucial measurements about the cross section for collisions. And then, pretty well all the British workers on the atom bomb project, which was called Tube Alloys, that was the cover name for it, Tube Alloys, they all went off to USA or Canada. But I wasn’t in any way connected with that, and I didn’t know what was going on.

*Do you know which scientists were involved in the Tube Alloys project?*

I’m sorry to say, I can’t remember the names of the two who were working with the lathe. And I don’t think I will recollect them either. Nor can I remember the names of the people who were working with the cyclotron. But actually that’s a matter of history that can looked up pretty easily.

[42:43]

*Mm. Thank you. I’d like you to talk about, if you could, the influence on your life and career of your other sort of, interest at Cambridge, which was in the, the literary and philosophical experiences that you had in the Ten Club. And, I was in particular interested in your memories of evenings with Ludwig Wittgenstein and, at the Ten Club. So, that’s two questions. So I’ll, I’ll put them to you one at a time. Let’s go for the specific first. Could you tell me about the evenings with Wittgenstein?*

Wittgenstein. Yes. I had a, a very close friend at Cambridge called John Butler, who was a brilliant fellow. He had been, not... He had been turned down for war service on medical grounds, and, it’s perhaps interesting, how he got to Cambridge. He, there was a BBC programme called *The Brains Trust*, on which sat C E M Joad, Julian Huxley, Commander Campbell, and various, maybe, sometimes others. C E M Joad became well-known as a radio figure. His catchphrase was ‘It all depends what you mean by’. And, he said something on *The Brains Trust* which my friend John Butler,
as a schoolboy, took issue with, and, wrote a letter to him about it. To which Joad replied, ‘Well come and have tea with me and we’ll discuss it.’ So John went to Joad’s house, had tea with him, told him that he wanted to go to Cambridge, but his stepfather was against it, and although his brother had, was going to Cambridge, his stepfather, who was a, quite a well-known barrister, was not prepared to pay to send John to, to Cambridge. So Joan wrote to John’s stepfather, urging that he should send John Butler to, to Cambridge. And so he duly arrived at King’s, where he read English in his first year, but didn’t get on with the Anglo-Saxon bit, which was compulsory I think, and changed to moral philosophy. Well, he had a most enormous collection of novels in his room, he had a whole side of a room filled with a bookcase, full of contemporary, mostly contemporary and nineteenth-century novels. He was a very learned chap. He became President of the Union. Quite eccentric in some ways. And, he formed a society called The Contemporary. And The Contemporary invited speakers, nearly all his novelists. There was, Arthur Koestler I remember was one of them, who, who told us that a novelist was like a man who had his feet in a bowl of hot water, and was looking out of the window.

Now quite apart from this Contemporary society, John’s room was frequently full of people, full of people I say, a few undergraduates, drinking coffee and discussing matters, as undergraduates do. And John thought it would be a good idea to invite Wittgenstein round to come and have a chat. Now Wittgenstein at the time was living in a room I think over the Great Gate of Trinity, in a very sparsely furnished room. John told me the only chair he had was a deckchair in which he sat. And he was somewhat alienated from the other dons in Trinity, and was quite glad I think to be invited around to meet some undergraduates. So he came on several occasions to John’s room, and I was invited, and, we sat in chairs, on chairs, and, talked.

One of the people who always came with Wittgenstein was Miss Anscombe, who I believe became his biographer later. [pause] Shall I tell you about one particular evening I remember? Well, we waited, and Wittgenstein came in and sat down, and we waited for him to say something. And there was total silence. Total silence. He had his head in his hands, and we just waited expectantly. Suddenly, there was an explosion. He said, ‘Bloody hell, if all your heads are as empty as mine is, we are not going to get far this evening.’ And at that point Frank Tuohy,a friend of mine who
became a novelist, asked some question about a poem involving Leda and the Swan, and then Wittgenstein was off, and we, we talked about Leda and the Swan. But, you see, his formal philosophical life was all somehow encapsulated to us it seemed in *The Tractatus*, which he had written long ago, and he had no patience with contemporary philosophy, moral philosophy, as it was taught at the time, with logical argument. His conversation was anything but logical, it was all allusive. Someone would say something and he would say, ‘No no no,’ and, and then somebody would say something else seemingly similar, and he would say, ‘Yes, yes, yes. Yes, go on, go on, go on, yes, I think we’re getting there.’ And it was that kind of poetical exchange, rather than any kind of argument about concepts. Which is what I think of as philosophy.

*Mm.*

[50:15]

But it, that’s, it was almost a dramatic effort. He, he was dressed in a very strange way. He wore these towelling tennis shirts, you know, made of towelling material, but not one, but, but several, one on top of the other. And I think they used to rotate from inside to outside. I was told that he often went to the rather grotty cinema in Chesterton, near Cambridge, just on the outskirts, where he would sit in the very front row, and the supposition was that he needed this huge screen in front of him to blot out his thoughts. Whether true or not, I don’t know. He had been a hospital porter during the war, this must have been just after the war I suppose, and, he had been in a very menial job during the war, and then he had come to Trinity where they had given him hospitality and this room. And he was kind enough to come and talk to us undergraduates.

[51:33]

We also had a, a talk I remember from Bertrand Russell at the time. He was writing his history of philosophy, and he had just finished his chapter on Nietzsche, and by way of talking to us, he read the draft of his chapter on Nietzsche. And, we had a little bit of to and fro afterwards, which was fun. And Bertrand Russell actually was a, a figure that I remember quite well, because, we used to invite him to talk to CUSIA, I think I mentioned that society last time we talked, Cambridge University Society for International Affairs.
Mm.

Which organised many meetings. And Bertrand Russell was one of the people we invited on at least one occasion. So he was a figure who became, I won’t say familiar but I remember very well.

[52:25]

And, what did you feel at that time about the relative merits of science or philosophy or literary criticism in coming to understand the world, the sort of ways of understanding the world around you? The relative merits let’s say of science and philosophy.

Well they never seemed to me to be in any sort of opposition. They were just two sorts of things, which were both fun to do. I mean, last time you mentioned C P Snow’s distinction of the two cultures and so on. But I, I don’t think I was quite, well I won’t say I wasn’t aware of it, because I think the undergraduates at King’s did very much distinguish between scientists and the arts people, and they were two rather different kettles of fish. Two different sorts of personality. But, speaking personally, I, I never really felt any, any tension or anything like that between the two. I thought both of them were interesting.

Two different sorts of personality?

Yes.

What were they, could you describe the two different sorts of...?

Well you know the, the arty people who talked about poetry and novels, novels particularly in my little circle, on the one hand, and were typically reading history or moral sciences or English, and the scientists who were interested in quite different things, and were typically studying engineering or natural sciences Tripos. I didn’t meet very many biologists actually, but also of course the biologists, and the medical students.
Was there any difference in the level of political engagement of the scientists and the art students?

Probably there was. I think the, if you had looked at the list of speakers at the Cambridge Union, you would have found that most of them were arts people, who were aiming at politics. They thought of the Cambridge Union as a, as a kind of nursery for the House of Commons, it was laid out like the House of Commons, it had dispatch boxes and a speaker or, who was the president, and the debates were carried on according to the, roughly speaking the rules of the House of Commons. So it was a, it was a sort of nursery for politicians.

Mm.

And, scientists weren’t going to be politicians.

[55:21]

Mm. Thank you. Now we’re getting to almost the point that we ended last time, but I was, I would like to ask you about your contribution to glaciology, which happened not long after the, the sort of science of glaciology was really established through Gerald Seligman’s society. And I, I wondered what the reaction was of other kinds of glaciologists to your mathematical and physics based glaciology. If you... I wonder what their reaction was to your papers or your approach to studying ice.

Yes. Well first of all so far as the history is concerned, of course, Switzerland was the centre of glaciology in the nineteenth century. It happens that there were a number of so-called students of nature in Britain, Victorians, who went on their holidays to Switzerland and became interested in the glaciers, and from a scientific point of view, people like Forbes, and particularly John Tyndall, they were the leaders in that. That was all before the, that was all in the nineteenth century. And then in the twentieth century there were quite well known Swiss glaciologists, particularly Sebastian Finsterwalder, was a real pioneer, and he, if you look at his papers you’ll see that they are quite mathematical. But among the great mass of papers, they are not, they are descriptive. Scott’s expedition to the Antarctic really opened up, he had several of
course, opened up Antarctic science. But there was no mathematics in that; it was, should I call it natural history? It was descriptive. None the worse for that. And the, the main scientist, Wright, was a very good scientist. The people who did the science on Scott’s expeditions were first-class scientists. The, so far as the Glaciological Society was concerned, yes, Seligman founded it. He was a, as I think I said last time, a skier and a man who wrote a book about the different kinds of snow. Someone, it was actually Garry Clark, who at a meeting of the Glaciological Society many years later was doing some sort of survey, a history of the society, and he undertook the project of looking at the journal of glaciology and counting the number of equations in each issue. And he drew a graph, the number of equations. And it was very distinctive that, when I started doing my work there was a great peak on this graph, in about 1952 I think, and then you got other peaks. And then, if you pick up a Journal of Glaciology today, you will find that almost every paper has equations in it. So, it’s something that’s stuck with the subject. It turned mathematical. Have I, have I answered that part of your question?

[59:25]

Yes. Well, you’ve confirmed what I thought, that, when you were starting to publish in, say, 1952 in the Journal of Glaciology, that, your mathematical approach would have, would have seemed then to be different from what was generally appearing in the journal. So my question was really, what was the reaction of other people involved in the society to your new way of studying glaciers? Particularly those kind of glaciologists who weren’t themselves mathematicians or physicists.

Well of course that was, that was almost all the, all the other glaciologists.

Yes.

So naturally they were a bit taken aback. And, some of them sort of threw up their hands in horror, said, ‘I can’t understand this stuff,’ and, others rather embraced it as a good way forward. I did actually take care to publish the most mathematical papers in proceedings of the Royal Society, and I kept the rather more descriptive ones perhaps, or, you might say understandable ones, to the Journal of Glaciology, purposely. Because I knew that the, well I felt it was kind of important that other scientists knew
that there was something going on in glaciology. Whereas for the glaciologists, they needed to know what it was that was going on in terms they could understand.

[01:01:04]

Could you indicate who were the members of the Glaciological Society who were throwing up their hands and saying, ‘I don’t understand this,’ and who were the ones who were pleased to embrace it as a, as a new way of approaching the subject?

Well, I mean, a particular example would be Bob Sharp, Bob Sharp at the, at Caltech. Now he embraced the new ways of thinking, and, became a very good friend of mine and was very, very generous to me. He’s always been a tremendous encouragement. I think he’s, he’s still alive, in fact he is. Yes. That was Bob Sharp. He had students, particularly Barclay Kamb, who married Linus Pauling’s daughter, kind of married the boss’s daughter as it were. He was a crystallographer. He, he was well equipped to do more mathematics on glaciology, and did so, and has made great, great advances. And there were other people, mostly on the West Coast of America, at, the centres were, Caltech, UCLA, Washington State University, and University of British Columbia in Vancouver. All the main centres as it were along the West Coast of America, they were the most alive recipients of this new way of thinking. Perhaps Britain was a little bit less so. Certainly the, there was a whole load of traditional geologists who didn’t think of this way of thinking, didn’t think anything of it at all. There were standard textbooks on glaciology which didn’t have that approach, like, there was the... I’ve forgotten some of the names. But, at Yale, I visited Yale, and there sat the, the guru of the Quaternary, the glacial period known to geologists, and... You see I’ve even forgotten his name.

That’s OK, we can add that.

But, he was certainly not receptive to it, quite, quite foreign to him.

[01:04:11]

The Cambridge Geography Department was very prominent in setting the tone of the approach to these matters, and these exhibitions we talked about all really emanated from the Cambridge Geography Department. So although they didn’t produce what I,
what’s now called mathematical glaciology themselves, they were certainly not, not averse to it or in any way antagonistic towards it. Does that give you a picture?

*Mm.*

There was also of course the, the Swiss and the French who were prominent, because of their work in the Alps, and there were Swiss glaciologists who were, who did do mathematical work. There was one called Hafley, a very nice man, who was very well known. He came at it from the point of view of soil mechanics, which is a perfectly good way of approaching the matter. He, he would have been more influential if he had, if he had adopted the basic hypothesis that Orowan introduced, that ice was plastic, preferably, more like a perfectly plastic material than a purely viscous material. That was the big distinction. I mean it isn’t such a, such an enormous distinction as is sometimes made out. They both result in movement. But some of the key peculiarities of glacial movement are due to the fact that it’s a non-linear material, obeying Glen’s law, and not the purely viscous material that was assumed by the Victorian, what Victorian glaciologists there were.

[01: 06:26]

*Mm. And, within the British members of the Glaciological Society, who were those members who were averse to a new mathematical approach?*

Did you say were averse?

*Yes.*

Well I think Hollingworth would be the one I would, I, I had personal experience with. I told you a little story about him last time. [pause] I’m not sure that I knew the glaciologists... the geologists that well. I wasn’t in that community. [pause] I mean you mustn’t, you mustn’t go away with the idea that there was some sort of battle going on and people saying, ‘I’m averse to this new approach.’ It didn’t work like that at all. They just went on doing what they had always done, that’s all. They simply didn’t embrace another way of doing things.
What was your view of... There’s a link between geography and the Glaciological Society, and, by the 1960s academic geography is being criticised for being descriptive and operating at quite a low intellectual level, within the discipline there were these arguments, and, in the Sixties you get new kinds of theoretical geography coming into Cambridge. William Bunge spoke on theoretical geography and Haggett and Chorley at Cambridge attempting to insist that geography at least attempts to develop theory instead of merely describing, producing data and describing things and describing things. And, geography was being criticised as being merely a kind of, travel description with a little bit of knowledge of geology. And, and really, when I was writing my PhD on geography, I sort of agreed really about geography, with that assessment of geography at this time, and, and I wondered whether, I wondered what your view of the kind of geographical glaciology was at this time.

You mentioned Haggett. Haggett came to Bristol, became, for a short time, a vice-chancellor. And... But I think he is very much into human geography now. [pause] So I don’t really know how to answer your question.

Well, when you first had an interest in glaciology, you might have started reading the literature, and some of the things that you read would have been these, really accounts of expeditions really, sort of...

Yes.

...descriptions of travel.

That’s right, yes.

And I wondered... And then you started to work in the kind of mathematics of ice.

Yes.
And so I just wondered what your view was of the, of these descriptive glaciological accounts, in terms of their value.

Well they were interesting. I mean, in any subject, you have to have a first descriptive stage before you can even know that it’s a subject. It’s true in, say, meteorology or any, or oceanography, any, any topic is bound to be descriptive before you can do any science at all with it. And, this was a necessary stage in the development of glaciology. It was, I mean it, it’s happened all over that, it isn’t quite that physics takes over, but it’s that physics tries to explain things that are otherwise just described. And of course what happened as far as I can see from outside in, say, geomorphology, is that, there’s tremendous emphasis on geomorphology in the schools I notice, that, they got hold of computers, without really understanding the basic ideas about, say, differential equations. And someone would have an idea about movement on slopes, or movement of stones in permafrost or something like that, and describe it to themselves, and then put it on a computer. Which is not the way to do computing. I’ve seen several examples of that. I think computing was really dangerous for some of the students in geomorphology, who wanted to put numbers into their, into their thoughts.

[01:11:31]

Tell me why.

Well, you see... I’m not sure that I’ve explained this before, but, the right approach is to, to reduce what you are saying to a precise model. My supervisor Orowan once said, ‘It’s better to argue precisely about precise models, than to argue inexactly about inexact models.’ And, what you have to do first is to idealise what you are talking about. And let me give you an example. A rock on a, on the surface of a permafrost. And, the rock moves, and, the rocks eventually find themselves in a pattern, it’s called patterned ground as you must know. So, to make a, to understand that, first of all you have to make a model of saying, well we’ll idealise the permafrost to be, something or other, a medium with certain properties; we’ll idealise the rocks to be, well, spheres, say; and now we’ve got something we can really talk about, and work out exactly what happens with spheres sitting on material of this kind, melting or whatever underneath. And, then you’ve got something which you can be sure about. What you
can’t be sure about is how good a model you have, and then you can argue with the, with the geographers who have actually seen this stuff and seen many examples of it and say, ‘Yes, well that’s sort of all right, but you’ve forgotten about,’ such and such a thing. And, then you, you make a better model. But when you’re, when you’ve got your model, and you mathematise it, very often the equations that describe its behaviour are complicated. And so what you then do is, you resort to numerical methods, proper numerical methods which you will find described in numerical mathematics textbooks. What you don’t do is just do a sort of, seat of the pants idea and say, well, let’s take a time step, and it goes down a little bit this way, and now we’ll take another time step and it goes down a little bit more, and so on, and not realise that what you are doing is a mathematical process which is unstable mathematically. There are numerical pitfalls in doing that kind of problem, and you have to understand them to, to put it on the computer at all. I mean, I learnt from bitter experience for example that if you solve the diffusion equation, which is a mathematical equation, I was trying to do it in a certain context which was unusual, and there were girls in the Engineering Department who were called, I think they were actually called computers. [laughs] Curious, but, I think they were called computers. And, I took my problem to them, and they, was able to, there was one of them who was particularly cooperative and worked on this thing with a hand calculator, and was in great distress because the, the answer was coming out not sort of uniformly but the numbers were jumping around all over the place. And, only later did I discover, I then discovered, that solving the diffusion equation forwards in this way is inherently unstable, and you have to invoke special methods, one called the Crank-Nicholson method, to overcome this instability. Now, I was doing what I, what I am criticising for the, for the geographers doing, just sort of, going straight at it like a bull at a, at a fence.

[01:16:19]

What precisely do they, do they do then which is wrong? They, are they observing something in the field, getting some numbers and then running it on the computer in some way without any mathematical understanding of what they’re doing, or are they... What are they, what are they doing that... I can, I can see, I can understand what you’re saying about the right way to do it, in other words, you have to understand the mathematics involved before you let the computer sort of take over
and run things, but, how far were the geographers getting, or the geomorphologists getting, before they put it into a computer? And when I say, when they’re putting it into a computer, what are they putting in and what is the computer doing?

[pause] Well often in geomorphology you are studying a process in time. Time is moving forward, and when you put it on a computer you have a time step you have to put in. And, sometimes if you take too large a step at a time, you get totally false results, because it goes unstable. I’m taking a particular example. And what you have to do is to take a time step which is small enough to avoid this. And, unless you know in advance what that time step is, is it seconds, is it hours, is it days, is it years, you’re liable to just get totally false results.

[01:18:02]

And, and for me and for listeners who are similar to me in their level of mathematical understanding, could you say what mathematical instability is? In other words, the, what is this pitfall that you can encounter if you haven’t sort of followed through the mathematics yourself, but let the computer step through it? Could you tell me a little bit about what the mathematical instability is, if it’s possible to describe it in a way that I will understand?

Well it’s a bit, you start with a sine curve, a wavy curve, and you have some theory that the curve has to move forward a little bit at a time. And, so really what you are doing is, is following, let us say, perpendiculars to that curve, and following along. Now you can really understand that if you have a sine curve, then the perpendiculars from near the troughs will eventually come together and cross. Now that’s an important phenomenon. It’s a sort of focusing. But if you didn’t know about that, and you took large time steps, then you would go along these perpendiculars right through this focus, right through it, and come up with another so-called curve at the other end which would be quite wrong. Because, you had missed the fact that they had crossed on the way there. I mean that’s, that’s just, I just made that up on the spur of the moment, but it’s the sort of thing that would be, mean that you, you just leapt in too large a step. You’d taken a year, say, instead of a second.
Mm. So you wouldn’t be able to spot that pattern, because you would be, either stepping over it...

You’d be steeping over it.

...or hitting it at the wrong place?

That’s right. Yes. Yes.

[01:20:08]

Thank you. Could you talk now about the origin of the move from the Department of Mineralogy [sound: minerology] and Petrology to...

Mineralogy and Petrology.

Min... to working at Bell Telephone...

Bell Telephone Labs. Yes. [pause] The appointment at the Mineralogy Department was definitely a three-year appointment. They used to do this for the, what they called the Demonstrators in the department. Demonstrator was what in other places would be called an assistant lecturer. And they were definitely three-year appointments, so there was no question that after three years something had to happen. So I had to get a job. I had had some... I must make sure I’ve got this in the right order. Yes, I had had some experience of looking for jobs in the late Forties, and, found it wasn’t all that easy, there wasn’t, there weren’t very many jobs available. And I, at the same time I had thought, I had seen friends going to America and coming back and talking about it, and, I thought it would be rather fun to go and to see what happened there. Was it like it was on the movies? So I thought, I’ll try and do that. So I went to, to Bragg and said I would rather like to go to America, and he said, ‘Oh I’ll write to my friend Buckley,’ who was the Vice-President of Research at Bell Telephone Labs at Murray Hill. And he wrote to Buckley, and Buckley said, ‘Yes, send him over.’ And, and I went. So it was all on an old boy basis. And, the salary they paid me there was astronomical by comparison with what I had been getting at Cambridge. I’ve forgotten the numbers now, but, I’d been getting about
£1,000 a year at Cambridge, and I think, I think they were offering £2,000 at Bell. Very generous. And, probably, probably plus the expenses of getting there, I can’t remember. And I got a small grant from a foundation, the Astor Foundation, to not only go to Bell’s but to kind of explore a bit of the United States. They were in favour of people going over and familiarising themselves with the States. So I got, I had that as well.

Anyway, I got to Bell’s, and, was welcomed by Bill Shockley. Bill Shockley’s group, as you may know, was the group that had fairly recently then invented the transistor, it’s very well known, and Shockley was, Shockley’s group was very, a very alive group, nearly all working on semiconductors as he called them. And did I... Maybe I said this in the last interview. I was the only, I think I was the, almost the only member of the group who was not working on semiconductors. Which at the time meant germanium, not silicon. There was a, Shockley himself was interested in dislocations in crystals, and there was another member of the group, Thornton Read, who was interested in dislocations in crystals, and Thornton Read wrote a definitive book on dislocations, which was of a special kind. It started with the idea that you could have these things geometrically, faults in crystals, not just the regular repeating pattern of atoms but faults in them which were called dislocations, and then, he wrote up what followed from that geometrically. It was, it was, you know, a bit like Euclid, you start with certain hypotheses and then you follow them through to see what they logically lead to. And it’s a marvellous, marvellous evolution. Simultaneously, unknown to Thornton Read, Alan Cottrell in England, he was later the Government Chief Scientist, he was also writing a book about dislocations, and both books came out at the same time, of totally different character. Anyway, I was in this group; most of the people round me were working on transistors, about which I knew nothing, and I was working on dislocations. I started by doing an experiment that Shockley was interested in, because it seemed to indicate, well it was to do with, to do with a single crystal of aluminium, and, inducing dislocations in it by, by banging it, which one of his chaps had done, and, it involved the electron microscope, it involved electron diffraction, so-called Kikuchi lines in electron diffraction, and their sensitivity to this dislocation phenomenon. So I did this experiment for a month or two with total non success, it really was, it didn’t lead to anything. But it involved my learning about electron diffraction and Kikuchi lines. And when it became apparent that this was not
a good way to go, I said to Shockley, ‘Well, you know what I’d really rather like to
do, is write a book on crystal physics,’ which I kind of started when I was at
Cambridge, but I had to stop it. Did I mention this at the last interview? I can’t
remember. So, he then did, he then passed my proposal around the group, and got
comments from them all.
[01:26:58]
There was a chap called Alan Holden in the group. I remember distinctly how he
started his reply. He said, ‘Where Wooster’s...’ Or something like, ‘Had Wooster
written a better book, I would question the need for this one.’ And, I found this
couraging. So, I was given permission to use Bell Labs’ time to write my book,
which was very generous of them. I do believe that Bell Labs at the time was the only
organisation in America, only commercial organisation, which had the financial
resources to back what today we call blue skies research, but looking back on it, they
were prepared to back anything that had the least thing to do with telephones, like,
Shannon on information theory, which was a very remote sort of thing at that time.
So... So I spent a lot of time at Bell’s writing the book.

[01:28:13]
Now, at one point in your unpublished autobiographical writing for the Royal Society
on this period you, you talk about work on dislocations, and you say that you, you
reached a point where it was developed to a point out of your mathematical reach, but
you were glad to have had a part in initiating, and this is ideas about non-Riemannian
space in which the infinite in... no, infinite parallelograms do not exist, the space is
said to have torsion. And, I was interested when you said that you had reached your
mathematical limits, because of course when I, when I’m reading this sort of thing, I
very quickly reach mine, not having done maths since GCSEs. And I wondered
whether you could say something about where you think the limits of your
mathematical understanding are, in a way that I’d be able to understand. It’s
interesting to me having such a low limit, that you would have a sense of where your
mathematical understanding of the world stops almost, where there is a sort of
ceiling, when it seems so...

Yes.
...far above what I’m...

Well it’s all relative isn’t it?

*Mm.*

[pause] Well I remember the, the paper in question that we’re talking about is on continuous distributions of dislocations, which sprang directly from an observation that I had made, namely, that if you take a single crystal of silver chloride, which I had been doing for my PhD research, take it in your fingers, I made them as long rods with a rectangular cross section, if you take it in your fingers and bend it, and look at it between crossed Polaroids, you see light and dark patterns in it which are due to the stresses that are induced in it. It’s called photoelasticity. And the only difference from conventional photoelasticity is that it’s taking place in a single crystal and not in, say, something like a piece of Perspex. So, you’ve got this crystal, it’s visibly bent like that, and it’s got these light and dark patterns between cross Polaroids in it. You then, and it’s obviously then, it’s gone plastic, it’s not, not going to spring back again, it’s plastic, so it’s obviously full of dislocations. You then put it in a furnace, heat it up, and look at it again. And it’s still bent, just as it was. But you look at it between crossed Polaroids and there’s no, there are no light and dark any more. So somehow the dislocations must have moved around in such a way as to relieve the stresses of the material. So, that’s an interesting thing.

[01:31:31]

But we know that the amount of curvature is directly related to the density, the number, of dislocations in the material. So, here we’ve got a piece of material full of dislocations, not, not exerting any long-range stresses that are visible, curved and with a density of dislocations in it. So I thought, well the two must be connected. And in fact they are, very closely connected. And, I called the... and I thought, well, the way to tackle this is not to think of the dislocations as a lot of little lines, or dots, but to think of them as all smudged out together, sort of, uniformly, uniform distribution of dislocation, rather than dislocations, that is to say, it’s continuously dislocated, which was a new concept. Not a difficult one, but, the idea was, the number of dislocations became infinite, but their individual strengths became zero, and if you multiply the
two together, the infinity times the zero, you actually in this case get a, a finite curvature. And that’s the basic idea about continuous distributions of dislocations. [01:33:00]

At the time I met a chap in New York, C... what’s his name? Oh dear, he’s important in this story. He worked with the Polaroid Corporation. [pause] I think it’s probably in the autobiography. And, I met him in his New York hotel where he was attending a conference, and I remember him ordering up a crate of beer for the afternoon, [laughs] which was not the sort of thing I was used to. And, C D West, C D West was his name, and, we talked about some experiments he had done with sapphire. These were rods of sapphire which had been heated up in a furnace, sapphire’s very hard stuff, and heated up in a furnace, and twisted into this shape. I’m, I’m showing a sort of, what would you describe it as, a loop?

Loop-the-loop.

A sort of loop-the-loop. And I think used as thread guides in the silk industry, where you want to guide the silk through a hole, and it’s going to go through, and you don’t want the guide to get worn away. So you make it of a very hard material, which is sapphire. And he had looked at this, these bent sapphire crystals, through crossed Nicols, just as I had been doing with mine, and seen that the, he saw a series of straight lines going through them, which he had drawn attention to. And, that was a key point in my whole approach to this issue. And so, although his stuff had been about winding the sapphire crystal around a circular mandrel, I thought, well does it have to be circular? No. So I took any old shape and imagined a crystal wound around it, and still got theoretically these straight lines. And that was the basis of half of this paper that I wrote. [01:35:13]

And all that was two-dimensional. So I thought, well, let’s make it three-dimensional. Now, curvature, you can think of easily in two dimensions, or, two dimensions actually like that, can’t you? But three-dimensional curvature’s a bit more tricky. You can have twist and you can have torsion and... And, to describe the three-dimensional curvature of a crystal was something I had to think out. And that involved my beloved tensors, which I had been writing my book all about. And, then I thought, well that must be connected with the number of dislocations. So there must
be a similar three-dimensional description of dislocations, of dislocation density. So I invented something called the dislocation density tensor, and the, and the curvature tensor. And I worked out the relation between them. And that’s the thing that’s lasted.

*Mm.*

At the time I didn’t realise that that would be the bit that would be remembered. It was just part of the paper.

[01:36:20]

*Could you remind listeners what a tensor is?*

What a tensor is? It’s, in mathematics you have things called matrices, which are tables of numbers. It turns out that in physics these tables of numbers are just what you need for describing properties of crystals which are different in different directions. To give you an example, if you measure, say, the electrical continuity of a crystal, you measure it in a certain direction, and if you measure it in a different direction you get a different number. And so, you clearly can’t talk about ‘the’ conductivity of a crystal, because it’s got a lot of different numbers. So, it’s possible to sum it all up by talking about ‘the’ conductivity, and the mathematical thing you need is a tensor or a matrix to do it. That’s what it is.

*And...*

You... Sorry.

*Sorry.*

I didn’t finish...

*And how, how... Go on, carry on, yes, please.*

[01:37:28]
Sorry, I, I lost the thread. I was... You asked a question about the...

Oh yes...

...work at Bell, and the limits of my mathematics.

Yes.

Yes. Just, just coming to that. Because, I got this relation between the dislocation density tensor and the curvature tensor, and left it at that. And incidentally, in that paper, I’m ashamed to say that I made two absolutely crashing errors. It didn’t affect the final result, but there were two errors I would never have made if I had known then what I know now.

Mm.

And, I won’t say what they are now, let anybody find them that wants them. But, the bit that’s survived is the bit that’s right. And, I didn’t... I sort of vaguely realised that I was talking about a funny sort of space, which was curved and dislocated, and, it turns out that in mathematics there are just such conceptual spaces. They’re not based on crystals at all, but they’re the sort of things that Einstein would have been interested in. In general theory of relativity he used what’s called Riemannian geometry, which is a geometry all described by tensors, which describes curved three-dimensional space. And that, if you wanted a crystal analogy of that, it would indeed be just a crystal which had been bent. So the lattice planes were no longer planes but curved, but curved all, in all dimensions. That, an elastically bent crystal would be corresponding to Einstein’s curved Riemannian space. But if you now dislocate the crystal, then you get what the mathematicians call a curved Riemannian space with torsion. And as I think you quoted just now, the thing about that for mathematicians is that if you try and draw a parallelogram in it by following a crystal direction this way, a crystal direction this way, the same crystal direction back that way, the same crystal direction back again that way, normally you think it would close, it would be a parallelogram. But in this particular space it doesn’t, it doesn’t close, because the space has what they call torsion, or in the crystal case, has dislocations in it.
So I didn’t know that, I couldn’t deal with that, the mathematics of, mathematical spaces with torsion, which is quite a, a highbrow art, and, that’s the limit of my mathematics. But later Bilby and his colleagues in Birmingham took up that aspect and said, well this is just Cartan’s idea of torsion, of a space with torsion, and, and as I say, turned it into the sort of mathematics that I couldn’t follow.

OK, thank you.

That’s the limit.

[01:40:43]

And you continued glaciological work at this time, and there was an episode involving Time magazine, which reported your calculations of shear stress at the base of Greenland ice sheet, and you say that the article in Time magazine gave you brief notoriety among your friends at Bell Labs as a, ‘a stay-at-home explorer and a lab-locked scientist’. Could you tell me about this episode please?

Well, at Cambridge I had been thinking about the implications of what I was doing for the Greenland ice sheet, and, I developed a formula which was extremely simple, that related the slope of the surface, the thickness of the ice, to the so-called shear stress on the base. The shear stress is the sort of sliding stress, doing that. And, taking a leaf out of Orowan’s book, I said, well what would happen if this... No no, what I said was, let’s apply this formula to Greenland as it actually is, where, I can get out a map and look at the contour lines and measure the slopes everywhere. And... What did I do about the thickness? [pause] Oh no, I said the shear stress at the bottom was the same everywhere. That was the sort of Orowan idea. Because that was the most that ice could stand. So, given that the shear stress at the bottom was as we thought at that time one bar as an assumption, and using the measured slopes, I could deduce the thickness of the ice. But it had only been measured, if at all, in one place, right in the middle, by a very early expedition which had used seismic shots to do the job. And they come up with the figure of 3,000 metres. So I came up with a, a contour map
essentially of the thickness, and therefore a contour map of the base. And I was
astonished to find that the base was well below sea level in the north. And I thought
this was sufficiently interesting to publish it, and the relevant paper was in Nature,
and that’s what Time magazine got hold of. It turned out unfortunately later that, the
slope I got from the then existing map was out by a factor of two, and the shear stress
that I had assumed on the bottom as one bar was actually half a bar. And the
combined error was a factor of four. So I got the thickness wrong by a factor of four.
So in fact although I predicted a vast, not lake, but, below sea... depression, in the
north of Greenland, in fact, although there are places where I think it’s below sea
level, it wasn’t anything like as below sea level as I predicted. But it was just a, a
shame that two errors of factors of two added together to give the wrong answer by a
factor of four.

[01:44:38]
I see. And the... And when you say that you gained notoriety as a stay-at-home
explorer, or a lab-locked scientist, who was, who was saying those things?

Well they all read Time magazine. It was just my, my colleagues whom I had coffee
with at Bell Labs. I mean to be on Time magazine for an American is a great, in Time
magazine, is a great honour you know. If you have a picture on the cover of course
you have to be president or, or a real top chap. But even to be mentioned in Time
magazine was, was a matter for awe.

[01:45:16]
And, it might be a good time to ask you to reflect on the relative importance for
glaciology of the field and the laboratory.

I don’t quite understand.

Could you say something about the, about how necessary it is to go to, say, Antarctica
or the Greenland ice sheet, in order to understand it. So in other words, the relative
importance of the field, the idea of doing field work in a place...

Yes.
...as opposed to the laboratory. Because I, I know for example when you were talking about the icefall work last time, that, when you were talking about the mathematics of it, you said in fact you could have worked out the reason for the formation of the waves and the surface by staying at home, but that being there was a kind of...

A stimulus.

Yes, a stimulus. And, and then we’ve got this episode where a stay-at-home explorer, lab-locked scientist, this is a comment on the field and the laboratory as relative, relatively valuable ways of coming to understand glaciers or pieces of ice on land. So, what would be your thoughts about the relative importance of field work and laboratory, field and the laboratory?

Well I’d say that nowadays it’s quite commonplace for applied mathematicians to do work in glaciology without having seen or still less set foot on a glacier. So it can’t be essential. But at that time, it did seem important [laughs] somehow to set foot on the very thing you were calculating about. And I suppose the fact of the matter is that it gave some inspiration for what you were doing. For example, one of the things I worked on was the patterns that, lines that crevasses made on glaciers, which is an interesting series of patterns, and, turns out to be important when you’re actually on a glacier, because you don’t want to fall in one. Actually, for example, one of the results of one of the calculations was, that if you are on a glacier and you’re walking up it, and you get stopped by crevasses, you tend to walk along the crevasse until you can find a way across. Well, one way of approaching it is to go to the extreme edge of the glacier, which is often a highly crevassed strip there, often covered with moraine. But if you persist and go even further and go right to the edge, you find a strip which is crevasse free, and that, there’s a perfectly good mathematical reason for that to do with the sliding of ice over rock. And it happens to be something that mountaineers know about. There’s a kind of piece of piece of applied glaciology for which, it’s rather fun to go to a glacier and try it out. One thing that I found on, at Austerdalsbreen that intrigued me was, that, you can drive three stakes into the ice, and line them up by eye, they can be like 100 metres apart, line them up by eye, with great precision. And you can move, you can signal to the person doing the last one to
put it in right there. And you get them right in line. You come back the following
day and they’re not quite in line. You get the last two in line and you see that it’s
slightly different from the one that’s next to you. You put a ruler there, and you
measure the distance. Say it’s a centimetre or two. One of the most precise
measurements you can make of curvature just by a ruler and three stakes. Now, I
would never have thought of that if I hadn’t been on a glacier. And curvature turns
out to be one of the, one of the results of doing calculations on glaciers.

[01:49:44]

In publishing in glaciology, did having been there lend authority to arguments that
you were making about glaciers and ice?

I think it probably did among the geologists. I don’t think it had any effect whatever
on the feeling among applied mathematicians: either it was right or it was wrong, and,
they judged it on the mathematics and the assumptions. But, I think probably the
geologists had slightly more faith in the results simply because they knew that I had
actually been there. It’s, I think it’s a bit illogical but, I think that was probably the,
could be an attitude.

Why did you pick the Greenland ice sheet to do this...

Calculation?

Yes.

Well, because the, I suppose the thickness had been measured by the seismic shot in
the middle, so we knew it was about 3,000 metres. Whereas in the Antarctic, no such
measurements had been done. The Antarctic is far more inaccessible than Greenland,
and, Greenland is a kind of mini-Antarctic.

[End of Track 6]
Could I ask you please to describe your sort of private living arrangements when you were in America, working at Bell’s?

Yes. When I got there, I arrived in New York and stayed a night or two in a hotel, and then was taken out by, company car I suppose. Oh, Bell’s had a, a headquarters in New York, and I was taken out by car to Murray Hill where the Bell Labs is. And there I was helped by a member of the staff to find somewhere to live. And he took me to a, a house owned by a Colonel and Mrs Canada, and there I was given a room. Colonel Canada was a was a, had been in the Army obviously, and, his wife, Mrs Canada, was a southerner. She said, I remember her saying, ‘Of course we’re really English ourselves, we’re from Virginia.’ And I stayed there all the time I was in America, it was a year. And I had a number of, they let out two rooms, and a number of people came to the other rooms, some of them joining what was called Kelly College. Kelly was the vice-president there, and he had set up a kind of training course for people who were going to work there, young people from, fresh from universities who were going to work there, and, they called it Kelly College. And a number of them came from time to time. But the more permanent person who came was Ian Ross, who later became Vice-President in charge of research at Bell’s.

Now, I had in the, 1990, around, I had a very brilliant research student called Joe Hajnal, and Joe Hajnal wanted to go to America to do research, just to see, just to try it. And I said, ‘Oh well, I’ll write to my friend the vice-president, Ian Ross,’ echoing the [laughs] words of Sir Lawrence Bragg who wrote to Kelly who was vice-president at that time. And to my dismay I got no reply from Ian Ross. I don’t know why. But, Joe Hajnal went to America anyway, so, it all worked out all right.

Anyway, Ian Ross and I used to go in every day to Bell’s. And, there was another fellow who lived there who had lost an arm in his army service by being crushed by a tank, and, he had a car that was adapted to one-armed driving. And he very kindly taught me to drive, in quite a different way from a normal driving instructor. And so I learnt how to drive a car, and bought a car, and drove in myself from that point onwards.
And, could you describe, do you remember your room in enough detail to describe it, your room in the house?

Oh it was right up in the top of the house with the beams, rafters there. And, an ordinary sort of attic room in a wooden house, in a very respectable district, a white clapperboard painted house. I watched the activities of the house opposite, where there was a huge white painted house, and the man who lived there used to jog round the block after breakfast every morning, get back to his house, and his wife then drove him round the block to the station to go to New York.

And, I know that when you first went out to America, you had mixed feelings about it, partly because you had recently falling in love with someone back in Britain. Correct? I wondered whether could tell me about that relationship and then tell me how you were feeling about that, having moved to America.

Well when I left Southampton I felt terrible. As I said, Audrey’s husband was also on board. Because I was leaving, well, somebody I loved. And, I was terribly lonely when I was in America for that reason. So, that, well she got married while I was away. She married a test pilot. Very romantic.

How did you meet Marjorie Wood?

How did I meet her? She came to a May Ball in Cambridge as the guest of a friend of mine, Oliver Lodge, of the Oliver Lodge family, who’s been a longstanding friend and whose family I’ve been, kind of kept up with. So that’s how I met her. She lived near Gloucester.

And can you tell me about your relationship with her, how it developed, or...?

Well it didn’t. You see I... it... she got married.
Oh, but, initially, from the point of meeting her to realising that you were in love with her, I mean what...

Oh extremely short. I mean, she came to this May Ball and, and I... as someone else’s guest. I met her during the, during the evening, and, night, and, we exchanged addresses and so on. And, it was difficult you know at that time to sort of, you couldn’t, you couldn’t just cross the country and, and go and visit somebody for no particular reason. [laughs] And, so I didn’t see very much of her. But then, I came to Bristol on a, on a mission to do with the, to do with physics, and it was so close to Gloucester that I went and visited Gloucester, and, met her there. And then I fell in love with her.

Can you describe her, appearance and dress and...

Very elegant, a beautiful voice. [pause] Very hard otherwise.

And, what did you, do you remember talking about certain sorts of things, or, doing certain kinds of things together that you wouldn’t do with other people?

Well I, we went for walks around the Gloucestershire area, but, I used to visit her up there, drive up in my Morris Minor as I then had, and, then come back to Bristol, and, we had a... It was all very short, a very very short time. And then came this rather sudden offer to go to America, and, and I went to Bragg and said, ‘Look, [laughs] I’m really in a dilemma here. It’s all been arranged. What do you think I ought to do?’ And, I said, ‘Would I be, would I be really letting them down very badly if I didn’t go?’ And he, he rather sort of thought, maybe I would. And... But I just decided, well, I’ll go and wait a year. But, it didn’t work out.

Did you tell Sir Lawrence Bragg that you were in love, and that was the...

Oh yes.

And what was, what did he say about that as a, as a condition or a reason for...?
Oh I don’t think he said anything very much. He, he very kindly came to, to my wedding to Georgiana a year or two later. He must have been a big puzzled I think. [laughs]

Mm. Did you attempt to correspond with Marjorie, having left for America?

Oh yes, yes I did. But it was a rather one-sided... Do you know, nobody else has asked me these questions, so, I don’t know how much I want to pursue this, but... Do, do go ahead and ask what you want to ask.

[10:00]
OK. Well, who... when you say it was one-sided, who was writing and, what was being written?

Oh I was writing and she was also writing back but not so much.

And when you, you had left for, to go to America, what was the sort of, agreement that you had made with Marjorie about the future?

She would think about it, really. It was in the air.

Did she ask you to stay, rather than to go?

[hesitates] No. I don’t think so.

And the plan then was to go for a year?

Yes.

What was her sort of, occupation or educational interest, or...?

She, she was what was called a, she worked out stresses in an aircraft factory. I don’t know quite what it involved. But it was the... In Gloucester there’s a, there was a well-known aircraft factory, was it called the Gloster Air...? I think it was called
Gloster Aircraft factory. They made Gloster planes I think. And, she did what was called the stressing. I don’t know what it involved really, but it involved working out, going through some numerical, mathematical procedures of working out stresses.

_She was a mathematician?_

No, not particularly I think. Well she must have been enough of a mathematician to do that.

_Mm. Was your conversation in any way scientific or mathematical?_

Not in the least.

_Literary and philosophical, or just...?_

I cannot remember, I really cannot remember. No, I can’t remember. We were very congenially suited you might say. But I cannot remember what we talked about.

_How were your feelings of her different from those for the undergraduate that you had really spent your three years with at university?_

Audrey you mean?

_Mm._

Well I guess they were a bit more mature. I mean when I was going around with Audrey, I had no idea of, no... Well, not no idea. I had few thoughts of marriage. In fact I think I knew I didn’t want to marry at that time. But, when I met Marjorie it was entirely different, I, I, I very much wanted to be married. I was just that much older.

_Mm. How old was she?_

We were both about thirty.
Mm. Did you propose?

Yes, oh yes.

Before you left?

Before I left? Oh yes, very much so. Yes yes.

So when you had left for America, was she considering the proposal?

[pause] I suppose you might say so, yes. Yes she was indeed. Yes.

And if it’s not asking a question that’s going to be sort of, an unhappy memory, how did you realise that, how did you come to realise that this wasn’t going to have a future, this relationship?

Well because she married, she married this other chap. He was a very, a very dashing fellow. His name, what was his name? Anyway, he had, he had had a career in flying. He was a test pilot for the Gloster Aircraft Company, he, he test flew the Jaguar aircraft which was a, a new fighter plane. And, had all sorts of adventures. The tail fell off when he was somewhere high up and he managed to land the thing without a tail. And, you can look him up, he’s, he’s won various air races and all sorts of trophies. A thoroughly dashing male figure.

[14:00]
Thank you. Now, I wonder whether you would be able to describe the, the meeting with your, with Georgiana, which happened I know in America.

Look, I don’t know how much I want to get... Georgiana loves talking, [laughs] describing this story, but I don’t know how much I want to go into it. [pause] My friend at Cambridge, Lamb, had a sister in America, New Jersey, and he, when he heard I was going to New Jersey he said, ‘I’ll get you introduced to my sister.’ So I duly called on her in New Jersey, and she kind of to get rid of this English visitor,
very kindly introduced me to her swimming pool, which was in New Jersey, and I became a member of this swimming club. While I was there I, I swam out to a raft on a lake where I dived in, and there was another girl on the raft who said, ‘Your legs weren’t together.’ [laughs] So I tried again and my legs still weren’t together. And, I became friendly with her. And, she lived in Orange County and was holding a party, Ann Ziemer was her name. And she, she was inviting a friend who was temporarily in New York, Georgiana in fact, who needed a kind of partner, a date they called it, at this church dance. So I was invited over to this church dance, for what they called a blind date. And, my car broke down when I was there, and I had to spend the night after the dance, because my car wouldn’t work. And in the morning it turned out to be a very trivial thing but just the sort of thing that goes wrong with an old banger that I had at the time, that the electric terminal on the battery had come loose so that it just needed tightening up and then the thing started up perfectly. So, [laughs] they thought it was all an excuse to spend the night, but actually, it was all bona fide. And, Georgiana, who was staying in this house, had to get into New York that morning to attend a dance class with Martha Graham in New York. And so I said, ‘Oh well I’ll drive you in.’ So I drove her in, and, here was my blind date. And, and when we got to New York she was late for her class, and nobody goes in to a dance class with Martha Graham late, just not done, so, we spent the day just going around New York. And I got to know Georgiana. And we went on from there.

_Could you say what attracted you to her, and her to you at the time?_

Well, you know, some people you can have a natural affinity with, and it was that. Very much so.

[17:50]

_And, and what happens next in your, in your life, both scientific and personal?_

My life. Oh. Well perhaps quite an interesting thing was that I made acquaintance with a lady who lived near Summit, New Jersey. I don’t remember how I met her. Her name was Gina Plungleon, and, she was an artist, and among other things she taught art, painting, to Einstein’s stepdaughter in Princeton, which is not too far away. And I used to go around to Gina’s house and pretend to be an artist. And I did a bit of
painting, I’ve got a few paintings upstairs, and one I did from her window. And she said, knowing that I was keen on physics, she said, ‘Oh well I’ll introduce you to Einstein. Easily do it, because I’m often there teaching art to, to his daughter.’ And there came the occasion when I needed to pick up, for some reason I don’t know why, I needed to pick up Gina in my car. I mean there was no kind of romance going on, she was a married, a middle-aged lady. And, when I picked her up, she was mortified because, she had just come from Einstein’s house and she had clean forgotten to ask me to come and pick her up from there, when I could have come in and met Einstein you see. So she was so, so mortified by this, she said, ‘I’ll try and make it up to you.’ And, a little while later she gave me a manuscript, a piece of paper, which she had been given by Einstein’s secretary. It had sort of, fallen off his desk. And, this, this is a handwritten manuscript, as I later find, relating to his current research work at that time, which now hangs framed in the library of the Physics Department here. I was able to present it to them when I retired. And that’s the origin of the Einstein manuscript owned by the Physics Department. As somebody said, this was the reason why Einstein never achieved his generalised theory, because the piece of paper involved fell off his desk.

[20:57]

Why did you go to Gina’s pretending to be an artist as you put it?

Why did I go? Well because it was kind of nice to meet people, and, I honestly cannot remember how I made the acquaintance. There’s probably some connection, but... I met a lot of people and made a lot of friends when I was over there. In fact, my friend, who later became, who was at Bell’s, George Dacey, whom I had most of my evening meals with, and later became Vice-President of the Sandia Corporation, he, he was introduced by me to Ann Ziemer, the friend on the diving raft, at my farewell party at, at Gina Plungeon’s and said, ‘How have you been hiding her all this time?’ And, promptly married her. So, I, I made good friends with him, and with Ian Ross, and with, with Ann Ziemer, and various people who used to come in and paint pictures at Gina Plungeon’s too.

