Cyril Hilsum CBE

Interviewed by

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17th January 2020

At the

BCS London Office,
25 Copthall Avenue, London, EC2
Kindly provided by BCS – The Chartered Institute for IT

Welcome to the Archives of Information Technology. It's the 17th of January 2020, and we are in London, at the British Computer Society. I am Elisabetta Mori, an interviewer with Archives of IT.

[00:14]

Today I'll be talking to Cyril Hilsum. Cyril Hilsum is a British physicist and material scientist, who carried out research on infrared devices and semiconductors, and played a key role in the development of flat-panel liquid crystal displays. He was awarded numerous prizes from several institutions. He was elected a Fellow of the Royal Academy of Engineering, a Fellow of the Royal Society, and an Honorary Member of the American National Academy of Engineering. He was awarded the Max Born Prize in 1987, and the Faraday Medal in 1988. He was awarded the CBE in 1990 for services to the electrical and electronics industry. In 2007 he was awarded the Royal Society's Royal Medal for his many outstanding contributions and for continuing to use his prodigious talents on behalf of industry, government and academia to this day. Welcome Cyril.

Yes, a bit difficult to live up to that isn't it.

[01:21]

Let's start with, where and when were you born?

I was born on the 17th of May 1925, in the East End of London, and, basically lived there for many years until the outbreak of war.

Can you describe yourself as a child?

Well, I was in a very poor family of course. My father was a street trader, I had two brothers, and we didn't have much to bless our lives with. But, it was a happy childhood. I distinguished myself quite often by taking what toys we were given to pieces and trying to work out how they worked, and then trying to assemble them, usually unsuccessfully. So I could say that, that's how I began my engineering life.

[02:19]

Who were the important influences on you in your early life?

Well I suppose it was this feeling that, life offered more than the East End had to provide, and that I needed to work hard and break myself away from this. It was very clear that, I was very quick at school, and, people knew that I was showing every signs of making progress. So, that I think is how it all happened.

Was there someone in particular that inspired you?

Well, I think I have to say that, in those days, things were much more flexible. I learnt to read before I was three properly through my elder brother, who was two and a half years older than me. And, I probably was a bit of a nuisance at home. So, my parents, who were pretty busy trying to earn a living in street trading, arranged for me to go to the elementary school when I was three. I don't know that I learnt a lot in the first few years, but it led to my progressing through the school, which had an infants part and the more normal part until eleven, and, it was there that I met the teacher of the senior class, the ten- to eleven-year-olds, and I was promoted to his class quite young. And his name was George Bennett, was a great influence on me, because he realised that I, and in fact a friend of mine, Jack Randall, who also made quite a lot of progress, would benefit from being allowed to work at our own speed. So he put us at the back of the class, so we weren't answering all the questions and inhibiting the, the actual advances of our fellow pupils. So Jack and I more or less worked on our own with Mr Bennett coming to see that we were actually working. And he wrote a nice report to my parents saying that, I had to be essentially encouraged to do my own thing and work ahead.
[04:48]
So, this was a, a fairly happy childhood. And, then I progressed. We all had to take an examination at eleven to leave the school, it was called the Eleven Plus, and there was very little doubt I was going to get that, and Jack as well, my friend. So we went to the secondary school, which was a grammar school, Raine's grammar school, which still exists, though it's under a different management now. And, I progressed through there pretty well for the first few years, well the first couple of years I suppose, until there was an event which, obviously everybody knows about, in 1939, when I was fourteen, and suddenly was extracted from London and first put, with of course the school as a whole, down to Brighton, until somebody realised that they had

actually evacuated us to nearer the enemy, and that this was not very sensible. So they then re-evacuated us, and this time they put us halfway between the Sandhurst military college and the Royal Aircraft Establishment at Farnborough. We were in Camberley, halfway between, so it's not surprising that at one stage I was bombed. It missed me, I happen to say. But again, it was reasonably happy there, and we were able to work. Again, we worked more or less on our own, advanced to the normal, first of all fourth, fifth and sixth forms, until we came to taking scholarships, and there I was able to win a scholarship to University College London.

[06:48]
What year was this?

The scholarship I won actually got me in UCL in 1943. And, it wasn't a kind of normal situation, because of course the war was still on, and most of the universities, as far as I know, were evacuated. UCL was evacuated in different bits, but physics was sent to Bangor in North Wales, which is where I spent most of the time for my degree. We didn't come back to London until the end of '44, beginning of '45. And, again, it was an interesting time being at university, and of course, learning a lot of physics.

Why did you choose physics?

I really, originally was interested in maths. I liked the logic of maths, and, I was happy with doing early maths at elementary school, and then, trying a lot more advanced maths. But, when I came up to UCL originally, I met Orson Wood, and he asked me what I wanted to do, and I said, 'Well I want to do physics for two years, get a degree, and then do a degree in maths lasting one year.' And he said, 'You can't. That's impossible.' I said, 'Well, you're allowed to by the regulations.' And he said, 'You may have looked at the regulations, but it's not practical. You'll just do honours physics with maths as a subsidiary.' So that's what I did. And, and that was very successful. I finished up with a good degree, and a continuing interest in both physics and maths.

[08:50]

And what did you do after your graduation?

Well, I have to explain what the situation was like. Remember, I graduated now in '45, it was a two-year degree, and, there were still restrictions. In fact, I wouldn't have been allowed to stay on if my birthday had been earlier in the year, because somewhat arbitrarily, they divided the annual population into two parts. If you were born before 1st of January in the academic year then you were called up. And in fact my friend Jack, who I mentioned earlier, he actually was called up, his birthday I think was November. And he became a petty radar officer, which is what would have happened to me. But in fact, because I was born in May, I was allowed to go to university as I have explained. Now that didn't mean to say that I was free once I graduated, because there was a system whereby you actually