[22:28]

Thank you. Now, could you describe the next sort of phase of your life, which is...
Just, shall I just go back a moment and say, there was a, a very nice fellow of Greek extraction called Jim Roros, who was a researcher at Bell Labs, and, the time came in the summer when we both wanted to take a summer break, and I used this Astor money to go on a trip across America with him. And we drove right across from the east coast all the way to the west coast, back through Canada and down, we went into Mexico, and I saw a great deal of the United States. And among other things I gave lectures on the way. And I particularly gave lectures at Caltech, at the invitation of Bob Sharp, who had already I think, put me in the way of a job in America I think, at Socorro in New Mexico, which I didn’t take. But, he was a kind of, patron, became a kind of patron, and was very anxious that I should do this glaciological work. And not much later, in 1959, we’ll come to that, he invited me over to America to give what he called [US accent] ‘American glaciology a shot in the arm,’ by giving lectures all over the place, to kind of propagate this new gospel. So that came about through my trip across the States when I was with Jim Roros. It’s interesting how all these things, one thing leads to another.

Mm.

But you were asking about what happened then, I think, when I came back?

Yes, I was, I’m going to ask you that, but, you just said, it’s interesting how one thing leads to another. I wondered whether you, you’ve ever considered a kind of, in a kind of mathematical or physical theory for, you know, the way lives develop, the way things happen in life, in the same way as you might with a... I know that you were interested in the traffic flow work of Lighthill and...

Whitham?

Yes. And applied that. I wondered whether you’ve ever thought about your own life or biography in that kind of way, the way one thing sort of, knocks on to another and the connections and that sort of thing. Possibly not.
This is, this is sociology and psychology, and the answer to your question is, no. That’s a brilliant idea of yours that you’ve just enunciated, but I’ve never thought of it. What you’re asking is a sort of mathematical theory of life, isn’t it?

_Mm._

I doubt whether it’ll ever happen.

_I wondered whether, I wonder whether you tend to, when you think about, well, presumably, you’ve said no, but when you look back on the past and these sort of things happening, does the fact that, does the fact that you’re a mathematician influence the way in which you think about those things, or order them?_

No I don’t think so.

_No._

No.

[25:46]

_OK, thank you. So can you tell me then about the next step which is coming to, which is, a) why not stay in America, and become an academic in America as many, as many British scientists are doing at this time, why come back?_

Well I consciously went there for one year only, and I stuck to it. I... You know when I went there, it was out of curiosity, and, in a way my curiosity was satisfied. It was indeed just like the movies, as I had naively expected. And, you know, the, the drive-ins and the salt dispensers and the, you know, they were all just as they were in the movies. And of course it was, it was shortly after the war, so I wasn’t quite prepared for the lavishness of American life. But, it was clear that I could earn much more money there than I could in England, but I felt a curious sort of tie to, to Britain and everything that was in it, and I decided this was it, I should come back. My American friends found this hard to understand, because so far as they were concerned Europe was just about to sink under the seas. And why go back to that
place when you could stay here in God’s own country? I found it... And of course when you, when you’re over there, the British people you meet are the people who’ve already made that decision, that they prefer it to staying in Britain. So it was, you don’t meet any support for your view if you decide you’re going back. So I suppose it was a bit of a stubbornness really that made me come back.

You said that you felt that you should come back.

Should?

Are you implying a kind of, almost a sort of, feeling of duty, or, or...?

Well I certainly wanted to come back to Marjorie, that’s true. But...

But by this time you had, you had met...

Georgiana.

Yes.

Yes that’s true. That is true. Yes.

So at the end of the year in America...

Confusing isn’t it?

Yes. So, so the decision at the end of the year in America to come back, still involved wanting to come back to see Marjorie?

I think it must have done, yes. Yes. Yes.

So although you had met Georgiana...

Yes. Yes, it wasn’t a, a sort of, romantic...
I see.

...completely romantic engagement as it were.

Anything else pulling you back to Britain apart from Marjorie?

Well, it was, I say, I think it was really stubbornness. Also, Mott, Nevill Mott had written to me saying, ‘Would you like to come and work in Bristol?’ And, ‘I can get you a grant,’ he said, ‘to work... Jack Mitchell is working here on silver chloride crystals, silver bromide,’ and, it was all to do with the explanation of the photographic process. And he had developed very careful techniques for looking at these silver bromide, silver chloride crystals under a microscope, and I had also done work on this material from a totally different point of view. And Mott thought it would be a good idea to bring these two techniques together. Would I come to Bristol, he’d get a grant and I’d work on this here. So, I said, ‘Yes.’ And I came, and... How did it work out? Yes. I did come and work on those topics, and, it was very difficult indeed actually to unite these two techniques, and we never succeeded. And I did later have, was it one or two research students who had a go too, and they were no more successful than I was. But, came the point when I said to Mott, ‘Look, what I’d really like to do is to go on writing this book.’ And, I remember him saying, ‘Yes well that’s just fine, but of course I can’t really continue to pay you from your grant when you’re not doing the work the grant is for.’ So I said, ‘Well all right, I’ll work for nothing, but I must get on with this book.’ Which I did.

And then, while all this was going on, I was applying for lectureships at, wherever they were going, and there was one going at Belfast I recall, and I was interviewed for that, and there was another going at Bristol. When I went to Belfast I was courageous enough to say, ‘Could I please see the library?’ And, he said, ‘Oh well, there isn’t actually... There’s a university library,’ and he showed it to me. ‘There aren’t any physics books there, and if they want physics books, they come to my room and I lend them to them.’ And the, the proposition was, that I would take over, they had taken over a house in one of the squares in Belfast which was a normal residence, and they were going to turn it into a lab, and that was where I was going to work. Well it was
all a bit sort of, difficult, and I decided that the whole thing was not a practical proposition. And so I turned that down and was offered the job at Bristol, which I accepted with great pleasure. I was very pleased about that. Was interviewed by Sir Philip Morris, the vice-chancellor, and the registrar, assistant registrar, Fraser, and, it was all done very informally. And I think Professor Tyndall and Professor Powell were on the interviewing board. And, it was all pretty informal. I don’t remember anything in the way of a, a sort of... They presumably had some kind of a CV but no kind of application from me saying why I thought I could do the job or anything of that kind. It was very, in those days you got jobs in an entirely different way from, from now. In fact I, I was just, when my daughter’s applying for all kinds of jobs, and we’re working on her applications, I don’t think I’ve actually applied in that way for any job in my life. It’s all quite different.

And, when were you appointed? This was 1953, but when...?

'53, yes. Started in October '53.

So by this time...

I came before, I came in, during the summer, and worked with this grant for a while.

[33:21]

And you married Georgiana in December 19...?

December ’53. Yes.

Could you then explain the development of the relationship with Georgiana from when you got back from America to your marriage in December?

Well, she, she lived on the West Coast, in San Francisco, which is a long way away, and her, she had been working temporarily in New York. And she was, she was very much into dance. And she was working with a company in, California I think it was. And her whole, her family was coming to a kind of, as the Americans do, ‘do Europe’. And she was coming with her family, her father, mother, sister, and her
brother who was in the American forces, in the American Air Force, already in England, based in Warrington. Interesting isn’t it, now, in 1953 the American Air Force was still here. And, they were doing, they landed at Plymouth, and they said, ‘Which is the way to London?’ [laughs] And, waved up the road. And they whizzed up to Scotland and to... Their travel agency in America only knew about the sort of, the expensive hotels in these places, so they were duly booked in at the, what is it, the big hotel in Edinburgh?

I don’t know.

At the, near the station I think. And then, Hotel Windermere of course, for the Lake District, at Windermere. And they were whizzing down from Windermere to Stratford-on-Avon, and then straight back to London, to Dover, and over to France, where they had family connections. But the tour did not include Bristol. And I was a bit upset by this, and, decided I had to make contact somehow. Georgiana will tell an entirely different story, but, I decided I had to make contact, so I kind of intercepted them at Stratford-on-Avon, at the Royal Shakespeare Hotel, where I met the family, and we had a nice time, and, I went for a stroll with Georgiana. And we agreed to go on a holiday together to France. And, the... So we made those arrangements. And then they whizzed off to, to Dover and to France, where they were on holiday in France. And then, and then I must have... Yes, I had arranged to meet Georgiana in Paris. And there was a, a strike, there was a French postal strike, and it was absolutely impossible... and telephone strike, impossible to communicate with France. So we had made these, I don’t know, tentative but, arrangements for a particular day, and I flew over, flew the car, the little Morris Minor, over on Silver City Airways to Le Touquet, expecting to be greeted by Georgiana there, to whom I had written, but received no reply. And I remember the, my father’s housekeeper in Hove, saying, ‘You must be sure you take disinfectant with you, because French, France is absolutely terrible now with all this strike, rubbish in the streets all over.’

[37:24]
And, I was a bit put out at not being met at Le Touquet, and wondered what to do. So I drove to Paris. The only address I had was the Guarantee National Trust, which is a bank, which was a forwarding address, at the Place de la Concorde. And I went in there and said, ‘What about the Wiebenson family, have you got any
communication? ’No we don’t know Monsieur.’ And, [laughs] well I knew that my, that Georgiana’s uncle stayed at a hotel in Paris called Hôtel Saint Pierre in the rue Saint Pierre. So I thought, well that’s a possible contact, I’ll go there. So I went to the Hôtel Saint Pierre, and I think I was... I didn’t have lunch there, but I think I had a picnic or something in the Luxembourg gardens, just close by. And as I was finishing my lunch and I came to go back to the hotel, the Wiebenson family drove by in an open car. [laughs] And, what had happened was, that Georgiana had written a letter to me, and because there was no post she had given it to her brother to post in England when he returned to England, and he had just lost it, he never posted it. So there would be communication. Anyway, her father, seeing me, practically chucked her out of the car and said, ‘Here, you can have her, she’s been, she’s been an awful nuisance with us.’ [laughs] Because of this Englishman who wanted to take her on holiday. [39:20]

So, there we were in Paris, with a kind of, idea of going on holiday. And we had a splendid holiday together, sleeping out. She had, Georgiana was not an heiress, she brought over with her a sleeping bag and a typewriter, and a few... and a parking ticket, which had never been paid. And that was her kind of dowry. So, we went on this very nice holiday. We drove down to the south, through Provence, and got to the south coast, had a lovely time. Drove back through Burgundy. And, parted on the best of terms. And, I think then after that, I was... The big thing on a trip like that was the currency allowance. We were only allowed, I think it was, was it £20 a year or £30 a year, but it was entered in your passport. And, that was the only foreign currency you could have. And I had already been abroad once that year, so there was, the big question was, would there be any money for the trip, not, not pounds but, because you couldn’t take those abroad, but, only this allowed foreign currency, which I had practically exhausted. So we had a pretty frugal sort of time there. But still we managed it very well. We had a, we used to ask for half portions and things at the restaurants.

[41:02]

So I came back to England and invited Georgiana to come and stay at home in my father’s house at Hove, which she did, and, and then we got married. And we wondered where we should get married, because, my home town was Hove really, and one possibility was, was Hove, but, I’d spent the rest of my grown-up life as it were in Cambridge. And I thought, suddenly thought of King’s College. So I
telephoned the dean and said, ‘Could we get married in the chapel?’ And he said, ‘Oh yes, oh splendid. We haven’t done it for years, and, but it has been done.’ So a special letter had to be written to the Archbishop of Canterbury by my future mother-in-law, who was in Spain, to get his permission for us to be married in King’s College Chapel. Well she as an American didn’t quite know how to address the Archbishop of Canterbury, and all she could find was notepaper with bullfighters on, [laughs] on the top of... That sort of thing. And I remember she put at the end, instead of ‘Your Grace’s obedient servant’, ‘Your gracious obedient servant’. Which, [laughs] wasn’t quite right. But nevertheless, we got the special licence. And got married in King’s College Chapel, which was lovely.

[42:43]

Who came to the wedding?

Who came to the wedding? Well, somewhere are the wedding photographs. But, her parents, who had, who had prolonged their stay in Europe; her sister; I think her uncle, who was a Francophile gourmet writer about French food and that sort of thing, his books are up there; and, Georgiana’s cousin. Now... Yes, yes, now she came. And, a lot of my friends of course. I had several of them who kindly said they would be ushers at the wedding, and, my good friend Gerald Infield was the best man. And, as I said before, Sir Lawrence Bragg came, and his wife, and we have pictures of them going in. Rather a good picture actually. So we had a pretty good, a pretty good show there. And, we had a little reception afterwards in the, put on by the King’s kitchens, at which the food was terrible. Sausage rolls were the order of the day, and sausage rolls come about the bottom of Georgiana’s list. So she, she wouldn’t speak to me in very glowing terms about that aspect of it. And then we went on our honeymoon in the Morris Minor down to, in very short hops, down to Devon, to a, to a place which we reached on New Year’s Eve, where the guests were interesting. They included the publisher of the Daily Express, who entertained us all with his Charlie Chaplin imitation. And, they knew we were a honeymoon couple and we were kind of received into the family for the New Year. It was a place called Nan Sidwell, which had been recommended to me by my friend David Lacey, who got married shortly before, sent me a postcard which said, ‘If you find yourself similarly
situated, John, I can recommend this place.’ And shortly afterwards I did find myself similarly situated, and chose that place. So there you are, some details.

[45:26]

Thank you. Could you describe the Department of Physics at Bristol, where you had recently been appointed? Would that then have been the H H Wills Laboratory?

H H Wills Physics Laboratory, is the proper title, yes.

Yes.

Well, the head of the department was Nevill Mott, and, Tyndall, who had been a big figure in Bristol University, also a physicist, had been really responsible for getting the place built. And he, he had been, I think not literally vice-chancellor but acting vice-chancellor of the university, so he was very prominent at the time. Then there was, there was a modest staff. And there was, it was what we call now the ‘old building.’ Subsequently there’s been, there has been a big extension to it, and another further extension is in the planning process at this moment. But this was what we called the old building. And it was a, it was a vast place. The ceilings were enormously high, because, Tyndall, when he was advising the architect, couldn’t foresee, he thought, how big or small apparatus might be in the future. So let’s be, let’s be generous about it, because it would have been paid for by the Wills family. It must have been an enormous amount of money. And so the floors were made very wide apart, so wide apart that in order to accommodate more people they later put in balconies in each of the offices, which fitted in comfortably. It was built, I was told, I think by Tyndall, that, you could take out all the floors and partitions in the inside and the building would still stand up. It had, not floor to ceiling but floor to top of building, windows, so the floors could, in principle, be put in at any level. Which for that time you know was pretty far-sighted. When it came to refurbish it a few years ago, however, they did not actually change the spacing of the floors, although they could have done, but it would have been too expensive.

Mm.
But when the time came to build the, the new extension in about 1964/5, that period, the architect had the job of having a more reasonable spacing between the floors, and he had to join the existing first, second, third, fourth floors on to first, second, third, fourth, fifth floors in the new wing. And he designed a most elaborate staircase to do this, and a lift which stopped at all the immediate stops. Because you needed to be able to get apparatus from one side of the building to the other without going up stairs.

Mm.

So it was quite an architectural feat. But I remember at the time being a bit scathing about the architectural style of the new wing, because, Bristol was pretty backward in its architectural aspirations, whereas other places had been busy over the post-war building phase, and, Coventry had got its new shopping centre pedestrianised, which was a new idea, and, other new universities were springing up. But Bristol was pretty backward architecturally, and I remember asking the vice-chancellor at the time, Sir Philip Morris, whether he would consider having an architectural competition for the extension of the building. And he had said, ‘Oh no no no no, it’s always unsatisfactory, they don’t understand these things.’ So he appointed the, the junior partner of the firm that had built the original building, Brentnall, who had built the Senate House and the engineering school, and later the new medical school, all Brentnall designed buildings, which to the modern eye look pretty dated. And, we were way behind the times.

Mm.

But, still, the building works, except of course for the ventilation and, and the usual problem with that which many of the buildings of that period, indeed modern buildings, fall down completely on, being overheated in summer and too cold in winter.

[50:32]

Mm. But the traditional design of the extension didn’t get in the way of modern physics within in?
I wouldn’t say got in the way of, not at all.

*Mm.*

No, we, we were comparatively well equipped.

*Mm.*

We specialised in low temperature in low temperature research. And we had a helium liquefier which was an essential for that sort of research. That fitted in perfectly. The workshop which used to be in the old building was moved out into a very ramshackle old, well not... a very ramshackle new building, one storey, and if you go there now, you will find that workshop completely demolished and the site of the archaeological dig for the old Royal Fort. Quite an interesting thing to visit actually.

*Mm.*

They’ve discovered quite a lot about the arrangements in the fort.

 Could you describe the, the laboratory in the old building to start with, in terms of the layout of benches and equipment and, pieces of technology and so on?

Yes. I think it must have been pretty crowded, because, the laboratory space that I was allotted was actually a, a hut on an outside roof. The first floor had a bit jutting out which had a, an asphalt roof, flat asphalt roof, and on this rather inviting space they have put a temporary hut, and that was the lab that I was asked to do these experiments in on silver chloride. And they were totally unsuccessful I’m afraid. But, it was not, it was better than the Cavendish, but, but not, not I would say very well equipped.

What were you using in that hut?

What was I using?
Yes.

Oh.

*In terms of equipment or...*

Well the most important thing was a microscope, polarising microscope. But the other equipment was for, possibly making single crystals, but I, I can’t remember actually to... I think, maybe we bought sheets of this silver chloride, but, we may have, we may well have, I think we did make single crystals ourselves. Mitchell certainly was doing all his own, his own manufacturing of the material he wanted to look at, and he as a chemist, I remember being somewhat scathing and sceptical about the rather crude methods that I was using for making the crystals, and he was concerned with matters of purity and things like that, whereas I couldn’t have cared less about the impurities. [pause] So, the conditions were fairly primitive.

[54:10]

*And could you describe the polarising microscope for people who wouldn’t ever have seen one and know what it does and...*

It’s a standard tool for mineralogists. And it’s like an ordinary microscope with, with an addition, namely two Polaroids, as you get in sunglasses. You put one Polaroid underneath the specimen and you put another Polaroid above it. And these are actually built in to the microscope. And they can be rotated relative to one another, and also, very importantly, the stage of the microscope, most microscopes have a stage that can be moved side to side and up and down, but, this microscope has a stage which you can rotate. That’s, that’s an essential part of it. And the rotation will be graduated and measurable. So, it’s like an ordinary microscope with these additions. There’s a third sort of thing you can put on which means you can rotate a crystal in three dimensions, but we didn’t use that very much. Does that explain your...?

Yes.

Yes.
And could you describe the, the new, if, if it did involve, the kind of, new laboratory, what was, what was new about the new laboratory in terms of physics?

The new building?

Yes.

Well of course it looked much more modern. The, the old building just looked Twenties, and was built of, in a style which you could describe as, a sort of mixture of Gothic and Classical. If you look at it, you’ll be astonished at the style. But, nowadays it’s, it’s all, it’s a listed building and it’s, it’s, it’s got a certain period charm about it, but the new building was strictly modern with large areas of glass, which were a mixed blessing because of the lack of climate control inside the building. It, it had... It used the old library. The library of the old building was a lovely, lovely room, oak-panelled, and it’s two rooms actually, and they, they extended that and, and, and that was the library for the whole Physics Department. In the new building it was mostly offices, new offices and labs, and we, I remember when it was just built, we were allowed to go into the, what had been a building site, and go and look at the accommodation and, as it were, put our names on a room which we thought we liked. Which we did. And it was all organised by Norman Thompson, who liaised with the architect, who was extremely good at this. The head of department was Maurice Pryce, he, he was a, he delegated the detail to Norman Thompson who did it magnificently.

But I remember meeting Maurice Pryce going down University Road one day when the lab was being built, and I said, ‘Where are you going?’ He said, ‘I’m going to the public library.’ ‘Why are you going to the public library?’ ‘I’m going to look up books on central heating.’ ‘Oh, why is that?’ ‘Well I’m going to the scheduling committee this afternoon, and I had to persuade them that heat goes up and not down.’ And the architect was proposing that the heat should come in from places, holes, in the ceilings, as indeed it still does. And the result is that the basement is absolutely
freezing and the heat for the basement actually heats the first floor and so on, up to the stiflingly hot fourth floor, where all the heat goes.

Mm.

That was... I don’t think the professional of heating engineer as such existed in 1964, but, that was the kind of thing that happened.

[58:53]
You realise of course that this project has got an interest in women in science as one of its aims.

I have gathered that.

[laughs] Yes. Could you comment on, could you comment on any female scientists in the Physics Department at Bristol?

In the Physics Department? Well, the librarian, Miss Littleton, was such. Now who was actually working? [pause] We have group photographs for almost every year, I think every year actually, at which you can count the number of women. They’re hanging up on the wall there. That would be a good way of doing it.

Mm.

But as for names, and, I’m struggling to find even one actually. [pause] I can’t remember at this moment even, even one. Isn’t that strange?

Even by 1964, would there...?

Even by 1964. Yes. I think the answer is, they were certainly not prominent. That’s an understatement.

[01:00:12]
Well another thing that I wanted to know about this period, I know that the, the Austerdalsbreen expeditions are 1955 to ’59, every year, and 1963. And we covered a lot of it last time. But I just wanted to ask about two things, three things. The first is, the role of Georgiana on the ’55 and ’56 expeditions.

Yes. By the way, the pronunciation is Austerbreen.

Austerbreen.

The, the, the valley is Austerdalsbre, b-r-e, bre, and then breen refers to the glacier.

Thank you.

The involvement of Georgiana. She came out with me the first year, you named it, I think it was...

’55.

...’55 is it?

Mm.

And, we knew nothing of course. And she, she got to see the routine, and, I think probably that year did most of the cooking, but certainly a lot of it, yah. And she learnt how things were done. We got most of the food free from, freeze-dried, there were government experiments going on to lay in supplies, emergency supplies for possible emergencies, to make tins of, like, kidneys, or, or, things you wouldn’t think perhaps of freeze-drying. But it was all, cod, freeze-dried cod, and we, they gave us, they gave them, gave us free tins in return for a, a kind of essay on the food when we came back, where we could report that the cod was very good and even the bones came through terribly well. The... This was the food that Georgiana would be concerned with. And the following year, she was in charge of the, of the food, commissariat, making sure we had enough of everything, and, and, arranging either to cook herself or to arrange rotas of cooks. It all worked out pretty well. Pat Baird, the
Canadian glaciologist, and explorer, was used to working in Baffin Island where there was lots of bad weather and you were confined to your tents for days at a time. And he knew that you had to take lots of flour with you to make bannock, because you could do that on a primus. So that, enormous quantities of flour were ordered for this Austerdalsbreen expedition through Pat Baird. And so, making bannock was one of the principal activities of the cook.

[01:03:34]
There was a, an occasion, since we’re talking about the commissariat, when the food was all stored in a little tent, separate tent, and we had cocoa in the evenings in mugs and we all sat round this big tent. It was an army, American army tent actually, and it would seat, well a dozen people with ease. And, the cook would sit doling out the cocoa, and the mugs would be passed round, and on this occasion the mug was passed first to George de Boer, a geographer, you may have heard his name.

*Mm.*

Do you know him?

*I, I’ve heard his name through...* 

Notting...

*But mainly through this.*

Nottingham.

*Mm.*

And, into this cocoa would be put a spoonful of evaporated milk powder. George de Boer had the first mug, and tasted it, and without a word he got to his feet, hurled the mug at the cook, and walked out of the tent. [laughs] It was a dramatic episode. And what had happened was, that the cement which we had used for fixing theodolite positions had been put in a container along with the foodstuffs, and some tidy person
had thought it was out of place and had put it together with the evaporated milk. So the result was that the cement was being put into the cocoa. [laughs]

*Who was the cook at that time?*

The cook?

*Mm.*

It wasn’t Georgiana. [laughs] It was somebody else. I can’t remember.

[01:05:28]

*What was his work at that place?*

George de Boer?

*Mm.*

He was very much a geographer. He just went round looking for things to measure and do, which was characteristic of Vaughan Lewis’s approach to field trips. He found a boulder which was sitting on an ice table. You may know that on glaciers the ice underneath boulders doesn’t melt as much as the other ice. So the boulders are left lying, sitting on pedestals of ice, and they’re called ice tables, the boulder being the table top. And, there was a big one just near where we were camping, and George de Boer was measuring the deformation of the ice caused by the boulder. What he did with the results, I have no idea, but that’s what he was doing.

*How was he measuring that?*

I don’t know. With a tape measure he was doing something. Wouldn’t be any good measuring to the ice surface, because it would be melting all the time. I don’t know what his fixed marks could have been. We had, in that first year, drills for the ice, which were terribly difficult to use, to make any progress at all. I watched people sitting on the handle of this brace, and turning it underneath them, trying to make
headway through the ice. And, I am no engineer and I didn’t know what the trouble was, but, Bill Ward, who led the later expeditions, knew exactly what to do to sharpen the drill. And he knew he had brought special files to sharpen the drills. And they went through them like butter. It was, it was very interesting to see the difference between the engineers’ approach and just the sort of, use what’s there approach of the geographers.

[01:07:19]
And, can you tell me more about Vaughan Lewis? I know, I know from another interviewee that he was prone to sort of periods of depression.

Yes.

And that, and from you that you took over the organisation of this work partly because of that. But, I wonder whether you knew any more about him. And I know that he came to Orowan with the model he had made of the glacier. Can you tell me anything more about him as a, a man and as a geographer?

Yes. Yes, I’d be glad to. I’d like to have a break at this point if we may.

[End of Track 7]
Could you tell me about Vaughan Lewis as a, as a geographer and as a person generally?

Well, I’ve told you how I first came in contact with him, when he came to see Orowan about his problem with the cirque glaciers. I think I had... The two, two words that come to mind are, first, he was first and foremost an enthusiast, and he had the gift of imbuing other people with enthusiasm as well. That was, that really was his great quality. He was very vigorous. We had a meeting I recall of the Glaciological Society called ‘Where do we go from here?’ And the rest of us were struggling to think out and say what we thought the main problems were, and Vaughan’s answer to this question, he said, ‘Where do we go from here? We go straight back to Norway.’ And I think that perhaps sums it up. He used to go to Norway every year, but there seemed to me to be no real sort of intellectual objective. He had this idea of rotational slip, which, I may be doing him an injustice here, but, it seemed to me came from two things, one is, that there are cirque, there are cirques and cirque glaciers where the ice does indeed slip and it rotates. It has to, because that’s the shape of them. And the other is, that there are what used to be called, and maybe still are called, thrust planes in glaciers, which may or may not be the thrust planes that the soil mechanics people talk about, and that are calculated mathematically, but are very obvious dirt bands. And when I show you the photographs, perhaps over lunch, I’ll show you pictures of them, and it’s easy to see how somebody who has this idea of rotational slip could think it must apply to these curved surfaces, which you can see in a cross section of a glacier. But, you know, in a way perhaps being over-critical, I don’t think it amounted very much, to very much more than just words. But there I’m being super-critical.

[02:52]

But Vaughan as a person was delightful. He was a, Welsh as you know, and, excitable, enthusiastic, and was very encouraging to students, enthused them with, with great vigour. That I think was his main contribution actually. He was a bit, I remember him being a bit... No I think I won’t, I won’t go on with that. But, I liked him very much.
And, a further thing I’d like to know about the trip is, as you might imagine, the gender relations on the trip involving the students, because you mention in the report of this that unusually for such an expedition you mixed the sexes in terms of the camping. And then you, you write that, ‘The men students readily accepted orders about backpacking from Leonora Berkeley.’

Berkeley [pronounced barclay].

Berkeley. Could you say what was, what lies behind that... Well can you explain what you mean? Who she was, or what that’s about.

Well she was early in on the whole series of expeditions, and she proved herself so efficient and such a good organiser of the activities that when it came to picking one of the students to be in charge, she was the obvious choice. And, because she was a girl, we obviously thought twice, would it OK? And we decided, yes, it would. And indeed it was. So, she was an outstanding example of, of a female being in charge of a whole load of males, and telling them what to do. But there were other female students on the expeditions of course.

So she was a student who came on one of the early ones?

Yes, geography student.

And then, was involved in organising...

At least I assume she was a student, but, she actually became a nurse afterwards, so maybe she was, perhaps she was a medical student. I don’t know, I’m not, I couldn’t swear she was a geography student, but somehow she, she got in on these expeditions. The official photographer for the expedition, Judith Thomas, was a woman, and there were later other, other women.

And there was Jean Clark as, as well, only separate.
Jean Clark was not concerned with Austerdalsbreen. She was, as it were, to the east, on the Jotunheimen. She had, she did her own thing there.

Yes. And, Leonora then was in charge of, what?

Leonora?

Leonora, in charge of, what?

Well there are so many details, when you’ve got a large party, a lot of details to, to work out. Who’s going to carry things from one place to another, and, have we got enough of this or that, or, who’s going to do the, be the measuring party tomorrow and who’s going to be the, the housekeeping party, and, and, well, you know, all the details of camping.

[06:40]

Mm. Right, thank you. And you also mention that a number of students who went on these expeditions developed later careers in glaciology. Are there some sort of, notable names of people who were students on these?

Well, I didn’t realise I had said that actually, because I don’t think I can name any.

Oh, OK. Perhaps you said, are likely to, or, I don’t know.

Well they certainly will have developed an interest.

Mm.

But, but I’m thinking... Well, there was a... George Ellison must have been, must have been on one of the trips at least, because he then proceeded to do his own expedition on similar lines in Iceland. So he must have been an example. George Ellison. Who else? [pause] I really don’t think there were any others actually. It would be nice to think there were. They weren’t all from Cambridge. Mostly they were from Cambridge but in later years, when it was really up to me, I recruited some
Bristol students and they were physicists. I don’t think any of them became glaciologists. So I’m not being very helpful there. But I think those are the facts actually.

[08:15]  
*And having organised the camp in mixed sex groups, unusually at this time, how did that work out, how did...?*

Worked out beautifully. I think the presence of girls on the expedition had a civilising effect on the men. There were some later expeditions when we were working not on the icefall but lower down on the glacier when the men students were behaving a little like, more like you would expect men students to behave, and, were teasing what few girls there were I think. It didn’t go very far, it was not a, it was not a source of, of great, of any difficulty really.

*Tressing in what sort of way?*

I don’t remember really. I think the usual sort of banter but I can’t really recall any.

*But not on these early ones? And what was the civilising effect then?’*

What was the civilising...? Well when, when there’s only men, they tend to be rougher and they swear more. And when there are women around, they don’t. I suspect it’s the same in the Army actually.

*Mm.*

I mean there was an extraordinary effect I noticed in Cambridge when I was in the Training Corps of these perfectly ordinary undergraduates, as soon as they put on khaki uniform and went to the training place, immediately became like old-fashioned, swearing soldiers, telling dirty jokes. It’s something about the uniform. It’s a dreadful thing actually, but, it’s a fact, sociological fact.

[10:26]
Thank you. The, the next episode in your life that I would like to ask about, but please jump in if you think that I’m, I’m missing things, was the, the period for, that, the California Institute of Technology in 1959, which seemed to have some connection with the IGY that had recently finished, and you went there with your wife and, and baby, lived in Pasadena, and made visits to various field parties. So, I suppose I’m asking in this case about the origin of that. Well I know about the origin of that visit, because you’ve explained how you came to know Bob Sharp at Caltech.

Yes.

But the, I mean, the story of that episode in your life had been sort of...

Well we went to, to Caltech I think... Let me see. We must have gone in two hops. We must have gone to New York and then crossed from New York. And we, we had accommodation in a house in Pasadena, which was very pleasant. We celebrated Thanksgiving there I recall. And I used to walk in to Caltech every day, it was within walking distance. It’s a lovely campus, and, it’s got this luxurious staff club called the Athenaeum, which, I think they boasted something like the largest carpet in the world. But it was a really luxurious place, built on lavish lines. And that was the staff club. The buildings themselves were very nicely designed, beautiful to look at. Have you ever been there?

No.

And, I was of course in the geology department. And I gave lectures there. Just trying to think. Yes, yes I remember now. Yes, I gave lectures there. And, I had recently been thinking about glacier waves, and I had written a, a mistaken paper called ‘Surging Glaciers’, because my idea was that surging glaciers were indeed examples of kinematic waves going down glaciers. I found I had been anticipated by, what it would be, fifty years. Finsterwalder had actually had the same idea, written a very good paper on this. But I had the thought that it would be rather fun to put in as the, what we call the, the driving function in a differential equation, not just ablation or, or accumulation, but a periodic change, a cyclic change, representing if you like the, the yearly cycle, and try and do the mathematics of what one might call the
frequency response of a glacier to a changing, seasonally changing snowfall and melting. Mathematically it’s called simply putting in a harmonic driving force, and trying to solve the equations. And I remember, that was quite a fruitful period because, I realised that the snout part of a glacier, that is the lowest part of a glacier, is inherently unstable. That had never occurred to anyone, least of all to me. But a simple argument shows that it’s inherently unstable, so you wonder then why, what stabilises it, why it doesn’t rush away. And, I mean there were simple answers to these questions, and the, the outcome of that was the first paper I wrote on the response of glaciers to seasonal and climatic changes. And I remember dreaming up that stuff at Caltech, or anyway, starting it there.

[15:14]
Barclay Kamb was a good colleague there. I mentioned his name before. He was primarily a crystallographer. He had worked out by X-ray analysis the crystal structure of all the different forms of ice. Could we break just for a moment?

[15:37]
Barclay Kamb was primarily a crystallographer. He had worked out the crystal structure of all or most of the different high pressure forms of ice. You may know that if you put ice under pressure there are phase changes. It changes into a different crystal structure according to how much pressure you put on it, and it depends on the temperature as well. And there are many many different phases. Extraordinary how the oxygen and hydrogens can adjust their relative positions and form repeating patterns of different kinds, sometimes interlocking lattices, and Barclay worked out those patterns. So he did some very fundamental work in ice. And he was also interested in its natural form in glaciers. So, he was a natural colleague there.

[16:38]
_Was there something about the, the set-up at, at this place that allowed him to do that work that couldn’t have been done in Britain for example in terms of the sort of, funding or...?_
Oh no, I don’t think so. I think, if the work on high pressure forms of ice had been done in Britain, that would have been a perfectly natural thing. It just happened, it just happened that it was done in America.

I see.

There’s nothing specifically American about it.

[17:12]

And, Bob Sharp wanted you come over to give American glaciology a shot in the arm.

That’s what he called it, yes.

What did he think it needed?

Well it needed first of all a grant, and he had obviously lobbied very hard for this, and he said, ‘You know John, the trouble is, that you get these ducks flying overhead, but you can never be sure that one’s going to fly over at dinner time.’ [laughs] That was his attitude towards landing grants. Quite right too. So he had landed this grant, which he was pleased about, and, invited me over, and it was used, part of it was used for, I got a salary, for paying for a trip for me to visit many centres of glaciology around the United States, which had been much stimulated, as you were saying, by the recent International Geophysical Year. So I visited... Should I just say where I visited?

Mm.

Well certainly University of British Columbia, which was becoming a centre. I visited Troy Pewe in Alaska, who was a permafrost, particularly permafrost man, and, I visited Tuso Wilson in Toronto, who told me, I remember vividly, about the very first ideas of plate tectonics. He had a, a globe, and he would draw great broad slashes across it where the plate boundaries were, and the idea of applying this idea, which had originated I think with Vine and Matthews in the mid-Atlantic ridge, applying it all over the globe, was a totally new one, and of course completely
revolutionised geophysics of the Earth. That’s a silly thing to say. Revolutionised geophysics. So, I met Tuso Wilson, and, I went to Ottawa, where they were keen on doing things with snow and ice, because of all the snow and ice in Canada. I went to Washington DC, and gave lectures there. And maybe there were other places too. So I got quite used to kind of going round and doing this lecturing to various audiences.

[20:00]

Why did Bob Sharp think that you were the person to administer the shot in the arm if you like? What was, what was he after?

Well it was this, this whole business of, of mathematising glaciology, really, which he thought was a good idea. Bob Sharp is a great teacher, and a great presenter, like Vaughan Lewis, a great enthusiast, and, he likes propagating ideas, popularising if you like. Very good at it.

So, what was, what did you see going on there when you visited these field sites, in terms of glaciology?

In terms of glaciology? Let me first tell you that he took me on a geological field trip where I was, I learnt something about geology. I learnt that geologists had very keen eyes for certain features which were not visible to the ordinary untutored eye. He took students on a field trip to a, a gulch, where there was a bridge, and asked them to go around, sniff around and see what they could find out, and actually there was a landslip, a potential landslip at a certain place. And, it wasn’t at all obvious, but that was their field trip. And, I learnt something about what geologists do on field trips, anyway on his field trips.

[21:41]

Now you said, what about these other visits? Well, they told me what they were doing, and, I was usually pretty interested in it, and, and I gave them a lecture and told them what I was doing. That’s the way these visits are planned. I saw a permafrost for example in Alaska which was fascinating because, I was up at Point Barrow, which is the northernmost point of Alaska. No glaciers up there, but, certainly permafrost. And, you, you must know about patterned, as a geographer, about patterned ground, and you know that under each of the sides of the polygons there’s
an ice wedge. Well, it happens that near Point Barrow these, I suppose the action of
the sea has worn away cliffs, so you can see cross-sections of these buried ice wedges,
and they go down for, I think forty feet was one of the figures mentioned. They’re big
things. And the whole business of, how on earth, why on earth they develop, was a
rather open question around that time. But, Art Lachenbruch made up a theory,
mathematical theory, which depended on showing that the, it wouldn’t happen if you
had ice which was simply elastic, and it wouldn’t happen if you had ice which was
perfectly plastic, but it would only happen if you had ice which was somewhere in
between. And, that was a, that was a mathematical approach to something that was
very very geomorphological. I was a great admirer of Art Lachenbruch’s work at that
time.

[23:45]
Now I didn’t know in your unpublished autobiography whether you were talking
about all of the glaciology you saw on the various places that you visited, or whether
you were just talking about Point Barrow. But you said that you, you didn’t find the,
the glaciology especially interesting, but you thought that the Eskimos certainly were,
and, so I wondered if you could comment on both of those things.

I don’t think I didn’t think glaciology interesting. I mean up in Point Barrow there are
no glaciers.

Mm.

The point doesn’t arise.

No.

Yes, I, I certainly enjoyed meeting the Eskimos. They had, I was allowed to borrow a
Jeep, and drove from the, I guess it was an army camp at Point Barrow, along the
shore to Barrow Eskimo settlement, and there I, I went to church with the Eskimos.
And I was given a special prayer book bound in sealskin with a special rabbit’s bit of
fur on it. I don’t think the rabbits came from Alaska somehow, but that was a very
special thing to have. And the whole, all the pews in the front of you were filled with
Eskimo men and women in very colourful kind of anorak. And every now and again a child would appear from underneath the anorak of one of the women and appear down below, and they struggled around and then climbed back onto their mother’s back, under these enormously thick, cold protecting clothing. And then I got into my Land Rover and it was, Jeep, and it was getting dark, and I wanted to switch on the lights. Looking for the switch, couldn’t find the switch. All these Eskimo kids around, they knew where the switch was in a Land Rover, a Jeep I should say, and so they switched them on for me, [laughs] I remember that. But, it was, it was, it was an interesting experience. And then driving back along the shore, back to the army camp.

*Could you, could you communicate with the Eskimos in some way?*

I must have been able to, otherwise I wouldn’t have been invited to church.

*Mm.*

It was probably, they probably spoke American.

*Mm.*

I can’t remember very well.

*And can you describe their, their settlement as you remember it?*

[eating biscuits] These biscuits are going to spoil your recording.

[End of Track 8]
OK, I wonder whether you could describe the Eskimo settlement as a physical place that you visited.

Well I’ve already told you how I went to church there.

Yes.

That was my only visit to it, so I can’t tell you very, any more than that.

You didn’t see their sort of village or settlement or...?

Oh yes, yes I was in the village. It was, it was built of wooden houses. They weren’t igloos, it was, after it was the summer time. But they would have, they would have made their livings I’m sure by going out and hunting on the sea ice. There was a, there was a bit of a tension at the time I think because the younger people in the village were going off, were going south into the United States proper, to be educated, and then coming back and not being satisfied with their home life. But that’s happened all over the world.

[01:13] Thank you. Now at Caltech you started to work on, as you’ve said, on links between the advance and retreat of glaciers and the rates of accumulation and ablation. And you mention the work of Lighthill and, sorry, I’ve forgotten this name again, the...

Whitham.

Whitham, on kinematic waves, and which then applied to traffic flow for example. And you thought this might apply equally well to the, the undulations on the surface of glaciers, which were, which had been pointed out in terms of the Trans-Antarctic expedition along the route of it, so which was at the time of the IGY wasn’t it. And I wondered whether you could explain the, the links between traffic flow, kinematic waves, and the waves on the surface of a expedition route.
No, I think there’s some confusion there.

Undoubtedly, yes.

There’s no connection that I know of between kinematic waves and the sort of waves, undulations, that we measured on Austerdalsbreen at the foot of icefalls. Nor I think is there much connection with the waves, the undulations on the surface of the Antarctic ice sheet, which were noted on the Trans-Antarctic expedition, by for example Fuchs and Hal Lister. I don’t think that’s got much to do with kinematic waves.

So the surface waves on the expedition, that’s, that’s related to your work on the roughness of the, of the bed, so that the, the surface waves reflect in some way the, the much...

Sorry, are you talking about the Norwegian work, or, or...?

No. More general, your theoretical work on, on glaciers, and...

Yes. I think, I think perhaps there’s a, it’s a, a source of confusion, and perhaps it’s partly my fault, that, I talk about kinematic waves on glaciers and people immediately think of undulations. But they’re not at all like that. Kinematic waves are, are more like shockwaves, as in traffic, when you get held up at a, at a congestion point, and there’s a... and that, that situation is passed back along the traffic flow. That’s a kinematic wave. But there’s no up and down about it, no undulation.

So it’s merely about how the glacier moves?

It’s about...

Or about how the ice sheet...
It’s about how it moves, but it’s not, it’s not to do with... People think waves, they think of sine waves, but it’s not to do with sine waves, they don’t play a, a special role in, with kinematic waves, to put it rather crudely.

_Could you then..._

They’re more like shockwaves.

[04:28]
_Could you then say how Lighthill and Whitham’s work applied to what you were thinking about in terms of ice movement?_

Well Lighthill and Whitham’s work on traffic flow introduced a new idea into the mathematics of what we call waves. But I think that’s a technical use of the word wave, which is not a very useful word in popularising it. There is a moving disturbance. We talk about for instance the Severn Bore as being a wave, but it’s not a, it does have undulations associated with it but that’s not the main phenomenon. It’s still, it’s still a wave. It happens to be a solitary wave. So, Lighthill and Whitham’s work on traffic flow is in some rather, in some sense applicable to, say, the floodings of the Nile, which is a long river which happens not to be supplied by tributaries towards its end. It’s just a long river subject to evaporation as it goes towards the sea. That’s much more, a much better analogy with what goes on in a glacier, which is melting at the surface just as the Nile is evaporating. But, the flow of the Nile, the periodic floodings of the Nile, are a much more complicated issue, and indeed I think Lighthill and Whitham’s paper on traffic flow is succeeded in a second paper by them specifically on long rivers, and that relates to the phenomenon that we’re talking about. I’ve given you a very confused explanation of quite a complicated topic.

[06:46]
_No, it’s OK. It might have been more helpful for me to ask at this time, at the end of the Fifties and the early Sixties, how, how was your theoretical work on ice developing, what were you...?_
I, I think at the time I was thinking very much of the response of glaciers and ice sheets to changing annual and seasonal and climate, seasonal is, is annual, changes. And which was of course a very important topic in glaciology and has recently become very prominent. But, it seemed to me to be important to relate the many observations there had been of the advance and retreat of the ends of glaciers, for instance in the Alps they go back to something like 1896, the records start, so you can see these records of forward and backward and so on, to relate them to, in some proper way, to changes of climate. And, of course there’s a seasonal, there’s an annual change, because, in the summer naturally it melts back, and in winter it’s all covered with snow. And in, when the snow melts you find it’s gone a little bit further forward, or perhaps further backwards. But that’s what they measure. And that must be to do with, whether it goes forward or backwards must be to do with whether it’s getting more and more snowy. And I wanted to connect these two things, the rate of feeding by the snow and the movement of the very end, which is what’s recorded. And maybe you can argue backwards from the movements of the snout to the, what had happened must have happened up in the higher reaches of the glacier, where it hadn’t been measured. In other words, infer climate change from glacier snout changes which had been recorded. And that was the, that was the, really the driving the force for all that work.

[09:29]

And I did do some, did produce some sort of, what are called algorithms, that is, a series of steps you can go through to infer one from the other. For example, let me give you an example. Later on, when I was at University of Washington, my colleague Untersteiner, Norbert Untersteiner, was approached by a copper mining company which had a copper mine in front of a glacier called the Berenden Glacier. They needed to dig a tunnel through the rock and get at the copper, and the tunnel mouth was not very far from the end of the glacier. And they wanted to know how long it would be before the glacier came forward and blocked off their tunnel mouth and stopped their whole mining process. So, they came to Untersteiner and asked him, and he said, ‘Well, Nye has got a theory about all this, we’ll ask him.’ So, I said, ‘Well what information have you got about the glacier?’ Have we got information about how fast it’s moving forward, and, how fast the ice is moving, and, and how wide it is, and how deep it is? And these, these were the essential numbers that need to go into my model.
We put them into my model, and rather confidently predicted that unless there was something quite fantastically unusual happened, nobody could predict the, the change of climate in the future, but, some changes are so extreme that you can virtually rule them out. If you ruled out totally extreme things, then, we could confidently say that the front of the mine would last for twenty years and even forty years. The forty years, the twenty years thing has passed now, forty years is coming up, and the mine is still, so far as I know, perfectly OK. And that’s, that’s what we predicted. So that was an application of the work that I was doing and thinking about in the period that you asked about, 1960s.

*And the equations would work in both directions so that you could, you could find the position of the snout from supposed changed in climate...*

Yes.

*...and that you could look at changes in the position of the snout and make arguments about past climate?*

Precisely. Yes.

[12:14]

*And, in your papers on this, you're, you’re looking obviously to test this equation that you’ve made, using, to test the equation that you’ve made using actual field measurements. And, there’s a Dr Meier and the South Cascade Glacier.*

Yes, that’s right.

*Why, why was that particular glacier apparently one of the only ones that you could use at that time to test your mathematical work?*

Well, Mark Meier had worked on the South Cascade Glacier for years, and, there were at that time very few, if any, other glaciers in the world which had been studied so, measured so meticulously. So it was an obvious candidate for using, the data that
he had was very extensive on how thick it was and how fast it was moving in different places, all this, all these essential geometrical, all this essential geometrical information you needed, and, also some idea of how the accumulation and ablation had altered with time. So, looking around for candidate glaciers to test these ideas on, it was an obvious choice.

Were there other people at this time working on... They probably weren’t at his time I don’t think worried about the glacier and ice retreat, or climate change, but were there other people working on this? In other words concerned to predict, you know, the future of...?

Well, I mean the natural idea is that if the glaciers are going back, it’s getting warmer, and if the glacier’s advancing, it’s getting colder. And this is merely a, an elaboration of that naïve idea. And the Swiss glaciers in particular had been measured, as I was mentioning, from 1896 onwards, and each year there was a little book of measurements on Swiss glaciers, they were very proud of them, quite rightly. Which was the record. So, yes, I mean the Swiss were very concerned about, what’s going to happen to our glaciers? Is our hotel still going to be able to see glaciers out of its windows? That kind of question.

Was there at this time a wider sort of popular and political or, perhaps just scientific interest in climate change and glaciers as indicators of that?

I don’t think people talked about climate change much. I mean they were, obviously people were very interested in the causes of the Ice Ages, and it was beginning to be generally accepted that the fundamental driving force was astronomical. They were the astronomical cycles, they were called the Milankovitch cycles, which were a rather more sophisticated way of interpreting the data, the astronomical data. You had to think about not just how much more sunlight, warmth, would be impinging on the earth, but also at what latitude it would be impinging and whether there were glaciers there. That kind of thing. And, it’s now generally recognised I think that the fundamental driving force for the Ice Ages is astronomical cycles. But at that time it was, it was only beginning to be understood; even though he, Milankovitch actually proposed his cycles I... would it be as far back as 1910?
I think it was, and, it’s sometimes argued that it was kind of his, his status as a kind of foreign researcher that sort of prejudiced the British, for example, geological community against his sort of theories, and it sort of wasn’t taken seriously.

It could have been, it could have been. But, I think it’s taken quite seriously now.

Yes. Yes.

And certainly the climate models are driven by cycles.

Mm.

And, there’s one prominent cycle which is 100,000 years, which coincides with the periodicity of the Ice Ages way back. So that you’ve got, by rule of thumb, one Ice Age every 100,000 years.

Mm. Mm.

There was a time, I haven’t kept up with all this, but there was a time when the difficulty was that the power in the 100,000 cycle oscillation was nothing like large enough to drive the whole thing. Whereas there were other cycles like the 20,000-year cycle which seemed to be more prominent, but didn’t seem to show up so much in the, in the ice record, the Ice Age record. I’m not sure how all this has been resolved, but those were the sort of questions that were being asked.

[17:43]

I notice that you gave a paper on this work for the Glaciological Society, and, at the end of it Gordon Robin says, I think he was in the chair, and he said something like, ‘I think Dr Nye hopes that someone will put him on to the right facts and figures so the theory can be tested out rather more exhaustively before handed on to someone like Professor Manley as a technique which he would use for his climate extrapolations.’ So there’s, there’s two things there. I mean, obviously, you would have liked, you know, some more facts and figures, because you needed sort of quite accurate, well,
not quite accurate, but, but quite a lot of measurements of sort of depth and position
and this kind of thing to test out the equation. But, I then was sort of, wondering what
Professor Manley was doing with his climate extrapolations, because this seems to be
quite an early, an early effort to model future climate.

Yes.

And so... But, are you able to tell me about Professor Manley’s work and...?

Professor Manley was professor at one of the London colleges, was it Royal
Holloway College? And, he was the great expert in compiling temperature records.
In this work you want to go back as far as you can, and of course the earliest records
are very unreliable because people used thermometers in a unusual way and they
didn’t take the usual precautions that are taken now with taking the temperatures
and... Manley was the chap who compiled the record of temperature going back as far
as we can, probably in London. So that was his great contribution.
[19:42]
If you met Professor Manley he would say, ‘Where do you come from?’ And you
would say...

Norfolk.

Norfolk. ‘Ah yes,’ he’d say, ‘Norfolk. You know, that had the record snowfall in
1892.’ He was that sort of person, who would, was a walking encyclopaedia about
changes of climate, from year to year.

And was he, as seems to be suggested by this, extrapolating forwards, predicting?

Well I can’t remember his role in that. He... I mean, it’s a perfectly reasonable thing
to do. What you do is, make a Fourier analysis of your record and assume that the,
it’s going to continue. And, people do this with the Stock Exchange and with the
weather and... There is some basis for, for extrapolation, like, it’s quite likely that the
weather tomorrow will be much like it is today. That sort of extrapolation. But I
don’t think Manley, he wasn’t a mathematician, and I don’t think he was particularly
into that kind of forecasting. He was a meteorologist and a, and a historian of, of temperature change.

Mm.

I mean, I know nothing about his other professional background. You may find that that’s a, a very small area of what he was interested in, but that’s the part I knew.

[End of Track 9]
OK, if I...

Are we on tape?

Yes. Mhm. Would you be able to tell me about family life in the late Fifties and...

In the late Fifties? Well we had our first child, daughter, Hilary, in 1957, and then, we had our second, Stephen, 1960, and a third, a second... a third child, a second daughter, in 1963, that was Carolyn. And, the two girls are still living close by in, in Bristol. My son is living now in America. But we’re very glad to be so close to the girls.

What was the effect on your working life of the coming children?

Having children? Well, Georgiana no longer came to the glacier of course. But, I don’t think there was very much effect on my working life. Perhaps I was rather selfish and, and took away from my fatherly duties, but, it really didn’t make such a lot of difference.

What did your fatherly duties involve when you weren’t at work?

Well the usual things. I mean I don’t think I was a very good father, but, Georgiana was such a good mother. I mean changing nappies and that sort of thing was not really my department. What else can I say? I mean, I enjoyed playing with them, and, Hilary used to climb the trees in the garden, and, swings and... Oh we went on some lovely holidays, really gorgeous holidays with them in the Gower, you know, the Gower Peninsular, Wales, with our very good friends the Black family, who I’m glad to say still live very close to us here in Litfield Road, just across there. We went on gorgeous holidays there, renting a cottage, and enjoying the sandy beaches. I was just looking at some photos the other day, and remembering about all that. That was a very pleasant time.
Could you describe in a little more detail the sorts of things that you did and...?

On holiday you mean?

Yes.

Well they were beach holidays largely, just messing around on the sand, you know. How else? What else did we do? Well we walked... We very much enjoyed walking, and we walked on the Gower, enjoying the scenery.

[03:00]
My wife’s father and mother lived in a house overlooking the Pacific Ocean, Arch Cape, and we used to, from time to time, manage to visit them there, and we had the same sort of feeling of being so close to the sea and thoroughly enjoying the, the marine atmosphere. It’s hard to be more specific.

[03:32]
Were there times that you can think of where family life related to your scientific work, in specific ways? I’m thinking of, the example that we had last time was of Lawrence Bragg doing his gardening and mixing oil, and noticing the bubbles. And, you’ve got, obviously, a great interest in the sort of materiality of the world. So I wondered whether it was possible for you, I’m sure it is actually, this may be fanciful, but, going on a beach holiday for example with the family, do you, do you look at the materials of the world in...?

Very much so.

Ah. Well could you tell me about that please?

Well, I mean I watch the waves, which are full of fascinating and unexplained phenomena. I watch the, the shore line and understand how the successive waves make a new pattern on the sand, and how they overlap, very much as successive glaciations actually cover the Earth. And, if you go to Scandinavia, you can find areas where there are striations which tell the geomorphologists what the glacier, which direction the glaciers have been moving in, and in some areas you can find
crossed striations. And if you look at water waves coming in on a beach, you understand how that can be, how successive swirls coming in have an overlapping area. And, I once wrote a paper about that, and, that was clearly inspired by looking at waves coming in on the beach.

*What about the packing of sand and arrangement of...?*

Well, Arch Cape where we went happens to be an area where you get singing sand. I still don’t know the complete explanation of singing sand, but I believe there is one. It’s a very mysterious phenomenon, because the note you get has nothing to do with the, any dimensions that are obvious in the sand.

[05:48]

*On these holidays then, did you combine holidaying with any actual measuring, or calculating?*

No, not at all. No, they were complete holidays, I didn’t take my work with me on holidays. Though I probably thought about it. But I didn’t, I didn’t take paper and pencil with me. I was going to say that I always regret not having studied the life sciences. It was a, as I think I mentioned in an earlier interview, at Stowe School it wasn’t possible to do both biology and physics and chemistry. The result is, I was totally ignorant of the life sciences. And, when you go on holidays of course you’re particularly conscious of the wildlife, birds and fishes and, whatever, and I was very consciously ignorant of that aspect of things and wished I could understand them better. I mean there’s plenty to, to excite a mind interested in geometry and symmetry and things like that, in shells and living creatures, the shape of a leaf, or a tree. I’m very interested in all that, but from a, from a very ignorant point of view.

[07:21]

*And, were you, were you talking to your, for example your wife about the things that you noticed about the sea and...?*

Probably, yes.
And what...

Probably boring her, yes. [laughs]

What would have been, what was her reaction to that kind of observation and conversation?

Well, I think interest, but it wasn’t her point of view. I mean for example, if you look at the sun setting from a, say a hotel window, setting over a sea, there’s often a distinct pattern of, a ribbon of light stretching from the setting sun towards you. And that ribbon of light tells you quite a lot about the state of the sea. It’s not always the same. Sometimes the horizon is lit up, and sometimes it’s more like a ribbon of light stretching towards you. And then you ask yourself, well why is the sea blue anyway? And you go back to, to the works of Lord Raleigh, and you ask, is it just reflecting the blue sky or is it actually the colour of the water, is it greenish or blueish? And, and you start thinking about, I start thinking about things like that. I think I’m quite a visually aware person, and I do kind of seek explanations for things. But usually without success.

[08:55]

As your children grew up, and you started to be able to have conversations with them, can you remember conversations about a scientific way of understanding the world with your children?

I probably was guilty of talking to them about it, but I don’t consciously remember any particular occasions. They showed no, no particular special interest or attitude for it. So... [pause] I suppose I could have been disappointed, but I, I don’t think I was. I just accepted it as a fact. I mean children are children, each one’s an individual, and I think it’s a great mistake to try and push them into some sort of preconceived mould.

And, perhaps as, as young, as young teenagers, did they, or, as older children, did they know what you did for a living, when you went off to the department at Bristol, did they know what you were doing?
I don’t think they did, and I think they’re only just realising that I’m still doing it. [laughs] Did I tell you the story of the little girl from nearby who was taken to the zoo?

No.

Well she was taken to the zoo to see the rhinoceroses, and I think they were all sort of white rhinoceros, rather a, a rare thing. And, she looked into the enclosure and she said, ‘There’s the baby rhinoceros, and there’s the mummy rhinoceros. Where’s the daddy rhinoceros?’ And she thought for a while and she said, ‘Oh I suppose he must be at the university.’ [laughs]

When did you...

So I think that’s probably the way I was looked at, from the point of view of the family.

So they were, they weren’t asking you questions about, you know, how, what have you been doing lately on, natural focusing of light?

I told them, but I’m not sure how interested they were. I did give them a, a sort of slide show of my Antarctic and polar travels one Christmas a few years ago, and they pronounced great interest in it, and, and said, ‘Oh you must do it again,’ and all that sort of thing. But, that was a one-off, a bit of a one-off occasion. I think they were all a bit surprised to see their father had gone to all these funny places.

And what is, how would you describe the extent of your, Georgiana’s interest in your scientific work?

Georgiana’s interest? Well supportive, definitely supportive. But... And I sometimes make attempts to, to explain things to her, and she makes attempts to understand them, but it’s not actually her metier. She is a, an artistic person and brought up to, with, with a passion for dance, and she’s still teaching dance actually. So her, for her
it’s much more body feelings than, than mind feelings. So we’re very opposite in many ways. But it works out very well.

[12:35]

And having this very particular interest in the way materials behave in the world, for example looking at the sea, many people say, there’s the sea, wouldn’t want to go swimming in there, or, that looks nice. But you’re very, very interested in the way the material behaves and the way different kinds of material behave. And, did you find it... Who did you find you were able to talk to about that kind of understanding of the world, and they would be, be observing in a similar way if you like?

I don’t think anybody precisely like that. But, there is a great tradition, a very good tradition, on having tea and coffee in my lab with the research students and the rest of the staff. It waxes and wanes. Sometimes it’s crowded, sometimes only a very few people, but it, it carries on from J J Thomson’s idea in the Cavendish of having, getting all the staff together for tea, and in many labs I’ve been to, they do this. But I’ve also been in labs where it’s an unknown thing. And the difference is very very striking about lack of interaction between people. So, any sort of, thoughts of the kind you are having would mean that I would be talking to other people in my lab. But, I mean, we would, if I had seen some natural phenomenon which was worthy of comment, I certainly would say, ‘Did you notice such-and-such?’ And, and discuss it with them. And I have two particular colleagues, close colleagues, whom I’ve been in contact with now for twenty, well thirty years or more, even forty I think, Michael Berry, Sir Michael Berry, and John Hannay, who share my kind of interest in curiosities. An example is, is Michael Berry’s passion for the Severn Bore. He regularly takes parties to see it, and keeps a little timetable of the, when the best bores are going to be. Because, you may know that sometimes there are good ones and sometimes there are rather miserable ones. So you pick your day from Michael Berry’s records. He gets them I suppose from the tide tables or, wherever. And, he is, he knows a lot about the bores, and you know a bore isn’t just a, a big wave coming down, it’s got surfers on it who surf the little ripples behind. Now those little ripples behind are what in mathematics we call an Airy function, and Airy functions are the very beginning of the sort of things discussed in my optics book that you were just mentioning. So, those particular colleagues I talked to regularly. And there have
been others. David Gibbs. Now David Gibbs, I remember saying at his funeral, every lab ought to have a David Gibbs. The sort of person you can go to when you have a problem and you need some sort of guidance or advice on it, and you say, ‘What do you think about this?’ And he will scratch his head and probably come up with something you never thought of, on almost any topic. With David it was music, acoustics, and that sort of thing, or, electrical phenomena. I mean he was absolutely marvellous at those things. With Michael Berry it’s almost any natural phenomenon. He’s a first-class theoretical, theoretical physicist, but not at all averse to doing experiments in his own bathroom and watching water going down plugholes, or looking at raindrops on windowpanes, or, all the little things around you that you don’t think of looking at. I want to tell you a bit later about water drops on windowpanes. Remind me.

[17:22]

OK. Thank you. OK. You, you moved into a rented flat in, in Bristol, and, you bought the house that we’re in at the moment in 1960...?

‘65, in this house, yes.

‘65. I wonder whether you tell me about your interest in gardening, which you developed here.

Yes. Well I had no interest whatever in gardening as a, as a youth. My father and mother had a garden, but I was a bit, a bit derisory about their listening to the radio programmes from, whoever was the radio pundit at the time on, on gardening. And, it didn’t interest me, until I had to look after my own garden at No. 21 Canynge Road. And I remember well buying the first plants, hadn’t a clue what to buy. But I got a, I suppose this is characteristic, I got out a book, and, got some advice from a book and went to the garden, wasn’t a garden centre then, they didn’t sell plants quite at garden centres, but I bought a few things and put ’em in, and they were the beginning of gardening. And then I put in some bulbs and realised I had to look after things, and there was an apple tree, and we stored the apples. There were tubs outside that had to be planted. And I, I rather got bitten by the gardening bug. And when I came here, the garden was a very formal Victorian garden with a path straight down the middle,
and very formal, straight flowerbeds on either side. And if you look at it now, you’ll see it’s quite different. It’s got a curving path, and a pond at the end, and much wider, the flowerbeds were four feet wide. You can’t do anything in a flowerbed four feet wide, they’ve got to be at least eight feet. And... Unless they’re on the shady side. And then, I, I really became very interested in gardening. I bought gardening books and listened to the radio gardening, *Gardener’s World* or whatever was the programme at the time. I was a great admirer of Geoff Hamilton. So, that’s my interest in gardening. Now, unfortunately for the last nine months I haven’t been able to do a thing in the garden, and that distresses me quite a lot.

[20:04]

*In the same way that you looked at the beach in a particular way, did you look at the garden and at plants and at soils and at growth in particular ways?*

Well I, I suppose, I did in a way, but, in a very unbiological way, because I knew nothing of biology, I know nothing of biology. But shapes and forms are interesting to me. There’s a lovely book by D’Arcy Thompson, quite a, quite an influential classic, on shape and form, and it’s about shapes of almost every natural object, particularly shells and tree rings and snowflakes, geometrical ideas, shape and form. That interests me very much. I have a, a friend, Tony Walsby, who was Professor of Botany here, and I used to have lunch with him most days, and he comes here and we talk together. He is a, he’s a botanist who’s somewhat inclined towards applying classical physics to, to what he is studying, and we, I often ask him questions. And if I want to know, if I have questions, as I often do, about cells and DNA and how the RNA works and, and what the mitochondria really are, and all that sort of stuff, I ask Tony, and he gives me very careful explanations, which I’m very glad of.

[21:48]

*Thank you. Could you tell me about the, the US Army trip to the Greenland ice cap?*

Yes. What happened there? The US Army had a desire to find out how they would allow troops to exist on an ice cap, and they made a camp called Camp Century, which was, I don’t how many tens of miles from the coast in northern Greenland. It might be as much as 100 miles. And there, they also wished to do science, partly
perhaps to justify their effort there, and there they made the first deep boring through the Greenland ice sheet, the Camp Century hole, which they, from which they took cores, and the cores gave them a record of the annual layers which have become so important in tracing our climate record. It was first done at Camp Century. So it was a place to go to. And I, I can’t remember how I managed to do it, but I must have persuaded somebody that I, I ought to go there. [laughs] And they very kindly said they’d take me there. I remember that I, I was sort of, half prepared to go and then, then the letter came, I think on a Friday, saying, ‘You will report at Mildenhall Air Force station on Monday, and take further orders.’ [laughs] And I... So I duly turned up at Mildenhall on Monday, Mildenhall, Suffolk, with my passport, and no money, no cheque, no travellers’ cheques. I was not prepared actually to go abroad at a second’s notice, with the banks closed. And, as I was waiting for my plane, an American colonel said, ‘Well, if there’s anything I can do for you, just ask.’ So I said, ‘Well as a matter of fact there is. I need some money.’ [laughs] And he immediately retreated, and said, ‘Oh go and get into some crap game somewhere.’ [laughs]

So I was flown over to the United States by military transport, where incidentally you always face backwards, which is a good thing, but, ought to do it on civilian planes, but they don’t. And flown to, Washington I think, DC, and then flown up to CRREL. No, no no, I think I flew up... No I flew up straight from Washington I think, and, flew straight up to Greenland, which is a gorgeous flight, because you pass very close to the North Magnetic Pole, and you have gorgeous auroras borealis to look at. And it was very spectacular, I remember going up there. You then fly to, I did fly to Thule, which is the American base, on the cost. They moved an Eskimo village to put Thule there, so the Eskimos lived in a new place a little bit further up the coast, and the idea was that the American troops would not interfere with their way of life. So the Eskimos were all well separated from the American camp.

I did go up there to visit the Eskimo village, and I found that they had, you could see Eskimo graves on the hillside, not of course dug down, because, you can’t, the ground is just too hard and rocky, so they were built up with slabs of stone. And, some of the American GIs had removed a slab, or cracked it, and, inside, there was the dead Eskimo with his, wearing his parker and, lying there.
They cracked it on purpose, or...?

Oh I think so probably, yes. And I watched an Eskimo there stalking a seal. They have a, a sort of, miniature sailing boat, a sort of child’s sailing boat kind of thing, which has a big sail on it, which they pushed in front of them as they creep up, and the seal can’t see them because they’ve got this white sail, until they get close enough to attack the seal. And I saw a hunter actually using one of these things, which was quite an experience.

But, Thule has a camp nearby called Camp Tuto, and Camp Tuto is pretty primitive. The huts are built on permafrost, and it, they’re all built up on stilts, because you can’t just build straight on the permafrost. It’s a pretty rough place. And it’s built where it is, they’ve got a, a tunnel from there right into the cliff of the Greenland ice sheet, a tunnel big enough to take trucks, and right, you go into this tunnel and you get a great view of what it’s like inside the ice sheet, with all the layers and imperfections showing up. And then at the end are the huts which the soldiers sleep in. These huts are insulated so well that they don’t need heating. In fact they put in, I think they were 100-watt, 1,000-watt electric heaters to keep the poor boys warm during the winter, but they didn’t have to switch ’em on. The body heat and the insulation was enough to keep the huts nicely warm. That’s what insulation will do. But that was at Camp Tuto. And you could see the structure of the ice which I was most interested in, in section. I had seen it before on the Mer de Glace with a tunnel that they dig for tourists, but the Tuto one was especially interesting. All the contortions in the ice, when it’s in close contact with a rock, for example.

Did you make a study of that?

No, just went as tourist really, just looking. And then, then I was taken on what they call a big swing. A big swing was a procession of tracked vehicles going across the surface of the ice sheet to Camp Century, which was, I don’t remember, it was probably somewhere between sixty and 100 miles. And these... It took three days, because they travelled so slowly. I think their rate of progress was a bit slower than walking pace, about two miles an hour or something like that. And they rumbled over
the, over the snow and ice, and sleeping in them was very like being in a, an express train going at about eighty miles an hour with that sort of noise. And actually you were going in these enormous caterpillar-tracked vehicles at about two miles an hour. Actually they, to be precise, the whole thing was dragged by what they called a, a big CAT, huge Caterpillar tractor, and it was dragging these Wanigans along, and they had sledge-runners. And what you were hearing was the crunch of the sledge-runners on the snow. They had refrigerators inside, and the supplies of food were kept on the roof, where they were cold enough. And the daily supplies needed were taken from the roof and put in the fridge, inside.

[30:37]

After two days, we were going along, rumbling along at night, and we picked up some flyers who had been on an exercise for, what do you call it, survival, survival training, and they had spent a couple, they’d spent a night in a snow hole and were expecting to be picked up by helicopter that day. But the helicopter couldn’t get through, so we had to pick ’em up the following night. And I remember them stumbling in, looking for bunks, and... [laughs] And, then eventually, we got near Camp Century, and I got the fun of sitting in the cab of the tractor that was pulling the whole thing. There was only one thing to remember, that was that when you started off, you mustn’t engage the clutch straight away, otherwise you will break all the couplings of the whole wagon load. So you had to go very gently, with two handles to steer, as you do with a tracked vehicle. And we were going through a white-out along a, a track, and I saw something which was, seemed to be just beside the track. And as the time went on, we didn’t seem to get any closer, and I realised that in the distance I was seeing Camp Century on the horizon. That’s what white-outs do, you don’t know what’s going on at all.

[32:15]

And eventually we reached Camp Century, which was a conception of a Swiss, very, what’s his name? Oh. Oh dear, he, he’s the, he is the engineer, do you know...

*Mm.*

He got the first, or one of the first Seligman Crystals. And he founded CRREL, the Cold Regions Engineering Laboratory of the US Army. He had the idea of taking a bulldozer, running it through the snow, and gradually digging a deeper and deeper
trench. I watched one of these trenches being dug with a bulldozer, and it was going as deep as it could. And there was a fundamental limit to how far down you can go, which is not obvious. But, what happens eventually is that the pile of snow which you're trying to move forward is so high that it's falling backwards over the top of the blade, and you can't then do anything more than that. So it's as deep as you can go. So he did, he, they dug these long trenches, put huts in them, and then roofed them over with corrugated metal. And then, the plan was then, take a snow plough and cover the whole thing in snow and make a snow roof, and then take away the metal. Actually they left the metal in place. I suppose there was no reason to take it away.

[34:00]

So, Camp Century was this, not network because it was a series of parallel tunnels joined together, with huts in them, with people living in them, and that's how troops were going to be accommodated. The whole thing was powered with a nuclear reactor, one of the first. And I've got a slide of me sitting at the controls of the nuclear reactor. [laughs] It had, I really don't understand how it worked, because it was a water, it was a water-cooled, a water... Anyway, you looked down in a great, to a great tank of water, without apparently danger of being irradiated badly, and there it was in the middle of the camp generating all the electricity. And if you looked at it closely you saw that the whole thing was on sledge-runners, it had been towed out there, across the surface.

[34:58]

*What was in the pool of water when you looked into it?*

Well it, it was, I guess it was a moderator for the neutrons. I don't, I don't know how a, a water reactor actually works. Never heard one described. But, it was all rather hair-raising and surprising.

*So the reactor has got skis, and then, what does the main body of it look like?*

I... Tanks.

*Just tanks of water?*
Well I think so. I can’t remember actually very well. I remember the control room quite well, but, what the actual... the actual reactor was extremely anonymous, except for its huge sledge-runners. It was by that time well, well below the surface, tens of feet below the surface, and I think probably roofed over.

*And, what did you hear said about the, the sort of, Cold War aims of this project from the soldiers presumably that were there?*

Yah. Yah. Well I met quite a lot of military people there. The, the reaction of the scientists was that, well they’re spending all these millions of dollars on this military installation; we might as well get two cents’ worth of science out of it. Why not? But it was primarily of course a military operation, and the scientists were riding on the back of it. Just as in Antarctica the South Pole station is run by the US Navy, Air Force, and, they do science there. But it’s there for military reasons and for political prestige reasons.

[37:00]

*Was there any talk from the American people there about Russians, about the Cold War, about why they were there and...?*

I was there, when would I go to...? I did talk to a...

‘61.

‘61. No, that was... No, not about... No, no, Vietnam was later. [pause] No, not really, I think they just felt that they were being, doing their job, and, they had... Whether they... I suppose they were, were they called up or were they, were they volunteers? I can’t actually remember. But they were just being, being soldiers. I flew back from Camp Century in a Caribou aircraft which took off on skis from the, from the ice, and landed on wheels back on the, on the mainland as it were. But, the... Thule was quite a place for, I mean it was a big American base, very important for them at the time. It was interesting being there, and, going to the officers’ mess and...
chatting to the officers. But the war aims or the Cold War somehow didn’t come into the conversation very much.

[38:43]

I had mild reservations about being mixed up with the military at all, but, I think I took the view of most of the scientists working there that there the facilities were, and how else were we going to look inside the Greenland ice sheet?

*How did you view the science that was going on there?*

Well it was very pioneering. There was a fellow who was entrusted with the job of boring this hole to the bottom, which was no mean feat, and he tried various things and they failed. He tried several times, and eventually he managed to do it, but it was not without a great struggle. Nowadays the technology has greatly improved but it was all of course based on the oil industries technology for boring holes and lining them with pipes.

[39:44]

*Thank you. Could you now tell me about your teaching of physics at Bristol, including presumably the teaching of ice physics in the 1950s.*

Well, the brief answer is, I didn’t teach ice physics at all, it wasn’t on the syllabus. I taught the ordinary undergraduate physics courses, and, I probably gave some postgraduate lectures. The undergraduate courses were handed out rather randomly, I mean you were supposed to be able to, it was the days when you were supposed to be able to lecture on anything. Quite embarrassing really. And… But they said, ‘Well it’s your turn to do,’ thermodynamics for example, and… So I go away and get out the textbooks on thermodynamics. Or, ‘It’s your turn to do,’ such-and-such. These duties were handed out in a very, well shall I say authoritarian way, at staff meetings, quite unexpectedly. ‘Oh, oh you’ll do such-and-such, won’t you?’ Mott says. ‘And you’ll do such-and-such.’ ‘Oh you’ll do a good teach… a good course on heat,’ or something like that. And, we even discussed the syllabuses in staff meetings. It was, it was very different from what it is now. We even had Maurice Pryce, who is a, a great polemicist and who was... and who was very very knowledgeable and learned, having controversial arguments with Eric Mendoza, who was hot stuff at
thermodynamics, on whether or not we should teach the so-called third law of thermodynamics to undergraduates. It all went over my head, but that we could have such a public discussion and argument over a syllabus in a staff meeting was ridiculous. Things were arranged so much more casually then. If we had a visitor, Mott had a visitor, halfway through the afternoon he might say, ‘Oh by the way, would you give us a seminar at the end of the afternoon?’ And the poor chap would be roped in and, and had to give an impromptu seminar. That was the kind of thing that happened.

You’ve set up some experimental work assisted by Michael Walford from the sort of...

Yes.

...mid-Sixties. Could you describe that?

I think it was later than the mid-Sixties, wasn’t it? I couldn’t... Oh maybe it was. Was it?

Perhaps mid, the mid-Sixties was when Michael arrived, was it, but...?

Yes, probably, yes. Yes. Well I had, you might say, given up experimental work and I was much keener on pencil and paper work. I hesitate, always hesitated to call myself a theoretical physicist, because I am quite ignorant of much of the subject matter of so-called theoretical physics, but I, I called it pencil and paper work. And Michael Walford, I think, I think the time was when they thought some sort of... What was... I think I was, I think briefly there was a, a joint course of physics and geology, and I was put in charge of that. It didn’t come to anything. But, it was the sort of time when, when a, a member of staff, as he became more senior, seemed to be owed some sort of back-up, in other words, another lecturer in his subject. And so, Michael Walford came as a candidate for this lectureship. He had been teaching in the West Indies, and he came as a, you might say a jack-of-all-trades. He was, he was prepared to put his hand to anything. He was just the person to set up a lab, which I didn’t have much taste for. I was doing work on sliding of glaciers on, on the rock beds, and thought that it might be a good idea to do some sort of laboratory
experiment to test out some of the ideas. And Mike Walford came at that time. And, Elizabeth Morris, that name perhaps means something to you?

Yes.

Joined me as my first, not my first research student but the first I had had on that kind of topic. And she set up a, Mike set up a lab, and I remember Liz Morris coming with an order that I should sign for, for hand tools for the, for the lab, you know, screwdrivers and things like that. She had to go down to Whiteladies Road and pick 'em up at a shop and, all that, all the dogsbody work, and setting up a lab from zero. And Mike Walford was extremely good at improvising anything.

[45:00]

So... I think it was quite successful. We, at that time the idea was, I think perhaps even born in Bristol, Bristol Physics Department, of having student projects, experimental projects I their third year. It’s now quite commonplace I think. But, at that time it was a pioneering effort, and each year we had to think up research topics for as many as we could for students. And then the students picked their projects, and, or their supervisors or whatever, and, I had a succession of students working on things that I had dreamed up, sort of not, not elaborate experiments, just investigating something for the first time, some, making the first steps in understanding something. I’ll give you an example of something which did not come off, and that is, I was sent a letter from a wing commander in Somerset, I think he moved to Wales, in which he looked at his garden bird bath, and noticed after a cold night that the ice formed over it had given rise to a triangular spike sticking out at an angle from the top of the ice. And he sent me a photograph of this thing. So, Mike Walford and I put our heads together and thought, what can this be, how can it be formed? And we made it a third year project. And so we had two students struggling for a year with freezing ice and buying a domestic freezer, and, trying to see how this phenomenon came about. I think we probably know, but I’m still not quite sure. And, there were a lot of, a number of students whom I got to do the experiments, and when I started the, my interest in optics, which we’ll come to, the very first thing I did was to get some students to get a sheet of reflecting metal and shine a, with a light bulb, ordinary light bulb, and noticed the reflections onto a screen, and take a lot of photographs of them.
And they were some of the best photographs of caustics that we made. That was the kind of student project that, that I had.

[47:43]

*How did the peak in the bird bath form, do you think?*

How did, what...? Well, my theory would be, it’s only a theory, that, you get so-called dendrites, the first thing you get when a pond freezes is dendrites. They’re crystals of ice which shoot across the surface and form a, typically a rather hexagonal sort of pattern, which is the symmetry of ice. They shoot across and they put up branches, like a snowflake, at sixty degrees. And it’s quite natural that after this process has been going on for some time, a short time, and the pond hasn’t completely frozen, that the space left will be triangular, because you’ve got these things going at sixty degrees to one another. So you’re left with a triangular hole. Now the water in the pond is asked to get colder, and it expands of course, water, when it freezes, and, so water comes out of this triangular space and promptly freezes. Now it’s got a triangular sort of chimney; more water comes up and it freezes again. And so you get a triangular chimney. And a Japanese student who knew I was interested in ice, presumably, sent me a, probably a Christmas card, with a beautiful photograph of one of these things on without any explanation. And I had a, another letter from, I think the same wing commander who had seen it again in his bird bath. None of us have ever written about it.

*Why do you think that you got the, the letter in the first place, why do you think you got it?*

From the wing commander?

*Yes.*

I suppose he had learnt that there was somebody in Bristol who was interested in ice. I, that’s all I can imagine.

[49:42]
Could you tell me about the significance on your work of the introduction of computing?

Yes.

I know...

It was very significant. There were these girls who, I think they were called computers but I may have got the wrong name, who worked in the Queen’s Building in the engineering lab, who did hand calculations with hand calculators, the sort of things that you used to have to turn a handle to do arithmetic with and, were I think by that time electrified. Brunsviga was the name, Brunsvigas. And they, they finished up with columns and columns of numbers. And they were happy to do this all day, or, anyway, they were paid to do it all day [laughs], whether they liked it or not. And, they were prepared to work out mathematical problems numerically, and I knowing of their existence got them to work out a particular problem that arose from my glaciology. And, I think I told you that I learnt the hard way that such calculations need doing with care and a bit of theory behind them. And they can go unstable, it’s well known now, but I didn’t know that. And... But they learnt it by bitter experience. And I remember, Christine Faithful was her name, saying, ‘It’s all going wrong, it doesn’t seem to work.’ Her numbers were flying all over the place. And, and I learnt about numerical instability. And then, we got... Let me see. We used slide rules for our calculations, and logarithm tables.

By the way, working on a glacier is hard because, the theodolites take readings extremely accurately, you read them to degrees, minutes and seconds, and even tenths of seconds. So, you’ve got a, a very long number, and four-figure logarithms are not good enough for that. You have to go to seven-figure logs, which come in a series of volumes which sit on a shelf. You can’t take those to a glacier. And that’s why you can’t work out your theodolite readings sitting on the ice. They all had to wait till we came back.

Now, I was thinking how we did calculations in those days. We used slide rules and we used log tables. There were no, certainly no electronic calculators. And the first
computer that we had was a, an IBM 1620, which came to the engineering
department, and I was friendly with the professor of mathematics, Mike Rogers, who
did numerical work, and he was the, he had spent a year in America learning about
computers, and so he was put in charge of the first Bristol University computer, which
was a cabinet about twice the size of this. And...

So that’s about...

I should think it would be about three feet by six feet, something like that. And, you
programmed it with punched cards. Punched cards were already a, they were I think
invented in the loom trade to make patterns, and the punchcards carried the
information about how the pattern was going to come out. So it was a technique that
was known about, but not used for computing.

But, punchcards were the way you wrote your program. You had this card... Do you
know about this?

No.

You had a card about that size, and a machine which punched holes, a pattern of holes
in it, as you typed on a keyboard. And you finished up then with a few hundred cards
with holes in them, and along the top of the card it wrote out what you had done in
plain English, in letters. So you didn’t have to look at the holes, you just saw what
you had typed. And then, to make sure you hadn’t made any typing mistakes, you
put... or it hadn’t made any mistakes, you put the thing into a card reader, which, as
they say, verified the cards, and, just checked that they were OK. The machine... I
was there when the machine was delivered, and it came with a packet of cards about
that big.

How big’s that would you say, for the recording?

Oh, say, nine inches long and three... four by four or something the other way. Mike
Rogers opens the package and the very first thing he does is to copy it, very wisely, on
one of these machines, so that it’s not just one copy. This was what was called the
compiler. You took the cards that you yourself had punched, and you put them on the end of this stack of cards which was the compiler; you put it into the card reader of the computer, the computer read the cards one by one, and now it had stored your instructions. You then switched on the computer, and lights flashed, and, the output came on a printer, which was a, of typewriter technology, and, of course it came out saying you’ve made a mistake. And you did it again, and again, and again, until you got every semicolon in the right place. The language used was FORTRAN, which is still used but in a much more sophisticated version.

[55:45]
And I put the first program on that computer, and the first program was indeed to measure... we’d been looking at photographs of those ogives on glaciers, to measure what sort of shape they ought to be. In other words, the distribution of velocity across the glacier, and with depth, bearing in mind that it’s slower at the sides than the bottom, and that the valley that it’s in could be, well I said, I did it first for a rectangular valley, typical mathematician [laughs] sort of, glacier, and then I did it for a semicircular one, and finally I did it for a much more realistic parabolic valley, working up to more and more sophisticated, complicated cross-sections. And, eventually... Of course the programs didn’t work, and my method of solving these equations was too naïve to work. They didn’t, as we say, converge on a solution. And it was only later, talking to, probably talking to Mike Rogers, that I hit on a way of actually ‘solving’ these equations. That’s ‘solve’ in quotation marks, because I mean, using the computer to get the numbers out. And I did eventually get out the, the numbers, which are what we like to think of as universal, that is, you’ve done it once and that’s it. And I joyfully sent off the paper to the Journal of Glaciology and sent the detailed results to what are called the World Data Centres, there were for of them in the world, which store this sort of stuff that’s too... Nowadays we’d put it as an addendum to the paper on the Web, but now... then, you sent it to the World Data Centre. I suppose they’re still sitting there. And the numbers are indeed still used. And somebody, some research student perhaps, repeated the calculations on a modern computer, and got the same answer, I’m glad to say.

[58:02]
What were you actually typing in on the keyboard, onto the punchcards?
You would type in things like, \texttt{do, d-o, j equals one two fifty}, and then a lot of instructions that they had to do many many times. And the thing about this computer was that it only stored 32,000 decimal digits, it’s what’s called a decimal machine, not a, not a binary machine. And, that was its limitation, 32,000 numbers. And as a result, my program wouldn’t fit into the machine. So I had to slim it down, and I slimmed it down by omitting all the comments. You know, to find your way through a program, you write comments on what you’re doing, so that you can, when you come back to it, know what it, what it was trying to do, in plain English. And it still wouldn’t fit in the machine. So I had to divide the whole programme into two. The first half was to kind of prepare the machine to do the work, that is tell it the shape of the valley and, all the, a lot of numbers that it needed to do before it could do the real thing. And then, that was one half, and the other half, it did the actual calculations. So I broke it down into two halves, without any comments. And just got it in with a, I think, twenty by twenty grid, which sounds pretty coarse, but it’s good enough for the purpose.

\textit{And, what were the other significant developments in computing through your career... Well actually, let’s, let’s...}

Could I, could I interrupt?

\textit{Sorry.}

Just to finish off about that computing. Of course as I said, it didn’t work. And then, a lot of other people were using the machine of course, and, and my version, that finally worked, was much later.

\textit{Ah.}

So I didn’t just go to the machine and get a result from it like that, but I did put the first unsuccessful program on the machine.

[01:00:14]
When did you get a successful run of that then, do you think? If that was... Well, it arrived in 1960, so, when would you have done the unsuccessful run of that, do you think?

In the early Sixties I think. But I know I did... I know one of the things I did when I went to Yale, in 1964 was it, or, yes, ’64 probably, was to write up that work. So it must have been successful in about ’63, ’64. It came out in ’65. I brought out three papers in ’65, which looked like a lot of work but actually had been sort of, gestating for much longer.

And, were there developments in computing that have influenced your work in glaciology after that?

I’ve been... Yes, there were. I mean, all that work on response of glaciers to climate change, all that really amounted to finally was a computer algorithm, the way you, the way you, given one set of data on the movement of the snout, you converted it into another set of data on the changing climate. And all that was a computer program. So... Let me see. I... There were two stages to that. I think I... Yes, I think I must have tried to do it analytically without a computer first of all. And then I was able to do it, that’s right, by computing and getting much more detailed results. But the first time I did it was definitely sort of, not numerical computing, it was analytical work. So the computer made a great difference to that, I was able to do it thoroughly with a computer, work which could only have been done unthoroughly by hand.

Was that by then the same computer that you were doing that on?

No, by then I think, well, the university had got a, had got a Ferranti, I think it was Ferranti computer, which worked rather differently. And then I went to Yale, where I worked at the Watson Center, which was the IBM computing centre. So they were, computers were getting larger and larger and more and more powerful. They were still dependent on valves, and the, even the computer at Yale was working on valves, but the very first one we got here, I suppose it was working on, it must have been working with solid state devices. And certainly the memory involved a whole lot of rings through which wires passed, and you could, you could address each ring with
two wires saying, I suppose it had zero and one or, I don’t remember quite how it worked, but, it was all very basic.

*Was that a Ferranti, the one with the two wires entering...?*

No, that was the first IBM, did I say, 1620 I think it was called.

*Yes.*

1620.

*Oh, I see.*

Yah.

[01:03:56]

*What was the next development in Bristol then, in terms of computing? The Ferranti, after the IBM.*

I hope I’ve got the, the name right. As far as I recall it was Ferranti, yes. Yes, I did a calculation with a Ferranti on... which was specially suited to it, on the, treating the whole glacier as if it were a perfectly plastic medium from the very beginning right to the end. And this involved, a glacier being a very long, thin thing as it were, this involved a long piece of paper, a graph, and a thin one, and it happened that the Ferranti machinery involved getting the output on a long roll of paper, which suited me perfectly, because the calculation involved going down from the top to the bottom, working out one thing, and then coming back up to the top to work out something else. And this wasn’t entirely successful because the roll of paper, by the time it had got to one end had slipped a bit, and so when it came back to the top, it was just a little bit out. So that was not so good for the calculation. But it, the computer itself and the way the roll was so long, was ideal.

[01:05:25]
And what about the effect of computers on more mundane aspects of being a scientist, how would you characterise the effect of computers on simply communicating and writing papers and...?

Well writing papers and communication didn’t come in for ages. I was kind of taken by surprise, because, computing for me was solving differential equations and the like, integrals, by numerical means. That was what I thought of as computing. And while I wasn’t looking, the kids were getting computers and playing games with them. And, computing as I thought of as an academic sort of activity, turned into something totally different, while I wasn’t looking. [laughs] And, then of course, we got, we got desk machines, like... Yes, we had a, we had a, a desk machine in the Theoretical Physics Department at the university, which was on a trolley like a dinner trolley, and we got it, we were allowed to take it into our own rooms and type away at it and do calculations. And the, was it... It was one of the keys, I think the zero button, zero button always wore out, and the, it had that sort of mechanical defect. So, but that was, that was the way the computer was used in the department, at first.

[01:07:14]

And I suppose, later, hand calculators, which were really revolutionary, came much later than that. I don’t remember the dates exactly but I do remember we gave our departing vice-chancellor a Hewlett-Packard computer on his retirement, and he became the chairman of the university pensions committee, USS I think it was called, and, he wasn’t very satisfied with the way the actuarial work was being done. So being typically, Alec Merrison was his name, he decided to do it for himself, on his new Hewlett-Packard calculator. And then disputed the actuarial predictions on what the university, I suppose what the university population was going to do in the way of dying and getting older and, the way the distribution changes, all these things that they have to bother about with pension schemes. Now he did it on this Hewlett-Packard computer that we gave him, and that must have been one of the first, I suppose it was a hand calculator. So it’s by such things that I date the development of computing. But I kept in touch with Mike Rogers, and, and I, actually, by the time I got onto the university computer committee, computers were quite different. They were, they were big mainframes, and we were sharing one with Bath University, and there were big administrative problems in sharing the computer with Bath. They were the, that was the, that was the talking point at that time I recall.
When was that, when you were having these shared mainframes?

With Bath. When was it? I’m awfully bad at these more recent dates. The older dates I can remember, but, but I, that’s a really hard one to answer. Probably it was in the middle Eighties was it, something like that. I know that the poor lady who was the assistant registrar was given the job of making a survey of how computing could be used in the different university departments. She was, knew nothing whatever about computing, and she was in some distress about this, quite rightly. But nobody knew very much about how to use computers in a business sort of sense. And so, it was all new ground. The engineers were the best at it. They, they knew what was available and how to use it and could talk the technical jargon, which I never learnt.

And as for glaciology, I think it’s true to say, around about 1970 will it be? Around about that time, I became somewhat disillusioned with the way glaciology was going. It was, it was indeed becoming computing. And clearly if you have very elaborate models and more and more computing power, you can do more and more, but it became less and less, something I could feel about. [knocks mic] Sorry about that.

Why is that?

It wasn’t my metier I think. I, I felt there was something artificial about it. Why was it? [pause] Well it was rather dull I think. [pause] I mean, a physicist always feels that it’s much more physicist-like to do something with, analytically, without algebra, than it is to do it with numbers. And I think I carried that bit of prejudice.

Could you explain that difference to people who won’t understand the difference between doing something algebraically and doing it simply with numbers?

Well, if you, if you say \(a+b=c\), and you want to sort of try it out, you say, let \(a\) be 1, let \(b\) be 2, and one plus two is three. If you write it as \(a+b=c\), you can then do algebraic manipulations on it, like, multiplying it by two, or, solving the equation for \(a\). That’s algebra. But if you’re doing numerical work, there are no symbols
involved. You work entirely with numbers. And what a computer does is multiply, is add numbers, subtract numbers and multiply them and divide them. And, a few logical operations, like ‘and’ and ‘not’, which are called logical operators, that’s what a computer does. And, with algebra, you, you start with symbols and you end with symbols. Does that make any sense to you?

Yes. Yes.

[01:13:02]
What people find very hard to understand I think about theoretical physics is, that you can feed in at one end of your work some equation which embodies some fact about the world; you can then go through a series of processes which are often highly technical and for which you need a lifetime’s training, and come out with another answer which is also in symbols, which tells you something about the result of your assumption at the beginning, and which you would never have dreamed of, and which you couldn’t possibly get to by just thinking. And that’s, that’s what physics is, theoretical physics. And people find that very hard to understand, how you can have faith in something which has been proved through a series of abstract algebraic steps.

Yes I, I follow that. So, so, you... So it can be sort of, unexpected and creative through doing that. How does that differ from what a computer does, working as it does, as you say, only with numbers and a couple of logical operations...

Yes.

...'not' and 'and' and that sort of thing.

Yes. Not a couple, more than a couple. But... Well with a computer you can only do it for one instance. You’ve got to feed certain numbers in. With algebra, it works for all numbers, all... the symbols you start with could have any values, and, and the result at the end would be valid. With a computer you would have to take a particular array of numbers and feed it in, and come out with a final array of numbers. And you, you have only done it for the once. You can never prove anything really with a
computer. Although there is a famous instance where people have almost done that, so-called three-colour problem.

_Go on._

[01:15:09]
But, but that’s the general idea.

_Could you say what the three-colour problem is? Or is it..._

It’s, it’s to do with maps. If you imagine the map of England with the counties, with their boundaries, how many colours would you need in your paintbox to paint every county in a colour, so that no two adjoining counties had the same colour. How many colours do you think you would need?

_And is that that’s been..._

I think it’s four, if I... I think it’s the four-colour problem actually. Did I say three?

_Right._

But it’s, I think it’s the four-colour problem. Now to prove that for any map you could dream up, except special things where every county meets in a point, special point or something like that, where they’ve got genuine boundaries between them, I think it’s true to say that for any map of that kind, provided it’s flat, and not on the surface of a globe or something like that, provided it’s flat you can prove that you only need four colours. Now, people tried to prove that for years. It was proved a few years ago, and as my memory serves me, probably wrongly, the proof did depend on, anyway the first proof depended on a computer searching through possibilities, very very fast, and, the only way of verifying this proof really would be to do it again on your own computer, which is very unlike the way mathematicians are accustomed to behave, when they look at a proof, study it very carefully, see if they can find any logical flaws in it, and if they can’t, they say, ‘That’s proved.’ And there’s your prize.
But if you say, ‘Well, actually this step involves using my computer, and, to verify it
you will have to do that with your computer too,’ that’s the difference.

[01:17:18]

Why is it not possible to get a computer to work algebraically and use symbols and,
so that you can...?

You can now. I mean this, what I’m doing now, since I’ve not been able to get
around, is to learn a mathematical program called Mathematica, which is a, an
astonishing intellectual feat, but essentially it does algebra on a computer, which is
possible but not the normal way that computers work. Normally they crunch
numbers, as you know, but for them to, to do algebra is a, is a considerable feat, and it
does, this program Mathematica does much more than that. So... You see when you,
when you do algebra, you spend a long time correcting your mistakes, and making
quite sure that, verifying your results, and the mistakes you’re liable to make are very
often quite trivial, like a minus instead of a plus, so your result at the end is, has the
wrong sign, or, dropping a factor of two somewhere along the line. And trying to find
out where it was that you dropped the factor of two. You know the result is wrong,
but where it went wrong takes sometimes takes hours, days to find out where you
made the mistake. A lot of, a lot of theoretical physics is of that very tedious kind.
Now if you can do all the algebra on a computer, you can be jolly sure with modern
computers that it doesn’t get it wrong. You maybe put in silly things, and you might
get out a silly answer, but, that’s your fault. But the computer asks to do something,
will do it consistently, you can put it in again and again, and out comes, there is the
same result every time. There was a time you know when computers depended on
valves, when the obstacle was, or was certainly thought to be, this goes back to World
War II, was thought to be the problem that the valves would blow during the
computation. And you would have to do it again and again and again. And they
would never last for long enough for you to get the answer out. And, I think the
Germans the other side were, were supposed to be deceived by us who emphasised
this problem with computing, it can’t possibly, can’t possibly break their codes with
computers, because the valves won’t last long enough. Until the actual trick was
found, and that was, never switch the valves off and on. Keep them glowing all the
time. And if you do that, they last for far longer than if you are continually switching
them on and off, which is reasonable because the filaments are expanding and contracting all the time. Of course they’re going to blow. But, it was a, it was a wartime secret, that, I believe.

[01:20:22]

And so, when you decided in the early Seventies then that you no longer found what was going on in glaciology interesting, what were people then doing with computers, which operated only in this numerical way, that you weren’t interested by or enthusiastic for?

Sorry, I got a bit waylaid there, because you said, I didn’t think that they were doing it, that it was interesting. I thought it was interesting, but I didn’t want to do it myself. Sorry, what was the rest of your, your question?

Yes, that, in that case that’s the answer. Yes. So, and why you didn’t want to do it yourself is what you’ve just described, that the computer’s working in this way, and you didn’t find that as interesting as proving things algebraically.

I think it, I think that’s true. Although I did realise that there were many problems in glaciology which were really best tackled numerically, and, people were developing glacier models on computers, and it became quite fashionable. It is indeed still very fashionable. By that time there were other people working on theoretical glaciology, particularly Hutter in Germany, who published a very weighty, fat tome on theoretical... I think the word ‘theoretical glaciology’ was probably coined about that time.

[01:21:50]

And... You know, we all have our prejudices and our, and our likes and dislikes. I just found even that just too, too, shall I say, analytical for my taste. Which must lie somewhere in between this rather lowbrow numericism of computers and the very highbrow abstractions of the higher mathematics. It’s somewhere between.

What about it was then too analytical?
Well, you see, when Hutter did his first paper on water going through tunnels, he made a great deal of the problem of describing, what was the word, not tortuosity, but the, there’s a sort of, mathematical word for describing a tube which is continually winding round and changing its shape. And, he made tremendous play with this, with this notion, which was, in its own right I suppose quite interesting, but, was not at all down to earth, it wasn’t going to make any difference to the, I thought, I thought, to the final results one got about glacier floods, which was what it was all about. My thoughts were very much on the objective at the end, and no, no interest in messing around with details which were not going to affect the result. I mean, I think one of the arts of physics is to get your finger on what matters first, concentrate on the, on the essentials and somehow combine them in a model. In a way I think that there are national, national fashions, national characteristics in the way people do this. In Britain, in, say, solid state physics, it’s considered quite OK to do, to work on a particular aspect of a solid, let’s say dislocations, and try and understand it properly, and then to work on another aspect like the electrical conductivity, and to understand it, and, a sort of patchwork. And, and you hope that by the time you’ve understood all these aspects, you’ve got some understanding of the whole thing. But it’s not a global picture in the first place. Whereas the French would go for the global thing right away, it has to be universal, and, they like a sort of philosophically complete thing and they, then they try to work it out. Of course, both methods have their advantages, but the British method I think has grown up from Victorian science. It’s what I would call a patchwork method, as opposed to the universalists like the French.

[25:06]

*Thank you. Would you be able to tell me about your work on the location of water in ice at melting point?*

Oh I...

*Which is, which is about sort of... Well let’s think. Yes, the behaviour of water within the crystalline structure of ice.*

Yes.
Which I think links to something that we talked about at lunch, which is, the relation of that to what Charles Frank was doing in terms of the way that the, the mantle behaved.

Yes.

And you said that you wanted to say something about his contribution to the plate, tectonics material. There’s quite a lot in that question. So, if you could...

Yes.

...tell the story of beginning to work in that area, on water in ice, then we will hopefully get to Charles Frank and, so on.

Yes. Now I’m trying to think whether Charles’s paper came before or after our work on ice. That’s quite important. He wrote a paper in Nature called ‘Fluid Flow in the Mantle’. And the general idea was that, the mantle of the Earth is partly molten, magma as we say, and partly solid rock, and that the molten part is where the grains of the rocky part come together, on a very, quite a small scale. If you’ve got a grain, crystal grain, and, it adjoins another one, you call it a crystal boundary, that’s a surface. You’ve now got a third grain, it makes a grain boundary with each of the other two, and the three together make contact along a line. Does that make sense to you?

Yes.

[01:27:09] And, Charles Frank thought, correctly, that the magma would be found in these places where three grains come together. Now that’s, the important thing about that is, that instead of having magma now in pockets, separated, you’ve got magma in a connected series of, of channels. And he was saying that when heat, when, magma will flow towards the surface of the Earth, and as it does so, in order to preserve volume, the rocks must go down. And that heat flow in the Earth is not just a matter of conductivity of heat through the rocks, which everybody had assumed it was; it’s
due also to the movement of the magma, and the magma, although small in volume, is moving faster than the rocks, in fact the rocks are going down and the magma’s coming up. So, when you talk about heat flow, geothermal flux as they call it, you’ve got to realise that there’s an important component arising from the molten component in the rocks. And he wrote a paper about that in *Nature*.

[01:28:33]

Now whether it was before or after that, I don’t know, but I was, it was certainly back in, in 1957 I think it was, the Chamonix, famous Chamonix conference, there was a paper by a man called Steinemann who said, whereas the water in a, in a piece of ice at the melting point, if there’s water inside it, where is it? And the general, general conclusion was that it wasn’t... that it was at the places where four grains meet together, which are points, not lines. Where three grains meet together, it’s a line; where four grains come together, it’s a point. So there’ll be little pockets of water at the places where four grains came together. And that was the general sort of view. But Steinemann’s paper was written in a somewhat, or shall I say obscure way, that it wasn’t quite clear whether that was the conclusion or not.

[01:29:33]

Now, when I visited Caltech... No, when I visited UCLA, which must have been in 1960...

4.

...4, Ron Shreve was doing experiments there, looking at the phenomenon of regelation. You know the phenomenon of regelation? Where you pull a wire through ice, an ice block, and the ice kind of heals up behind it, and leaves it unbroken. He was doing experiments on that. And, he put his pieces of ice under water. Now normally if you have a piece of ice, it’s got a sort of rather rough, wavy surface, and it’s hard to look into it, and see anything distinctly, because everything’s all, all wrong because of the wavy surface. But if you put it under water, the refractive indices of ice and water are almost the same. So, you can see what’s happening in the ice perfectly. And you can visibly see, I could visibly see with Ron Schreve pieces of ice, that there was water running along in lines, not in, not in pockets. And, these lines must be the places where three grains intersect. So, so I said, ah, well, the water is in what I call the veins, veins between the crystals. Now why would that be? Well, I
took this idea to Charles Frank, and, he is very, he was, very good at, what shall I, how shall I call them? Arguments which don’t, don’t depend on doing elaborate calculations. You see if you present that problem to a conventional physicist, he will say, ‘Oh well you must look for the lowest energy state.’ Now what are the energies? Well the energies are the energies of the surfaces involved. Just as surface tension involves an energy, so you must work out what are the energies involved in the different configurations, and say which has the lowest energy, and that’s where you will find the water. Well that’s quite a hard task geometrically, because you’ve got to test this or that arrangement and work out the surface area and how much it’s going to change when you change the dimensions and all that. Charles Frank’s attitude was entirely different. He said, let’s suppose as a hypothesis that the water is... Say, let’s suppose it’s in a vein. Right, let’s suppose that the vein is wider at one point than it is at another. Well that’s going to change the curvature of the, of the tube, of the vein. That will have an effect on the melting point, because, that’s what curvature does. So if you’ve got a change in the melting point, the temperature’s different here from there. If the temperature’s different, heat will flow. So heat will flow from one place to another. Which way will it go? Well he worked out which way it went. It’s either going in the direction where it’s going to refreeze the vein to being uniform, or it’s going to go in a direction which is going to open it up more. So he worked that out and said, ‘Oh well, that’s why these veins are straight and they don’t have bumps on them,’ or... So that explains why they’re straight. Then you extend these arguments, what’s going to happen if you have a four... You see if you think about the geometry, there are four veins meeting in a pocket, which is roughly tetrahedral in shape. It’s not a regular tetrahedron with flat sides; it’s a tetrahedron with curved sides. And it’s not even a tetrahedron because it doesn’t have points to it. It has channels running off. So, you’ve got this funny shape, and, is that better for the water than being simply in the veins, or simply in a blob at the four-grain junction? And the conclusion was, just by these thoughts, that, it was actually in the veins and at these tetrahedron places. And Charles and I worked this out together, and, I, I wrote a paper on it for Nature.

[01:34:20]

And since I had had so many discussions with Charles Frank on it, you know, arguing about whether this configuration was unstable with respect to another one and so on, I spent a lot of time with him, and I said when I’d written a paper, ‘Don’t you think you
ought to be a joint author with this? You’ve done so much.’ And he rubbed his beard and said, ‘Well I suppose perhaps I should.’ [laughs] And, it’s a typical sort of, Frank reluctance. So, so the paper is under joint authorship of, I think it’s probably Nye and Frank rather than Frank and Nye, for that reason.

[01:34:58]

And... Now, how this related to his work on the mantle, as I said, chronologically it’s rather important, but I, I can’t actually remember but we can easily look that up. Anyway, the fact of the matter is, that the location of the water in ice was certainly established by both theory and by looking through a microscope. No question about it. Then, later, this became, this translated to the, to the mantle of the Earth, became the whole theory of how the magma in the Earth came up and came in to pipes and made volcanoes, and the whole, the whole story of movement of fluid through the rocks. Which became then pretty central to plate tectonics and subduction zones, and the whole story of how the plates move.

[01:36:05]

Now, parallel with this work, I think, I’m pretty sure actually after this work, the people in Cambridge also realised that this was a, a problem. It’s a problem actually which is common to all metallurgy in alloys, the same thing arises. But anyway, they realised in Cambridge that this was a problem that needed to be solved. And they didn’t do it our way, and Charles Frank’s way, they did it I think the hard way of calculating the energies involved in different configurations, and getting a computer program from the car industry, who are interested, which is interested in panels of various shapes. And for some reason the car industry had a computer program which could tell them what they needed to know about the energies. And this was how they worked out that this was the way the magma had to be in rocks, and applied it to rock, to, to magma flow within the mantle. And, and this I think was the work that, the person you were speaking to in Cambridge about plate tectonics...

Dan McKenzie?

Sam... Was it Dan McKenzie? Dan McKenzie. If I’ve got the correct person. I think I have. He wrote up of course an account of this, and a kind of history of the rock mechanics, with quite... And I don’t think he knew about the work on ice, which I
think preceded it. So that’s, I think the history of the, of the whole thing is slightly wrong from that point of view.

_In missing out Charles Frank’s and your work on..._

Missing out the work on ice.

_Mm._

Yes. Yes.

_Thank you._

And incidentally, I have a paper in the _Journal of Glaciology_ working out the exact shape of these funny tetrahedra, which I worked out on the computer. You were asking...

_On Mathematica?_ 

On the, on the... at Bristol University.

_Oh on the Bristol one, yes._

Yes, one of the things I did on, on it.

_Mm._

Which was quite fun actually, I enjoyed doing it.

[01:38:21]

_What did that involve then, working that out on the computer, the shape of the junctions?_
Well it involved, involved something which is called, relaxation techniques. You...
It’s equivalent, although it doesn’t seem to be, it’s equivalent to the problem, the following problem. If you have a, a box, or, let’s say you have three planes, one here, one here and one here, making a corner. And you decide to put a drop of water into the corner, and you say, I know what the angle of contact of the water with the, with the wood of the box is, shall we say, twenty degrees. And I also know, it’s not a right-angled box, the sides of it are actually at 120 degrees to one another. That’s a perfectly definite problem. What shape is the little drop of water going to be? What shape is it, is it going to be? It’s obviously going to stretch little arms towards the corners; it might even run along the corners. It does. Those are the veins. And where the veins meet at the corner is one face of this tetrahedron I’ve talked about. You put four of these together, and there’s your model of the water in ice. So it’s like the water in the corner of a box.

And how did the computer allow you to...

Work it out?

Yes.

Well, you, as with all computing, you break it down. It is a continuous surface, you break it down into little squares, little blocks. You put a grid on it. And so you can’t work it out exactly unless the grid is extremely fine, infinitely fine. But you do work it out on a coarse grid first and then make it finer and see whether it’s getting more and more the same. So you, you do it grid-wise, and you... I can’t remember exactly, but I think, first of all you assume a curve that it’s going to make along the junction between the water and the wood of the box, in that analogy. You just say, well, suppose it’s a circle or something simple, and you give it to the computer, and you say, now work out whether it’s in equilibrium, is surface tension going to be satisfied? The laws of surface tension are rather simple to say but rather difficult to apply. And are they going to be satisfied or not? And so, adjust the edge to be more in harmony with what surface tension wants. And now ask, now work out whether it’s in equilibrium, and go on doing that until it doesn’t go any, make any change. That is the, that is the final surface.
And, did the computer actually draw that in any way, did it visualise it in some...?

I drew it afterwards. And I remember there was a programme called Gino, which drew as it were in three dimensions, and, I, I don’t know how I’d do it nowadays, but, I remember being on the phone to the chap in Bath who happened to be in charge of the computer, and saying, ‘I’m putting in this command and it doesn’t work at all as it should do. I’m saying “Gino”, and nothing happens.’ Well he said, ‘Oh that’s ridiculous.’ I said, ‘Well I’ve written a program consisting of one line, and it doesn’t work.’ ‘One line?’ he said. ‘Well, how are you spelling it?’ So I, I said, ‘Well do you have to... I think it’s lower case.’ ‘Ah well you put it in upper case and it’ll work.’ That was the sort of thing you had. It’s just like with, you know, the Net or something now, it’s very fussy about upper case and lower case and that sort of thing. So, it was worked out on the computer and, it... I drew... Yes, I drew the pictures with a graph plotter. Do you know what a graph plotter looks like?

No.

[01:42:35]
Well it’s a lovely thing. It’s mechanical, and it has four pens in it and an arm, and you write your program, and the arm rushes round, picks up the red pen, and puts a dot on the paper where it’s been told to. Then the next command is, move to so-and-so. And there’s a red line. Now make the next section blue. It lets go of it, grabs a blue pen and puts it down and makes another one. And it’s continually drawing these, very often tiny little lines you see to make a curve. And you finish up with a beautiful drawing.

So you can, you can slowly watch it emerge, like a sort of dot-to-dot, in making the...

Well, you are supposed not to see the dots at the end of it. It’s all supposed to be, they’re so close together that it looks like a curve.

And this ended up drawing the 3D shape of the space that...?
It ended up drawing a... Yes, it ended up drawing a projection of what it would look like from any point of view. So you could look at it from a certain distance and see this, see this shape, and it would draw out the lines that gave the shape. And I found, interestingly enough, I hadn’t thought of this before, that I could put the viewpoint inside it and see what it looked like from inside, looking out. [laughs] And that was quite fun.

[01:43:55]

And could you actually get ice submerged as you say, and look into it with a, a particular kind of microscope and see...

Yes.

...see these water spaces and see the shape of them actually...

Absolutely, absolutely. And I had a research student, a very good and patient research student, who, Heidy Mader, who’s now a lecturer in the Earth Sciences Department, that was her PhD project. Because the grains of ice actually have different contact angles, the contact angle with the water on the wooden box would have been, say, twenty degrees and constant, but with ice, it depends on the orientation of the ice crystal that the water is against. So it’s not quite so simple as the, as the naïve approach would indicate. And, she spent hours, patient hours looking through a microscope at these ice specimens and turning them round, and measuring the angles actually. We found a way of measuring the angles.

[01:45:07]

I’ll tell you another... Is this going on too long?

No.

Well, you see Mike Walford was a very good experimentalist and he had set up this glaciology lab on the top floor of the Physics Department, and we bought a walk-in fridge for Heidy Mader to do her experiments in. And she bought a great thick parker and sat inside, in a lonely way, for hours, looking through her microscope. And the temperature had to be adjusted exactly right, and measured. Well this is done with
thermocouples, which are electrical devices which measure the temperature accurately. And she found that the thermocouples weren’t giving reasonable answers, they were giving results which are all over the place. There was indeed electrical interference from somewhere. So, we tried to trace this electrical interference with somebody switching on a motor in the other labs, or what, and couldn’t trace it at all. And Mike Walford one day said, ‘Well let’s listen to it.’ So he put on some headphones and plugged them in, and, he heard human voices. He heard communications traffic from the BRI, from the local hospital. We were on the top of the hill and these were doctors paging each other in the corridors, and sending out signals which were spoiling all our experiments. So, so Mike said, ‘Oh well we’ve got to stop that, and obviously we can’t stop the paging system in the BRI for a trivial experiment like this, so we’ll line the whole fridge with silver foil and make it a Faraday enclosure,’ which means you can’t get any spurious electromagnetic waves getting in.

Heidy went in. She could still get the interference. So there was only one place it could be coming in, and that was the mains cable which was going in and running her apparatus. The signals were getting onto the mains somewhere between us and the BRI, and then getting into her little box, which she was sitting in. So, the answer to that was clearly, you don’t use mains at all; you run it all off batteries inside. So we did, and it worked beautifully. And on that depended her very well-known observations on these angles inside ice.

So, I summed all this up in a paper in Japan, at the Sapporo conference, and they, the Japanese laid on a magnificent banquet, as they do, and one of the centrepieces of this banquet was an iced swan, it was a swan made of ice. And I looked at this thing, and, I could see, because it was so nice and shiny, because it was melting a bit, I could see the grain, the grain boundaries, not the grain boundaries but the veins. And I could see the veins, and I could see roughly what size they were. Well they were visible to me. So they must be beyond a certain size. Now my paper the following morning to the conference was precisely on predicting theoretically how big the veins would be, or were. And, there was a curve in the paper showing how they grow, very very slowly, gradually, and reach the sort of size that’s visible in about three or four days, or a week or something like that. So I said, at the banquet, ‘That swan was made a
week ago.’ [laughs] And, which I think went down quite well. And the following morning I told them the reason why.

[01:49:28]

Thank you. Was Heidy your first female research student in glaciology, or was that Liz Morris?

No, Liz Morris was the first female student.

She became the head of BAS didn’t she?

Not the head of BAS, no.

Or of the glaciology part of it.

No, no she was a prominent glaciologist at BAS, and she moved to the Scott Polar, because, when she was promoted at BAS, she could only be promoted by being in charge of more people; being the Civil Service, that didn’t, that didn’t appeal to her. So she moved to the Scott Polar, and went on doing her research at the Scott Polar, I think kind of seconded, seconded [correct pronunciation] is the word, from BAS.

I see.

I think it was some, some arrangement like that.

So Liz Morris started in about, I suppose in the late Sixties, was that?

Must have done I think, yes.

And Heidy some time after that, in the Seventies?

Some time after that, yes.

[End of Track 10]
Could you tell me next about the visits to Antarctica with the National Science Foundation in the late Sixties?

Yes. Yes, there was a, there was a, an American in charge of the whole glaciological effort of the National Science Foundation called Crary, and, I met him at a conference, and said I was rather keen to go to Antarctica. And he wasn’t too enthused about this I think at first, but I was later contacted by him and invited to go there and act as its sort of general consultant. So, I accepted the invitation with great enthusiasm, because I thought it would be great fun to see this area that I had read so much about. And, it must have cost them, I don’t know how much, but, of course they have all the facilities all there, but the, the cost of the travel and all that must have been enormous. But they flew me over to Washington DC, and then Washington to, Santa, what was it? San Bernardino in California, air base; from San Bernardino to Hawaii. And by the time we got to Hawaii I was, well I was sitting by, side-by-side with the commander of the troops who made up the rest of the plane. And I said, ‘Well they’ll be glad of a sleep when we get to Hawaii.’ He said, ‘Not a bit of it. You’ll find they’ll be out on the town tonight.’ [laughs] So, we landed bleary-eyed in the late afternoon and the troops were certainly out on the town that night. Amazing really, they’d flown all the way from Washington DC, and, I forget how long the hops were, about, eight-hour hops I think they were. And, from Hawaii we flew to Samoa.

A story about Samoa. I was sitting with the commander on one side and on the other side of me was another scientist called Twomey. And we all got out at Samoa to stretch our legs, and were told to keep close by because the plane might take off fairly soon. And Twomey, I’ve forgotten his Christian name now, I think it was John, Twomey made friends with a local sitting at the bar in the local, it was not much more than an airstrip really, and, he turned out to be a local schoolmaster who offered to show him round the island. Well it’s only a small island of course. It reminded me very much of the, my picture of Treasure Island and Gauguin’s sort of, thing. The greens were just like that, viridian. And, off Twomey goes with the schoolmaster, and the pilot is told that there’s a tropical storm approaching, and indeed you can see it.
So he decides to take off quick. So we’re all told, ‘Get in, get in the plane.’ And we all get back in the plane, except for Twomey, whose wallet and passport are lying on the seat beside me, together with his jacket. It’s very hot there, and we had come from Washington DC. And, the plane took off without him. The officer says to me, ‘If he were in my command, I’d have him court-martialed.’ [laughs] Anyway, Twomey didn’t catch up with us for a week, because he had to wait for the next military plane which was going in the right direction and had room for him and so on. So... But he got away with it and had quite a good time apparently in Samoa. Gosh. [mic noises] Excuse me. Sorry about that.

*It’s OK.*

Did you interrupt it?

*No it’s fine.*

[04:31]

So the next stop was Christchurch, New Zealand, where I waited for a plane for a week. They called it Operation Deep Freeze. And, whereas the trip out to Christchurch had been on a, I guess on a jet plane, the trip to the Antarctic from Christchurch was on a propeller plane. And, they only flew at fairly irregular intervals, and so I had to spend a week in Christchurch, not very reluctantly, just...

Excuse me. [mic noises] Just watching for the plane, waiting for the plane. Christchurch, I visited the university there, and found it an extraordinary sort of reversion to the Hove of my childhood. It was that sort of period. It hadn’t come on that, as fast as we had, for better or worse. I think, just before I arrived petrol was not available on a Sunday. It was that kind of place.

[06:00]

Anyway, from Christchurch they fly me to McMurdo, which is the main American base in Antarctica, at the end of the Ross Sea, very close to where Scott and his expeditions landed, and Shackleton. So the whole area was close to what was left of those expeditions in terms of huts and various things they had left behind. So I was able to see Scott’s hut, which was just, almost within, well within walking distance of the McMurdo base. And then one day, we went out in a helicopter to the area where
Shackleton had worked and made his base. And the idea was to stop there and be picked up by the helicopter that evening. But we took sleeping bags in case we were stranded, and indeed we needed them, because, the radio said, the weather’s closed in and the helicopter’s not coming. So there we were, in this little meteorological hut where we could, it was a group of about four of us I think, maybe five, and there was only room for five people standing in the little hut. And we were all huddled together there against the weather. And, clearly we couldn’t, it was quite clear we couldn’t spend the night like that. So, one person said, ‘Well, I volunteer to sleep in Shackleton’s hut,’ which we could see a few hundred yards away, but which was a kind of museum you see, you weren’t supposed to go and, certainly not sleep there. So, I think I said, ‘Well I’ll volunteer to come with you.’ [laughs] And finally we all volunteered to sleep in Shackleton’s hut, which we did. And there we, there we slept surrounded by these, we didn’t dare to sleep on the beds there, there were bedsteads but they were so, the springs were so rusted, we knew they’d break if we lay on them, so we just lay on the floor. But there were sledges and boxes labelled with the men’s names and, supplies, tins of things, ice axes, ropes, you know, all the, all the clutter of an Antarctic expedition. And it was extraordinary really. And I slept there on the floorboards. And the next thing I knew, it was morning, I woke up in the morning with a shout of, ‘Helicopter’s here.’ Now you know they keep these helicopters, they, conscious, they purposely keep their rotors going just to hurry you up, because it lends a sense of urgency to the whole procedure. So, we then rushed to get, rolled up our sleeping bags and, and our kit, and dashed to the helicopter. And I was late for breakfast at McMurdo for about the third time. I think I went without breakfast for several days. [laughs]

[09:15]

And, the mess at McMurdo you see was designed for the, for the naval personnel, not really for the scientists. In fact in many ways the place wasn’t really set up for the scientists, they were kind of guests of the, of the naval people. But the, nevertheless the chief scientist had a meeting of course, periodic meeting with the chief of the, of the Navy people, and said what he wanted in the form of personnel being flown by helicopters to here, there or, or wherever. And the purpose of the military were there to help the scientists. But the mess was set up in a, in a services way. And when the arm of he sergeant came down, nobody else got in there. And we were, not exactly subject to military discipline but it would have been very impolite you know as kind
of, civilian guests, to try to do anything which was not by the book. So, you were in a
way bound by these, these rather silly military rules. And it was quite difficult
actually to, if you were going out on a field trip, to find some food to take with you,
because you couldn’t, they didn’t issue special little packs or sandwiches or anything
like that, you had to go into the kitchen and ask, ‘Please could you make something
for us?’ And they supposed so, and... And, there was a load of chocolate bars which
had been unfortunately dropped by accident from an aeroplane, had lain in the snow
for ages, and a chocolate bar that’s been lying in the snow for ages has an
extraordinary constituency, it’s very very brittle and tough, and not the sort of thing
that’s very good for refreshing you on field work. But, the idea of a, a kind of field
ration just wasn’t there, because the troops didn’t go into anywhere else except their
base, so they didn’t need it.
[11:29]
So the scientists, although they were kind served for transport by the services there,
actually were not, were quite hampered. And the, if you wanted to do something like
go in camp, you had to raid the store where you found that all the best things had
already been taken, and there was nothing left. So, it, it had its disadvantages. But,
the accommodation was superb. I had a lovely hut. I mean we had a hut. And, and
the, the booze was supplied courtesy of American Customs who had confiscated...
When they confiscate bottles at the frontiers, they give them to those poor chaps
starving down in the Antarctic you see. So there was no shortage of hard liquor.
And, altogether it was pretty luxurious.
[12:33]
In the hut, just to give you an illustration, in the hut where I was quartered there was a
chap called, a geologist called Rutford, and his job was to get flown to Enderby Land,
which is quite a long way away, and join a geological party there. But in order to do
so the weather had to be good at, in both the beginning and the end of the journey.
And do you know, he waited a month before he could get off. Every morning he went
out with his kit to the airstrip to see whether the weather was any good. Each
morning he came back. And this went on for a month. I know a month because I, I
wasn’t there all the time but he was still doing it when I went, and he was doing it
when I came back. Eventually, I came back there and, he’s gone, Rutford, he’s gone
off to Enderby Land. In a day or two he was back, because his wife had got ill while
he was away and he felt he ought to get back to the United States to be with her. So,
that was the sort of patience that you need to work in Antarctica. Merely an
illustration.

[13:53]
But I was most fortunate, I was flown, first of all to Byrd Station, which was another
big American station, this time on the ice cap, where they had drilled a hole to the
bottom. And I saw this drilling taking place, it was, it was, as in Greenland,
technology developed in the oil industry. There was a fellow called Klondike in
charge of it. He looked like an old-timer gold digger. And they were measuring this
hole at Byrd Station. It was one of the, I guess it was the second hole in through an
ice sheet. And they dated the cores as they came out.

[14:40]
Went back to McMurdo, and I guess, probably the next thing... Oh I explored in the
region of McMurdo, and one day we went out to see the, do you remember Cherry-
Garrard, do you know, that name means something to you?

Mm.

_The Worst Journey in the World_?

_The longest journey. Yes, The Worst Journey, yes._

That’s right. Well, he went out to see what the Emperor penguins were doing in the
winter. You may remember that they built themselves a stone shelter, and, the
blizzards, protect themselves from the blizzards. Well, the helicopter landed us
sufficiently close so that they could forage around and find that stone shelter, which
we did. Most interesting. It was still standing there. And, under the stones, top
stones, you could still see the remnants of the canvas that had been made into the roof.
And the floor was penguin skins. And it was really quite a thrill to be there and see
that sight, which, of course they were there in winter and the weather was terrible.
We were there in summer and it was beautiful. But we went from there to the
penguin rookery at the end of Ross Island, what’s the name of it, its, its other names
escape me. And, to get to the penguin rookery we had to cross a piece of open ground
with nests of skuas, all in the little crevices in the rocks. And the skuas don’t like that
at all, and they come zooming along, aiming straight at you, and then go zooming up
again. And the only way to defend yourself against them is to take a handful of gravel and just chuck it in the air as they go by, and that, that sends them off. But, it was a hair-raising experience. We had to get across this place to get as far as, get to the penguin rookery at the other end. Penguin rookery was itself a most interesting place for me, although I’m not a naturalist, and I spent a whole day among the penguins, which were coming ashore from ice floes and finding their nest. It was so densely populated that all the nesting places, which were in the tiniest little depressions in the rocks, were all taken. So the newcomers had to trudge for half a mile or so inland, finding places. And they were all in little groups, and there were clearly teenager penguins who were outside these family groups just creating mayhem by going into other family groups and getting chucked out, and, you could watch their habits, really quite interestingly.

[17:38]
There was a, I watched them on a floating ice floe, floating fairly close to shore, and there, they were standing on it and diving in the water, and then, you know how very clumsily they move, climbing on to the ice proper as it were, the mainland, although it was Ross Island, and, then stumbling ashore. But there was a holdup. They were all gathered together on this ice floe because, there was a leopard seal swimming around, and they were certainly not going to dive in when there’s a leopard seal there. So they all stood there watching this leopard seal. And clearly didn’t know whether it had gone or not, until one of them, sort of on purpose, by accident, nudged his neighbour, [laughs] ducked him in. And they all looked to see what would happen to him. And he was all right. So they then started off and came ashore. So, I did have a little bit of a naturalist’s observation of life in the Antarctic at that point.

*Did you have any sort of, close-up relations with...*

Penguins?

*Yes.*

No I didn’t make friends with any of them, they were all in family groups, close together. I think they would have resented it.
So back to McMurdo, I suppose. Yes. And, and then I went to the South Pole, which was an experience in itself. You fly for hours in a Hercules, and you come in to land at the South Pole, not as a, a normal plane would and making a, a certain fairly steep angle of descent to the runway, but you fly for minutes, ten minutes, quarter of an hour, very low altitude, lined up for the landing strip. There was going to be no obstacles, because it’s a featureless wilderness. And they lined it up, and then they land as gently as they can on their skis on the landing strip. And they keep the engines running, so that they won’t freeze.

I stayed at the South Pole for several days, and, there’s a kind of false South Pole that they show you at the station with arrows pointing in all directions, to London and new York and Tokyo, which isn’t the South Pole at all because, of course the South Pole is actually moving. The ice is moving there. And the real South Pole is about half a mile away, where there’s a great flagstaff with the Stars and Stripes flying from it, and surrounded by a little circle of, I don’t know what, I can’t remember. I do remember going the half mile on my own, just for fun, and, just to be there, and walking around the South Pole several times, [laughs] just, just thinking of the hours I was, the time zones that I was going through, and hoping that, well I didn’t hope but I was jolly well sure that it was not going to, the weather wasn’t going to come down so I couldn’t go back to the South Pole Station, because that would have been fatal. But the weather was great.

And, South Pole Station of course is entirely underneath the ice surface, it’s entirely underground. It’s all been snowed under, and, it consists of, shored-up, shored up with wood, or it did, tunnels, with a, a mess and places to sleep, sort of, dormitory places. [mic noises] One of the things I was asked to advise on, I think it was only just to give an excuse really for me being there, was to advise on where the new station should be located. [mic noises] Excuse me. [pause-drinking] The idea was, they were going to build a brand new station, it was going to last for twenty years, and it had to be located so that during the middle of its life, in ten years’ time, it would be over, exactly over the geographical South Pole. And, that was, that was how the
siting was going to be. And then, the four quadrants were designated, I mean there
was a protected quadrant where they were going to do work on the snow and there
was a quadrant where, I think the wind blew in a certain direction and all the fumes
from the stoves and so on were blown in that direction. And there were two other
quadrants which had... Oh there was an emergency hut or station to be in another
quadrant. Because fire is a great hazard in those polar situations. And I’ve forgotten
what the fourth quadrant was. But it was all great fun. And the, one of the things I, I
was shown was the water supply, which was, as you might imagine, a great hole in the
ice. By, when I got there, it was like the, it was like a bottle, it had a neck and it
opened out. Imagine the bottom half of an egg timer. And I was lowered on a, on a
wooden seat, through this hole about this size, into this chamber, huge chamber of ice,
from which they drew their water. And an electric light bulb was dangled down at the
same time so that I could look around. And there was I dangling in space, and this
huge cave which wasn’t full of water, the water only came up about, I don’t know,
three-quarters of the way or something like that. But that was their water supply.
And out I was pulled.
[23:49]
There was a, a most interesting cave there which was, whose sides were covered in
frost crystals, and if I had been more, well equipped and knew more about it, I would
have done something about it, because, it was very visible, that there were different
habits of the snowflakes in different sections of the tunnel. You know the growth of
snowflakes is dependent on humidity and temperature, and clearly the different
sections of this tunnel had experienced different conditions. And it was very visible
to the eye, that the form of the frost, which was lovely snow crystals, was different in,
I think four different sections of this tunnel. I’d love to have studied that. [mic
noises] It was [inaud], there were only a few people there.
[24:47]
And one of the experiments was a very, called the very long wave experiment. I don’t
know if that was what it was called, but that’s what it was. It involved an antenna, a
wire, stretching for twenty miles. And it was centre-fed, so ten miles along it, there
was a feeding place which sent out the signals. These are the signals that travel
through the Earth’s atmosphere, through the, get into the ionosphere, and they follow
the lines of magnetic field of the Earth, and they’re heard on the other side of the
Earth at the foot of the magnetic field line on which they enter as a sort of whistling
noise, they’re called whistlers, and that was because, people heard them on their radios as a whistling noise. And I suppose what it was, was a lightning storm at the other end of the line of magnetic field coming all the way from Antarctica to the, to England. And there they heard these whistlers. And this long antenna was designed to study this effect.

So I went out, actually on Christmas Day, in a snow-tracked vehicle, and visited the people who were at the transmitting station, we went downstairs into the, into the snow, and, there was a, a caged chamber containing the most frightening looking transformers, developing frighteningly high voltages, all caged in. The sort of place where when you open the door you have to discharge all the capacitors with a pole to make sure you’re not electrocuted. And these chaps were, just two of them, were just preparing to settle down there for the winter. I thought it a pretty dangerous sort of place to be in the winter. So... Otherwise, it was furnished very like a motel, it had motel kind of furniture, and, a little bit of America underneath the snow. And on, I think it must have been on the way back, our vehicles broke down, and we had to wait a long time while the, some sort of back-up vehicle came. I can’t remember quite how we were rescued, but we were there, standing and sitting in the snow for a long, long time, before anything happened. And then got back to the Pole Station. Those were the kind of activities that I was engaged in.

By the way, I had a pretty bad toothache when I was in Antarctica, and I chased the dentist around from station to station. There was a dentist, but he was going from station to station, and only sometimes was he at the station that I was at. And he happened to be at the South Pole at the same time as I was, and they had just taken delivery of an X-ray machine at the South Pole, which looked very frightening. It was a big thing, probably suitable for body scans, but they simply wanted to take an X-ray of my tooth. Had this vast aperture, and they had never used it before, so they had no idea what exposure to use. [laughs] And I sat in front of this thing and had my tooth X-rayed. And when I, when I took the little film back to Bristol, to a dentist in Whiteladies Road, she looked, gave one look at it and said, ‘Oh we must take this again. It’s, it’s overexposed, we’ll take another one.’ So, that was my encounter with the dentist.
Got back to McMurdo, and then was helicoptered about sixty miles away to what they call the Taylor... was it the Taylor or the Wright? There are two dry valleys there which were discovered by Scott’s expedition, and which have the very unusual feature in Antarctica of being bare of snow in the summertime. They were just bare valleys. And, and very dry, because there was no water there. And they were bordered by the most magnificent glaciers which were being in... I realised this later, that being in the Antarctic, instead of finishing up with gradually sloping snouts as the glaciers in the Alps do, they finish up in sheer cliffs of ice. It’s a topic that I immediately seized on as worthy of explanation, why are they sloping snouts in the Alps and cliff snouts in the Antarctic? Well, one of the reasons it hasn’t been studied is that nearly all the glaciers in the Antarctic end in the sea, where they carve off into the sea, so it’s not, it’s a different problem. These end on dry land, in the dry valleys, and they’re all the same, they all have cliffs, and the cliffs have boulders of ice falling off them and forming a, a rubble of ice, an apron around the foot of the cliff, and then the only source of ablation is evaporation. So what keeps the whole thing, you might say in equilibrium, a steady state, is the evaporation of ice from these boulders and the continual supply of the boulders from the ice cliff. And Meserve Glacier where we were was gradually moving forward and giving a supply of ice to the whole process. So it was a, it was a new, a different sort of balance of accumulation and ablation, which I’ve never seen described in any of the journals. Probably it’s well understood, but it cries out for explanation as to why these glaciers end in cliffs. And I have tried to think how to work this out properly. And it’s, it’s actually a very difficult problem, if you do it mathematically, because you’ve got a, even with treacle, going like this across a surface, if it doesn’t stick to it, which treacle probably would, as the blob spreads out, what starts as the top surface folds over and becomes the bottom surface. Now mathematically that’s very hard to, to deal with. And then you’ve got three surfaces, you’ve got the surfaces it’s sitting on, you’ve got its lower surface, and its upper surface, all three. And the whole thing has to work out mechanically. It’s a very interesting problem. It’s probably in the journal somewhere but I’ve never seen it properly dealt with. If I was still in glaciology I would, that’s a problem I probably would put on a computer.

Mm.
I think it could probably be mathematised and, and done. But someone should do it.

[32:04]

Did you do any glaciological work while you were there?

Yes, there was a party from Ohio State University, which I joined in that dry valley, that was the purpose of going there. They were, they had a hut there with about, accommodates about four people. I think there were three of us there most of the time. And we, well they were making measurements on this glacier, Meserve Glacier, which they did by walking out of the hut, walking up a ladder up the cliff that I have described, getting on to the top of the cliff, walking... then they found themselves on the top of the glacier, and walking across it and putting their stakes in and measuring the velocity and that sort of thing. And even made a tunnel, a little tunnel, at the foot of the cliff, and they were dating the ice.

[32:56]

When I got there the little tunnel at the bottom was used for storing the food. The food was interesting. It consisted of two things: steak and frozen, lobster I think it was, or was it shrimps? Either lobster or shrimps. So one night it was steak, the next night it was shrimps, and then it was steak again. And each time we had to thaw out these, these frozen packages, which we did on a big stove at the end of the hut. By the time I got there, the cave in which the food was stored had contracted so much under the weight of the ice that in order to get in I had to wriggle through on my tummy, and I couldn’t get in until I had taken off my anorak. And I had to take off my anorak, slide in, pulling myself forward, get in, grab a, a steak or a frozen whatever, turn round, and then somehow get out. And there was a, I think there was a rope attached to a, something, a rock or something, outside, and I think one pulled on the rock, on the rope, and slithered out of this very contracted tunnel. How long it lasted after my departure, I don’t know, but it was obviously gradually, every day, getting smaller and smaller. And I just hoped it wouldn’t get small while you were inside. Well you certainly didn’t want to eat when you were inside. [laughs]

[34:36]

How were they dating the snow?
How were they dating it?

*Mm.*

By, some sort of isotopic method. I was asked to gather the sample for, that they wanted at particular depths, and I did it by getting my ice axe and knocking off a lump and watching where it fell, and then picking it up and putting it in a, in a bag, and then going back to the hut and melting it and putting it in a bottle. What happened to them after that, I’ve no idea, but it was some sort of radioactive isotope method of dating. You know, the isotope, relative abundance of the isotopes of elements is a good way of dating things. It could have been oxygen, could have been oxygen 16 to 18 or something like that.

[35:29]

*Mm.*  OK.  Thank you.

Are we still in the Antarctic?

*If, if there is more, yes.*

Well, there’s not much more. I think I, I was there in Meserve Glacier for, I think it was three weeks, it might have been a month, and, it did become a bit tedious at times, but, I enjoyed it. And the helicopter came in from time to time. I’ll give you an example of how a tiny little bit of science goes an awful long way. There were two geologists doing seismic explosions, making explosions and recording the seismic echoes. And their apparatus wasn’t working because, when they pushed in the plunger to make the explosion, there wasn’t enough juice from their accumulators, batteries, to charge the device that recorded the echo. So they had to, or they were, they had radioed for a helicopter to come in, get a hold, get the batteries, take them back to McMurdo, and recharge them and bring ’em back. That’s two round trips. And I said, ‘Well, have you tried using the batteries in... are you using them in series or parallel?’ ‘Well we don’t know,’ they said. So I looked at them and said, ‘Well that’s one way round. There’s another way of doing it, you can put ’em in parallel,’ or did I say series? I can’t remember. And, they did that, and it worked beautifully.
So no helicopter. I must have saved the price of two round trips. Now that was much less than the, than the cost of my own visit to Antarctica, but it was a, it was an example of the kind of thing that a visiting scientist could do. Typically, you got a PhD student, who had been left with some apparatus by his supervisor, who had probably set it up, not explained it too well to the student, told the student to stay there and take the readings. Gone back to the USA. Of course the stuff sometimes didn’t work, and the student who didn’t know quite what it was all for was at a loss to know quite what to do. And I think I was able to... I talked to everybody I met and said, ‘What are you doing?’ And, I think occasionally I was able to suggest to them things they might do to help. I think that was my role.

And, was this the first time that you had seen people geophysically attempting to measure the depth of ice? I know you had seen, you had seen a sort of...

Oh you mean the...

...drilling and... Yes, the sort of, the sounding.

Well... Seismic explosions.

Mm.

They weren’t... They were done in the dry valley by geologists to examine reflecting strata.

Oh, mm.

Not, not the bottom of a glacier.

[38:33]  
Mm. I see. [pause] Shall we move on now to the international glaciological work? I think the first thing that I’ve got is, a meeting of the International Glaciological Society in Iceland in 1970, and you had been President from ’66 till ’69. But I
wondered whether you would want to more generally talk about your work in international glaciology, and in the kind of organisation of, of it.

Yes. As you, as you well know, the Glaciological Society, it was started as the British Glaciological Society, which was founded by the amateur enthusiast Gerald Seligman, who was a wealthy man. And, it turned into the International Glaciological Society at a date that you can determine, but, what happened obviously was that the Journal of Glaciology, which was edited by Seligman, was a publication vehicle for not just British glaciologists but overseas as well, and the whole society began to have many overseas members. And it didn’t really make sense much to call it the British Glaciological Society. Although I believe Gerald Seligman was pretty much in favour of keeping it all British. I do remember some of the discussions. But anyway, it turned itself into the International Glaciological Society, and the first president, I’m pretty sure at the insistence of Gerald Seligman, was Sir Vivian Fuchs, Bunny Fuchs, who was President... you can, you can find that out. By the way, John Glen will be a source of very clear chronological information about all this stuff. [40:43]

So, they had their headquarters originally I think at Gerald’s house, Gerald Seligman’s house, down in Ashford in Kent, where he edited the journal. They then got a room, premises you might say, in Cambridge, in the Scott Polar Research Institute, which housed them, and they’re still there. So the... And the Secretary was Hilda Richardson, who was an extremely influential figure in the whole development of the society. She was a figure who was much admired by all the visitors who came there, and she was a tremendous organiser. So that she... And she was devoted to the society, so she organised meetings and symposia, and, the finances, and, generally set the society on its feet. There was a, they did have a treasurer as well as, she was the secretary, they had a treasurer who looked after the finances as well. So... But the, the finances were always a bit precarious and, so it was a heavy responsibility for the treasurer and for her. [42:12]

I recall for example when the journal was going very well and we were being asked to publish the, publish conference proceedings, because librarians like to have volumes which are in series, they don’t much like odd issues, and the conference organisers equally find it hard to know how to set about publishing their conference proceedings.
And the society was constantly being asked to publish these in the *Journal of Glaciology* and it was getting a bit difficult. And the idea was to start an entirely new series called *Annals of Glaciology*, which would be essentially devoted to conference proceedings. Now financially that was quite a big step, and, I remember Hilda feeling pretty apprehensive about it all, and we had a meeting where we discussed the whole project, and, well, we strengthened her hand, and said we think it’s OK. And it was, I think John Glen will tell you, that it was an enormous success. He was the, he was on the editorial side of the *Journal* and *Annals*, and knows all about that side of things. But *Annals* has gone from strength to strength. And as we predicted, the librarians, having taken issue number one, are going to buy issue number two as well. So it’s good for conference organisers, and it’s good for glaciology.

[43:52]

*Why do you think that Hilda Richardson was so devoted to the society?*

She was, she was a very good person, personally, she made very good friends. She was a very good hostess, she invited people to her house near Cambridge and looked after them. She had a tremendous international, what’s the word, following, group. She knew, she knew everybody, because of these overseas conferences that were organised. She happened to have a second side to her life, a society called the Soroptimists, which always puzzled most of us, because it seemed so un-Hilda like, but, it was, it was a second side of her life apparently. And, the Soroptimists knew nothing about the glaciology, and the glaciologists knew nothing about the Soroptimists. But at her funeral both sides of her double life were talked about, and, praised.

[45:07]

The Soroptimists was a, I don’t know what it was, it was a bit like perhaps the Rotary Club. It, it involved professional women. It was a women’s thing, and, it involved them joining all over the world, and they had meetings all over the world to celebrate their meeting, and, and how many members they had. It never seemed to me to have any sort of guiding objectives, but they loved belonging to it. They came to Bristol for one of their meetings, and Hilda stayed here and, and I asked her about it. But, they thought of a slogan for the meeting, which was, ‘Venture Further’. Well, fine, but the actual ceremony in the Colstan Hall consisted of them pinning medals on each
other for having recruited so many members, or held so many meetings. And, Hilda had lots of medals, well she was the President and was pinning lots of medals on lots of women in evening dresses. That was my rather, [laughs] remote and prejudiced view of the Soroptimists. But, so far as the Glaciology Society was concerned, she was absolutely ideal as a secretary. She was the heart and soul of the, of the society.

*Did she have another job of some kind, a paid job, or was this...?*

No, she didn’t. No, her husband worked at, is it Philips in Cambridge? Or, or the...

*Pye was it?*

Pye. What became, Pye became Cambridge Instrument Company, or was it vice versa? But he, he worked for that, Cambridge Instrument Company I think it was, he worked for them.

*And could she have been secretary of any society, or did she have an interest in glaciology?*

Well she had, I think she had been a member of the Cambridge Geography Department, so she had been a student in among all this activity, stimulated by Vaughn Lewis and his people, so, I think that was her background really. But she never... Oh she did come with us on one of the Austerdalsbreen expeditions, but we, you know, lots of people came, and... She enjoyed that, and she loved field trips too, glaciological field trips. But for her it was a kind of family, which she looked after. She didn’t have any family of her own.

*And, the Soroptimists’ society, which, was it sort of centred in Cambridge, was it...? No?*

No, I don’t think so. But, she happened to be President one year, or maybe more than one year. But, I think it was a worldwide society, so I don’t think it was centred in Cambridge. You could look that up on the Internet I expect.
Did you know any other members of it?

None at all.

[48:27]

Thank you. Could you talk, in terms of the International Glaciological Society, of a proposal from America to dispose of high-level radioactive waste under the Antarctic ice sheet.

Yes. It originated in, in a city in America, was it Kentucky or...? Anyway, there had been a proposal to bury long-lived radioactive waste in some sort of, caverns I suppose, underneath an American city. I’ve forgotten which it was. But they, I suppose the ‘not in my back yard’ approach applied, and they said, well why not Antarctica? I mean nobody is living there. Well that’s a great place. It could be dumped down at the bottom, and we could just leave it there. Problem solved. So, there was a meeting to see if we thought this was a good idea, since that time I suppose the Glaciological Society was as close as you could get to an expert opinion. Well we knew nothing whatever about what happened to water at the bottom of the Antarctic ice sheet, whether indeed there was any water there, and even if there was any water, was it connected with the sea or, or not? Absolutely nothing was known. But we did know that if you put radioactive material down there, there was quite a, a possibility of eventually finding itself in the ocean. And we said, no, we think it’s far too dangerous.

[50:20]

There was a thing called the Philberth probe. A Philberth probe was a, I think it was a, a device which was heated, it was probably heated by radioactivity, and you, you let it down on a wire and it melted its way gradually down to the bottom of the Antarctic. He was a German, Philberth, he was a, happened to be a Roman Catholic, and the, the, how was it, the, the implications of this, he thought through to the extent that he thought that it was a pretty dangerous sort of technology, and it shouldn’t be generally adopted by nations, because it could have been used for example to take a package of radioactive material down to the bottom. So he didn’t want to lose the, the authorship of this idea; at the same time he didn’t want to make it public. So what he did was,
deposit the papers describing it in the Vatican Library, where I imagine it still is to this day. Interesting way of publishing but not publishing.

How did you know that that's where he had put it?


Did you meet him through the society?

I met him, I think I met him in Chamonix, at that meeting in ’56 or 7. There were two Philberths, you’ll have to get the right one. I think this was W Philberth.

[end of session]

[End of Track 11]
OK, could I start today just by asking a few supplementary questions to do with some things that we discussed last time and the time before. And the first thing is, I was curious as to why the Austerdalsbreen expeditions had an official photographer, Judith Thomas, I wondered why.

Yes. Well there were a lot of people involved, and it seemed quite reasonable, since everybody was taking photographs, to have someone who would make a record that was as it were the official record. Actually a cine film was made, and, something went wrong in the processing, and I think it was a colour film that was processed as black and white, or vice versa. So, the whole film was useless.

Oh.

Which was a great pity.

Mm.

But, Judith Thomas produced a lot of photographs which we, which were exhibited and we went up and looked at them and had a good old exhibition of them.

Do you know where those are held now, if they are, were they organised into an archive, or...?

No I don’t, but I, if I, if I were looking for them, I think I’d go first to the Geography Department in Cambridge.

Do you know anything of Judith Thomas’s background?

[pause] No, I know nothing at all. No.

Mm.
I think she had some connection with Cambridge.

[01:30]
Right, thank you. Last time we spoke a little about Gordon Manley, a meteorologist and historian of climate, and he was involved in the Glaciological Society early on. But we didn’t talk about Hubert Lamb, who was another early member of the Glaciological Society who was also interested in the history of climate I think. And I wondered if you could tell me anything that you know of him at the time and his motivations and work and so on.

No, I’m afraid I’m going to disappoint you. I, I had no contact at all with him.

Ah.

I, I... He is, he is not a name that, that is... You’ve just reminded me of his name, but I had completely forgotten any, anything that I had to do with him.

Ah, OK. So perhaps not a prominent member of the society at the time.

Not central, no.

[02:20]
Yes, thank you. OK. Now, in the period of our project, I wonder whether you could comment on differences between British and American glaciology, given that you had pretty good experience of both through, with American glaciology through the various visits that you spoke about last time. And I wondered whether there was a, there was anything you can say about distinctive styles of British and American glaciology, or different scales or approaches or, that sort of thing.

Well yes, there were differences. They were just the ones you would expect. The North Americans were more high tech. For example, when we camped on Austerdalsbreen, we camped on a moraine in the glacier, and the floor of the tent was just rocks. When I camped on the Athabasca Glacier in British Columbia, the floor of the tent was a piece of plywood, it was very different. They just shipped out a
hexagonal piece of plywood and guide the tent down to the edges of the piece of plywood, which was far more comfortable than sleeping on, with a lilo on rocks. Or, if you wanted to drill holes, as we did, in a glacier to take stakes, here we used a hand brace and bit, like that.

Mm.

I have a picture of Professor Hoinkes doing just that on a glacier in Austria. And, I would contrast that with a corresponding picture I have of an American glaciologist with a power source, and drilling from a, with the drill being driven by a power unit, exactly the sort of difference you would expect between Europe and North America.

*And difference... What about differences in sort of, intellectual aims or content or, priorities? Leaving aside the sort of logistical advantages they had.*

Well I think the, maybe the Americans seized on the, the new approach to glaciology perhaps a little quicker than the Europeans did. I mean they were anxious to grasp the opportunity of dealing with glaciers in a new way, whereas in Europe it had been a traditional subject for 100 years, and, that was the way they did things. It was, again, as you would expect. The Americans were keen to do something new.

*Is it because of, then, is it then because of a lack of tradition in America, so that they... Was it all newer to them, in the sense that you said that, it was partly a European tradition that sort of, was conservative, is it...?*

Yes. Well, it’s not that there was no American tradition. It was not so long obviously as the European one. I mean there was a lot of geographical field work in America, simply because they had very exciting glaciers to look at, in Alaska particularly. So that, they had a lot of background of what there was to study. It was just a matter of how it should be studied. They had a geographical tradition just as we had.

Mm.
It is perhaps interesting that in Europe glaciology was and is studied in geography departments, whereas in America it’s studied in geology departments. Now why that is, I have no idea, but it is a fact.

*Does that influence, does the fact that it’s studied in a geology department rather than a geography department affect the content of glaciology?*

I don’t know really. In fact I seldom visited a geography department in America. I suspect it’s a sort of poor relation to geology, but I’m not sure.

*I think quite a lot of geography departments actually closed in the Sixties because...*

Is that so?

*...they just sort of lost ground I think.*

They’ve got, I know they have a very different attitude towards mapping in the United States, and, perhaps that’s also a component.

*What is that difference?*

A friend of mine said to me, ‘You know John, the Americans are just not map conscious.’ They know about road maps, getting from A to B, but an Ordnance Survey map would be a rather unusual thing to an American. They do have the American, rather, maps put out I think by the geologists in America, which are very detailed of course, but there’s nothing like, didn’t seem to be anything like the Ordnance Survey.

*Mm. Do you remember which friend it was that pointed out...?*

Andrew Lang.

[07:45]
And, a sort of final point about differences between America and Britain. What was the sort of process of the sharing of data between American and British glaciology? I suppose especially when we get on to things like radio echo-sounding where pieces of equipment are generating data, and presumably individual scientists are keen to publish on that data, but on the other hand glaciology progresses through, you know, sharing and that sort of thing. Were there different attitudes in America and Britain to the...

None at all that I could detect. [pause] I think it was probably after the first IGY, International Geophysical Year, that the so-called World Data Centres were set up, one in Moscow, one in... this was for glaciology, one in Moscow, one in Cambridge, England, and one in, was it Boulder, Colorado? I think. And, so, that data was completely international.

Mm.

The only case I can think of as being distinctive is, when I was working later on on sea ice, the question arose as to how thick the ice is in the Arctic, and the only way of discovering that of course is by boring through it and seeing how thick it is. Well I said the only way, but the other way is to use submarine data, the nuclear submarines were going on courses underneath the ice. And, you could also of course survey the top surface of the ice, and deduce the thickness. However, the data was secret, and, the only data that we had that was in the public domain was from the, one of the British nuclear submarines, I think it was the Dreadnought. And that was open source data. But there was one thing that they did not disclose, and that was, the time of the data. So that, presumably one can deduce how fast it could go or something like that. But that was, that data was available when I was working at AIDJEX. Whereas, there must have been masses of comparable data from the American submarines which was simply not published, well not made available.

Did you attempt to, to sort of ask for it, or, have it opened?

I think they probably did. It wasn’t me personally, but, I think the people who were running AIDJEX would most certainly have tried to get hold of it.
Mm.

They were in with the Washington administrators and they would most certainly have asked, but been refused.

[11:00]
Mm. Thank you. This is, I'm hopping back quite a long way, but I noticed that we hadn’t said very much about it, and, and that is, in 1950 you went, I think this was the first time you went to look at glaciers, you went on a sightseeing trip to the Swiss Alps with Max Perutz and John Glen, and I think possibly you might have said Professor Hollingworth as well, but I, I previously thought it was just John Glen and Max Perutz.

No, the previous year, the Royal Society had given a grant for Max Perutz, I think Max Perutz, Hollingworth, and Orowan, to go. The second year the Royal Society grant was very kindly handed to John Glen and me, only. Perutz happened to be out there for the first few days, and kind of initiated us into walking around on glaciers. That was, he, he wasn’t part of the grant.

Ah, OK. Well I wonder whether you could, in as much detail as you can remember, just tell the story of time spent with Max Perutz discovering glaciers, or walking on glaciers, the Mer de Glace I think was one of them.

[pause] Yes, you’re right, I think we must have met at the Montenvert, which is the end of the railway up to the Mer de Glace. That’s been the traditional place to get on to the Mer de Glace. And we, you go down ladders on to the glacier, with all the tourists, and, and you slide around on the rather slippery ice. I think Max must have been with us then, and took us further up the glacier than the tourists usually go. He... I’m, I’m trying to think if we went on any other glaciers with him. I suspect not actually. Though we might have done. We certainly, after that we visited the Arolla Glacier, and I’m pretty sure Max was not with us at that time. And then we caught up with him again later on when we went up to the Jungfrau firn on the railway and helped him with his pipe experiment. That was his role.
And what was he, what was he keen to show you? I’m taking you further up than the tourists on the Mer de Glace. What sort of things was he keen to...?

Well, when you go on a glacier for the first time, there are all kinds of extraordinary things you never see as it were on dry land. That’s what’s fascinating about glaciers. There are for example ice tables which, if you get a big boulder on a glacier it protects the ice underneath from melting. And eventually the big ice boulder is left on a pedestal of ice. They’re called glacier tables, and some of them can be enormous. And I mean, you could have a rock up, halfway up the height of this room, sitting on an ice table. I have a picture of John Glen standing under such a boulder. Maybe he showed it to you when you visited.

Yes, I think he’s got an enlargement actually in...

Yes I, I, I sent it to him quite recently.

Ah. Yes, yes he’s got that framed in his house, yes.

Has he? Yes.

Could you give me a sense of the sort of, personal interaction between the three of you? Given that we’ve got three, three individuals on a glacier. I mean can you give me a sense of the sort of informal relations between you at that...?

Yes. I think there’s only one particular remark that I remember, and that was that, John Glen was as interested as I was in all these new things, the little cryoconite, the little stones on glaciers that melt themselves down into the glacier which is the opposite of the glacier table. If you have a stone about a foot wide, then it doesn’t sit on a table, nor does it melt itself down into the ice. That’s the size that became very important to us on Austerdalsbreen, because it enabled us to come back to the same place knowing that it wouldn’t have slipped.
That’s the kind of thing we were looking at. And, I remember John as, habitually being very curious, quite rightly, and asking all sorts of questions till finally Max said, ‘John, you ask too many questions.’ I think perhaps that gives a little bit of the flavour.

[16:03]

And, I was interested in why, why the Royal Society... Well, I suppose why Max Perutz was supported sort of officially in this work. I think I know why he himself was interested in it. The reason I’m asking is, during the war, it’s fairly obvious why certain people were working on certain things, for example with the ships cracking. And I was wondering why this work on ice was seen by people, like the Royal Society were funding it, or, or the, or Cambridge, why this was, why this was seen as important work to do, if you see what I mean, why, why these trips happened, why Max was directed into this, or at least allowed to pursue this, if you see...

I think allowed to pursue it is, is the right phrase. There was no, no question, no sense that he was being directed into it. After all it was after the war, and, the Government wouldn’t have any such power. Max of course had been involved in this Project Habakkuk, the iceberg aircraft carrier. So he, he knew that technical material about ice, and, he was also really interested as a mountaineer. So it was, if, if an application were made to the Royal Society for them to initiate Orowan into real ice rather than sitting at a desk thinking about it, and they needed somebody to go along and help him, and Max wanted to go, and Hollingworth was always anxious to get walking on the glaciers, it was quite natural that they would support the work. Because, you might say, what’s in it for the Royal Society? Well, familiarising a theorist, Orowan, with the real thing.

Which, which was considered a good thing. It’s interesting now you know, there are theorists who have never actually been anywhere near a glacier. It’s not considered
odd in the least. But, it was, it is actually a little bit strange to work on something you’ve never seen.

*Mm.*

But now when we work on ice caps on Mars, [laughs] after all, it’s the same thing isn’t it?

*And do you remember who was suggesting that you ought to go on this trip yourself? Who was saying then that it would be a good idea for you to...?*

Probably, probably Perutz and Orowan. The money was there, and, they rather generously decided they had had enough of tramping around together and why not let the young ones get on with it?

[19:11]

*And were you aware at this time, perhaps on these trips, of, for example, John Glen’s particular motivation for wanting to understand the physical world around him, and the differences perhaps between your and his motivations for scientific work?*

I think our motivations were very much the same. I had done this work on the analogy between glacier flow and the compression of a block between rough plates, and the result of that had been to suggest that there were not only thrust planes in glaciers, as Vaughan Lewis was very keen to promote, but also the corresponding slip surfaces in the higher reaches of glaciers going in the other direction. So, you might say almost as an excuse for, for something being there, we looked specially for these active... We, I know, had a name for them, they weren’t thrust planes, they were the reverse. Slip planes. And we found indeed one I think on our trip, and, anxiously put a metal ruler in it and took a photograph, which is actually in one of the published papers, so excited were we at least of finding something new. But that was in a way an excuse for being there.

*And what was Max Perutz’s reaction to your discovery of this and your...?*
I don’t think... it was, it was nothing, nothing very exciting really.

[21:08]

Thank you. Could you explain the origin of the project that you were involved in between 1973 and 1979 I think, AIDJEX project, Antarctic Ice Dynamics Joint Experiment?

Yes. They usually said AIDJEX [pronounced ayjex].

Thank you.

Yes. I had a colleague, I have a colleague, in America, Norbert Untersteiner, and he started this Arctic Ice Dynamics Joint Experiment, and invited me over to be a consultant. Which I was very glad to do. And, I visited them as you were saying for about a period of six years. And the advantage to me was that I knew nothing initially about sea ice, which is a rather distinct topic from land glaciers, but in the course of that work I did learn something about sea ice. What is it you would particularly like to know about that?

Well, the aims of the project, the reason why...

Right.

...for example they were interested in the movement.

Yes, right. Well, the great leaders in sea ice work were the Russians, they had, because of the sea ice that they had, and I suppose particularly the route across the north of Siberia, they were, they were the authors of the standard works on sea ice. There was American work, but not as advanced probably as the Russian. And the idea was to try to predict the motion of sea ice in the Arctic from first principles. After all we had tried to do that for glaciers, why not for sea ice? If you take a, an isolated ice floe, just a tabular piece of ice floating on the ocean, and you say it’s subject to certain forces, in fact, the wind on the top surface, the resistance of the water on the bottom surface, and what you might be more surprised about, the
Coriolis force, which is due to the Earth’s rotation but which is vital in the force balance. If you just take this into account and say, well now, we know which way the wind is blowing; where’s this ice floe going to go? It doesn’t go actually in the direction that the wind is, is pushing it exactly, because of the Coriolis force. And you make a map of the Arctic Ocean, and you think that thereby you are going to explain the motion of the whole ice cover of the Arctic Ocean, you find that it all sort of streams over the coast of Siberia, which is clearly absurd.

*Mm.*

So there’s something missing. It’s stopped from sailing over Siberia actually by the coast. So how does the coast stop it? By exerting a force backwards on it which brings into play the mechanical properties of this sea ice. And you have to have a model which takes into account the response of the ice to stress, to force. For example, it breaks up as we know into leads, and it also has in it pressure ridges, where the ice is forced up into ridges. These are the things that mostly obstruct the polar explorers who tried to get to the North Pole.

*Mm.*

So, the idea was that the AIDJEX project, which was funded by the National Science Foundation, should form a group of scientists based in Seattle with what they called a modelling group, which was headed by Max Coon. And the modelling group’s object was to produce a model so far as possible based on first principles, which would have as its objective the prediction of the motion of the ice given that they knew about the wind and the ocean currents, and the... I’ve forgotten what it’s called. The, oh the ocean tilt. The ocean is not, as you might think, flat, but it, it’s tilted a little bit and the ice floes downhill. [laughs] Roughly speaking. Or, or is it uphill? I’ve forgotten now. Anyway, taking account of everything that they could think of, can they predict the motion of the ice? And it was a very big project. And we had all kinds of meetings as to what were the most important aspects to look at, and... And my contribution was, I suppose my experience with the land glaciers and how that had worked.

[26:44]
I found the whole experience extremely interesting. It brought together oceanographers and meteorologists, primarily. And there were, although the meteorologists and oceanographers were really studying the same thing, that is the motion of a fluid on the surface of the Earth, their approaches were very different as you can imagine, and their language was quite different. For example, if a meteorologist talks about a northerly wind, and a sailor talks about a northerly current, they mean exactly the opposite. That sort of language difficulty had to be ironed out between them.

_Did they have different intellectual approaches to, to approaching the study of the fluid, the particular fluid that they were interested in, so that it wasn’t just the language that was different, but the...?_

No, it wasn’t just the language. No no, they were both based on physics. And, that was fine. I mean, it was just... You know there’s weather in the oceans as they have discovered, it’s just on a large, much larger timescale than it is in the, in the atmosphere. So, there are great similarities between them.

[28:05]
_Do you remember key debates about approaches to studying this, the movement of this ice or predicting the movement of it? In other words, key sort of, possible disagreement about the, the right way to consider it, or to..._

Well we had very little idea as to how to proceed, and, there were questions about, frequency response and whether... Actually I’ve forgotten the details of that, so perhaps I’d better not go into it. But certainly there were, there were major issues to be sorted out. I’m trying to remember what the, what the key factors might be. [pause] There was... Of course AIDJEX were, the EX at the end of the acronym, the first expedition, they were building up to a climax. It was a, a project of limited time duration, building up to an experiment at the end, and the experiment was to be, to do with observations of the ice movement north of, in the Beaufort Sea, which is north of Alaska, at a, a station on the ice called Big Bear. And Big Bear was the centre of the experiment, and then there were to be three other stations at the corners of a triangle around Big Bear. There was a good deal of experience of people who had camped on
ice floes throughout the winter, throughout the year, and, there was a kind of
comppetition for who had stayed on an ice floe for longest, and these tough North
Americans were vying for this position. So there were plans to make observations of
the motion of the ice at these stations, Big Bear and the other three, based on what
they called data buoys, which were devices used by the Navy and, I think nowadays
we talk about navsats; they talked about satnav. And it was using satellites, which
were just coming in, to decide where a given ground object was. So there were going
to be observations of the motion of the ice, and it was all to be correlated with
measurements on the ice itself in one grand experiment with aircraft flying over the
top and satellites observing the motion from much higher up.

[31:29]
And it was a, I suppose, the most elaborate experiment of the kind that had ever been
contemplated. Its success was somewhat mixed I think. But, certainly they
accumulated an enormous amount of new knowledge. For example, the whole
concept of the, what was called the ice thickness distribution was introduced. That
means, how much first year ice of a thin ice you have, how much slightly thicker ice,
how much thicker still, how much really thick ice, in the distribution of thicknesses?
And this distribution was really important for the mechanical strength of the ice, and it
became an important parameter as they say in the whole model.

Mm.

The whole model was, was a numerical model, put on a computer. And so there were
computing experts in the group who wrote the programs. There were people who,
who dealt with the mechanics of the ice. But it was, it was building up to a huge
computer program which would incorporate the mechanical properties of ice within it,
and thus be first principles as we said.

And how did... What was your view of that, given what you’ve said before about the
use of computing in some cases to produce models?

Oh, quite different. I thought, I really thought this was the only way to do it. If you,
if you really wanted to do it, how else could you, could you approach it? And, I was
all in favour of it.
And when you said mixed success, you’ve suggested some things that were successful about it. What wasn’t?

Well, they built up a group containing a great deal of expertise, and, then at the end of the experiment it was disbanded. NSF, National Science Foundation, decided that they couldn’t support such a thing as a permanent feature. So that they’d built up all this expertise and it, I don’t know what happened to it afterwards really. The, the experiment itself was in full swing when the news came through that a great crack had appeared right through the central camp, Big Bear, and the two halves of the camp landed up I think a quarter of a mile or more away from each other. Of course by that time people in the Arctic knew that what you must not do is to join your huts together with electric cables, because, then, the electric cables just pulled the huts to pieces. So they had cables joined with plugs which separated if a strain was put on them. So the mess hall ended up quarter of a mile away from the sleeping quarters I think, and, but still happily living on this broken piece of ice. And that rather disrupted the experiment.

Were you... Did you see that final stage, when you say there were planes flying over the data buoys on particular ice floes, and this sort of mass of activity, were you then when this was...?

I never visited Big Bear. I did see it from the air.

Yes, could you describe that flight? You mention it in the paper, the, the flight on the remote sensing aircraft.

I made two flights on the remote sensing aircraft. I’ll tell you about them in succession. The first was not to do with AIDJEX, it was to do with an experiment called BESEX, Bering Straits Experiment, which was a, a joint experiment with the Russians. And the idea was to correlate the view you get from a satellite with what’s actually happening at sea level. For example, can you just by examining the satellite
photograph decide how high the waves are? They had, they had satellites, they had the remote sensing aircraft, and they had ships, all observing at the same time, I think from both countries, which was quite a, an effort in coordination. And the night before our flight went over, the meteorologist said, ‘We’ll have to call the thing off because there’s a storm coming up.’ And the Russians said, ‘Yes, you have to call it off.’ And then the American meteorologist said, ‘Well I think we can see a window where we could fly tomorrow.’ And, a big discussion took place over the radio. And finally the Russians agreed, they should go off. We did indeed take off, I was on board, and, it was a very interesting experience. There was a Conveyor 990, which was rather like the size of a 707, and down one side were lots of experiments, pieces of apparatus, and down the other side were the ordinary seats. And I sat next to a chap who had his feet up on a big box, and I said, ‘What’s your role in this operation?’ And he said, ‘I am the computer operator, programmer.’ And, I said, ‘Well what do you do?’ ‘Well I don’t have anything to do unless something goes wrong.’ And so he sat there with his feet on the computer for the flight. Others were operating their experiments at various wavelengths, microwaves, optical wavelengths and so on, looking down at the ice. And we had television screens all along which alternately showed the view looking down and also the data on the flight, and the angle of the, angle of yaw of the aircraft and its altitude and all the things that, that, to do with the flight course. So we, and we were all in communication with one another over the intercom.

[38:36]
We flew first, I remember the chap in front of me was counting the seals on the ice floes, and...

*How literally was he doing that?*

Just as we flew over the ice floes, he saw ice floes with black spots on and said, ‘There are twelve on that one,’ and then there are six on the next one, and...

*By merely looking out of the window or...?*

Looking out the window, yes. [laughs] So far as I know. Maybe he was doing something more sophisticated that I didn’t know.
We, we then went up to high altitude where the jet was more at home of course up at 30,000 feet or so, and stayed there while they did the experiment. And, I know the Russians came over the radio saying ‘Congratulations’ to the meteorologists, ‘you were right.’ The storm did not overtake us. And we then went down to virtually sea level, like 300 feet, and to zoom along in a jet at 300 feet is, quite an experience. I remember we went through the clouds, and, every now again we could see the sea, and a ship would appear. They must have been very surprised. But there’s, standing at the end was the flight manager who told us that we were using up too much fuel at that low altitude, and we must, we must go up, so fasten your seatbelts. Which we did. And the jet zoomed up at full power, and we were pushed down into our seats. And eventually, at I think about 30,000 feet, it pulled out, and I felt the springs of the seat coming up, and the fellow in the, across the aisle from me had a coffee carton, and he just threw it in the air, and it went on a beautiful slow parabola. We weren’t weightless but, we had lost a good deal of weight. [laughs] That was quite an experience. That was the BESEX experiment.

Were there other British scientists involved in that?

I think Bill Campbell, he is an American, was, I think he was my intermediary with that, for that flight. But I don’t think the AIDJEX people were actually involved in that.

And, and how... why were you asked to go, on this one?

Oh, I think really they were wanting to give me a thrill. It was a kind of present I think.

But the other flight of the, of the remote sensing aircraft, was to go out and fly over the station, Big Bear, which meant going out over the Beaufort Sea, and doing a search pattern. A search pattern is going out, turning, making a U-turn coming back, making a U-turn going back, over a sort of grid. And, we all looked out of the windows looking for the station. But I was the first to see the station. And I saw these little black huts down, it was quite a long way away you know, and, just, just
made them out. And we were very excited, and said, ‘Well, the angle’s about forty-five degrees as far as I can tell, and I see we’re flying at 30,000 feet; that means it must be 30,000 feet to one side,’ which is, what, six miles. Is it six? Yes, six miles. ‘And, our flight paths are about six miles across. So I think next time we come round we’ll go right over it.’ And, so they came back, and sure enough, we all saw it. It was just immediately underneath. I’ve forgotten whether we saw it on the television camera or out of the windows.

[42:55]
So, we flew over Big Bear, which was the... I, I really don’t know what the scientific purpose was, but they had all these instruments on the plane, so no doubt they were looking at it with what’s called SLAR, s-l-a-r, sideways looking radar, and, getting images, visual images, visual, visible light images, as well as the radio wave images.

And then we all got back to Anchorage, which is the US base where it started from. And, the aircraft went back to the very place on the airfield from which it had started, in order to check the drift in the inertial navigation system. It was only a few feet out I think.

[43:54]
And it must have been that night that the officers’ mess had its gala night. We had been having, been having sirloin beef one night, and then king crab, crab claws, the next night, and on the gala night we had both together. All the officers were dressed up in their dress uniforms, very very smart, with the ladies in their best evening attire, and we glaciologists slunk round the edges of the room, disgracing the whole proceedings in our field clothes, and feeling very ashamed. [laughs] That was my visit to, up there.

[44:55]

Thank you. The first one you mentioned which was a joint project between the Russians and the Americans...

Yes, BESEX.

When was that? Was that at the same period as the...?
The same period, yes. I’m trying to remember. I couldn’t tell you the date straight off.

*Was it connected to the other project in any way, or...?*

Not really, no.

No.

No, they were really disconnected.

*And, do you know the aims of that project, why the Americans and Russians were together, doing what they were doing?*

Well it was the, it was the Cold War period, and, if you could get some science done jointly, you were doing rather well in improving relations. And Norbert Untersteiner, who was in charge of the AIDJEX project, was very keen on collaborating with the Russians, and indeed planned what he called the Nansen Drift Project, which was to repeat Nansen’s Drift across the Arctic Ocean. And managed somehow to, was it exchange or, or get hold of one of their data buoys. It was all done very, a very clandestine way. But, they were trying to cooperate in, at the scientific level, whatever the political atmosphere might have been.

[46:32]

*Did you have any sense of the difficulties of organising the, the BESEY...*

**BESEX.**

...**BESEX project, given... I mean I know that you wrote about the difficulties of even having Russian delegates speaking at conferences, you wrote a paper in Nature on Russia, or a note in Nature on Russians at conferences, but, did you have any sense of the, the difficulties in establishing the BESEX project from that point of view?**
Really, I know very little about the BESEX project. It was far from central in my activities.

*Mm.*

AIDJEX I was very close to indeed, but BESEX was, was just a, a little thing on the side.

*Yes.*

But I mentioned it because it was one of my flights in the remote sensing aircraft.

[47:20]

*Mm. Thank you. And... So in the BESEX aircraft, did you yourself have a role in the science that was going on, or, in terms of...?*

No. No, I was learning about what people were doing, and as I say, looking out of the window, trying to find the Big Bear station. By the way, having found the station and coming back to Anchorage, I remember now that I had to stand drinks all round to the other American scientists, and that was no mean task, because each one asked for his own special cocktail, which had to be mixed in the very same specified way. Of course I had no idea what they were, and had to have a bit of a help. But I remember that as quite a, a difficult experience.

*Just for being the first one to spot the...?*

Yes, that’s right, yes.

[48:15]

*Mm. And, what were the aims of that, of the, of this project in terms of understanding the way that ice moved north of Alaska, into... Given that there was military involvement in this project, I wonder what the aims of coming to understand that were.*
There were, there was almost no military involvement in this project, that I know of.

*I mention that because of your, the officers...*

Officers’ mess?

Yes.

Oh well they, they operated, I mean the aircraft was operating out of the US Air Force base at Anchorage, for convenience. It was not a military aircraft.

*Mm.*

It was a, very much a civilian aircraft, I think run by the National Science Foundation. It crashed actually shortly after that, at San Francisco, and killed everybody on board.

*Gosh.* I just wondered, because at this time, this... You have a sense in this part of the, in the Northern Hemisphere, of, coming to understand the way that ice moves for example is useful in terms of submarine activity or establishing sort of, listening stations and this sort of thing. I wondered whether there was any, any sense that there was that?

No.

No.

No, I don’t think so. [pause] No, it was far more to do, if it had a, if you are asking for practical implications, it’s far more to do with predicting clear water for ships to navigate. I mean, the, the passage over the north of Siberia was closed by ice, but it was open for some time, and, it would be nice to be able to predict, do weather forecasting as it were, ice forecasting, as to where the clear water was going to be. The Northwest Passage was a case in point.

[50:33]
And why had the Americans been camping out on ice floes at all? I mean...

Why had they been camping out on ice floes?

You mentioned the competition between them.

Yes.

But why were they, why were any of them camping out on them?

Why were they doing that? I think that perhaps did have some sort of military implications, but, I think at that time the Americans were trying to see how they could, what would happen if they, say, had troops on an ice floe, and, how long they could stay there and, that sort of thing. Probably there was something like that in the background, but I know nothing about it. There was a... I know there were military involved because, there was a rather notorious case of a, of a military unit which was camped on an ice floe, and there was a pressure ridge, and pressure ridges are great ridges as the name implies of boulders of ice. And they get bigger and bigger and bigger. And the American officers’ reaction to this as it approached his camp was to get out the bulldozers and push it back. But, he found that the bulldozers were overwhelmed by the, the ice boulders coming over the top of their blades. So that, they were learning things about what it’s like on an ice floe. That’s the only thing I can, I can tell you. But, you see, Camp Century in Greenland was a military experiment to see how you could have troops deployed on, not a temporary basis but semi-permanently, on a big ice cap, and it would not be unreasonable to think, how could you deploy troops on large tabular icebergs? The base at Thule in north-west Greenland was a military base, they moved an Eskimo village twenty miles north, and, that was their big base in north-west Greenland.

[53:03]

When you visited that, what did you see in terms of the, the layout and the logistics in the place and the personnel?

Oh it was just, it was tall radio masts and, wooden huts.
With armed soldiers, or...?

There were no arms in sight. There were at that time of course radar, probably still are, observation stations to detect missiles.

Yes, the DEW sites.

The DEW, the DEW Line, early warning sites.

Yes.

Yes, it’s coming back to me now. Yes. And of course, the people manning those sites had to, had to live, and how should they live? Under the ice or on top in tents or what? Or in huts? All that had to be worked out. And, meanwhile the, the receivers themselves were being snowed on and getting buried, and you had to learn about what to do about it. Did you just jack them up a bit higher each year, or, or what? All those techniques had to be learnt I guess. I was, I was never party to any of that stuff.

Did you see any of the listening, early warning sites or...?

I don’t think I did. I suppose Thule must have had big dishes, but I don’t particularly remember them. They had big masts, I know, because the aircraft came in. But, there must have been dishes all over the place.

And at Camp Century itself, what did you see in terms of the physical layout of...?

At Camp Century? Well of course it was entirely beneath the surface. You, you went down hatches and you climbed down a ladder into this network of tunnels underneath the ice. From on top there’s very little to see. And there was all this accommodation for troops underneath in these tunnels. In these tunnels there were, there were huts, and huts were nice and cosy, and, there was a mess hut and, huts to sleep in and, all that sort of thing. It was a, a military camp underneath the ice.
And again, there, any, any sense of armed occupation or...?

I don’t think I ever saw a weapon in the whole, whole experience, no. No they didn’t have anything, they didn’t expect Russians to come in and invade them.

And, I know there was a nuclear reactor there. Were there sort of dishes and radio masts and that sort of thing at Camp Century?

There was a great... Let me see. Oh no, the drill, the drilling rig I think was entirely under, I think was entirely under the surface, unlike Byrd Station where a lot of it was above the surface. So... [pause] There was very little to see there really, it was all underneath.

[56:30]
What did you see of the drilling? Because I think this was the first very deep core, wasn’t it?

Yes. Yes. Yes, well it was all based on oil drilling technology, which was of course very advanced. And they drew on the personnel doing that. But, drilling through the ice is different from drilling through rock. And, essentially they were melting their way down, and hoping to bring up cores of ice, which of course had become very important in this new interesting climate change. But all this was pioneering work to, working out how to make the hole and how to get the cores out. And there was a guy called, was it Hansen who was, that was his job, to make it work. And he had many unsuccessful attempts, until he got it right, and got to the bottom. A great event.

And did you see the cores coming up and being...?

Yes. Yes.

What did they do with them once they pulled up a...?

They laid them out horizontally, and sliced them into sections and sent them to labs for further analysis, analysis of the ratio of the oxygen isotopes for example, and the
different layers gave information as to the ages. There was a particular layer not too far down which was due to the hydrogen bomb explosion in the Pacific, which had laid down a layer all over the Earth, and this was very recognisable in the cores.

*And were they sent to laboratories back in America?*

Yes, particularly to CRREL, c-r-r-e-l, yes, in Hanover.

*Mm.*

Yes.

*And, you remember seeing them laid out horizontally and chopped up?*

Yes.

*Do you remember anything about the way in which they were labelled, or, I don’t know how you would label a core made of ice, but, the way in which they were sort of, how they kept a track of what was from where?*

No I don’t actually. Chester Langway was the guy who was the expert on analysing them. I don’t think he, he was kind of, so much with the drilling, but he was very much concerned with what you did with the cores when they came out. And, that kind of thing would be in his domain. But, I don’t think it was a particular problem.

[59:23]

*Mm. Thank you. Now I know while you were working on the AIDJEX project, that, you’ve mentioned in the piece of autobiographical writing that you’ve given to the Royal Society, that you worked at that time with Alan Thorndike on mapping floe wave... sorry, mappings of floe fields like the wind, and this was at the same time as the AIDJEX project, is that right?*

Yes.
Could you say a little about, about what that involved? And I think it may be important to define singularities. So... Yes, so what was that work and how did it come about, and what it involved experimentally and so on?

Well the modelling people were producing maps with arrows on them, a field of arrows which indicated the motion of the ice. Or it could, it could also depict the motion of the wind, the motion of the air. And these were fields of arrows which are called vector fields. Now Alan Thorndike is a very bright fellow, and, he knew that mathematicians used what’s called, they define what are called mathematical functions as mappings. I being not a mathematician was not so familiar with this idea, but you, what it means is that you transfer a series of arrows on one map to a separate map which is not a position map but a velocity map. It shows where the ends of the arrows are as it were. And, you map velocities, you map positions to velocities. And, this is the way mathematicians think of these things. And, mathematicians also say that one of the things interesting about these mappings is what they call singularities, that is, places where the mapping kind of goes wrong. For example, if you, if you take a map of England, and you literally fold it on top of itself and it’s, it’s transparent, then, clearly something funny has happened at the place where it’s folded. Harrogate has come on top of Bournemouth. But, this, at this place, where it’s folded, something’s come on top of itself. And this is a, a special place on the map, and it’s called a singularity. And there’s a whole mathematical theory of these mapping singularities. But mathematicians talk about them and, write books about them indeed, but they don’t actually do it.

And we had these fields, real fields of vectors, meaning real observations. And we said, well the thing to do is to map them. So we made these, we did this mapping, and became interested in these so-called singularities in the mappings, and had to learn about what kind of singularities there are.

When you... With the example of Britain folded onto itself, it seems sort of, a slightly artificial thing to do, and you can see why things would go wrong at the fold, but, in these vector maps, based on actual observations of the ice floes, how is it that, how is it that the singularities occur, what goes wrong, given that, you know, we’re not
folding the ice floe in half? So, what makes the singularity in velocity of the floe or the position of the floe, what makes that appear in reality?

Well two places you can imagine in the Arctic might have the same velocity, and in the velocity map they come on top of one another.

OK. Because...

But in the real map of course they are two separate places.

Oh, so it’s when you’ve, you’ve, you’re not... So, you take out the sort of spatial data and it’s just...

Indeed. You concentrate on the velocities. And, there’s a theorem that there can be only two kinds of singularities in such maps. There are the folds that I’ve just described, and all the pleats. If you, you can also have, well, at the end of a pleat, a sort of point, if you understand what I mean.

Mm.

Where two folds come together. That’s, that point is a singularity. And mathematicians prove that there are only these two kinds of singularities and nothing more complicated can occur as they say, generically, that means, unless you do something very special. If you took that piece of paper there and folded it in four, then at the corner something very special would happen, there’ll be four sheets, one on top of the other, instead of just two. But, but that wouldn’t happen if you just crumpled it up. It wouldn’t happen, as we’d say, generically. It wouldn’t happen by accident. You would have to fold the paper very very carefully to make it happen. So that, this is the kind of thing mathematicians liked, what’s going to happen quite generally. And, there were only these two kinds of singularities. And so we said, well let’s actually do it with the AIDJEX data, and see what kind of pictures we get. And we did it, and, Alan did it for the, for the winds all over the world, just to see what would happen. And came up with the most interesting results.

[01:05:49]
And, when I came back to England I got in touch with the Met Office and said, ‘Had you thought of this way of, of looking at the weather?’ And they hadn’t. And thought they might perhaps put a research student on to it, but never actually did. But the question we were interested in is, to what extent can you say that two patterns of velocity are similar? Suppose you have results from the AIDJEX model, and you want to compare them with what actually happened, in the way of velocities, you’ve got a prediction from the model as to how the velocities are going to be, and you’ve got observations of how they are. And you want to say, well how good is our prediction? So you’ve got to compare two velocity maps, two maps with arrows on. And, you want... You could sort of go over it and come up with some sort of figure of merit. But, it could be that the two fields were utterly different, because one of them was folded and the other wasn’t. And, so, you might say, well are the singularities in the right place?

[01:07:11]

*Why would one be folded? That’s what I...*

Well as I pointed out, Harrogate would be over Bournemouth, and, somewhere else would be over itself.

*But I mean, if you’ve got the computer producing a model of the velocities of these ice floes, and then you’ve got an observed map...*

Yes.

...of the, of the floes... I’m perhaps not understanding the idea of the fold, but, why would one of those be folded? Why are they not both merely different kinds of depiction?

Slightly different. Yes. Well, if you regard them as continuous rather than just a series of discreet arrows, but a continuous field, then, as I say, each one will have its singularities in it, which are not obvious, but which you can work out. And if the two fields have different singularities, then you might say that they differ not just quantitatively but qualitatively. Because in order to make them match, you’ve got to
open up the fold. You’ve got to take that pleat, and you’ve got to pull the material out so that the pleat’s not there, in order to make... And that’s a drastic thing to do. But otherwise, you’ll never make them match. They’re fundamentally different, if the singularities are different in the two. That was the general point of view. And this could be done, say, for the weather over England. I don’t know why they don’t do it, but it would be a test of their forecasts. Did they get, did they get the singularities in the right places?

_Mm._

It could be done. That, that was the sort of work that I did with Alan Thorndike. He was the more mathematically-minded, and, perhaps I was slightly more pragmatic about it.

But we thoroughly enjoyed doing this, and we took it to, to a degree which was way beyond anything to do with ice physics. We, at that time, and we’re coming on perhaps to another topic now, catastrophe theory became public. And, singularity theory and catastrophe theory are very closely related. So we did it for, for higher dimensional systems, vectors in three dimensions instead of just two. Imagine a three-dimensional map of winds which, of course that’s what the wind is, it is a three-dimensional thing, and, and mapping that into a three-dimensional velocity space, and what are the singularities then? Well, there are new possibilities of singularities which we investigated and wrote papers about.

Thank you. Before we do go on to the, the catastrophe theory book and the work on optics, can you, and I know that what I’m going to ask about now leads into that as well, through the building of a small model connected to this, but, could you talk about your, the development of your interest in radio-echo sounding, which seems to have occurred in the early Seventies?

Yes. Radio-echo sounding was pioneered by, you might say an American who... [pause] The American aircraft were flying over Greenland with altimeters, to tell them how high above the ice they were. And occasionally it was said they found they
had landed on the top of the Greenland ice sheet. This story may be apocryphal, but that’s what they say. In other words, their radio altimeters had given them completely the wrong result. In fact, they were looking not at the top surface of the ice but the bottom surface where the rock was. It was discovered with a great surprise, a degree of surprise, that radio waves could penetrate ice. And, it took a long time before this idea was exploited, but once the penny dropped, then radio-echo sounding was, evolved as a science. And it was taken up by the Scott Polar Research Institute in Cambridge, Stan Evans was the pioneer there, and he built the apparatus, and it was used for finding out the thickness of ice in Antarctica, which was a very important parameter as they say in working out how much ice there is. And, we didn’t, we didn’t really know whether the ice was above, well above, whether the... sorry, the rock bed of Antarctica, was well above sea level or well below sea level, because there were no holes to drill down. But the radar was clearly the right way to do it. And, I remember strongly urging Gordon Robin, the director of the Scott Polar Research Institute, to get the apparatus into an aeroplane. Now, he didn’t need any urging at all, he wanted to do that, and that was his great contribution, of getting the apparatus into an aeroplane, and then going on many many flights, cross-crossing Antarctica, and they’ve come up, that’s why the best map of the thickness of the ice in the Antarctic is done by the Scott Polar Research Institute, with all these criss-crossing flights. So, radio-echo sounding had really become an important technique.

[01:13:45]

At the same time, I don’t know quite why, I became interested in the wave aspects of this, namely that if you sent a pulse, as they did, down a pulse, if I may indicate it in the air like this, I’m drawing my finger horizontally, and then with a wiggle in it, and then the wiggle stops and it goes on, that’s a pulse. And inside the pulse there are waves. It’s going up and down inside the pulse. And you send a pulse down, a pulse of radio waves, and what comes back is something which is not like the original pulse, it’s much longer, drawn out. It’s still got waves inside it, inside an envelope as we say, but it’s got a long tail on it. And the reason for the long tail is that only some of the waves have come straight back from the bottom; other waves have gone out slightly sideways, and taken longer to come back. So, they come at the end of the echo. So the echo is long drawn out. Moreover, if you do this on a, on a ground, in a sledge, on the ground, and move the sledge along, the pulse changes, the retaining pulse changes very quickly. And there was a paper published, I’ve forgotten quite
where, which I looked at carefully, and saw that the, the wave inside the pulse was behaving in a very sensitive way. So I thought it would be a good idea to investigate this, and got two students in our third year at Bristol Physics Department to rig up an ultrasonic analogue to the radio. It turns out that if you substitute sound waves in air for radio waves in a vacuum, or ice indeed, that, you can scale everything down. The radio waves had a wavelength of metres, the ultrasonic sound waves had a wavelength of millimetres. So, everything’s in scale, so the thickness of the Antarctic ice sheet which was like 3,000 metres, became in the lab three metres, 1,000 times less. And you could, my colleague Michael Walford designed two little ultrasonic, a transmitter and a receiver, put them side by side, and we shone these down onto a piece of crinkled kitchen foil which pretended to be the bottom of Antarctica, and we moved it along and watched how this pulse changed on an oscilloscope. And noticed that, as we expected, as you moved it only a few millimetres sideways, although the pulse was much the same, the wave inside the pulse, in the tail, changed drastically.

*On the returning echo?*

On the returning echo. And we got two students to do this, look at this in more detail as a project, and they produced Polaroid photographs from the oscilloscope, which were later published, and the interest of it was, that this was a way of measuring how fast the ice was moving. Because what you did was to sit at a place on the surface of the ice and then wait for time to move you, and watch your echo coming back, and it would change. And you could then work out how fast the ice was moving, because you then went back to the place where the echo was what it was when you started, and said, that’s the, geographically the same place as I started at. And I have been moved on to a few metres further on. And you just measure that few metres and there’s your, there’s your, your velocity.

*How does the change in the tail of the echo help in calculating the velocity?*

Well you, you start with the tail looking like that, and drawing a sort of wiggle, and then as time goes on it changes, it looks like that. You go back to the place now where you can recover the original return and say, that must be where I started.
Because that’s what was below?

That was what was below. So your datum is the rock bed.

Ah.

The rock bed has stayed where it is, and you have moved relative to that. And that’s, that’s only a short distance you see in, in a big ice sheet, it’s only, you’re measuring, you’re measuring on a scale of a wavelength. It’s, it’s only a metre or so.

So the, the oscilloscope or the radio-echo sounding equipment, let’s say, you’ve got out there, allows you to sort of, fix yourself, not as they used to do to a sort of, a rock that you can see on the land, but...

Precisely.

...to a bit of rock under the...

Precisely. That’s exactly the point.

And that allows you to do the velocity calculation.

[10:19:42]

Indeed, yes. Yes. So, so the students showed that this was possible in the lab, and, Mike Walford went out to the Antarctic, was it the Antarctic or...? [pause] He went to the Fleming Glacier, named after the Archbishop of Norwich now. He became a, became a, he was a cleric and, the glacier was named after him, he was a scientist. And they put the radio-echo sounder on a sledge, and did a survey, made a map of this returning pulse. And then they did the same thing a few, was it weeks or days later, and they had another map. And then they could see how they could get the maps in register, and that was the, that was the velocity, the movement. So, it worked on the Fleming Glacier.

Who did... You said they, Walford and who else was...?
Mike Walford did the radar experiment, and Charles Swithinbank was with him, and did the conventional thing, and, trained a theodolite on there by mountain tops, and worked out how the, how the velocity as he would say, really was. [laughs] Actually there’s an interesting footnote to that, because, when we were going to report this at the conference in Cambridge, thee was a terrible discrepancy between the two observations, a factor of, I think it was fifty per cent, was out. And, I was very worried by this, because I was sure that the radar thing was being done properly. And I quizzed the, must have been Charles, on how he had done his survey, and he told me, and I thought about it, and, I think it was overnight I realised that it was wrong, he had, he had worked it out wrongly. And the factor involved was the square root of two. So, we were due to report these results the following morning, and I remember getting in touch with him immediately after breakfast, or was it before breakfast, and saying, ‘Look Charles, I think there’s a mistake in your, in your calculations. It’s a factor, root of two, which is 1.414. And, he recognised there was a mistake, and indeed was saved from a gaffe in the conference by our being able to say that they agreed.

_Do you remember the particular nature of the mistake he had made in working out the...?_

Well it was to do with how you change the angles that you measure with a theodolite into the actual movement on the ground. I can’t remember, it wasn’t... Well it... No, no frankly I can’t actually remember the nature of it, but it was a, sort of, something, something you could easily do if you weren’t thinking about it quite precisely enough.

_Does this imply that Charles had made this mistake in calculating velocity in all, in all of his previous work up to that point?_

Oh I doubt it.

_Or just this..._

I mean Charles is a very careful worker.
Ah, so this was something that you could just accidentally do on one occasion, rather than...?

Oh I think so, yes. I mean I wouldn’t, I think he’s a very careful observer, and he, he just sort of missed a point. I can’t, I wish I could tell you exactly what it was, but, I know it was the square root of two.

What do you remember of that conversation with him where you, you pointed it out as a way of, you know, saving him from...?

Well it was a perfectly friendly sort of conversation. I think he was surprised, and, and admitted, very properly, that, yeah, he’d got it wrong.

And what was the reaction at...

And everybody makes mistakes.

What was the reaction of the people who had been using radio-echo sounding to measure thickness of this new technique that you had discovered of measuring velocity?

Well, there was a researcher at the Scott Polar called Chris Doake, who took it up with a will, and, he indeed repeated as it were Mike Walford’s kind of experiment, and reckoned that it could show... [pause] You see there were, there was a possibility that the bottom of the glacier had a layer of rocks in it which was moving with the ice. And you can get an echo from that. So if there was a discrepancy between the radar measurements and the normal velocity measures on the surface, you could attribute it to the sliding of this layer of rocks on the bottom, relative to the real fixed bottom. And so he was, he was rather pushing that idea, that you could get a, measure the difference between the two. The thing with Swithinbank we found agreed, but of course it got more refined I suppose later, and Chris Doake pushed it along those lines. So, the answer is, took it up enthusiastically.
Mm.

Yes.

[01:25:56]

And could you describe as, say a little bit more in terms of describing the, the model that you made in, in the Department of Physics in Birmingham of the, in terms of what it looked like?

Do you mean the ultrasonic model?

Yes. Yes.

Well it was a, a Dexion frame.

What’s that?

Dexion, you know Meccano perhaps? Well it’s a rather large version of Meccano, with angle girders with holes in and you can bolt them together in any way. Much used in labs for just, lashing up some framework to hold things. And, it stood about a couple of metres high, and the cooking foil was on the lab floor, and the, the ultrasonic transmission receiver were on some sort of holder which had knobs which you could screw, you could translate it in X and Y, horizontally, east and west, east, west, north, south as it were, by turning knobs. And then beside it was an oscilloscope showing you the wave form. That’s what it consisted of.

And you say that, it scaled down so that the thickness in the laboratory of the ice sheet as it were had to be three metres, did you say?

I think it was actually in fact about a metre and a half.

So was that a metre and a half of cooking foil, or a metre and a half of something else with cooking foil or...?
No, it’s a metre and a half of air between the receiver transmitter and the horizontal cooking foil. The cooking was on the floor of the lab.

Because that is the bedrock of [inaud].

That’s the bedrock, yup. It was crumbled, to be... Because you needed a rough surface to do this.

What decisions did you make about the particular way in which it ought to be crumpled, given what it was representing?

Nothing in particular. We just crumpled it and hoped for the best.

[End of Track 12]
Do you want to test the levels? So am I speaking at about the right level now?

That’s fine, yup.

Good.

Now, just before we continue on radio-echo sounding, could you say something which you just mentioned off the recording about the use of Meccano at Birmingham, by Michael Walford in...

Sorry, use of Meccano at...?

In, in the Birmingham laboratory by Michael Walford?

Not Birmingham. No, Birmingham doesn’t come into it.

Oh sorry, at Bristol. Yes, yes, sorry.

Bristol.

Yes, yes. Sorry.

[laughs] You’ve just been interviewing John Glen.

Yes. [laughs]

No no. There was a, a laboratory essentially set up by Mike Walford in the Bristol Physics Department. And, in experiments of the sort we’ve been discussing about, say, the ultrasonic analogue of radio-echo sounding, we built a frame out of Dexion. And, there was also a use for Meccano in the lab, and I remember Mike going out and buying a Meccano set to lash up some little framework that he needed. It’s very useful stuff in a lab, because it’s flexible and you can build things to your own whim,
without going to the workshop and asking them to cut brass and steel. It’s all angle-girders and nuts and bolts or what you want.

*Mm.* And, do you remember any particular piece of equipment, presumably smaller than the ultrasonic one that he made using Meccano rather than Dexion?

I don’t think I can actually. There must have been several, but I, I can’t, I can’t lay my finger on one of them. Sorry.

[02:00]

*Thank you. And, could you tell me the story of the, I think partly failed attempt to continue the radio-echo sounding velocity measurements at the South Pole?*

Yes. Yes, there was a, a series of observations made at the South Pole by, I guess they must have been astronomical means, and the accuracy of these improved with time. But they went back to the very beginnings of the American occupation of the South Pole. I know that the very first observation was questioned, because, it seemed a bit out of line, but, it had been made by a, a less senior person in the American Army, and therefore was discarded. Interesting reason for discarding a point. [laughs] But, there was this series, so we knew roughly how fast the ice at the South Pole was moving. I’ve frankly forgotten how much it is, but it’s very, very small, it’s, I think it’s of the order of a metre a year. And, we thought it would be jolly nice to take our radio-echo sounder to the South Pole and take some observations so we could measure it maybe within a season quite reasonably. So Michael Walford devised what he called a slot antenna, which was a big box made of, aluminium I think. Imagine a, a big packing case sized box with a, with a big flange on an opening in it, and laid flat on the snow surface so that the aperture faced downwards, and this would act as an antenna which would help to direct the beam where we wanted it, namely straight down. If you just, as was customary, used what’s called a dipole antenna, which is just a piece of metal a few metres long, as a, as an antenna, to transmit and receive, then it, it transmits in all directions of course, and it would be much more economical if you could send the power straight down. And that’s what the idea was. So this big box was transported to the Antarctic. And we had a, a research person with us in the group called Bob Oakberg, who was a giant of a man, and very keen on
the outdoors. He was American, and, was a great sort of field man, bearded. He bicycled all over Bristol, and, having bought a powerful Jaguar car and decided, instead of high performance cars he’d use a bicycle. And, he did that. And anyway, he was keen to go out and do this experiment. He wasn’t really an experimenter, he was a, he was really a theorist, a very good one. But he turned his, would turn his hand to anything, and, he was, Mike Walford devised all the circuitry and so on and, and initiated Bob Oakberg in how to use it, and, out he went. And, unfortunately he, you know, the South Pole is at a considerable altitude, it’s, it’s 3,000 metres high. And you fly out there, and you, you don’t realise that you’re at a great height. And, Bob I suppose, being an active chap, exerted himself as soon as he got there, and the result was, what they call a pulmonary embolism. I got a cable from the American authorities saying he’s got a pulmonary embolism and is going to be evacuated. And I went to, to our local expert, Rogers, who was a, he had been on, I think an Everest expedition, as a physiologist, and knew all about high altitude sickness. And I said, ‘What’s a pulmonary embolism?’ And he told me that, it’s extremely dangerous, and if nothing’s done you just die. But, if you can get the patient down to ground level quickly, then, everything will be fine and he’ll, he’ll wonder what all the fuss was about. And that’s precisely what they were able to do with Bob Oakberg. They took him back to McMurdo, and, where he was fine, and he then had three weeks I think to spend doing nothing. So he went off and joined a, a local expedition which was going to do some mountain climbing, and had a fine time for three weeks.

Meanwhile, I had to write a report to, to the, to NERC to say what had happened to their grant. And, had to say, ‘Well unfortunately we just didn’t do the experiment.’ And, they did, I suppose which was quite an unusual thing, and said that the, that the grant had been sort of, a failure. Which indeed it had been. But there was nothing you could do about it, it was.

[08:16]

_And, the piece of equipment?_

Still sits at the South Pole. Presumably buried by snow, by this time. And maybe some later generation will pick it up with ground-penetrating radar and wonder what it is.
And so Bob Oakley had perhaps started with it, had he, with the...?

Bob Oakberg.

Oakberg, with the piece of equipment, he had started the work had he and then developed the symptoms?

I think the piece of equipment was actually at the South Pole, whether transported along with Bob or not, I don’t know.

[08:53]

Mm. Thank you. Now, this takes us to about 1973 I think, when we have the beginnings of a new research interest in optics, but, I think that there is a connection between the equipment which was in the laboratory in Bristol which was modelling the bedrock with aluminium foil, and your use of this piece of equipment, and your discovery in using it of wave dislocations. And I think there’s a link which I don’t understand yet between your sort of experiments with that and the development of interest in optics. So, I wonder whether you could explain that.

Yes. I was using the equipment in our glaciological equipment in our glaciological lab, and watching the oscilloscope and watching the form of the pulse that came back. And I noticed that as I turned the knob to move the transmitter receiver along, as I explained before, the, the wave, the oscillation, inside the pulse, changes. Now, on occasion, it’s useful to think about it as a wave inside an envelope, which is the bounding curve, up and down, which limits it. On occasion the envelope simply went down to zero. It crossed itself. So that the pulse in time was diminishing to nothing and then reappearing. And, I counted the number of crests and found that when this happened, the number of crests changed by one. That is, before and after I had gone through this place, before I could count the number of crests, and it might have been four, and then in the same time on the oscilloscope, after going through that place it was five. In other words, a new wave had kind of been born at the point where the amplitude was zero. And I thought, well what’s going on here? Why is there one more? And my mind turned to what are called dislocations, and we’ve talked about
that, dislocations in crystals, where essentially a plane of atoms comes to an end. An edge. Now if you, in your mind, substitute wave fronts for crystal planes, you see this is a place where a wave front has come to an edge and stopped, which is an odd thing for a wave front to do. And I remember talking to colleagues in the lab about this, and, saying, ‘I think we’ve got wave dislocations here.’ And, Charles Frank was one of the people I talked to, he was one of the pioneers of dislocations in crystals, so he was exactly the right person to talk to. And we got these, we kind of drew out how it must be in, in space. You see what you were seeing on the oscilloscope was a record in time, and, wave fronts exist in space. So we drew out, or I drew out, what must be going on in space, and we talked about tridents and forks, forks and tridents. A fork is when one goes into two, and a trident is when one goes into three. And this was quite confusing for a time, until I realised that the, what we were talking about as lines were actually both places where the phase is 0 and pi, and not 0 and 2 pi. 0 and 2 pi means a complete revolution; 0 and pi means only half a revolution. And, we no longer talk about forks and tridents, but, for a time that was a, a bit difficult. And, then, then they became very simple objects called, I call them wave dislocations.

And then of course the next thing to do was to try and mathematise them, and write down a formula for them, which I found quite difficult. And I knew that in crystals, if you have two crystals which are slightly misaligned, you have between them what’s called a grain boundary, and if they’re only slightly misaligned, it’s a special sort of grain boundary that consists of a series of dislocations, one above the other. They’re at an angle. And so I thought, what I’ll do is try and make two waves which meet at an angle, and I’ll do it so that the two waves are made so that one is predominant on one side of the plane and the other is predominant on the other side. One is predominant on the left and the other is predominant on the right, so they’ll be like two crystal grains. And then, then there should be a wall of dislocations between them, and I would then zoom in on one of these and see what it is mathematically. Which I did, and, found that the mathematical formulation was extremely simple, it was just a, it was a mixture between polar coordinates and, and ordinary, Cartesian coordinates, which I hadn’t really thought of at the time. And, wrote, indeed wrote a paper about these dislocations in waves. And, the interesting thing about it was that in crystal dislocations you can’t go right to the dislocation because there the atoms are all disordered. It’s called the disordered core of the dislocation. Whereas in the
waves you could go as close as you liked to the bad place, the dislocation, and, it was described perfectly by the mathematics. So it was a, it was, there was a certain perfection about the wave dislocation, which was absent in the crystal dislocation. That was nice.

[15:46]
So I wrote the paper and, showed it to Michael Berry, who was my close colleague in the lab and who already had latched on to some of the aspects of radio-echo sounding and what happens when a wave is returned scattered back from a rough surface. Quite a complicated statistical problem. And, he took this thing away, and, produced a lot of mathematical extensions to it, showed that these dislocations can bounce off one another, and, if you write down the right formula, and they can do interesting things, pass through one another, and, or be born in pairs and all sorts of interesting things. So, we said, well we’d better write a paper together. And so that’s the paper that’s now always quoted as the beginning of singularity theory in waves, which is 1974 I think, Nye and Berry.

[17:08]

*Could you say what the relationship is between singularities and wave dislocations, in other words, what does singularities mean in this context? Are they the same thing?*

Singularities mathematically are nothing more than special places, places where something unusual happens, and in this case, what is special is that the phase of the wave is indeterminate. Now that’s a funny thing to say perhaps, but, a wave has two aspects to it. It has an amplitude, which means how high up and down it goes, and it has a phase, and the phase goes from 0 to 2π at every wavelength. Phase, think of going around a clock. Once round is 2π. And, 2π, the phase goes round 2π as you go from one wave to the next. And at any given point in a wave field, you can say the amplitude is such-and-such, and the phase is such-and-such. Now when the, when you’re at one of these dislocations, the amplitude is zero, on the oscilloscope the envelope went down to zero. The amplitude is zero, and if there’s no amplitude, there’s no phase. So the phase is indeterminate, the amplitude is zero but the phase is indeterminate. A good analogy, and it’s a perfect analogy actually, is the, is the tides in the North Sea. We’ve used this quite a lot. They were first analysed by Whewell of Trinity in the 1850s I think it was, and he noted down the times of high tide down
the east coast of England, and all the way up Holland and Denmark, Scandinavia, and so he came up with a lot of times of high tide. And they went along like the hands of a clock. And the question was, well then how do you reconcile the time of high tide in Norway with the time of the high tide in East Anglia, or Newcastle let’s say? And he drew hypothetical what he called cotidal lines in the North Sea. These are places, a line of cotidal lines is place where it’s high tide all at the same time. So along this line it’s high tide at one o’clock; at this line it’s high tide at two o’clock; and this, three. And the lines actually went round like the hand of a clock. So, what about the middle? At the middle point, what time is it high tide? It’s high tide all the time. Which means no tide at all. The amplitude is zero at this point. And the phase, that is the time, is indeterminate, and that was called an amphidromic point. And an amphidromic point in the tide is the exact analogy of the dislocation in a wave.

[20:41]

Did you apply any of this to waves yourself, or...?

Well it was already applied to waves, it was discovered in waves.

I wonder, did you, did your development of this theory, did you then reapply it to the study of waves?

Oh yes, I’d been doing that for about, well ever since 1974 when the paper was published.

To, literally to sea waves?

Oh no, not to, not to sea waves, no.

No.

No, no, mostly to electromagnetic waves, and to sound waves, what are called... You see there are, electromagnetic waves are fairly complicated, they are vector waves; electric fields and magnetic fields are represented by arrows. They are vector fields, and, vector waves. And that’s slightly more complicated than, say, a sound wave
where the wave is characterised by its amplitude and its phase. And that's it. Those are called, those are called scalar waves, scalar because they’re not vectors. And the electromagnetic wave is called a vector wave. And for a long time I and others dealt with these, not many others actually, dealt with these scalar wave dislocations, and, I was anxious to know what happened to them in light waves, which are electromagnetic waves. Now it’s a well-known approximation in optics that you just as it were pretend that light is a scalar wave, and if you do that, you can explain almost everything that happens in the way of interference of light. But, what you cannot explain obviously is polarisation effects which are represented by vectors. Polarisation is, takes place in a definite direction. Whereas a sound wave is just a sound wave. You can’t have a polarised sound wave, they’re longitudinal waves, or indeed transverse waves, but, there we are. There are differences between scalar waves and vector waves. And, with vector waves, I was very puzzled, because I couldn’t see how you could actually have a, a zero of, of amplitude and how it could be a, a dislocation. And it turned out that there were strictly speaking no dislocations in electromagnetic waves like sound waves and radio waves, but their place is taken by lines of circular polarisation, which I call C lines. And C lines are the basic singularities in vector waves, just as dislocations are the basic singularities in scalar waves.

[24:07]

Are there, are the any natural systems that don’t have these problems or slight mistakes or singularities or...? It seems that, in the early work on crystals and in this work, what you’re interested in is, things where, various scales where things sort of go wrong, things aren’t perfect. And so, two questions follow from that. One is, are there, are you just focusing on systems where there are these sort of mistakes, in other words, are there systems where everything is sort of perfect and there aren’t these singularities? And the other question is, philosophically, how do you, how does this make you feel about nature, the world, existence, the fact that, looking at it as you have, you tend to find these sort of mistakes or sort of, crumples or singularities in...

Yes. Well, take the crystal case. There are such things as perfect crystals, and people try to make them, try to grow crystals, without any crystal dislocations in them. And, that’s an important activity. For example, in semiconductor work single crystals of
silicon or germanium are very important, and there’s a great art in growing single, large single crystals. They don’t occur naturally. If you take any kind of natural or, or ordinary way of growing crystals, they’re full of dislocations, singularities. Likewise, if you take any natural wave phenomenon, take the tides, look at a map of the tides all over the world, and you will find that there are many, many amphidromic points, these are these places that are analogous to dislocations. The Black Sea has one right in the middle; the North Sea has two or three. There’s one in the Irish Sea. And, they’re a natural phenomenon, the Earth, the tides in the Earth being a natural, in the oceans, being a natural phenomenon. So they’re very reasonable to study them. Now you say, what are the philosophical implications? Well, nature is not perfect. Indeed, but these things have a sort of perfection of their own. They have a definite structure. Just as, say, I don’t know, it just suddenly occurs to me, the knots in a spider’s web probably have a natural, have a structure which the spider knows all about. I don’t know what they look like. But, nature is full of, imperfections is the right word, and when you study these imperfections in detail, you’re often surprised to find that they have a certain similarity to one another. For example, in a crystal, sometimes the anatomy is completely missing. It’s called a vacancy. Well one vacancy is just alike another vacancy. And you, you can be interested in vacancies, they are indeed quite important in, in nuclear piles, because they, the radiation kicks out atoms and, and the material behaves accordingly, it changes its size. So, imperfections are not just sort of terrible things, they’re, they’re often of great interest and beauty in their own right. The bottom, the edge of crack in glass, is an imperfection. Glass shouldn’t be cracked. But if you look very closely at the bottom of the crack, then you find interesting things, which are the clue to how strong it’s going to be. Imperfections are everywhere, and interesting.

[28:56]

Can you, can you say something about why you think that you in particular are interested in these imperfections, as opposed to anything else? I mean what in particular draws you to having an interest in finding these discontinuities?

Well clearly with the waves, it was the astonishing revelation that waves were like crystals, which is a link that I and others had never thought of making. I mean that, that was clearly the reason I became interested in wave dislocations, and having
alighted on them, I wanted to find out more and more about them. And as I found out more and more about them, they became more and more interesting, to me anyway, if not to others.

[29:42]

*The... You say that you could, in the case of waves you could zoom in on the particular, on this particular, on one particular singularity, and explain it mathematically. Could you, using particular techniques, see these sort of, visually see these dislocations? Apart from the oscilloscope. But, were you able to sort of, see them in, as vectors, you know, with direction? I’m wondering whether this is the way to move on to polarisation singularities, and the work of a particular piece of equipment which a research student of yours invented, the optically modulated scatterer.*

Yes.

*I wondered if you could say what that was and...*

Yes, there is a link there, yes. That’s a strong link. How did that happen? The man’s name is Jo Hajnal, h-a-j-n-a-l, and, he is now a professor at, I think it’s the Hammersmith Hospital. [clearing throat] Excuse me. Now how did that arise? It was, it was probably a third year project again for students. [pause] Yes, I think I had probably written a first little paper about singularities in electromagnetic waves, and was eager to know more about them. And thought... And, and I knew that there was lab apparatus used to teach students about waves which was available, and used microwaves. A sort of, analogue of optics. You can focus microwaves and, and make them interfere, and you can teach students about waves using apparatus readily available using microwaves. And knowing that, I thought, well it would be quite fun to set up something which would try to make dislocations in microwaves.

[32:06]

So, I set this problem to Jo Hajnal, and he, being a very ingenious person, thought of a good way of making a little detector which could be moved around and could tell you what was the signal, the amplitude and the phase, of the wave at any given point. You see the normal way of picking up a wave is to put essentially a microphone there, like
these that we’re wearing. These are detectors. But, they’re a bit blunt. And what you need is something which will measure the amplitude and phase of the wave, whatever direction it’s coming from, at a particular point. So he, he invented what he, what we called later the optically modulated scatterer, that’s to say, you make a microwave, it comes out from a horn into the lab, and, you say, I want to know what the signal is at this particular point. So at this particular point, you put a little scatterer, anything, a little bead, which scatters the waves, and then you pick up the waves in some, in a horn, another horn somewhere else which is a receiving horn. And you, according to the intensity of the waves you pick up, you know that the scatterer is at a place perhaps where the wave field is very strong, or if you put it at a place where the wave field is very weak, of course you’re not going to get anything in your receiver. If you put it at a place where the wave field is strong, you get a strong signal in the receiver. But the trouble with that is that, your receiver is going to get, receive waves from, from reflections in the room and, straight from the transmitter, and how are you going to pick up this tiny little signal coming from this tiny little bead? So what you do is, you make the bead oscillate in its scattering power, you make it into a strong scatterer and a weak scatterer, on some, in some kind of, in some way. And what Jo Hajnal did was to take a, what was it, a, [pause] an existing piece of electronic equipment with two wires coming out of it, which he bent round to make into a little dipole. And this dipole antenna was the scatterer. And the bead in the middle was something whose resistance could be changed, electrical resistance could be changed, by an optical signal. If you, if you shone light on it, it would, its resistance would change. So he took an optical fibre and joined it up to this little bead. And on the end of the optical fibre was a, a generator of light, low frequency light. The microwaves were in the megahertz region and this was in the kilohertz region, 1,000 times slower. And then, in the receiver you look for a signal which is modulated at a kilohertz rate, and if you see something that’s fluctuating in a kilohertz, you know it must be coming from a little bead, and not from somewhere else, because that’s the only thing that’s fluctuating at kilohertz. And you then join the... Well, that, that’s essentially the explanation.

[36:40]

So, he made this, and, set it up in our lab, and it was a big gantry made, you won’t be surprised to hear, of Dexion, and up on the top was a, the main part of a travelling microscope which was for us simply a platform that could be, with turning a screw,
could be moved around, and then dangling down from it, hanging on a, some kind of support, I think three cottons or something like that, was this little bead. And, shining on to it were various transmitters making a, making a, a more complicated wave field, in fact three transmitters shining on to it, making an interference pattern between three lots of waves. And then by moving the bead through this interference pattern, in three dimensions, and having a receiving horn, he could work out what the field was at every point. This was quite new. Because the standard ways of measuring microwave field involved putting a rather bulky piece of equipment, a receiver microphone like this, at the place concerned. And that didn’t do the trick at all. For one thing, it disturbed the field itself, and, secondly, it pointed in a particular direction, and, there were great disadvantages to that. So, this optically modulated scatterer was really a very ingenious device. It did have forerunners, I mean, Jo Hajnal was not the only person who had thought of this kind of thing, but, there had previously been done, an experiment done with a, a little bead or scatterer, which was made to spin round very fast on a piece of cotton. And, you looked for the spinning speed in the received signal, but that was a bit crude, and to do it optically was a great advance, and with an optic fibre still better.

So that was the optically modulated scatterer. And Jo did his, I think he did his thesis, anyway, he, he became a research assistant on a grant at the lab. And we came, we, we decided we would enter it for the prize competition put up by the National Physical Laboratory for, for, there was a, a prize just being started I think at that time, and we were very gratified to get the award as being the best thing that had been put in. I don’t know what the competition was, but, it was an interesting award because it was a, a sum of money, I think about, oh it was about £2,000 I think, which we divided between us. And, it also consisted, more importantly, of the idea of a contract between the NPL and your university which would support further work on it for so many years. And that was very important to us. So we accepted the award with great pleasure.

*Whereabouts was the National Physical Laboratory?*

Teddington. In Teddington.
Mm. And so, just so I understand, by moving the bead, you could recognise that it was the place where the bead was in the receiver, because that was the only thing that was fluctuating at this lower speed.

Yes.

And so you could... And you’ve got three microwave fields sort of... Interfering...

...interfering with each other as a way, to possibly generate...

Correct.

...singularities. And you could put the, you could, in space, you could put the bead anywhere you wanted in the room...

Correct.

...and, and, and then on the receiver you could get a picture of the, the, the fields all coming together at that particular point where the bead was.

You measured the strength and polarisation importantly, and the phase, or, everything you needed to know, at that particular place in space.

And were you able to find singularities by doing that?

Absolutely. Yes, indeed. I mean, the idea of the three horns was to set up a pattern, which, we knew roughly what it ought to be, and so, it wasn’t just a random thing as it might be in this room, but it was a, it was a rather well defined situation. And so we knew roughly how it ought to be, and, then we measured how it really was. And it wasn’t these, there were no dislocations, but there were these so-called C lines, in two
dimensions, C points. So it varied... Which I had already kind of predicted theoretically, and then they were measured experimentally.

[42:15]

*At this time were you telling your family about what you were doing with the, in the laboratory with the...*

I’m sure I was, yes, I was very excited about it.

*Do you remember their reaction, or their comments on it?*

Well you know what it is when you explain science to anybody who is not a scientist, their very first question is, ‘What use is it?’ And, actually this is quite useful, and so it wasn’t difficult to answer that.

*What did you say when they said, ‘What use is it’?*

I can’t remember now. But... I mean it hasn’t been used, for example, has not been used to test the, the danger of mobile phones. But if you wanted to know in great detail the electromagnetic field around a mobile phone, this would be the way to do it. Actually, when we moved the work to the NPL, I hit on, a rather good idea which was to... How was it? [pause] Not to have a separate receiver to get the signal, but to use the transmitter also as a receiver. And to do that, made it a very compact arrangement whereby the optic fibre, then, signal was generated back at the, at the transmitter. And, there’s a very, a very nice theorem in not only electromagnetism but in much of physics, called the reciprocity theorem, which roughly says that if you have a source here and a receiver here, there’ll be a certain level of signal, and if you, if you then go the other way and make this into a transmitter, and this into a receiver, exactly the same thing will happen. And that is because, if you like, if you want to think of it this way, the rays have all gone either this way or that way. They’ve been reversed. Of course that’s ray theory which is not wave theory, but, the same thing applies for waves, and it’s called the reciprocity, and that’s absolutely essential for the working of this device where the transmitter does double duty as a transmitter and a receiver.
And it’s the essence of why the thing works at all. It’s difficult to explain in words.

[45:17]

Thank you. Now, could you tell me about the book which came out at about this time in English by René Thom?

Thom [pronounced Tom].

Thom. Mm.

Yes, that was before, that was a good deal before this, this episode. That was in the Seventies, when I was at AIDJEX. It was a book in French, and, I never saw the French, later I saw the French version, but, I didn’t know about it. But it was translated into English and that was the key event. It was a very idiosyncratic book. Thom was an extraordinarily clever mathematician who had won the Fields Medal which is like the Nobel Prize in mathematics. He hadn’t won the Fields Medal for this, but he had, he had it. And, his book was called... What was it called? Well it had the word morphology in it. And he was interested in embryology, how the embryo develops, as the mathematicians say, topologically. I mean the alimentary canal goes right through us, and makes us into what a mathematician would call a torus, but you and I would call a motorcar tyre, a ring. We’ve got a hole through us, and topologically that’s what the mathematicians concentrate on. And, he was a topologist, and he, he kind of, he didn’t actually prove Thom’s theorem himself, but he bullied, I am told, the various French mathematicians into putting together a whole series of important mathematical steps to bring them all together and result in something which is now called Thom’s theorem. Quite rightly.

[47:34]

So he was a... And he wrote this idiosyncratic book. I say idiosyncratic, because it’s, there’s not much mathematics in it, and there’s a lot of talk about embryology. It also says that it’s about singularities, and, there are pictures of these occurring in light. They’re rather smudgy photographs of cusps and folds; folds we’ve talked about, and
cusps are the analogue of the pleats. I had done a certain amount of work in optics as you know, with a polarising microscope, and I thought I could do very much than those pictures. So I, when I came back to Bristol I just had a go. A very very simple apparatus, like, a piece of metal sheet, not crinkled this time but just gently folded, and, gently undulating you might say, and holding a, a little bulb some distance from it, and then seeing what the reflection was on a screen. And if you do that, you will see very very beautiful caustic patterns, they’re called caustics. Caustics are places where the light is bright. It comes from, caustic comes from the word for burning, a caustic glass they call it, a magnifying glass that concentrated the Sun’s rays. And, caustics are something that were already well known in optics, but were regarded as unusual and, difficult, maybe difficult to study. And what Thom’s theorem did was really to say, these are objects in their own right, and they occur in a hierarchy an interesting series of, getting more and more complicated, which he named and, and studied in his book. Their names are, the fold, the cusp, and then getting more complicated, the swallowtail, the elliptic umbilic, the hyperbolic umbilic. An I think he went on to the parabolic umbilic, and the butterfly. And he stopped there, because, those last two occur in four dimensions, and he thought that, there are four dimensions of space, time. So that’s where we stop for applications. And, he called these the elementary catastrophes.

[50:56]

Now simultaneously in Russia the mathematician Vladimir Arnold, who had also classified these, these, what were now, what are now called catastrophes, singularities, and, had gone much further beyond the butterfly, and had made, seen that it just went on forever getting more and more complicated. And he had a notation for, for naming these things. I didn’t know anything about Arnold’s work until later, but I did, did latch on to Thom’s work, because, simply because I thought I could take better photographs than the ones in the book. Came back to Bristol, and there, through Michael Berry, became acquainted with work going on at Warwick University. Christopher Zeeman led the group there, and I, it may be that, it may be that Christopher Zeeman coined the word catastrophe theory for this. I, I can look this up, I must... can’t remember the title of Thom’s book at the moment. Structural Stability and Morphology or something like that.

[52:26]
Anyway, at Warwick with Christopher Zeeman worked Ian Stewart, you may, you may know the name, he is, he often comes on the television. And, Ian Stewart produced a, what we used to call a mimeographed little booklet of a few sheets of A4, with pictures of these catastrophes. I think he called them the famous five or something like that. And we became acquainted then with these names that I’ve just mentioned to you, swallowtails and butterflies and so forth.

And I went on with these experiments, and, talked about them to Michael Berry, and we realised a, a funny sort of thing, that they must apply to the patterns you get on the bottom of the swimming pools where the sun shines through the top surface and forms that dancing pattern on the bottom. It’s very hard to make out what’s happening. David Hockney paints pictures of such things. And we, actually we have a, one of the David Hockneys up in the, reproduction up in the lab. And, you will see there bright lines meeting in threes, nodes of threes. And it’s interesting, it was interesting to us, that none of Thom’s catastrophes consisted of three bright lines, caustics, coming together. They might have done but they just don’t. Just doesn’t happen. So we wondered what these things could be. And I said, ‘Well let’s settle it experimentally.’ And I asked our glassblower to make a, a disc of ice, of glass, and to make in it three grooves joining together at the centre. And I said, ‘Don’t take any particular trouble about the accuracy of it, because that’s the whole point. It has to be generic.’ So he did this, and, the idea of course was that it formed a kind of lens, like the top of the swimming pool water, but the, being grooves, instead of being ridges, instead of being a converging lens it was a diverging lens, but it was easier for him to draw grooves in the top of the molten piece of glass than to make ridges. So I said, ‘Well fine, just make grooves.’ And so he gave it to me. And it had a, a glass handle on it. He decided to put a glass handle on it, or maybe I asked him to, I can’t remember. So we called it a lollipop, glass lollipop.

And, I took this piece of equipment, and I used a white light, pinhole source, I can’t remember exactly how it was... Oh I think it was a, a microscope lamp I think with an iris diaphragm in front of it. And, looked through a microscope at the corresponding pattern that you would get with light coming through it. And I remember it was a Saturday morning, and I saw these three bright lines, but they weren’t three bright lines, they were, each of them was double, each of them was double. And, then, there
was no special thing... That was fine, they were folds, you see, folds of bright lines in, in optics.

[56:37]

Then I noticed a tiny little bright point, bright point in the middle, which I assumed was coming from, a reflection from somewhere else in the room, as you might see a little bright point reflected in, almost anything. But then I saw that the bright point when it reached a fold, did something to the fold. It kind of moved it. It was clearly implicated in the whole phenomenon. And excitedly I rang up Michael Berry and said, ‘Look, I’ve seen this little point, and it, it’s going to explain our, our triple junction.’ And indeed the, if you look at the little bright point, focusing the microscope through it, it turned into a triangle. And, that was another caustic, caustic inside a caustic if you like, which we later realised was connected with the elliptic umbilic. But the, the point was, we had kind of solved the problem of the triple junctions, and we wrote a little paper in *Nature* on triple junctions, how triple junctions were quite usual in nature, and we used the patterns on the, on antelopes I think, or was it giraffes, the patterns on the sides of giraffes as an illustration, or mud cracks, where you get lots of triple junctions. But in optics, although you seem to get them, actually they were, they had a fine structure which I had seen in the microscope and we showed pretty pictures and they put one of the pictures on the cover of *Nature*.

[58:19]

*So, seeing that the three lines meeting were in fact double, or folded, and seeing the bright spot, that allowed, that explained why... that allowed you to make, say that this was actually one of the five catastrophe models, and, whereas previously when you had the three lines, you thought, it didn’t seem to fit.*

That’s right.

*But by... Mm.*

Yes.

[58:43]
And, this was Saturday morning you say. Where were you doing, where were you shining a light through the bit of glass?

In the glaciology lab, in the Physics Department.

Was it normal to be in there on a Saturday?

I think it probably was, but I didn’t have much time in those days, I had a lot of teaching to do, and, hours were precious.

So you often went in on a Saturday?

I, I must have done I think, yes. You know, we used to have, when I first came to Bristol we had Saturday morning lectures. Students came in on Saturday mornings. As they did at Cambridge.

Mm.

This idea of Saturday being a holiday is comparatively new, you may know.

[59:28]
And, the first interest in the swimming pool, the, the patterns on the floor of the swimming pool, is there any reason why you, you spotted that? In other words, did you go swimming at this time and, and see this sort of thing? Was there a particular, was there a particular place where you spotted that happening and then decided that, then decided to study it, or, were you just familiar with this sort of pattern much more generally?

I was... I mean, we used to go for holidays in the Mediterranean, and, I used a mask and flippers. And, was constantly fascinated by this pattern on the sea bed, on the sand, normally in shallow water. And, it goes so fast, it changes so fast, you can’t make out what’s going on. Hockney just gives an artist’s impression of... But if you look at it, it isn’t like that. And, it, it’s, it is so hard to, to perceive what’s really going on. I’ve taken, in Corsica, I’ve taken high speed photographs of this pattern reflected
on the white hulls of yachts, and I used 1000th of a second exposure, to freeze it sufficiently to resolve it into these basic caustics. So yes, every time I went on holiday in the Mediterranean with my mask and my flippers, I saw this pattern on the bottom of the sea. And of course having realised about the triple junctions, I became more interested in it.

_So, had you already been on these holidays before Thom’s book came out and...?_

Frankly I can’t remember, but, well certainly we, I had seen this pattern on the bottom of swimming pools, and particularly at, not in the pools but in, at the sea, used to see this pattern. Well everybody sees it, but they don’t think it’s interesting. You see, that’s the change that’s taken place, that, if you said to somebody, ‘What about that pattern on the bottom of the sea?’ They’d say, ‘Oh yeah, but it’s, of course we know what that is, it’s just the sun and it’s refracted through the top surface.’ And if you knew all the details of exactly how the surface of the sea was, you could just compute it. But it wouldn’t be interesting, would it? Now you see you would say, ‘Ah, but it’s very interesting, because the patterns have a certain structure to them, and each pattern is in some ways the same, they’re just distorted versions of one another. There are generic features about these patterns, which always occur. And only a very limited number of them, like five.

_Are there still thought to be five?_

It isn’t a matter of thinking about it, it’s a mathematical theorem. It’s either proved or disproved.

_And has that held... Has that held up, has Thom’s...?_

Absolutely. Absolutely, yes, it always was. It was a perfectly rigorous theorem. Oh yes.

[01:02:54]
And, apart from the sea floor and the Mediterranean on holiday, the side of boats in Corsica, can you think of any other particular places or sites where you were seeing things that fed into your thinking about this?

Well there was one particular episode that’s perhaps worth repeating. I used to walk to the lab and back along Clifton Park, which is a road near here. And one night it was raining, and I wear spectacles as you see, and there were little raindrops on my spectacles. And there were the street lights, sodium lights. And I was used to seeing through these little drops and seeing these caustic patterns through my glasses, and noticing that it was the same for every street light. But on this occasion, I saw that the, well they were always the same pattern, it was a bright fold at the top, which I knew all about, and, and then there were cusps, they were clear enough, and, that was, that was nothing strange about that. But on this occasion, I saw little, little spots, bright spots just underneath the fold, and as they hit the fold, the fold changed and they turned into cusps. And what I was seeing actually was so-called parabolic umbilic. And I realised that, because I had looked at these pictures in diagrams in Thom’s book, and realised that one of the so-called unfoldings of the parabolic umbilic is the impact of a little triangle on a fold, resulting in a, in a cusp. I think Thom has a name for it, the mushroom I think he calls it, he has a name for everything. And, that was, that was to me a revelation, because I realised then that you could actually kind of manufacture, that’s a very bad word for it, but you could manufacture a parabolic umbilic. So in the lab I then put a little drop of water on a glass slide and looked at it through a microscope. I think I’d done that before. But this time I tilted the microscope slide, actually the whole microscope, because you want to look at it, keep on looking at it, and, you had to change, move the light source as well, and, keep on looking at it, and, then you see this caustic evolving, until it became the same shape as I had seen it through my glasses. And I could see the little, little spot inside moving through and producing a cusp. In Clifton Park it had happened dynamically, at every step I saw these things jumping up and jumping up and down. And I saw these three cusps appearing, three cusps, three cusps, three cusps. And every cusp had a little bright point. In the lab I could do it slowly, and I could tilt the microscope until the little point went through the fold and made the cusp. And this was the so-called parabolic umbilic. And I could take photographs of it, and, was delighted to publish a paper in *Philosophical Transactions of the Royal Society* about the parabolic umbilic,
and its companion, which is even higher up in the hierarchy, called the symbolic umbilic, which is related to it.

[01:07:00]

*Is there something about the, the spacing or, or the kind of lights or the time of day that you’re walking that meant that you were likely to see what you saw in that particular place, rather than any other street?*

No. I mean, what you need are, distant street lights, distant points. These happen to be sodium lights, which are monochromatic. There were also white lights. And, inside these folds you see typically a series of interference fringes, which we know all about. With the sodium light, they’re very clear indeed and there are many of them, because it’s monochromatic light, that’s a well known phenomenon. With the white light, there are far more of them than you would expect. The standard result, which I had learnt about at school actually, was, with white light, because of the different wavelengths, it blurs out the fringes, and you can only see two or three coloured fringes inside your rainbow which is the fold. And, this, this was remarkable, to see, like, six white light fringes, and remained remarkable for a long time, until Mike Berry and I figured it out, that it was a physiological effect, that it’s to do with the way the eye perceives colour. And there’s a picture in my book of these fringes, and if you put the book on the other end, I think as much as twenty metres away, then your eye sees them as black and white fringes, not as coloured fringes. It’s a physiological effect.

[01:19:17]

*Mm. Thank you. When you were, when you were swimming with your mask and flippers on holiday, and you said you were very interested in the patterns on the floor of the sea, can you give me a sense of sort of, how long you were doing that for? I mean in...*

How long was I staring at them you mean?

*[laughs] On a typical visit.*
Well, the water’s warm. [laughs] So you just pause in your swimming, because, when you’re swimming, you ruffle up the water; you want to keep it nice and, smooth, and, just float and, and look. Oh I should think a few minutes at a time.

*Did your family, when you came back to, came back to the beach, comment on the fact... So you were just sort of, laying in the water...*

Yes.

...*trying not to move.*

Yes.

*Did they comment on what you were doing?*

[laughs] It must have seemed strange. No, I just said, ‘I’ve been looking at those patterns again,’ you know, it was... [laughs]

*And did your, did your... What sort of age were your children when you were going on those holidays?*

Oh we didn’t take them on those holidays. These were, this was later. The children were getting into their, it’s in the Seventies, the children were getting into their twenties.

*Ah.*

So, sort of family holidays with children was a different phase. No this was with Georgiana.

*Mm. So you would be reporting back to Georgiana on the...*

Yes, particularly we used to go to the same restaurant in Bonifacio in Corsica, and there were always the same white boats on the quay, and I was fascinated by these. I
also took pictures of the sunlight. You know, if you stand on a rock and look at the sea and the sun is over there, you get a glare from the sun. And it’s, it’s being reflected in all these little waves underneath you. And it’s a series of spots. And they dance around in a way that you can’t really analyse, everything’s happening so quickly. I got some students to do this in the lab to see what’s really happening, and, in Sardinia I took, again, a photograph at 1000th of a second, and those spots turn out to be little, little loops, each one as a little loop. It’s a, they’re two little bright points which are born at a certain place, and they go round, and then annihilate each other, in little loops. As, just as you would expect actually. That’s, that’s the shimmering of sunlight on water analysed microscopically. Well not literally microscopically but on a small scale. And very slowed down.

*And how do the, how does that relate to the five...*

Five catastrophes?

*Yes.*

How does it relate? [pause] I can’t remember offhand. [pause] It’s a different, a slightly different phenomenon. You’re looking in a, in a different space. You’re not looking at caustics. You’re looking at the orbits of reflected points. It’s all one subject, but, they’re not literally catastrophes. You’re looking at the images of a point, which is, these are images of points, not caustics received on a screen.

*Mm.*

It’s more akin to what the astronomers do now and look at very very distant quasars, and see them, the light bent through a, gravitationally, through a cloud of gravitational matter, and they see several different images of the same star. Very strange when you think about it. You know, you’re looking at the same star but you’ll see it several times over. And those are different point images of the same star. It’s more that geometry than the caustic geometry.
Mm. And what was Georgiana’s response to you when you went to this restaurant being fascinated by the boats?

Rather than her? [laughs] Well I don’t know. It’s just, revisiting an old, an old and very favourite restaurant. We usually ordered the same thing.

Thank you.

[End of Track 13]
Could you describe how this work on caustics developed around the, the particular structure of a certain number of those caustics, when you examine...

Yes.

...the increasing levels of detail.

Yes.

And there’s I think an associated historical link back to an earlier part of your career at the Cavendish where something was going on regarding cusps.

In a mathematical lab. Yes. We’ve talked about hierarchies, but, this is a different sense of the word. The caustic is the coarsest member, the coarsest way of describing light singularities. In other words, at first glance it just looks like a bright line, and it’s brighter on one side than it is on the other. But if you look more closely, and if you use monochromatic light, you see that there are interference fringes, as we’ve just discussed, inside the caustic. That’s the next level of detail.

[01:22]

We often talk about the caustics as the skeleton on which the wave flesh is put, and the flesh is the diffraction patterns. Each caustic, each fold cusp, swallowtail and so on, has its own characteristic diffraction pattern. That’s a very important result, which was known. And, I suppose my own contribution has been actually to look at them carefully with the microscope, and, also to analyse them in terms of the dislocations that they necessarily contain. And each of these, I’ll call them catastrophes, each of the catastrophes has its own diffraction pattern, and each of the diffraction patterns has its own pattern of dislocations. So there are really three levels there, caustic, diffraction pattern, dislocations. Now the next level would be to take on board the fact that light is an electromagnetic phenomenon, and therefore scalar theory is not adequate, and you need a vector theory, and to look at the, the C lines associated with these diffraction patterns. This is, this is not something that’s, that’s yet been done, thoroughly anyway. So there’s a whole scale there.
And then, if you want to be really, want to go on, you look at these, these interference places where there’s no amplitude, and you say that, well actually, that’s just classical optics. In quantum mechanics, if you looked very carefully at these places, you would see though it’s statistically completely black, there would be the occasional as it were flash of light. This is quantum uncertainty. And that aspect of them has not been studied at all, so far as I know.

Mm.

But it’s kind of interesting that you can go from the very large to the very small, encountering at every stage something interesting and indeed important.

Mm.

Oh yes, the link with Cambridge. When I was there in 1946, unknown to me there was a man called Pearcey working in the, what was called the Mathematical Laboratory, and, he was working out what we now know as the Pearcey pattern. And the Pearcey pattern is none other than the pattern of interference that you get associated with a cusp. He didn’t know that it was the beginning of a whole hierarchy, but indeed it was. He was the first... Actually the first one to work these things out was George, Sir George Airy, the Astronomer Royal, in I think it was 1836, who worked out the pattern associated with a fold. This is the pattern you see in a rainbow. If you look inside a rainbow, under the right conditions, you will see a series of bands behind it, and these, these are explained by Sir George Airy, and they are something which we now call the Airy function, which is a, a well known mathematical function. Each of these catastrophes has its own mathematical function associated with it to describe the diffraction. And, you may be interested to know that the most recent addition of the big, used to call it a handbook of mathematical functions and their numerical values, it was called Abramowitz and Stegun, and every applied mathematician needed a copy of this, these tables, to look up the numbers. They don’t need them now because of the, because computers do it for them. But there are formulae before each chapter in Abramowitz and Stegun saying how the
formulae are calculated, which are the only bit of the book which a lot of people really used. But now a completely new edition has gone online from NIST, the organisation, the standards organisation in America, which has a chapter on each of these mathematical functions. And I think largely owing to Michael Berry’s influence there’s a new chapter on these new diffraction integrals which arise from catastrophe theory, which we’ve been very interested in and which I am, of which I have looked at the structure in terms of dislocations.

[06:54]

Mm. Thank you. And could you say a little bit more about what Pearcey was doing in 1946?

What he was doing? Well, the Mathematical Laboratory had a, an analogue computer which was called an integrator. It consisted of a series of discs rather like gramophone turntables, and, these... And it had lots of axel rods with gear wheels, and one of the rods turned and then the others turned because of the gear wheels. And that turned the discs, and, by, as it were, the gramophone head moving over the disc. So, it integrated your function. And it had, the more of these platform discs it had, the more powerful it was. It took a long time to set it up from doing one differential equation to solving another one, and in order to do that efficiently, they built a Meccano version of this integrator, and my friend Walter Cairns, Warren Cairns, who worked as I mentioned in our group there, was making some new what are called torque amplifiers for this Meccano machine, and they needed torque amplifiers because if you turn a rod, an axel, and it’s got to turn a whole lot of others, the friction is eventually going to stop you doing what you want to do. So, you need a, a thing called a torque amplifier which magnifies, amplifies the torque, the twist that you’re applying to the rod. And he was... It’s an ingenious device with a, with a string round a, a sort of cotton reel, and which spins, and amplifies the torque.

Mm.

So it’s, he was working on that at the time. So there’s a link which I was completely ignorant of. There’s also another link that is perhaps historically interesting, that, in the Cavendish at that time the radio astronomy group was going strong, and working
with them was John Findlay, who was getting pictures of returning echoes from the ionosphere, radio echoes from the ionosphere. And these pictures showed interleaving waves, and sometimes the waves came to an end. And these actually were dislocations. But he didn’t in any way remark on them, although there they were, and they became key to what we later did.

[09:59]

Mm. Thank you. And what was the role of the Meccano replica in planning the larger...?

Well, as I said, it took a long time to change the machine from working out one differential equation to working on another one. And in order to see how to do this, and how to arrange the discs, you could do it so easily on a Meccano model, and then you went to the big machine, Metrovick machine, which was, filled the whole lab, and was sitting in a series of glass cabinets rather like the cabinet, horizontal cabinets in a museum. Looked rather like that. And, which were difficult to alter because you had to move heavy pieces of steel around and unscrew things and screw them up again and, and you wanted to mock it up first, in Meccano.

And are you able to say how a series of discs of discs in a room in that way can do mathematics if you like, can solve a differential equation? I mean, perhaps starting with what the output is, and...

It’s called a, it’s an analogue computer, and, frankly, I don’t think I’ve really thought of it enough to give you a simple explanation, but, what you are trying to do is to integrate a function.

What does that mean?

Integrate? Well, suppose you have a curve drawn on X/Y axes, and, you ask for what is the area under the curve, that’s an integral. You’re given the curve; the area is the integral.

Mm.
And, in mathematics you are given a curve which is a function, and you are asked to integrate it. And you, you try to do this algebraically, but, some of them are complicated and you can’t do it algebraically so you do it, nowadays we’d say numerically, but then you, you used, you could use an analogue computer to do what’s now done numerically with a, with a computer.

*Mm.*

And the analogue computer was a disc with this, like the, the playing head being moved different distances from the centre.

*And so, do you...*

I wish I could give you a, a better explanation, but I can probably come up with one with some thought.

*And, what was the nature of the output of it, how did it give its answer if you like?*

[pause] I suppose there were little revolution counters on the end of the axels.

*Mm.*

Something like a gas metre I would imagine. [laughs]

*Thank you.*

As a guess.

[12:46]

*Now, the next thing that I’d like to explore is the, is your connection with Russia in various ways, in terms of visits, but also in terms of your support for, I think it’s Scientists for the Release of Soviet Refuseniks that was a London-based organisation.*
Yes, that’s right, yes.

And also your role in organising conferences for the International Glaciological Society, and which included Russian delegates obviously and there were problems with that.

Yes.

But, I suppose, if you could give us a sense of the timing of the, the relations with Russia.

Yes. Yes, I’ll try and do that. Actually, I think it started with my relations with my colleague Shumsky, Petr Shumsky, whom I had met at conferences, who was the well-respected Soviet glaciologist. And, I had written this book on physical properties of crystals, published in 1957, and, Shumsky wrote to me in, I don’t know when it would be, around, the end of the Sixties, saying... Of course the book had already by then been translated into Russian, under the influence of the rather prominent Soviet crystallographer Shubnikov. And, in the late Sixties Shumsky wrote to me and said, ‘We are bringing out a second edition of your book on crystals. Would you like to come to Moscow, and what we will do is the following. You will get royalties on the second edition, and these can be used for paying for your wife to tour around with you. You yourself will be the guest of the Soviet Academy, and will travel around. So we can... you can bring both, both of you.’ So we did that, we were rather pleased.

[15:00]

And, so we went to Moscow, and Shumsky... Actually we were under the charge of a series of Russian glaciologists in turn, and Shumsky was just one of them. But I remember the time came when we needed to collect the money for the royalties, and Shumsky took us to an ancient mansion in Moscow where the publishing firm was. The publishing firm was called Mir, which means peace. And it had the old, very worn parquet floorings that you get in those old mansions. And we went in to the office of the publisher, and he explained to me the basis on which the royalties were calculated. He said, ‘Of course, I know that in England the royalties are based on the number of copies, but that would be very unfair in Russia, if somebody has a book
which is studied in all the schools in the Soviet Union, then that would not be fair. So instead we pay by the words, and a figure counts for so many words, and an equation counts for so many words. And we calculate then, and then we work out the royalties. And here they are.’ And here was a big envelope stuffed with bank notes. ‘Would you like to count them?’ he said. [laughs] I said, ‘No, thanks very much.’ And then he said, ‘Do you have any questions?’ So I said, ‘Yes. What about the first edition?’ ‘Ah,’ he said, ‘that is too long ago.’ [laughs] You see, before I went in, Shumsky had very carefully pointed out to me with some trepidation I think that you mustn’t ask any questions, you have no rights at all in this matter. They don’t need to pay you any royalties. But they’re just doing so. Because they, they were in total breach of the, what was it, the Berne Agreement I think it was, about, about licensing and, and royalties. So that’s what happened there.

[17:17]

And then we were taken on a great tour. We went to, all round Moscow, Leningrad, to the Antarctic Institute at Leningrad. And then we were flown to Tashkent in Azerbaijan, where I was to be the guest of the Azerbaijan Academy of Sciences, or maybe it’s the Tashkent Academy of Sciences. And while we were in the plane the message came through that, ‘We cannot receive Professor Nye.’ So, we were already on our way. And, we arrived in Tashkent without the red carpet treatment that they would have laid on. But the students had rallied around and done magnificently. They knew that Professor Nye was coming, and they arranged a marvellous picnic in one of the parks, local parks, where we had a huge mountain of shashlik, what do they call, the rice thing with lamb in it. There’s a name for it, it’s just gone. Not shashlik, it’s... Anyway, that was the staple for the, for the, for the picnic.

[18:54]

The conditions were, were a bit primitive all over Russia. I remember on the picnic my wife wanted to use the toilet, and the students were very worried about this, because they said, ‘You won’t want to use it.’ [laughs] Quite right. And, when we got to the next... We got to Samarkand. Was it in Samarkand? [pause] They were, again, they were unprepared for us, because, we really shouldn’t have been there. We were not the guest of the Academy you see. And our conductor, our host, Sasha Krenka, was quite worried about this, and, treated it as, as a bit of an obstacle. And, we had got with us some chocolate and some whisky, and, he chided us one evening because we had eaten our chocolate and our whisky, and it was our iron rations for
emergencies when there wasn’t any dinner available. So, it wasn’t exactly the smooth operation that the rest of the trip had been.

Why had the official welcome not come, why had they decided that...?

I have no idea, no idea. I think they, typically they, they arranged these things always at the very last moment, and they communicate by telegrams, and, not by letter or telephone. But, there’s always a telegram, or there was, that said, ‘We cannot come to the conference,’ or, ‘We will arrive tomorrow,’ or, whatever. It was very much last minute, hand to mouth sort of... I think that was Russian life actually.

Mm.

[20:51]
But I visited Moscow on other occasions later as you were referring to, and they were quite different. There was a conference there which I was... No I don’t think I was the president of the International Commission of Snow and Ice at that time, but, anyway, I went to a conference there and, we were treated of course officially as you are at conferences and...

[21:24]
But the perhaps more interesting occasions were when I went to visit, as you referred to, the Russian refuseniks. It was this group who wanted to leave Russia and go to Israel, and for this they needed an exit visa. And when they applied for an exit visa, they tended to be demoted in some way. For example, if they were scientists working in the lab, in a lab, they might well find themselves as a technician, and without access to the library any more. Those sort of things happened very frequently. And, there was quite a group of these, and, in order to keep up their interest in science and their intellectual life they arranged seminars, typically a Sunday seminar run by Brailovsky. When I got there, Brailovsky was in jail, but his wife was carrying on. He was in jail on some excuse, violating Article 96 of the Penal Code I think it was. Anyway, I was telephoned one evening by my friend John Ziman, a professor of physics here, and said, ‘Would you like to come to Moscow?’ And he explained to me that there were these visits arranged. And I said, ‘Yes.’ I’ve never... If I’m asked to give a seminar, I’ve never refused. So why should I refuse now? Slightly peculiar
circumstances, but it’s the same situation. And these, it wasn’t in any way illegal to go and talk to Russian scientists. But, it happened to be a rather unusual group, these refuseniks. So John Ziman and I, they always sent out pairs of scientists to support themselves as it were, [laughs] morally as they went out there, because it was a peculiar situation. And we went, we got to the airport, and my friend Sasha Krenka was not there to meet us as I hoped, and we waited around for an hour, and, then got a taxi into the city, and, got to our hotel, and, and then went to visit the refuseniks.

John Ziman had a whole lot of telephone numbers in his diary, which he had written in code. I think he enjoyed that. So he had to be able to read them. But we were worried, or he was worried, that they might be searched and find all these telephone numbers and, ask questions about them. And I said, ‘What will you say if you’re asked questions? “We hear you are going to this, to this refuseniks seminar.”’ And he would say, ‘Well that’s very interesting, please, tell me about that.’ And...

[laughs] Anyway, we, we got out to the, to the apartment, which was way out on the outskirts of Moscow, at the end of the Metro line, and we found it, and we were, we were especially there, I think possibly it was a Sunday morning, and they were having a kind of committee meeting in this tiny little apartment with the child’s toys all stuffed away on top of the wardrobe, and, very little room, with a blackboard. I’m pointing to this size. A foot and a half at the most across. Less than that I think. And a piece of chalk which might just have been taken out of the hillside, greasy chalk which made hardly any impression.

But there was a committee meeting going on as I say, and, they were talking about their next meeting and their relations with the authorities and, how Mrs Brailovsky’s husband was in jail. And one of them came over to me with a manuscript and said, ‘Please, I would like you to take this and have it published in the West.’ And I looked at it, and I didn’t understand anything about it at all. And, I said, ‘Well...’ He said, ‘I won’t give it to you now, but I’ll give it to you tomorrow morning. Where can I...?’ I said, ‘Well come to my hotel and give it to me.’ ‘No I can’t come to your hotel. There is a baker’s shop just a few doors up from the hotel.’ And at this point a great hush fell on the room, and they all fell completely silent, pointing at the, at the light thing, and, they brought out little pads, these little one-time pads where you write, get them as Christmas presents for children, and you write on them and then you go like
that and it all disappears. And they conversed with one another with these pads, passing the pads one to another and then shaking their heads and passing them back. And eventually came to a consensus, and they said, ‘No, you mustn’t take the manuscript out. We have decided that it might be bad for Brailovsky who is in jail,’ because, they might have something, they might in some way blame him for this. And the seminar had in fact been stopped the previous Sunday by the police, who had been at the bottom of the staircase, and stopped people going in. But this time, when we were there, we saw nothing of them. We went up in the tiny little creaking elevator to the apartment, and, gave our seminars. And I remember talking about the elliptic umbilic, and they were... Of course I had to speak in English, and they were pretty good on their English. But, when it came to a work like that, they would have a little conversation between them as to what it meant. ‘Umblic, umbilicus?’ And they, they talked about that. And, actually, it was the most, I think it was the most lively audience I have ever lectured to. They were fascinated by every word, even though some of them were chemists and biologists, or even economists, but for some reason the physicists and the mathematicians were to the fore. And, I don’t know why that was, but, it was the case. And, they were, as I say, extraordinarily alive in a way that I had never seen in a little seminar audience. So that was quite an experience.

[29:04] While there, did you see any other evidence of, well, KGB, sort of, limitations on, on freedoms?

Oh. Well they were all around, because of course, they’re not supposed to be conversing with, with foreigners. My friend Sasha Krenka, whom I met on several occasions, was a geographer, and the geographers had special privileges, they were able to travel abroad as other scientists were not allowed to. So he had quite an experience of, say, South America. And, he, he invited both John Ziman and myself to dinner in his house, and his son was there who professed not to understand English; in fact he did. And, John Ziman and Sasha talked a lot about politics. And then the son suddenly came to life and talked to us in English [laughs], which was strange. But, I think that was just fear on his part. But, Sasha Krenka told me that he mustn’t be seen to be associating too much with Nye, because that would simply go on his
record, and then when he wanted to go to a conference, that would be a point against him. That was the kind of thing.

[30:47]

On another occasion I went there and the, I went to the Geography Institute, as a sort of courtesy, and, gave them a little lecture. And I asked to meet one of my friends there, whom I knew reasonably well actually, and, he shook his head, he couldn’t, he couldn’t do it. And, another one, who was a little bolder and was an Antarctic scientist, a very good Antarctic scientist, invited me back to his apartment for lunch. We went down the stairs, and, there was a lady in the bottom foyer, sort of a hall, selling fish. She had been to the market early in the morning and she was selling fish to these professional chaps as they came down the stairs for their lunch. So he bought a fish, and we took it back to his apartment, where, we talked. And he, he unplugged the telephone and said, ‘You know, that’s where they put the bugs.’ And, and then we talked quite freely. He, he asked for... I said, ‘What would you like me to send?’ And he said he would like oil paints, because he painted in oils, and I made up a parcel of oil paints, particularly white, which he wanted, what’s the name for it? And... But I’m not sure that it ever arrived.

[32:35]

I had, I also discovered that there was a geophysics seminar on similar lines in Moscow, and I was invited to, to address them. I went out there, and we sat round a table and talked about geophysics. And one of the chaps there said, please could he have reprints? And I said, ‘Of course, certainly I can, but I’ve left, I’ve got them all in the hotel.’ ‘Oh,’ he said, ‘I’ll pick them up tomorrow morning. I’m staying in the big hotel where all the foreigners stay,’ at that time, near the Kremlin. The very big one. And, I said, ‘Well, I’ll given them to you at my hotel.’ ‘No I can’t come into your hotel,’ he said, ‘we’ll meet outside.’ So we met outside, and, he had this carrier bag that all Russians carry around with them in case there’s a bargain going in the shops, for something just come in. And, he said, ‘We will walk.’ And we walked, and we walked, and we walked to, to a park. And traditionally we sat down on the park bench. And I took the reprints out of my briefcase and he put them into his carrier bag, just like they do in the movies. [laughs] And, I’m sure we were not being observed, but, that was his concern. And, I went back to my hotel and he went to his lab.
And do you know what reprints they were?

Yes, they were about optics. They were the, the *Philosophical Transaction* one with the, all the photographs in.

There was another occasion I was going to mention to you about, what was it? Oh yes. Yes, I was due to give a seminar at the, to the refuseniks, on a second occasion this was, and I had been to visit them in the morning, and it, it didn’t occur to me that I wouldn’t get back to my hotel, say, for lunch. But during the morning it was arranged that I should go and meet another Russian scientist who had somehow become estranged from the group. And, they said, ‘You just catch this bus, and you go so many stops along, and then he will meet you at the stop.’ So they took me to the bus stop, and I got in, and when I got in I realised I was in trouble, because, the whole bus was full, and the windows were all steamed up, and I had no idea where we were going. And every now and again it came to a stop, and people got out. And I showed this address to the lady who was sitting beside me. She shook her head. And, the driver was sitting in a glass cabinet way over, I think it was one of these double buses. And, clearly nobody was going to help me find this address, and, it kept on stopping, and, I thought, if I get out at the wrong stop, I’m scuppered. I don’t even know the number of the bus. And, I guessed, providentially, it came to a, came near a stop, and I got up, and I walked to near the exit. And as it came to a stop I looked at this woman, and she very gently nodded her head, very gently, inconspicuously. And she nodded her head and I nipped out. And there was my scientist whom I was supposed to meet, standing there. Gives you an idea of the terror that they had of saying anything to a foreigner.

*Mm.*

What would have happened if I had got out at the wrong stop, I cannot imagine, because I had no map, and, I had already tried to get a taxi and nobody, no taxi driver would take me. So, I would have been...

*Stranded.*
[laughs] Absolutely, I don’t know what I would have done. I think, in a way it’s the most terrifying experiences of my life. But, anyway, I, I talked to the, to the scientist and went, eventually, back to my hotel, and got my notes for my seminar, and I was pretty late, and thought, I won’t go by the Metro, because, which I knew; I’ll, I’ll get a taxi. There was a whole rank of taxis out there. They all shook their heads, ‘Nyet.’ ‘Nyet.’ ‘Nyet.’ I couldn’t get them to take me. I don’t know what I should have done, given them a hefty dollar bribe or something, I don’t know. But, simply couldn’t do anything. So got to the Metro, was already very late by this time, and, then got to the far end, and, tried to find the place where the seminar was. Didn’t succeed in finding it. So, telephoned them, and went back to the Metro station, and, and they said, ‘Stay where you are. Don’t move. We’ll come and get you.’ So the... Excuse me, my wife’s just come in.

[End of Track 14]
Could I ask you please to continue the story from the point we left it. You are at the Metro station and you phoned them and they...

The Metro station is almost at the end of the line, and, I think I was at the one second from the end of the line, for some reason. And, I rang the, my friend up and said, ‘I’m at a Metro station. How do I get to you?’ And the said, ‘Stay exactly where you are, don’t move.’ So I waited, and, was duly picked up by one of the organisers of the seminar. They had been waiting patiently for hours there, literally hours. [laughs] I mean no British audience would last that long. It may have been as much as two or three hours. And, the man accompanying me was a Professor Baum, who was a micro, who was a biologist, and he had brought with him The Biochemists’ Songbook, which he had written, and he was keeping them entertained by teaching them songs from The Biochemists’ Songbook, which is a, a way of making the introduction to biochemistry a little more palatable because there’s so much detail and odd names that have to be remembered. And he had done it with his students by inventing a songbook. That went down very well I think. So I had an excellent introduction to my, to my much delayed lecture.

And was this the second group of seminar of the refuseniks that you had...?

It must have been, yes. Because the first time I went out with John Ziman, the second time I went out with Harold Baum.

Mm. And, do you remember that particular meeting in enough detail to describe the setting and the people there and...?

The people there? It was much the same as the, the first. No progress had been made. I mean there were some new faces and some old ones. But the enthusiasm was exactly the same. I, I’ve forgotten what I lectured on the second time, but I know I, I had... Oh yes, I think I must have lectured on, on wave dislocations. And, I had wised up sufficiently to know not to rely on any kind of audiovisual devices, certainly not on their blackboard, so I had got a series of A4 sheets and just written on them in
very large letters, and held them up. That’s what I remember about, about that second time.

_Do you remember their response to your, you know..._ 

Enthusiasm, yes.

_Mm._

Great enthusiasm.

[02:51]

_And do you know... I don’t know, it would be difficult to tell probably from what you said, but do you know whether things like handing over the papers on the bench in the bag and the giving of the seminars, do you have any sense that there was an influence on Soviet science, perhaps after the period where the refuseniks were, sort of, pushed to one side, do you have any sense that things developed there that wouldn’t have otherwise?_

No, I, I mean, I simply don’t know. I don’t know what happened. I mean they were, what they were doing was, it just occurs to me, rather what Hitler did. They were kicking out their best people. I mean, you know the book, _Hitler’s Gift to the West_, or some such thing.

_Mm._

Meaning, all the Jewish scientists that he, that fled from Germany in the Thirties, here was, here was the Russian Government essentially kicking out their best Jewish scientists. Of course there were lots of them who perhaps knew enough not to apply for visas to Israel, but these chaps that I was talking to were, were very good scientists.

[04:13]
Mm. And could you say how, from, perhaps at the same time or after these seminars, you developed an interest in supporting the Scientists for the Release of Soviet Refuseniks, which I think is a London-based group?

Well that was the very society which kind of sent me out to Moscow. It was under their auspices that I went.

I see. And, about the, your writing of letters in support of particular...

That was all, that was all done under the auspices of that society, yes. Which, it was John Ziman who put me in touch with them, and once they knew my address, I, I just got letter after letter from them.

And do you remember the response to any of your letters to the Soviet authorities in relation to...?

I don’t think I ever had any response. But we were simply told that these prison governors, if they received a lot of letters for one particular prisoner, would maybe treat that prisoner a little more leniently.

[05:21]

Mm. Now before we, before we go on further, could you tell me, or give me a sense of the relationship between you and your parents? I realise we may have to go back a bit, but the development of it. I think, probably from the time of the Cavendish onwards. It’s just that looking back at the summaries of the recordings, I don’t have much of a sense of your relations with your parents from that sort of period onwards...

Yes.

...in terms of, how often you saw them, what...

Yes well, my mother died when I was at Cambridge, that would be back in the, in the Forties. On my father’s side, he, he lived until he was in his nineties, and he had been
born in 1881, so, you can work that out. He, he died in the, in the Seventies I guess. And, by that time he had left our family home in Hove and moved to Oxford. Now we frequently visited Oxford to see him. He had a, a rather nice cottage in Oxford. And my brother, Peter, also lived in Oxford, lived close by him, by my father in Oxford. So, so, the relations consisted of, rather frequent visits up to Oxford to see him.

Mm.

He also came down to Bristol from time to time. He came on holiday with us. He celebrated, I think it was his eightieth birthday, a significant birthday, with us in Spain, at Tarragona. And, it was a time when Georgiana and I were going camping in various places round the Mediterranean, and we, one holiday we went and camped on Elba, off the coast of Italy as you know, and my father came and joined us there on Elba. And, we shared that holiday together with him. We, we were camped in our little tent up further into the island, and he was staying in a hotel down at the main place which he called Porto... It’s Portoferrai. But, another, another time I, I guess before I was... Would it be before I was married? No. I... I remember meeting him for a holiday in the south of France at Menton, on the Riviera. [laughs] I had, I had picked out a little hotel which I thought was rather cosy in the middle of the town, and, showed it to him with some pride when he arrived, and said, ‘Daddy, I’ve found a nice little hotel.’ He looked at it and he said, ‘Yes, it’ll do for one night.’ And then went and sought out his idea of a proper hotel, which had a nice airy dining room in a sort of Edwardian sense, and, we moved. [laughs] But it was a nice holiday

[09:12]

*Did your father ever comment on your career or your work?*

Not really. I mean I think he was glad that I was making a success of things, because, that was his idea in life, to be a success. But, of course the details of the science were not for him. I think he was pleased.

[09:37]
And, you say that your, your mother died in, while you were still at Cambridge, as a research student, or as an undergraduate?

No, I was... Let me see. I know the digs where I was staying, it was in the late Forties, I was, I was a research student or just about... Oh wait a moment. Yes, I was a research student, I remember. I was in the Cavendish, working with Orowan. And, I was giving a series of lectures, and I had to break it off and go home. Yes. So that’s when it happened, yes. I was very shaken by it.

Mm. If you feel inclined to, of course you don’t have to, but could you say what effect that had on you, your mother’s death and your sort of, or how it affected, I don’t know...

Well she was, she was always a semi-invalid you see. She had gastric trouble. I think probably she had cancer, but it was never admitted at the time. It was those days you, you didn’t mention the word. And, I was... I remember kind of growing away from my mother in the sense that I, we had no intellectual meeting ground. And, I mean now I think I, I could have managed that much better, and I regret that. But, I would see her, we wouldn’t have much to talk about, and I would give her books as presents, which were not really a suitable thing to do. So, I regret not having, having been closer to my mother, but her death affected me very much.

Did it alter your, the way that you thought of your life or, life itself or, yourself?

I don’t know. How does death affect anyone?

Mm.

I don’t know. It, it’s one of those things that happen, inevitably.

And, do you remember your father’s response to it, and...?

Oh very much. He was absolutely devastated, and, wore a black tie for the rest of his life.
Mm.

She was absolutely everything to him. So that his whole period in Oxford afterwards was really in the shadow of Mamie’s death.

Did he move to Oxford soon afterwards, is that, does that explain the move?

Probably, yes. Yes I think he did. I think there was nothing much to detain him in Hove, and Peter was living in Oxford, so it was a rather obvious place to go. He moved from a substantial house in Hove, which he had designed himself, to a, a nice, rather cosy cottage in Old Marston, where the furniture that was suitable at Hove was not at all suitable. But it was somehow fitted in.

[12:51]
And, what would you say of the relations between you and your brother over this sort of, same sort of period from Cambridge onwards?

Well, it was, again a matter of going up from time to time to Oxford and, and seeing him. So we were not close, but, but we saw each other as frequently as I saw my father.

Remind me what your brother’s job was?

He was reader in soil science at Oxford. He, he had started in, as a, as a chemist, a soil scientist, soil chemist, and gone out to the Gold Coast as it then was, Ghana as it now is, and, then come back to a job in the International Atomic Energy Authority at Vienna, and then come to, to a post in Oxford. You know, readerships in Oxford are equivalent to professorships elsewhere, in fact they’ve turned them all into professors now. So essentially he was a professor at Oxford. And, he died not too long ago, only a few years ago. So... And I used to see him in his old age at, when they moved from Oxford to Bournemouth.

Was there any overlap in your scientific work at all?
The only one I can think of is that he wrote a, he wrote the standard textbook on how nutrients flow through the soil to reach the roots of plants. And this involves both diffusion of the nutrients in the, in water, and the physical motion of the water through the soil, both of which I was interested in as a physicist. And, he wrote the standard textbook on this with his colleague Tinker, and Nye and Tinker is the book people quote. And then it came to a second edition, and by that time he was getting extremely frail and ill and couldn’t really deal with the proofs, and I offered to, to go through the proofs. Which I did, and corrected them, and made some mathematical amendments to them.

Did you talk to him about those amendments?

I didn’t discuss them, I discussed them with his co-author, Tinker. But, I don’t think I discussed... I might have discussed them with him, but, he was, I think he was by that time sort of beyond that kind of discussion. I’ll tell you what the kind of thing was. Of course you’re not a mathematician, so you wouldn’t know the difference between partial differentiation and differentiation.

No.

Well, that was, that was the particular thing they had, trying to make it simple to non-mathematicians, they had made mathematical errors by using, not using partial derivatives, and this is one of the things I corrected. The other thing was, in tables. You know there’s a standard ambiguity in tables of numbers that, you can either start here at, say, at the top of the table, L, this is a length, L in metres, and then you have a lot of numbers, and these are the number of metres the length is; or you can say... I, I’ve forgotten what the distinction is now, but, it makes it totally ambiguous as to which way round it is. Is it the length divided by metres, or, are each of the quantities metres? Or, if you, say, times twelve at the top of the table, does it mean you’ve got to multiply every number by twelve, or is it that the thing times twelve is what it says in the table?

Mm.
It’s that kind of ambiguity which I hope I managed to clear up in this book.

*And did you tell him about your work and, and do you remember any of his responses to that, in optics or in glaciology?*

No, I don’t think so. No I don’t think that... I think from time to time we may have just mentioned it, but I, there was no real overlap.

*Yes. OK, thank you.*

We were... Did I, did I mention we were, we were for some time I think the only, was it, the only brothers who were both FRSs at the same time.

*Mm.*

I know the Longuet-Higginses are such a pair. But, I think we were the only pair for a time.

*Mm. So your, your father would have been pleased on two counts in terms of his view of...*

My father will have been very pleased, yes.

*...the importance of success.*

Yes, indeed.

[17:52]

*Mm. OK. Now, there was a story from last time that I failed to follow up on in enough detail, and that was, your time as President of the International Glaciological Society. One of the things that happened while you were President of that society is that you were approached regarding a plan to put nuclear waste in the bottom of Antarctica?*
Correct. Yes.

And, this was towards the end of the recording last time.

Yes.

And I wanted to know a little bit more about who was approaching you with that.

Yes. It was connected with a university in the central part of the United States.
Where would it? Was it Kansas? I think there had been a proposal to dump this stuff in a salt mine in, it was probably Kansas, or Kentucky, I, I can’t remember which one. And, they had scratched their heads and thought, well it would be much better to put it in Antarctica. Anyway it’s off our doorstep. And, then, the glaciologists got wind of this, and many of them thought that was a stupid idea, because we, we really don’t know what is at the bottom of Antarctica, and, it’s a good guess that there’s some water there, because we knew a little bit about Greenland. And, if there’s water there, that’s bad because, eventually the stuff will get out into the ocean. And we’re talking here about a timescale of, do you know, 250,000 years. So who can look ahead that far? I mean that’s the, some multiple of the half life of one of the isotopes involved, probably one of the plutonium isotopes. So, the glaciologists were all horrified, and so somehow we, we had a meeting to say so. I don’t know who we reported to. I, I don’t know who commissioned as it were the meeting. It may be we just did it off our own bats and felt we were the ones representing glaciology whose opinions should be made known to whatever authorities were going to, particularly the Americans, were going to deal with this, and tell them what a bad idea it was.

Mm.

Well of course they still haven’t solved the problem.

No. I wonder why they, the, the request for sort of, advice didn’t go to, well, an American sort of cold regions department or a...
Yes, indeed. Yes, it might well have done. Maybe they considered it. But we were
the international body after all concerned with, with ice. I mean there was the, also
the International Commission of Snow and Ice, but, but the, the Glaciological Society
felt in some sense responsible for what happened to the world’s ice. There was
always a toast at all the dinners saying, ‘To the glaciers, on which we all depend.’
[laughs] And of course they’re getting smaller and smaller.

[21:11]
Yes. Speaking of which, the International Commission of Snow and Ice, you were the
President of that, 1971 to ’75. Is that right?

Do you want to get the dates right? I’ve got a sheet here with them on if you want
to...

Ah.

Shall we look at it now?

Yes.

Right. [pause] ’62 to ’64, I was a member of the Committee on Glacier Variations. I
was, actually I was President at one time. [pause] Member of the Board of the
Permanent Service on the Fluctuations of Glaciers, 1971 to ’75. That almost certainly
is the same... Oh yes I’ve got it down here. President of the International
Commission of Snow and ice, ’71 to ’75. Yup.

Let’s take that earlier period then, ’62 to ’64, first, where you were involved in the
subsection on the, on...

’62 to ’64. Oh, oh there was, there was a Committee on Glacier Variations at that
time, which I don’t remember much about. And apparently I was a member of that
committee.

What was the nature of interest in glacier variations at that time, ’62 to ’64?
It was, it was something which was started by the Swiss, and they, since about 1895, had kept an official eye on their glaciers, on which they depended as it were. And, most years, or maybe every year, a book was published, a red book was published saying how much each individual glacier had gone forward or backwards. And that was done on a year-to-year basis. And I suppose, in 1962 the International Commission was, ICSI it was called, was interested in this, and, I think sponsored these books as they came out, and they were mostly concerned with Swiss glaciers. But we felt that the whole idea should be extended worldwide, and since we were an international body we were people to do that. So we had a, a committee on glacier variations, variations meaning, are they going forward or backwards?

And, at that time when you were suggesting that it ought to be expanded worldwide, what was the, the reason put forward for recording this worldwide?

I think a lot of this activity stemmed from the International Geophysical Year of 1958. I think it was, ’57/58.

Mm. Yes.

So there was a lot of increased interest as a result of that, and increased coordination throughout the nations of the world. So it probably, if you’re looking for, for roots, it probably stemmed from that.

Did it stem from any kinds of concern, or...?

No. I don’t think... I mean, nowadays, we’re enormously concerned about climate change. Nobody was interested in those days in that, it wasn’t on the agenda, at all. It was a rather esoteric pursuit.

So the reason for studying world glacier variations was a matter of, just interest rather than concern?
It was a, it was of concern I think to Swiss hotel keepers, who needed a view of the glacier, and were concerned if it was disappearing around the corner. And the Swiss after all, living so close to their glaciers, they’re more conscious, much more conscious of them than we are. But, it wasn’t a, a concern about climate, was it getting colder or warmer? That was of incidental interest. But to glaciologists, it was of interest as to why they were going forwards or backwards, and what connection they had had with the, with the weather, with the, indeed with the changing climate of precipitation, snowfall and, and ablation in the summer. And that was the driving force for my work at that time, in that period, on the connection between the two. The driver is the snowfall, or lack of it, and the thing that’s driven is the end of the glacier, which is what’s measured, or, in those days was what was measured. And the connection between the two is not direct. You can have a very... a period when there’s a lot of precipitation, but when the glacier’s still going back. And of course, in those days they were still, now, as now, they’re going back, but for the first time in my life I saw an advancing glacier when I went to Norway a few years ago. And it is a remarkable sight to see a, a cliff of ice, not quite a cliff but a, an edge of ice, moving through a virgin field, pushing down birch trees as it, as it goes. They just lean over and, and get overrun by the ice. It’s a very striking thing to see ice moving on to green fields. In all the glaciers I’ve seen, and most people will have seen, the glacier is surrounded by bare rocks, a very bare region, but simply because they’re retreating.

[27:38]

Mhm. Yes, thank you. And then, following this, an inventory of exiting glaciers you worked on.

Yes.

Could you tell me what that work involved, how it started?

Well that, I think it was... I didn’t start it, literally; I think it was probably Professor Hoinkes, who was a predecessor of mine as Chairman of the Commission on Snow and Ice, who said, ‘If we are the body, international body recognised as concerned with that, we ought to start by knowing how much there is. So we should have a, a worldwide inventory.’ And this was before the days of satellites, so the inventory had
to be based on ground observation and on aerial photographs from aircraft. And that was set in train, and it was a big project. And in the course of it, satellite observations came in, the first one was called the ERTS satellite, ERTS[sic] I think, ERTS satellite, and, that made, made it possible, as you might say, to count the glaciers.

[28:53]
We had no foresight of there being a great interest in climate change, but the general idea was, we’ve got this permanent service on the fluctuations of glaciers, variations, which shows what they’re doing coming forwards and backwards each year, but this is an inventory of what it’s like, a snapshot if you like. And possibly, people will feel motivated to repeat this in fifty years’ time, and take another snapshot. And, that was just our idea. And, I think it was a good one.

*Mm.*

Because, because that inventory will be a very good index of what it was like then, and what it’s like now.

[29:35]
*Were you envisaging any, or predicting, or envisaging any particular kinds of change in the world’s inventory in terms of numbers or...?*

In terms... I don’t think so. I did... Did I mention to you the calculation I did on the Berenden Glacier when I was, this was when I was at AIDJEX, it was brought to my attention by Norbert Untersteiner, that there was a copper mine in British Columbia.

*Yes.*

In front of the Berenden Glacier.

*And you could, you could reassure them...*

Reassure them that it was most unlikely that the copper mine would be overwhelmed within the next twenty years, and, and almost forty years to... And we’re somewhere between the twenty-year and forty-year mark now. And I shall send a congratulatory
telegram to my co-author, Norbert Untersteiner, when that date arrives. I must look up when it is.

[30:32]

*Mm.* Thank you. *There’s further work in glaciology that you wanted to describe concerning, Norway and, I forget the particular name of the features that...*

Oh there was, there was Mars perhaps.

*Mars to come, but before, I think...*

Before that? The floods in Iceland.

*Yes.*

Ah that. *Yes.* There was a, there was a meeting of the International Glaciological Society in Reykjavik, Iceland, and we did a field trip to see the site of the glacier outbursts that took place on the south coast. The big ice cap is called Vatnajökull, Vatnajökull. And, every twenty years, or subsequently every ten years, there was an enormous outburst of water from under the glacier into the sea. And that was the reason why there was no road along the south coast of Iceland. Because it would, not because it would be washed away, but because the bridges built over the various streams would be in the wrong places.

*Mm.*

The Icelander who took us round was Sigurdur Thorarinsson, who was a volcanologist, and we actually, interestingly we... Did I tell you about our expedition to Hekla? Hekla had erupted. Perhaps I should. It just happened we had this meeting arranged for June, and, Hekla had erupted back in, was it April? April I think that year, erupted with enormous force. And, a farm, say, twenty miles from Hekla, had telephoned Sigurdur Thorarinsson saying, ‘There are great rocks going overhead the size of motorcars.’ And he said, ‘Don’t worry, it’s always worst at the beginning. It’ll die down.’ Well by the time we got there, it had indeed died down,
but it hadn’t stopped. And we went along to see it. And what we saw was as follows. We approached it from the side from which the wind was blowing, because, there’s a lot of fine ash being blown, being exuded, blown out, and it was going in the opposite direction. So we approached it from the proper side. There was a, there were large rocks being thrown into the air from the crater, and then coming down again. And there were medium size rocks, about this size, being blown out, and landing on the sides. And it was building up a new cone there. And we started climbing up this new cone, which was like climbing up a pile of ashes. And Sigurdur said to me, said to all of us, ‘Nobody is to go closer than I go.’ He was the expert you see in the whole of Iceland. So I thought, well I’ll be all right if I stick close by him. So I stuck close behind him. And we trekked up this cone, going sort of sideways, not straight up, and I was about, oh, how far? Ten yards perhaps behind him? Ten paces behind him. And, we were taking photographs. And we arranged that, while you were looking through a viewfinder, of course you weren’t looking to see what might be coming to hit you. And just to be sure, we had a, an arrangement so that when you took a photograph, you had a friend looking just to warn you if there was one coming in your direction. And that worked fine. But while I was walking up with Sigurdur, one of these bombs as they were called landed slap between us, midway between us. And, I remember it landing, it was red hot on the underside, and it slowly rolled down to the bottom. It was, the whole thing was at the angle of repose. And it, it slowly rolled down to the bottom. And Sigurdur said, ‘I think this is close enough.’ [laughs] So we went back. That was my experience of Hekla. But that was nothing to do with glaciology.

[35:15]
The glaciological interest for me was that this flood that I spoke of in the south of Iceland was, occurred in this way, that there’s a volcano; it isn’t really a volcano, it’s a hot spot in the middle of the Vatnajökull ice cap called Grímsvötn. And, there’s a lake in the middle of the ice cap, and the lake level goes up and up and up, although it’s covered with a plug of ice, but the lake level goes up, and at a certain moment it all drains out, suddenly. And there’s a most enormous flood, so much so that at the peak of the discharge the flow is one third of that of the River Amazon, and greater than the River Congo, which are the two biggest rivers in the world. So it’s a big flood. But of course it only lasts a short time. In a few days, I think about two and a half days, it’s all over, and the lake is, is drained. But the mystery was, why did the,
why did it all happen before the lake reached the top and just overflowed, and why did it stop so suddenly, before the lake had, had drained out completely? That was very surprising, and had to be explained.

[36:51]

Well, Roethlisburger, Hans Roethlisburger in Switzerland, had worked out that when there’s water flowing through a tunnel in ice, there are two opposing factors at work. One is the fact that the tunnel is trying to contract under the weight of the ice, and the other is that the water’s flowing through it and by the heat of friction is keeping the channel open. And he worked out how there can be a balance between these two forces. With the Jokulhlaup, I thought... that’s, the Jokulhlaup is a technical Iceland term for this phenomenon, we had a conference on it, and a poor Icelander had to stand in front of us and say Jokulhlaup ten times so that we could get the pronunciation right. He, Roethlisburger had worked out the steady state, and I thought that what we needed was a, a different sort of theory which would deal with the rapid opening of this tunnel and the rapid shutting of it, how did it happen and why did it happen? Why did it all happen so suddenly? So I tried to work this out, and wrote down the relevant equations, which was a slightly sort of, unusual job for me, because it was a little bit more engineering than, than physics. But actually, the simplification of the equations showed a remarkable thing, that one can try and predict what’s called the discharge curve, that’s to say, the record of how much water is coming out at a given time, that the flood has a sudden onset, and it has a, then a gradual decay. And the onset followed a certain curve, which I was able to work out from rather, well, not through simple physics but it came out from the equations. And I was astonished to discover that it fitted the observed curve almost perfectly. To have a, something you dream up at a desk fit some observations that have been made under very rough field conditions so exactly, was really a very strange thing to happen.

[39:24]

The observations were made in an interesting way. The glaciologist had flown over the flood in a helicopter, and noted the height of the water against various rocks. And then later when the flood had all died down, he measured the height of the marks on the rocks. So was able to give a record then of how the height of the flood had changed with time.
Mm. Can you say how your prediction of that curve, which was very close, can you say how that came, as you put it, came out of the equations? I'm interested in how you, how you could predict a curve from, as you say, working at a desk, especially as it was so successful in...

It was a certain, as we say, power law, with an asymptote. It was a curve... it wasn’t an exponential curve as you might have guessed, but a, a curve which had an asymptote, that is, it went off to infinity. And it went on, kept on this curve up to a certain point when, I could explain, when, when it stops and turns over and the flood suddenly dies away. [pause] Well it comes out from the, there are equations of flow-through pipes which are done on a much smaller scale, and you can scale them up and guess what will happen at larger scales. Nobody’s done a measurement on something as huge as that, but they’ve done engineering measurements on flow-through pipes. And I used those flow equations, which are very empirical, for that bit of the problem. There were five equations, one said that the, one was to do with the conservation of mass, one was to do with the conservation of energy, where the heat was going, and then there was a, a law for the contraction of the tunnel under the weight of ice, and there was a fifth equation which was also relevant. And it was a simultaneous set of equations. And, by making various simplifications I could, I could work out what was happening at various stages of the flood, and was able to work out rather exactly what was happening at the beginning of the flood, which was the most important bit, well, most significant, obvious bit of the curve, not, not important or, or significant, but obvious part of the curve. [pause] Later on, other people took up this idea and did the whole thing on, by computing, and, and worked out the whole curve exactly.

Why mechanically then did it flood when it wasn’t full, and only expel some and not all of the water?

Yes. Well it was to do with the two competing processes of heat being produced by the violent rush of the water, and the inexorable plastic deformation that was taking place to close the tunnel. And when you set one against the other, eventually the plastic closure won. It was going on all the time. And the other process was also
going on all the time, but eventually the plastic process overcame the other one, and it stopped it pretty, pretty quick.

_Mm._

Roughly speaking, that’s what happened.

_That’s why it stops?_

That’s why it stopped. And why did it start, you say?

_YES._

Well it started because, around any enclosed body of water of that kind, there’s a certain place where it’s eventually going to come out, a sort of sill. You might think of it as the lowest place; topographically it’s a saddle in the contour lines. And that place was sealed by the weight of the ice, so that, the water didn’t... The question was, well why doesn’t the water just trickle through in a slow, steady stream, and you get a, a river at the end? Why does it happen suddenly? That was the, that was the big question. And... Well the breaking of the seal, as I called it, was, was really a hydrostatic event, that is to say it was to do with the gradual build-up of the pressure. And, that was not quite, not absolutely trivial to work out the, the hydrostatics of it, but it, that’s what broke the seal, which... You see it was sealed over a, over a whole length by the weight of the ice, and gradually that length that was sealed was contracting until it became a point, and then, whoosh, the water came through.

_Mm._

[44:50]

It was something like that. So, as a result of that, and at Sigurdur Thorarinsson’s suggestion, I think it must have been, an Icelandic research student, Helgi Bjornsson, came and worked at Bristol. Now Helgi had, knew all about the surveys of the area and all the levels and so on, and, he worked with me at Bristol. I had already done the theory, and was able then to use his surveys to put in the right numbers. So, we
eventually published two papers, he published a paper on the survey and I published a paper on the theory. And, subsequently as I just said, other people worked on this and did it all, did the whole curve and got it out from a computer. But that work has been used in all kinds of other glacier outburst events which happen all over the world, and it’s quite a hazard you know with, when you have a dam as it were made of ice, it’s a dangerous thing, because it’s going to go.

_Mm._

And the more you know about how it’s going to behave, the better for the villages just downstream. So it’s, it’s become quite important in those contexts. I’m very glad about it. It’s one of the most sort of, practically useful things I think I’ve done.

[46:29]

*And, and how was it that you came to be doing it in the first place, who, who invited you to help to solve that?*

Nobody invited me. I think I, I just wondered why it happened.

_Mm._

Sigurdur Thorarinsson, who had written a paper about it, and, there was a Norwegian who was involved in... I think he had said, he had said something like, the faster the water flows, the, the faster the ice will melt, and, so it will go catastrophically, which is the essence of it. I think it was a Norwegian who had said that. But nobody had put it into mathematics. So I, really what I investigated was, the stability or instability of water flowing through a, a tunnel in ice, and in some cases it can be stable, and in other cases it can be unstable. And in the paper I asked the question, well why is it then that you can have lakes perched on the top of glaciers? Why don’t they just drain away through, through fissures, the same as this thing does in Iceland? And, there are good reasons for that. And, if the veins in ice, which I think we discussed last time, are continuous, as I was maintaining, why don’t they, why doesn’t the water in those go downwards and heat up the ice, and why don’t they grow into a Jokulhlaup?
Mm.

Which they clearly don’t. But these are contradictions which need to be explained. And, that was what I did in this paper.

[48:17]

And is this a feature that you worked on without seeing it yourself?

Yes.

Yes. Did you at any stage go and...?

Well I saw the Vatnajökull after a flood was all over, that was when I went to see Hekla, but I didn’t see the flood itself, which was a rare phenomenon.

Mm.

I saw the results of it, and, we... They have now built a road along the south coast, I think partly perhaps because the floods have become less frequent. They used to be every twenty years; now they’re every ten years, or even closer.

Mm.

There are good reasons for that. To do with the thinning of the ice cap.

Right. I see. So, you were able through working on this from a distance to offer predictions about when it would happen. As well as working out the mathematics of how it happens, were you able to offer a sort of, prediction service?

Yes. Well, I mean, it was obviously going to be cyclic, and I think there’s been a recent paper which offers to, to work out the, the periodicity, but I’m, I don’t think I particularly worked out that it would be twenty years. I mean I knew it was to do with the rate of filing of the lake and, and that; I more concerned with how it happened. And, and all these, these paradoxes of, why it wasn’t happening all the time, with the
lakes perched on a glacier, or with all these veins in a glacier. Why were they... That was a very peculiar situation. You had all this water inside any glacier, any temperate glacier, sitting there and flowing downwards and not doing this, this trick of becoming a flood.

[50:12]

*Why, can you say why then water in glaciers tend not to produce these tunnels?*

Well, it’s to do with the dimensions. Are you... You’re not familiar with the simple harmonic motion equation are you? But, that’s the, that’s the essential mathematical thing. It’s rather like a pendulum that swings and has friction in it, and normally a pendulum dies down in its swings, because the friction is positive, but if you have that term in the equation negative, then something entirely different happens. And, that’s what’s called an instability, and it’s a matter of working out the sign of that term. Is it positive or negative? And in some of the cases I mentioned it’s positive, and everything’s safe; in other cases it’s negative and everything is very unsafe, unstable. It’s, it’s hard to explain it without mathematics.

*So, if it’s negative, does it do things, tend to speed up then rather than slow down?*

Well it’s, it’s an unstable system, rather than a stable system. A pendulum that’s going to come to rest is a stable system, it comes to rest. A pendulum that’s got a different kind of frictional behaviour, which we don’t have, would, wouldn’t come to rest, it would just go spinning round.

*Mm. OK, yes. So, it just so happened that this particular place in Iceland with the particular topography or arrangement of things meant that it had this negative...*

The particular dimensions.

*Mm.*

I mean there’s a vast range of possible dimensions from the veins in a glacier which are only, a fraction of a millimetre thick, and a glacier tunnel which is many metres
thick, and a tunnel which is thirty miles or fifty miles long, and, and a little short vein. There are tremendous differences in scale to be put in, and they make all the difference.

[52:31]

Mm. Thank you. Could you say how you came to be working in relation to NASA planned missions to Mars?

Ah yes. Just remembering who my, who my sort of, sponsor was. [pause] Oh yes, the, there’s what they call a Martian community in America, and a leading light in it is Steve Clifford, and he wanted to, the general idea was that satellites, and even telescopes, observe that there are ice caps on Mars, a north polar cap and a south polar cap. And if they’re going to understand these caps, they ought to draw on the expertise of terrestrial glaciologists. So there was a thought to bring together the Martian community, who were astronomers and know all about the climate on Mars and the, the winds and all that, all that astronomical aspect of it, and the terrestrial Earth-bound glaciologists who know something about ice caps. And so these two groups were brought together, very sensibly, to see what sort of information exchange there could be. And as terrestrial glaciologists we learnt all kinds of things that we didn’t dream of about Mars.

[54:20]

We learnt that its atmosphere was quite different, that the gravity, the strength of gravity was different, and, all kinds of important differences which had to be thought about. And it was, I think it was a very successful meting of minds there.

When you say we as glaciologists, who were the glaciologists dealing with...?

Who were the other glaciologists? Yes. I rather think Stan Paterson was one of them, but I can’t swear to that. Stan Paterson had written a lovely book on, called The Physics of Glaciers, he is a Canadian. Who else was there? Oh yes... [pause] Do you know, I’ve rather forgotten who the, who the other Europeans were. I think there, there were about three or four of them altogether.

And when was this?
They didn’t involve the... Sorry?

_Sorry. I was going to ask, when it was?_

When it was? [pause] In the Nineties.

_And where were you brought together to?_

We had meetings in various places, in America, Houston, Texas, and the... It’s called the Lunar and Planetary... Is it the Lunar and Planetary Institute, in Houston. But we met in various other places as well. We had, the last meeting I went to was at Lake Louise in British Columbia. The general idea was to have a meeting in a place which could have a field trip to look at something which was vaguely connected with Mars, the geomorphologists were to the fore here, because they, you can see this geomorphology on Mars, and they were looking for analogues on Earth. For example, most of the mid latitudes on Mars are covered in the, in the satellite pictures with what look very much like meandering rivulets.

_Mm._

And so they say, ‘Well that looks like water.’ And they’re interested in water on Mars, because, water means life. So, a great deal of the evidence for life or water on Mars was geomorphological evidence which looked like rivers, and, there’s a lot of, a lot of the field trips were concerned with that kind of thing. [pause] You said, when did I go there? I’m looking up the paper I wrote and that might be, that would be a clue to the date. [pause-looking through material] Oh yes. I see that we published a paper in 2000, two papers in, in the year 2000. And, yes, three papers in the year 2000 on it. So that, that would give you the dates. It would be shortly before that. [58:25]

The way it worked was this. Bill Durham I met at one of these meetings, was an expert on the creep of materials, that is, what happens when you put a, a stress on a rock or some material, and it slowly gives way by creep. And he had machines which measured how fast this was. One of the big difficulties of course is, it’s so slow, and
you normally have to have experiments running for a very long time, but he managed to do experiments in a reasonable period of time. He did it by I think, not the standard technique of suddenly putting the stress on and then waiting to see what happened, but deforming at a given rate by sheer, by brute force, and seeing how the force changed. [59:28]

Anyway, he had this idea that, he had measured the mechanical strength of carbon dioxide. Now, there are really only two possibilities for what that white cap is on Mars. It’s either solid carbon dioxide or solid water, solid ice. Because the atmosphere of Mars is largely carbon dioxide. And when it snows, what’s happening is that the atmosphere itself is freezing, not like our snowflakes which come from clouds. It’s as if the air was freezing, and, and dumping itself on the ground. Rather strange isn’t it?

Mm.

So... And the measurements had been made from satellites picking up the signals from carbon dioxide over these caps. So the suggestion was that they were made of carbon dioxide, and were white. Well Durham had made creep experiments and found that carbon, solid CO₂ is rather weak, certainly weaker than ice. And so he was saying to himself, well, if they’re as weak as that, why don’t they just collapse? And... But he didn’t know how to calculate it. And we had a chat at one of these meetings, and I said, ‘Well I think I know how to calculate, how to, how fast it will go, if it’s in a cake, sitting on, on rock. And went away and tried to do it. I think I had two goes at this actually. And, he and I collaborated, and we came to the conclusion that it couldn’t be CO₂, or they would have collapsed long ago. They wouldn’t be there. So what was happening was, that there was a thin frosting of CO₂ that happened from precipitation in the winter, and, underneath that was a great cake of ice. The carbon dioxide was merely superficial, and it wasn’t surprising that it was there because the atmosphere is made of carbon dioxide. And it was what they called the seasonal cap, because you could see it contracting and expanding as the seasons went on, and that was just the, the CO₂ patch doing that. [01:02:04]
Why could the, why could satellites or a remote sensing not see the ice, why could it only see...?

Because it wasn’t visible, it was underneath the CO\textsubscript{2}.

But when the CO\textsubscript{2} cap received it...

Yes.

Wasn’t there a margin...

You might, you might well ask, but, there are a lot of mysteries. I don’t know.

Mm.

And, the satellites, to make a satellite go over the Pole, is not the usual way they go. They make orbits which miss the Pole. So there wasn’t a lot of satellite coverage.

Mm.

They’re very strange ice caps. The northern ice cap has a great spiral rift going through it, and when I went to the first meeting, I remember saying that, ‘You know, I think the most interesting thing about this whole ice cap is that the spiral is right-handed. Why isn’t it left-handed?’ They didn’t think that was particularly interesting. [laughs] I said, ‘It must be something to do with the rotation of the planet. But, why, why does that make a right-hand spiral?’ And, there was a, I had a sort of shot at explaining why it might conceivably be so with a very far-fetched kind of model, but at least it gave a right-handed spiral. And, but, they weren’t interested in that, surprisingly.
[01:03:32]

But, there were a lot of interesting things about the caps. And really, the work that I and Durham published was associated very closely with topographical observations made by other means of, which showed the surface of the, of Mars, and showed a sort of hump on it where the ice cap was, and it was pretty clear that it was something
sitting on a, on a rock base, but who know what was actually inside it? And was it really a lump of ice, or not? Anyway, we stuck our necks out and published a paper saying that, it couldn’t be CO₂ because it would have collapsed. It must be H₂O.

[01:04:23]
*What was the response to that article, given that you were arguing that essentially there’s water on Mars in that way...*

Yes.

...*although frozen?*

Yes. Well, I think people accepted it. I mean there’s a paper here, I see, which may amuse you. We’re talking about 2000 aren’t we. It was a paper written as a result of that conference. It said, ‘The study that...’ Sorry, ‘The state and future of Mars polar science and exploration. Mars polar science special issue. S T Clifford and fifty-two others.’ I am one of the fifty-two others. You know in particle physics it’s quite common for a paper from CERN to have fifty-two authors, but in other areas of physics, it’s totally unheard of. And, this is unusual, in my, in the circles in which I move. Nearly all these papers are single author, by me, with a, some of them with two or possibly three authors. Fifty-two authors. Ridiculous.

[01:05:40]
*When you work on a problem such as this, which, you’ve got some, you’ve got those topographical measurements and you might have some satellite imagery, but other than that, you’re not doing field work or experimental work. And, a couple of times, and what you’ve just said about the Mars work, you said, ‘I had a couple of goes at this,’ or, ‘I had a bash at this.’ Now, if you were doing a kind of laboratory experiment, I could ask you, ‘So what did you do, what equipment did you use, what was the procedure?’ It’s slightly different here, because, I assume that you, you’re not setting up equipment, you’re not, that sort of thing. So... But could you take me through the kind of, what it actually means when you say, ‘I had a bash at it’? And both in terms of how long a bash is...*
Yes.

...what does it involve, and, I mean in, in sort of, very practical details like, I mean, do you wake up at, you know, what time do you get up, where do you go to do this bashing, and what are you using, what limited equipment, you know, was around you?

Yes. Yes. Well of course, one starts with what one knows. And, the whole theory of the Greenland ice sheet and Antarctica is based on what’s called mass balance, which roughly means that as much comes in as goes out, and the whole thing is in balance. It’s a balance between the snowfall and the outflow at the edges. And, that’s what glaciology is all about. A flow takes place in between the two, inflow and the outflow. But Mars is different, because there’s no precipitation to speak of. So it’s much more like having a, putting a pool of treacle on a plate, and watching it squash out without putting any more treacle on top. And, that’s a different problem. Actually, it had been, there had been a glaciological paper published on how you do that problem, and I had rather ignored it, because I said, ‘Well this just isn’t relevant to real glaciers which are fed from on top, and, and then lose material at the edges.’ So I didn’t pay any attention. But, when I saw the Mars, what was happening on Mars, I thought, oh that, of course that’s, our approach is wrong, we should go back to this other paper. And, and I used... Actually I think I did use it myself, but it, it was essentially that previous idea. And, and I applied that physics to it, and, and thereby came out with the, with the answer. I mean it is, it’s a question of timescales, how long would a CO$_2$ cap survive? And, if, if the answer is, 100 years, and even maybe 100 years ago astronomers were seeing white patches on Mars, I’m getting the timescales probably completely wrong, then you say, well they can’t be CO$_2$. But, it had been there a long time. And, that was the basis for the, for the prediction. Well not the prediction, the conclusion.

So one thing you will have done then is to get out the, the journal that had this paper in, and, look at that.

Looked at that, yes.
But, at this stage, let’s say this was the late Nineties, where would you be working on...?

In my room in the Physics Department.

OK. And was this post, pre or post retirement, sort of official retirement?

Oh definitely post.

Post.

Definitely post.

But you’ve retained a room there?

And, indeed I have, yes.

Would you, would you do any of this sort of work here, at home?

Well I’m working all the time. I mean, particularly in my bath, or gardening, or, lying in bed. If I have ideas I scribble them down, so that I won’t forget them. I do now, but I, I don’t think I used to.

So, you think about these things all the time. In fact I’m merely repeating Newton I think. He was asked, how do you, ‘How do you think of these things Mr Newton?’ And he said, ‘By thinking constantly upon them, Madam.’ And, I think that’s true. You just ponder. And I think, you need pondering time because, when you’re actually sitting at your desk, engaged in some calculation, you’re following the calculation, but it’s when you’re away from the desk, if you think, perhaps I don’t need to do this after all. I ought to be doing something quite different. But you have to leave the desk in order to have that thought. It works something like that for me. How it works for other people, I, I wouldn’t know.
And are you more likely to think about those sorts of things at certain times? You’ve mentioned, you mentioned, I know you mentioned bed, bath and garden, but...

[laughs] I think less in the garden actually, because, one’s attention is beautifully distracted in the garden.

I was just interested, because over lunch Georgiana said that when you were on the family holidays, they could all tell when you were thinking about something, about a sort of problem like that. So I wondered whether there were, there were times when...

Set times.

Yes. When it sort of, set times when you are able to think in that way, or, do you find that it can...

No, I don’t think so. It’s just, it’s in the background of your thoughts.

Mm.

I think, in the early hours of the morning in bed is, is often a very fruitful time, because you are detached.

Mm.

That’s my experience.

Detached from...?

Minutiae of questions. But, you can think of, totally different approaches perhaps.

Mm.

Or, something to follow up later. Something like that.
And when you are at your desk, actually doing the calculation, what does that practically involve?

What does it practically involve?

Well, what I’m thinking is, the most limited equipment for that would be a pencil and a piece of paper for example, but...

Frankly I don’t use a pencil. I always use a ballpoint. And my friend Michael Berry uses a proper fountain pen.

So you’re at your desk following a calculation.

Yes.

And you’ve got a ballpoint pen and a bit of paper. Anything else?

Well nowadays I use the computer, a lot. I mean, as I, as you know, I’m learning this program Mathematica, which does the algebra for me. And, whereas it used to consist of writing down symbols of algebra on a piece of paper, and then painstakingly looking through and finding out where you could have made the mistake, which was frequently a factor of two, or a factor of pi, or a plus or minus sign, a lot of time is spent by me anyway on checking that I’ve got it right. You always have to check your result somehow. Everybody makes mistakes. But, it’s important to know when you’ve made a mistake, and that’s what you have to do all the time. Because it’s so easy just to slip a, slip a sign somewhere. But, even with this Mathematica, it has to be the same thing. But I’m doing a lot more on the screen than I did on a, on pen and paper.

When did you first start to use a computer for this sort of thing? When did it first start to take over a bit from?
Well I think the, I think I mentioned that when the first computer mainframe as we, well not a mainframe, the first computer was delivered in Bristol, it was, it must have been 1960 or thereabouts. And I, I used it right from the beginning. But, as a routine tool, I have, of course it’s, it’s come in since one had personal computers. And, there was a time when you went to ‘the computer room’, and, communicated with the university mainframe from a terminal. And, before that, you actually walked to a certain building, the computer building, and punched your cards, and then came back later in the day to see whether it had got any result from them. And it told you that you had left out a semicolon. And you made another trip back and put the semicolon in and it was still wrong. And you did this many times. So, this is going back. Now going forward, you, you went to the computer room, and worked at a terminal and your, the machine that did the work was the central university computer, which then became shared between Bristol and Bath. And these mainframe machines became bigger and bigger. And then there was a revolution when the personal computer came in, and everybody could have a machine on their own desk, which was utterly different. Previously I had been competing with the students, with, to use the keyboards, and the, often the students had given them a bad time and the plugs had got rather worn out and you might have a bad one, and, and anyway the room was down in the basement of the building and I was up on the third floor, take all my notes down, and then find a machine that was vacant, and, do what I could. And then find it was time to give a lecture or do something else, and take all the notes back upstairs. It was a very tedious business. But having then a machine on one’s own desk was fantastic. I first...

[01:16:52]
You see word processors were coming in for the secretaries, and we used to get them electric typewriters which had special balls on them with mathematical symbols, and, they, they typed up the mathematics, very tediously for them. And then, then they started using word processors, and, the results would come out on, on printers I guess. And, it became much easier for them to make changes in manuscripts, cutting and pasting. When I was at AIDJEX for example, cutting and pasting was a literal process, with scissors and sticky tape. And you, a typical manuscript or typescript the secretary was working from would be a piece of sort of A4, or American sized paper which is different, all stuck together in pieces. And, she would work through this, and type away. And then you would make amendments and put arrows in, and, this bit’s
got to go here, and then, this chunk’s got to be replaced by something I’m writing now, and, it will give you, labelled ‘A’, and then another piece ‘B’ goes in somewhere else. And, ‘Please do it again.’ And, and they, they put up with this.

[01:18:25]

So that was the way that, that scientific papers were produced. And a secretary was, were doing one of my papers I think on a word processor, and I was, I’m rather finicky about putting in commas and full stops and so on in the right places and not getting it wrong, and, held back a little bit from making these amendments, because they were so trivial and meant that she had to, to do them. So I used to slink into her office after five o’clock when she had gone home and just put in the commas and full stops, or spaces, as I thought they ought to be. She, she finally found out about this, and didn’t like it.

[01:19:17]

But, I got a little more familiar with how to use a computer. I remember experimenting with this, moving the files around and, and leaving them in the wrong places, and she found out I had moved her files around on the screen. So, I think it was some time after that that I got myself a Mac. It was one of the, not one of the first Macs but it was a lovely little thing. It had the computer and the screen and everything all in one piece, with a little handle behind. It was very portable, and I bought it because I thought I could carry it from then lab home very easily. It was a lovely thing. And so I learnt then how to use a computer myself, and particularly to do word processing on it.

[01:20:13]

*Roughly when was that, do you think, when you had that very early Mac?*

Oh roughly when was that? Oh dear. I’m very bad about dates for that kind of thing. Let me see. I know when I was at AIDJEX, they were, they were doing this cutting and pasting, but there was a, for calculations, there was a, a communication to their main computer, which was, what was the telephone connection to a computer called? It’s...

*Is it...*
A modem.

Ah, OK.

It’s literally a cradle with a phone in it, and, and you put the phone in the modem and it translated what you were saying on your keyboard to the, to the main computer. That was, that was a modem, telephone line. But that was for doing calculations at AIDJEX. They didn’t have word processing there, the secretaries were typing it out. So that was in the Seventies, wasn’t it? So, I suppose in the Eighties, probably, the...

Let me see what papers I was publishing at the time, it will remind me. [pause] You can find all this out so easily just from the history of computing. [pause] But I think in the, in the Eighties we were probably getting the secretaries to type our papers on word processors.

[01:21:48]

There was a terrible period when journals were asking us to produce what they called camera-ready copy. Camera-ready copy was awful. You had to do the publishers’ work for them by being your own art editor, and get everything right so that their cameras just photographed what you had got and that’s what was published. It was a horrible system. It was, you know, it was, it was pre-Guttenberg. Guttenberg at least had the type in a pre-arranged line, whereas with, with this stuff, you could paste the letters anywhere, right way up or sideways or tilted or whatever. It was, it was a very poor intermediate stage in publishing, in my opinion.

Mm.

But it’s what we, what we had to do. And, camera-ready copy was horrible. But, nowadays people actually type their papers in what is virtually the journal’s style, which is going to be seen on the printed page. And, and even more, there won’t be any printed pages, it’ll just be e-published, only accessible through a computer. So, it’s gone through pretty quick stages of evolution.

Mm. So if in the 1980s the secretaries were typing your papers on word processors and you were sneaking in and getting discovered having moved files around...
Yes.

*Perhaps, you got your early Mac, at the end of the Eighties?*

The early... Oh the Mac.

*Mm. With the, with the carry handle.*

Yes, probably the Eighties. Could be the Nineties, but... I can’t see anything here that will remind me. I’m surprised by some of these dates, so I, I don’t rely on my memory for them. Yes, there’s one here that was definitely done by camera-ready copy. That was in 1979. So... Yah.

[01:24:08]

*So having, having got the Mac which you were able to carry between office and home...*

Yes.

*You started to write your own papers on, on the Mac rather than asking the secretaries to do so?*

Yes.

*And then, could you explain the sort of, the timing and the influence of Mathematica, or, or at least take the development of your... You’ve given a very good description of the way in which computing related to your work, especially publishing. What happened from the period of the Mac onwards?*

Yes. Well, can I say that the, the word processing on computers very much suited my style of composition, which consisted of writing down stuff as I thought it should be, and then realising that I needed to turn it round, and put whole passages, transfer them from one place to another. That’s the way I write. And, Word is precisely what you need for that style of composition. I’m always putting phrases in different orders, to
make them as clear as I can, and, and I think I’m rather conscious of, of what it sounds like. Sometimes, one order is much better than another, although they mean the same thing. And you can do that on a word processor so easily. So, so there was that period.

[01:25:40]

Then, then comes the question of, doing calculations. I learnt FORTRAN first of all on the first computers. It was, it wasn’t even called FORTRAN 1, it was just FORTRAN. Later it’s been greatly developed. Then there were, I had a, a big, a big change when I was recommended to use something called True BASIC. Now BASIC sounds like a pretty sort of, low level language, but True BASIC is entirely different. True BASIC is a very good computer program for, computer package application, for working out problems in physics. It’s highly flexible, you can make it, in principle you can make it do almost everything you want, provided you, you have enough steps in the, in the program. I didn’t use Mathematica, although my friend Michael Berry had already adopted Mathematica with great enthusiasm, because I found it rather inflexible. For example, if you, if you draw a graph in Mathematica, it does it for you in an instant, but if you say, but I don’t want the axes to be labelled in just that way, I want them to be labelled slightly differently, then you really have to work at it, to make it do what you want. I had it, for example, use, I was asked to write a, an article for an encyclopaedia on physical properties of crystals, and, since I wrote my book, it’s been possible to generate beautiful three-dimensional pictures on computers of, imagine a three-dimensional solid with shading on it, so you can see, well its shape, where it bulges out, by the lighting. Well you can do that now beautifully on a computer, but you need Mathematica to do that. And I got a, a very bright research student to generate these pictures for me from the equations which I gave him, and, which he did very willingly, and I said, ‘But I don’t want the, the numbers just there, I want them a little higher up.’ And he worked for ages to try and move the numbers a little bit higher up. That’s the sort of inflexibility that I perceived in Mathematica. And that’s why I didn’t adopt it as Mike Berry did immediately.

[01:28:36]

Now quite... And I... But I did have a try, I suppose, a couple of years ago I suppose, briefly, to use it, and again found it really quite hard to adapt to what I wanted. But now with this period where I’ve been immobilised, I thought this was a golden opportunity for really learning the, the thing. You know the, the volume of
instructions is that thick, it’s a really, really thick volume, and you don’t use it
because you’ve used the ‘Help’ on the machine. You just click on a command, and
press ‘Help’, and up comes the, all the instructions as to how to use that particular
command. It’s really rather beautiful. You know, Stephen Wolfram who invented it
was so enthusiastic about it that he regarded it as, as a revolution, comparable with the
Utsonian revolution. He was a bit taken to task I think in some of the reviews for
being so ambitious as that, but, it is a remarkable set of programs. But you do, it does
take a bit of time to learn, and I now, after this long time, feel that I can use it, use...
You only use a little corner of it you see for what you want to use, want to know, want
to do. The rest of it is for use by other people who are doing their problems, say,
statistics or, I don’t know, running the London Underground, or, whatever your job is.
It’s... Or doing a census.

Mm.

It manipulates lists very efficiently. Well I don’t deal with lists really, but that’s what
it’s all based on.

Mm.

But it, I’m beginning to use it now with more confidence and ability. I was spending
an inordinate amount of time trying to get the thing right. It’s very fussy about
brackets and semicolons and commas and, you’ve got to get it exactly right or it
won’t, won’t allow you to go on. Most computer languages are like that, but,
Mathematica is particularly vicious in... But, it tries to help. It puts up great things,
notices in red letters when you’re doing something wrong, tells you what you’re doing
wrong. If you can understand its messages, which are written in its own jargon, and
you then have to go to the ‘Help’ to find out what they mean. It’s, it’s that kind of a
language. Beautiful, but, but difficult.

[01:31:22]

Has it allowed you to pursue things that without it you wouldn’t have been able to
because of the speed at which it can work?
Not the speed, no. It does things very quickly, or the Mac does things quite quick
enough for me. Although there are problems where you can get it to work for ages.
No, it’s, it’s lovely really. I mean it, this work I’ve been in recently, currently doing,
involves a lot of algebra, which I wouldn’t undertake if I hadn’t got, if I couldn’t do it
on Mathematica. It would just be too tedious, and the checking of it would be just
ridiculous, it would be a total waste of time and effort. But, but you can make it do
the algebra. You’ve still got to check it in some way, you always have to check what
you’ve done in some way or another, but you can check it numerically or you can
check bits of it at a time, and sections of it. So it’s, it’s a lovely tool to use.

[01:32:27]

Thank you. Now, another question that arose at lunchtime was about the way in
which you generate new ideas for things to work on throughout your career. And, I
wonder whether you could talk about the, the way in which you tend to discover
something new that you want to work on, or something. For instance, we, we know
how your interest in glaciology started, because of the particular circumstances of
your research work at the Cavendish and the people around there. But throughout
your career, how have you, how have sort of changes in direction or new interests
tended to arise?

Well, one thing leads to another. And, nothing is really quite accidental. It’s often a
matter of connecting things together. [pause] I mean, you wouldn’t think that the
study of ice in the Arctic Ocean would have anything to do with optics, but the
connection is singularities, and, if you are interested in sea ice and you are also
interested in optics, then, if you are lucky you suddenly see that there’s a connection
between them. This catastrophe theory, have we talked about catastrophe theory?

A little, but not...

Well, it was, catastrophe theory is really the basis of all this work on optics. It’s quite
wrongly called, well not quite wrongly but, it’s called catastrophe theory but that can
be misleading. It’s to do with sudden changes. For example, if you, if you are in a
canoe and you stand up, you know what’s liable to happen. You capsize. The capsize
is a sudden event. And, catastrophe theory is a way of dealing with such sudden
changes. It’s a theory which engineers have known about for a long time, they call it bifurcation, and the, I think the first application, the engineering application, was to a strut called an Euler strut, the Euler we were talking about, which is just a, a slender strut, and you push, you put weight on it from the top. And of course, as you know, first it just takes the weight, and then suddenly it goes sideways and buckles. That’s a, that’s a catastrophe in this sense. And the engineers knew about this, and called it bifurcation theory.

Then, Thom and his co-workers and Arnold in Russia were working on mathematics of these singularities, which was miles from engineering. And Christopher Zeeman in particular at Warwick took up this idea and applied it in all sorts of contexts. For example, one of his favourite ones was prison riots, where tension builds up and builds up and then bang, there’s a riot. Or, aggression in dogs, which is a bit similar where the dog, two dogs facing one another, and then suddenly they decide they’re going to fight, or suddenly they decide to, to retreat. And that’s a sudden decision.

He applied it to anorexia nervosa, and said he could identify, he had been talking about it to a doctor, and he could identify, in the doctor’s descriptions he could identify geometry, which he reckoned made it analogous to the butterfly catastrophe. Which is a rather complicated one. Taking a simpler one like the cusp, what did he apply the cusp to? Well he made a machine which is called, it’s now called the Zeeman Machine, and, it’s made up of pulleys, a rotating disc, and pieces of elastic. And, you turn the disc, and then suddenly it decides to flip over. And that was a catastrophe, according to where you’re holding the elastic. And where you hold the end of the elastic, if you hold it within a certain curve, which looks like this, I mean take an astroid curve, if you are one side of the curve it’s all happy and stable, and if you’re on the other side of the curve it’s liable to flick over from one stable position to another.

So, he used catastrophe theory in such a variety of circumstances. Tree lines on mountains, going backwards and forwards. Crashes on the Hong Kong Stock Exchange. [laughs] And, there came a great, a great kind of, surge of contrary opinion, and there was a very serious article written by a guy in America who said, ‘Really this is all a load of nonsense,’ and, questioned a lot of these applications. But conspicuously did not mention either bifurcation theory in engineering or our work on
optics, which in my opinion is thoroughly sound, whereas the sociological applications are by their nature much more controversial but simply because of the difficulty in measuring things. In engineering, you have the, the elastic energy of a system, the potential energy of a system, and that’s a very definite thing that engineers calculate. And if you are, if you are in the bottom of what they call a potential well, then everything is stable. If you put a bead in the bottom of a well, it just goes like this; if you put the bead at the top of a mountain, it flops off on either side, that’s unstable. And, that’s the way engineers distinguish between stability and instability. So, engineers knew all about this, but the sociologists of course have difficulty measuring the right variables. When it’s aggression in dogs or prison riots, what do you actually measure? And so, Zeeman really had quite a, a controversy on his hands. And he did say I think at one point, ‘I think we oversold it.’ But, he was a very good mathematician, and, he popularised this whole idea of catastrophe theory, to the extent that it was the main topic in *Time* magazine at one point. But, there came this questioning which kind of put the whole thing under a cloud, because some of the applications were maybe a little bit doubtful.

*Mm.*

That’s the way I would put it. But there were other applications, two in particular, the engineering application and the optics application, which were absolutely sound, because in engineering there’s the potential, and in optics the corresponding thing is the path, the length of path, the ray path, which has to be minimised or maximised. And, that’s a, you can measure that, it’s a very definite quantity. So it’s mathematically perfectly sound, but that doesn’t go for all the other applications. Maybe some of them, not all of them.

[01:41:38]

*Mm. Mm, thank you. I want to ask a question about, which we touched on a bit before, about the, the motivation for studying the kind of materiality of the world around you. Now, the scientist that I interviewed recently would, would say and did say that his reason for studying materials is that it’s a way of uncovering the secrets of God’s creation if you like, which is one, which was one motivation for looking at particular materials and particular processes and attempting to describe how things*
work and how things related to each other. So that’s, that’s a kind of, that might be at one end of reasons for studying materials. I wonder whether you could talk about your own motivation, which I think is going to be very different from that, in coming to understand materials, the world’s materials, in particular light and ice but others as well. And, perhaps in relation to this, you could comment on what you talk about as mathematical beauty in these sorts of systems at the beginning of the book on natural focusing. You talk about math... Well, a kind of, aesthetic beauty in the caustics themselves, that’s on the one hand, but on the other hand beauty in the description of them mathematically. It’s quite a big question, so, you might want to break it up a bit, in the way you answer it.

It’s very difficult to answer. I’m just interested in things I get interested in. Why they’re those particular things is, is probably a mystery to me and to everybody, but, it’s partly perhaps a matter of upbringing. I mean, and I think I’ve mentioned to you, I, I always regret never having learnt any biology. So, I think I’m not so curious about the plants I grow in the garden, as I am about a phenomenon I might notice in the sky, in the clouds, or, the way the sunlight is reflected or something like that. So, it’s partly a matter of your background I think, or my background, and, which decides what I’m interested in and what I’m not so interested in but would like to be interested in. But apart from that, it’s, I think it’s actually pure self-indulgence and enjoyment. I, I really enjoy getting a result, getting an elegant result out in any field really, whether I’m making something with my hands or, or finding the solution to some equations. It’s just right.

Mm.

And that’s a great source of satisfaction. As for understanding nature, I find that a rather high-flown description of anything that I would do. Yes, I am interested in, in it. I mean I don’t think it’s anything whatever to do with creation by God, but, nature does exist, and it’s, and there are some very mysterious things about it. I mean the most mysterious thing, if we’re going to get very, very deep into this, is that it’s intelligible at all. I mean, why shouldn’t it be just total chaos? But for some reason things are unravellable, you can actually look at them and, and find they’re put together in what we perceive as a logical way. And, I mean that’s, that’s always been,
and maybe always will be, a tremendous mystery. It’s why science is possible. So what one’s doing really is, probing at little corners of this great mystery. I mean that’s a very high-flown way of putting it, but it is very mysterious in the deepest analysis. And that’s an entirely material analysis, nothing to do with morality or spirituality or anything of that kind. It’s just, it’s just very odd. [laughs]

Mm.

And, I suppose it, in a way these things are rather like glorified crossword puzzles, but a long step away from a crossword puzzle because a crossword puzzle is set by a human setter. Whereas the problems that I and my fellow scientists are working on are not set by anybody. You’re up against nature. And if you’re wrong, you’re wrong. And, I mean nature doesn’t just set problems, it just is, and, it’s consistent, that is, that’s the big thing, it’s consistent. And, you are trying to understand the nature of its consistency in certain corners that you happen to light upon. Why you light upon those corners, as I’ve just mentioned, is I think a, a matter of upbringing and experience and, so forth. Does that begin to give any sort of answer to your question?

[01:47:35]

Very much so, yes. And you... I asked earlier about the fact that you seem to be interested in sort of spots where systems sort of go wrong or aren’t quite right, or aren’t perfect dislocations and, and singularities. And, and you said not, because they’re... not because they’re wrong or mistakes or disordered, but because, even when you look into these mistakes or bits that seem to be sort of mis-programmed, they themselves have a kind of, internal structure that can be explained.

Mm. Mm.

Have you in any field that you’ve been working in found a, a dislocation of some kind, or a singularity of some kind, which, which doesn’t seem to have a kind of internal structure itself that you can explore, that is just wrong?

Doesn’t fit in to that pattern?
Yes.

The answer is, yes, probably. But as you were speaking, I was thinking of my mathematics lectures at Cambridge, where we were told about matrices, and, we were told that sometimes a matrix becomes what they call singular, which means that it somehow can’t be used in the usual way. And I used to think, well I don’t need to bother about that, because it hardly ever happens. And, and so I totally put it on one side. And it was only later that I discovered that these are the really interesting things about matrices, when they become singular in some way, there’s a particular technical sense in which they do. So I’ve learnt from thinking that odd things don’t matter, to realising that sometimes they’re the very essence of what does matter. Now, that’s a terrible generalisation, but, it comes from their experience in mathematics. And, and I suppose it’s, I think it’s purely accidental that I’m working on singularities, both in waves and in crystals. I mean I really think that’s just a, really a coincidence, but, of course they’re related because if I hadn’t known about singularities in crystals, I wouldn’t have discovered them in waves. But to that, only to that extent, that’s the kind of thing that, there’s a happy coincidence.

Mm.

I think it... You know, many scientific discoveries are made by accident. When Alexander Fleming went on holiday he left his Petri dishes with their covers off, and when he came back he noticed that one of them had grown a mould and there were no bacteria around it, and he discovered penicillin. That was just a mistake. [laughs]

Mm.

But I think a lot of, even in theoretical physics, I think a lot of things come just out of the blue, out of, by accident. But I really think perhaps it’s more than that, it’s that you’ve, you’ve exposed yourself to thinking about them all the time. But a lot of the things I do come from remarks by colleagues, made as I’ve mentioned I think at coffee time. A most important part of a lab.
There have certainly been key colleagues, yes. Well the, there’s a phenomenon in quantum mechanics called the Aharonov-Bohm effect, which was discovered by two people who were visiting Bristol, Aharonov and Bohm, and, I remember that being discussed in our coffee room. And I remember also, since it involved, involved sending a beam of electrons past a, you can think of it like a pillar, a magnetic field, and discussing what happens. Charles Frank said, ‘Well what about using a whisker of iron?’ Now, whiskers were things from solid state physics from crystal physics, which were known about at that time because they had become, they had been a phenomenon at Bell Labs, short-circuiting circuits, circuits, short-circuited themselves, because, the metal had grown whiskers and sent a current to the chassis of the, of the, way it shouldn’t have been. And they discovered them with tin actually, and tin whiskers were then investigated and found to be little single crystals with what’s called a screw dislocation in them. And Charles Frank knew all about that, and said, ‘Well why don’t we use a, a, an iron, an iron whisker?’ And Bob Chambers, who was standing around, knew about electron microscopes, said, ‘Well I’ll set it up if you like.’ And, thereby experimentally proved the Aharonov-Bohm effect, which is I might say the, I think it’s sometimes described as the only real advance in quantum mechanics since the Twenties. And, my colleague, Michael Berry, subsequently generalised it, and, showed that it was a very common phenomenon throughout the whole of quantum mechanics. So it’s now called the Berry phase. He gave a lecture in the Great Hall of Bristol University. I’m, I’m answering your question as to how ideas come.

Yes yes.

He gave a lecture in the hall of, Great Hall of the university in which he explained the Aharonov-Bohm effect, and what we now know as the Berry phase, but which he is modest enough to call the geometrical phase. You can’t, you can never call something after yourself. [laughs] And, my other colleague, John Hannay, was in the
audience, heard Michael Berry finish his lecture by saying, ‘Of course this is purely a quantum effect.’ Now John Hannay came back a week or two later and said, ‘You know, it isn’t; it occurs in classical mechanics too. It’s what’s called an anholonomic effect.’ Have I got the pronunciation right? And, he is a, John has now become an expert on anholonomies, which he will explain to you, occur all around you in everyday life. But he obviously had that idea when he was listening to Michael Berry’s lecture, it came to him just like that. Now this chap saying it’s just a quantum effect, but, why shouldn’t it happen on a big scale in, in classical physics? And immediately thought of a context in which it did. That idea came to him just like that. And, the experimental verification of the Aharonov-Bohm effect came to Bob Chambers just because the conversation was about that. And the iron whisker idea came to Charles Frank, because he knew about iron whiskers and could realise that they were useful in this context.

[01:56:00]

What are the everyday examples of the, of that effect in classical mechanics that...

Yes. That John Hannay...

Yes.

...did. Yes.

You said that he, if he told you about it, he’d say that these things happen all around us.

Yes. Well, I mean a good example, you will think this is trivial, is when you park a car. What you do is, you go through a series of motions, turn the steering wheel, and then you change the gears to reverse, and then you turn the steering wheel the other way and then you turn the steering wheel this way. You go through a, a cycle of motions, and the result of this cycle of motions is that the car hasn’t come back to the same place, as you might at first have thought, but it’s moved out from the curb. So, it’s a, it’s a, a series of cyclic motions which results in a, a net movement in that case. I mean that’s just a very simple example of it, but, there are many other cases. I’ll
give you a, I think I can give you another one that might perhaps be interesting.
Suppose you stood at the North Pole with an arrow in your hand, holding it above your head, like that, and you said, ‘I’m going to walk down to the Equator.’ And you walk down to the Equator, and you turned round, a right angle, and you walked along the Equator. And then having gone round the Equator for some distance, you turn round again and headed back to the North Pole. You will find that the arrow in your hand has actually, you’re facing a different way, when you get back to the North Pole. If you think it out, you will, it’ll, you will realise, but you have to think about it.

OK.

[01:58:01]
And, that’s an anholonomy. And the interesting thing about that one is that the angle that the arrow has turned by when you get back to the North Pole is related to the area of the spherical triangle that you have executed on the surface of the Earth. It’s a case of an angle in the plane being equal to a solid angle on a sphere, which is a most extraordinary thing when you think about it.

Mm.

That you can have an angle measured in a plane which is equal to what’s called a solid angle, I don’t know whether you’re familiar with the idea, but which is a three-dimensional angle. And, that is, that’s the kind of thing that they’re talking about.

[End of Track 15]
Could you tell me about the development of the work with the National Physical Laboratory?

Ah yes. I mentioned that, Jo Hajnal and I were given a, jointly a prize for, I’ve forgotten what the title was, it was sort of, measuring invention, something of that kind. And it involved not only money but also a contract with Bristol University for developing the work further. And I thought this over, and, Jo Hajnal had already decided to take jobs elsewhere to get away from Bristol. He had been a, I think he had been an undergraduate and a research student, and postdoc as well, and he thought it was time to seek new pastures. So, I thought it over and thought that the, really it would be much better if the work were done at the NPL than in Bristol, partly because, I had no experience really in electronics, and I would be totally reliant on the person who was actually going to do, physically do the work, and, I didn’t know who that would be. So, better done at the NPL. So the arrangement was that we engaged, Hygate, what was his Christian name? Gosh. John Hygate? I don’t know. I’ll get you his Christian name later. We engaged him, and he was officially a research assistant, paid by Bristol University, but doing his laboratory work at the NPL. Because the NPL had screened rooms, these are special rooms for doing experiments involving radio waves to make sure the radio waves don’t get out. Because the other people are rather concerned about that, it interferes with their radio sets and, and all their other things. So that they have these wire cages in which the experiments are done. So that’s the way, that’s the way it was done. And, and, Hygate...

MRS NYE: Graham Hygate.

Graham Hygate, was very successful in building apparatus to do this. In fact he did it extremely well. And, it was done within the relevant group at NPL which was a measurement group for, for electromagnetic waves. They’re, they were mostly concerned with things like standards of radio antennas, and standards of interference between one piece of apparatus and another. You know it’s a pretty important thing when you have a lot of black boxes, that one’s not going to interfere with another one. And there had to be stringent regulations laid down by the Government on what you
could and couldn’t do. They were responsible for all that sort of work. Anyway, it was, it was pretty successful. And, Graham Hygate found it not really to his liking in the way of management. He, he felt he wasn’t appreciated there, and indeed he was right I think. And, so he decided to go. So we were left then with the work not fully completed by any means, and, we needed somebody else. And I found it extremely hard to find anybody else. The job was advertised, I rather think twice.

So, we engaged him, and he came and worked. And he was probably as different from me as anybody you could imagine, I mean from the point of view of science. He, he was an engineer, and, he wasn’t at home at all with theoretical ideas or equations or anything like that, but he could put things together with his hands. And, with some trepidation we put him on the job. And he, he made these little gadgets and, and we, by that time we were realising that it was time to commission work from elsewhere, and it was necessary to go to a, we needed two people, we needed a solid state person to do the, the gadget itself, and we needed a, another half of the work was concerned with the, the wave aspect of the whole situation. So there were two quite separate things.

And, I got a little committee together in Bristol to try and sort some of these things out, including, managed to include people from our engineering lab, who were very helpful. And, my friend David Gibbs also was very helpful on these things. I wrote a, a series of reports to them, not because they asked for them, but because I thought I ought to get it down on paper. So they amounted to really quite a large number, I was just looking, I think about twenty at least, maybe thirty, of these reports, which were by no means your standard sort of government report, due every month or every quarter or whatever, but every now and again when we had reached, when we had done something, I thought I would write it down and make a report of it. Which I think is a much more sensible way of writing a report than the standard Civil Service way. They, they quite accepted this. Of course they didn’t read the report so it didn’t matter to them.

So the work went ahead, and, [mute], not being, not being a physicist really, used to ring me up from the NPL. And there was a time when I was on the telephone to him, literally every day, kind of directing the experiment at the NPL from my desk at
Bristol. And it worked surprisingly well. He sent me data by the, by email, and, files of data, and I worked on them, and, he would then do what I told him to do next. When he started, he, he got this device going, and he made it, made every conceivable measurement. He moved it around in all directions. And he came back to me with a great pile of graphs, which you can churn out from a machine. And, they were useless, because, he hadn’t interpreted them in any way. And I had to tell him that the proper way to do experiments was to get a result and then look at it, and then decide what to do next, but not just to do it over and over and over and over again, hoping something would pop out. And, he had a lot to learn. But, the result was that the whole project really was quite successful, and we, we worked on their horn, a standard Atlanta horn, which is a brass thing, like this, which is an antenna, and which was a standard which they kept very carefully in case you dented it in any way, because it was a standard horn with a standard gain. And we measured the gain in a, by putting our little probe actually inside the horn. It’s like measuring the, imagine a musical instrument, and putting a microphone actually down the spout of the trumpet, or cornet, whatever it is, and seeing what’s going on actually inside the pipe, rather than just listening to what the sound is outside.

[09:00]
And I for the first time thought, physically, what’s really going on in this? What’s going on for example with an open organ pipe, where you have a pipe which may be closed at the end and you have these waves inside which you’re taught about at school, standing waves, or, and you open the end, then something different happens. But what’s happening at the open end? Why is it... It’s just an open end, why is it reflecting the, the sound back in any way at all? Why doesn’t it just come out? And, those were the sort of questions. And I had a theory about this, how it worked, based on what’s called edge diffraction, that the waves come along and they encounter the edge of the horn, and then they’re scattered in all directions, and, some of them are scattered back into the horn, and it just looks as if the wave is being reflected back into the horn. And that’s why, when you have one of these horns, the gain as it’s called, the power that it’s radiating, fluctuates with frequency, because of the organ-like, pipe-like wave inside the horn. And I worked all this out to the extent that, it is possible, in fact the question was asked at one of the meetings, is it possible we don’t need a standard horn any more, because we can work it out theoretically?
Mhm.

[10:23] So I think, I think that was very successful. Another little thing that he did, for example, was, there are things called patch antennas, which you find on the sides of aircraft, they’re just planar antennas, no wires around, just patches. And, we were able to measure the field extremely close to these antennas, which is not something you can normally do, and map it out. And, found to our surprise, or to the engineers’ surprise, that the power was not coming from the antenna at all, as it was designed to do, but it was coming from the connection, from the bends in the connections to the antennas. When a signal was coming along a wire, and it encounters a right-angle bend, it radiates off radiation. And it was, it was all to do with how the thing was wired up behind, that was what was radiating, and not the patches that they had designed should send the waves out. So, it was that kind of thing was, I think, really quite successful.

[Closed between 11:29 – 13:25]

[13:26] And, finally the, the time came when [mute] was taken over from Bristol by the NPL, that’s to say, they, instead of being paid on this grant, they took him on to their permanent staff, which was a slight surprise, but they did. And they did it in a very, very poor way. They, they, to suit them, they wanted to terminate his contract in, I think it was June or something like that, and take him on. And he was with us until October, or thereabouts, and, just peremptorily they, they decided to, to take him on. And, this had all kinds of implications which resulted in a meeting between me and the NPL in which I had to produce the letter and the contract showing when his contract came to an end, accompanied by a letter I got from our finance and personnel department saying that they couldn’t possibly terminate his contract unilaterally, they would be, they would have been sued, they could have been sued for unfair dismissal and all sorts of things like that. And the NPL people were, you know, totally mindless in, in a case like that. It was, it was a very poor style of management. It was bullying in fact.

[Closed between 15:01 – 15:21] So, [mute] eventually, he had, he had done pretty well actually in getting a PhD from Manchester, and then getting a permanent job at the NPL, but he decided to go back
to Beijing. He was... You see he did things by the seat of his pants. He, he, he wouldn’t use an equation or... He would do it entirely differently from me. He would make something that worked. He had something in his fingers which, which did it. But, of course you make terrible mistakes doing that sort of thing, and, and hoping that Nye would somehow manage to sort it all out. But, he eventually decided to go back to Beijing, where, so far as I know, he still is, and, he’s gone from strength to strength. Instead of working on his own and trying to get this idea adopted commercially, he’s got about ten people working away for him up there.

To get what idea adopted?

The optically modulated scatterer, to get it generally used around institutes that want to measure electromagnetic fields accurately, point by point, and go as close as they like to the, to metal. You can’t do that with a, an ordinary microphone. You must at all costs keep it away from metallic things, because they interfere with its operation. With this little bead, you can go almost, you can go within a fraction of a wavelength of even a metallic plate and still measure the field.

[17:17]

Had the design of the little bead gadget changed from the first...

Oh it did change.

...model to when he left to go back to China?

Oh, it became much smaller, and much more efficient, much more sensitive. Jo Hajnal just picked, went down to the stores and picked up what was available, and, made use of it, when he was able to design the thing right from scratch, and it was a matter of designing a very fast electronic switch, and, making it very small at the same time. And we commissioned, was it Ferranti, I think, was it Ferranti? to make the thing for us. So it became, it became optimised. That was the whole project really, to optimise the, the device, and to make it generally usable.

[18:21]
And I, I think that you may have used this device with a, a black screen with slits at some point.

Oh yes. That’s right, yes. Yes, I mean, I thought that the interesting thing to do with this would be to use it for some fundamental physical experiments, not for example for detecting the fields around a mobile phone, but to take a classical problem, and instead of attacking it purely theoretically, actually to measure it and see whether it agreed with the solution. There’s a famous solution by Sommerfeld for what happens if you have a, a metal plate with an edge, and you shine a wave at it, and it’s an exact solution of the, of Maxwell’s equations, which are the equations which govern all these phenomena. And we actually set this up and measured what happened, and of course naturally it agreed with what’s predicted.

[19:27]

But, a more interesting thing was to use it not in front of a metal plate, but what’s called a black screen. Now a black screen is something that they talk about in the physics books, without quite explaining what it is. It’s a, it’s a screen which is infinitely thin, and absorbs the waves that hit it. It doesn’t reflect anything, and it’s very thin. Now, it’s been explained by, it’s actually in standard textbooks, that this is impossible. You can’t have such a thing. Because, anything that’s going to, to absorb, has to be a certain thickness. It’s got to absorb it so that the wave dies down within the material. And that means it can’t be very thin, and nor can it be just a plane, a thin plane, because that would reflect the, the radiation. And indeed, almost anything is a perfect reflector if you, if you use it at glancing incidence; even a road outside, you’ve probably noticed, that, if you have lights very very low down on the road, the road is a reflector. So everything, it’s like a mirror, when it’s a glancing incidence. So that the whole idea of a, a black screen is something that’s a bit, questionable, and yet the standard diffraction theories say, let there be a black screen which does these things, and then the diffraction will be as, as then computed, calculated. The result was, that there were actually two different, two rival theories as to what would happen if you really did have a, a black screen, one due to a Victorian physicist, Kirchhoff, with two h’s, and the other due to an equally well-known scientist, Sommerfeld. And they, they had different results, different formulae. So we thought it would be interesting to set it up and see which one was right, if either.
Which we did. And it turned out that Sommerfeld actually was better than Kirchhoff, although Kirchhoff is the generally known about theory.

[22:06]

*What did you set up in order to test that exactly?*

Well we had to set up a black screen, and as I say, a perfect one is impossible.

*Mm.*

So we, we went to, I think it was Malvern, which is the, used to be the Royal Radar Establishment, where they know all about, all about absorbers. Because, in defence, if you want to make an aeroplane invisible to radar, you cover it with absorber, so it won’t reflect anything. And they gave us some very expensive material, it was about that thick. Oh it’s...

*A centimetre, under a centimetre, eight millimetres?*

Oh it was a few millimetres thick.

*Mm.*

And, we made a screen out of that. It was so expensive that we couldn’t make the whole screen out of that, we had to sort of patch it out at distances with, with a less expensive absorber. Well we did this thing which is, is loaded, it’s, it’s not a perfect material, it’s loaded with carbon particles, so it’s a composite material, and therefore, more difficult to calculate about if you want to. So it violates the, it’s beginning to violate the general principle that I mentioned. But it’s as close physically as you can get to what’s, to the theoretical black screen. And we used that.

*And, and put the... using the, the bead...*

Using the bead, measuring extremely close to the edge of the screen. And especially looking on the illuminated side of the screen, where it’s supposed not to reflect
anything, and seeing what happened there. And moving the little probe around and making a map of what the field was. And I collaborated with my friend John Hannay on the theory of this, and he was very smart about it indeed, and contributed a great deal to the theory of this and we wrote a joint paper called ‘Diffraction at the Edge of a Black Screen’ or something like... a black half-plane is the official title I think. And that was the most interesting work for me anyway.

*And what was the source for that, was it the, the National...*

Source of waves?

*Yes. Was it the standard horn of the National Physical Laboratory, or was it the, the Bristol set-up with the...?*

It was actually a, a standard horn I think.

*Mm.*

Didn’t really matter. I mean it didn’t matter where it was coming from, but, it was coming

*Mm. And presumably you, you didn’t find that everything was absorbed; there was a, a reflection there?*

Oh no, no, in fact the manufacturer’s specification was wildly optimistic. It was reflecting far more than the manufacturer said. I don’t know how they measured it, but, it’s difficult.

[24:59] The experiments in Bristol, there was a point where Jo Hajnal came to me and said, ‘I’m getting very strange results, I really don’t understand what’s going on here. Look, it’s all, it’s not what we expect.’ And I thought about it, and I said, ‘Well, I think it’s the, you are getting waves reflected from the wall of the lab.’ And he said, ‘Oh yes, of course, he would have thought about that himself, in a given, a very short amount of time.’ So we had to somehow get rid of that. And, I think I wrote to
Malvern asking if they could, we could have some absorber. We couldn’t afford to buy any because it’s so expensive. And they lent us some. But before they lent us any, Jo had made a rather marvellous horn out of cardboard which ducted the surplus microwaves out through the window. Now before you can duct them out of the window, of course you can’t really leave the window open all the time, so he had to cut a circle out of the glass. And that needed the bursar’s approval, because it was mutilating the building. That took as long a time as it took for, to get the absorber from Malvern. But, we used the absorber in the end.

[26:17]

_Were Malvern interested in your device as a way of developing absorption for...?_

No they knew all about it because they were part of the NPL by that time.

_Mm._

And in fact the director of the relevant unit at NPL came and displaced Ray Clark, I think, Ray Cooke.

_Mm._

I think. But, I struggled to get this device adopted commercially and I, I actually wrote to the corresponding lab in Denmark, which does these things. I told the NPL I was doing this of course. And wrote a paper frankly saying that, I hoped that this idea would be taken up by industry, but, nothing’s happened. It’s a great disappointment.

_No interest whatsoever?_

No interest whatsoever. I’ve sent, I’ve written to a number of possible establishments, but no.

_Who have you written to?_
Oh dear. Well the Danish one was the, was the best hope. There was one in America, and, and I’ve just talked about it at various places. And said to the audience, ‘You know, if there’s any... This is, this is free, you know, I haven’t, it’s not patented.’ So there it is. But it hasn’t been taken up. It’s, it’s a bit disappointment to me.

*And was it ever used at the NPL to measure the radiation around a mobile phone, or that sort of...?*

One of the difficulties about using it for a mobile phone was, that, it involved taking a, a, a lead from the apparatus and putting it into the mobile phone, where the, where the signal was being generated. And if you go back to the mobile phone people and say, ‘Look this is bad, this is doing bad things to the head of the person telephoning,’ they’ll say, ‘Ah but you’ve mutilated our phone. It isn’t as we gave it to you, it’s got a hole in it.’ So they, they couldn’t actually literally do the experiment that I would have thought would measure the, what they wanted, with a mobile phone that is. But the one in the group was making frequent visits to the hospital here, where they were actually doing experiments with mobile phones and patients and, and phantom heads, to simulate the human head and, and that sort of thing. But no evidence has been found, I believe, that they’re in any way dangerous, but of course they had to be sure.

[29:15]

*Could I ask you a question now about your archives, about unpublished material that you have, places where you’ve already deposited it, and places where you intend to, that sort of thing, including some sense of the amount of each category of material, one category of which might be photographs, another might be papers, and handwritten calculations, that sort of thing.*

Yes. I mean, if there’s anything worth preserving, it’s already been published, really, that’s the answer. I have written for the Royal Society, as you know, a, a sort of, notes for my, anybody who’s going to write the biographical memoir, which in my experience is a great help to the, to the writer, because otherwise they have to start from scratch. So that’s, that’s already deposited at the Royal Society. I know that Charles Frank’s papers have been deposited in our university library, which has an archive in the basement, and I know that Cecil Powell’s stuff is also there, together...
with Brunel’s notebooks. They’re all stored in specially air-conditioned, great sort of cylinders, this size, with great wheels on the end, rather like a morgue I suppose. And the stuff is all in there. So if anything needed to be preserved, that’s the place I think to put it, the university library. But I can’t imagine anybody would want to preserve anything that I may leave behind, that hasn’t been published.

[31:02]

*What do you have in terms of unpublished... whether you think it’s worth preserving or not, what do you have?*

Unpublished? Well nothing really. I mean I’m planning shortly to go to the lab and prune my filing cabinets with Georgiana’s help, and throw out as much as I can, so that I can liberate the room. But, there’ll be a certain amount left, but, I doubt whether anything except photographs will be of interest. Photographs are special, because, they’re kind of unique. And you’re interested in some of these photographs for example.

*Mm.*

So, I think what I’ll do is, chuck out everything I can, but I’ll certainly preserve photographs. And, as for notes made in writing papers, I mean, I really think they’re, they’re not of interest to anybody except the author. I’ve kept, in some cases I’ve kept a few notes of that sort. But there are a number of problems I’ve worked on which haven’t come to any sort of conclusion or fruition. I suppose that’s natural, normal. Quit a lot of problems of that kind, I might just keep those.

*So this is a decision you are going to make when you look at the filing cabinet?*

Yes, I think so. Yes.

[32:24]

*And what does... And so that contains notes does it, on, on projects?*
Well it’s bound to contain stuff that I’ve worked on which I haven’t published, because it’s not worth publishing. I can’t imagine therefore that it’ll be of any interest to anybody. Otherwise I would have published it. So, I think it’s a rather academic question.

*And, correspondence, that sort of thing?*

Well there’s a certain amount of correspondence I suppose with, with well-known people, but it’s not a tremendous interest. I’ve put aside various letters I think that I’ve received, they’re the sort of key letters, like letters of appointment to my positions, that sort of thing. But I don’t think there’s anything of, of enormous value or interest there. I’m just... I can’t think of a single letter frankly that would be of any interest to anybody except myself.

[33:24]

*And, you said that you would retain the photographs, because they’re unique.*

I think so, yes.

*Having retained them, have you any sense of where you would want those to be archived, if at all?*

Well I think the library at, the university library is the obvious place. I’ve got a whole drawer full of old-fashioned glass slides, for old-fashioned lantern slides, you know, the 3¼ x 3¼.

*Mhm.*

And, I think the ones of the bubble model would be worth preserving.

*Yes.*

And I’ve also got the original negatives, quarter-plates, of the bubble model. Not... I mean, there were masses of course, and I’ve... but I’ve got a box of the ones which
were published, and, and the ones which are any good. You don’t want to keep the over-exposed and the ruined ones, you just, just the ones that were, that were worth keeping. I’ve got those in a box. I think those would be worth preserving.

And you think probably the university library?

I would think so, yes.

Thank you.

Do you have any other suggestions?

Well, partly asking because the British Library is interested in collecting material in relation to interviews, so, if you...

Yes.

If... Well that’s another option, for this sort of thing.

A third option is the Bath outfit that I mentioned to you last time.

Mm.

They’ve got, they do archive, I think.

[34:50]

And, the final question is, could you comment on your experience of being interviewed for National Live Stories? Which can include things that you felt about it outside of the interviews themselves, and... Yes.

Well I was surprised. I was surprised by the amount of detail that you were interested in, and by the length. I mean when you first suggested a day or perhaps another day, I thought, well that’s a rather long time. And it’s turned out to be three days. So, I think that’s my principal reaction, how much detail you were interested in. But, apart
from that, I found it a most interesting experience. Everybody likes talking about
themselves, and you don’t often get the opportunity. In fact never. [laughs] So, sort
of summing things up in this way has been an interesting experience for me, and I,
I’m grateful for it.

Thank you very much.

[End of Track 16]

[End of Interview]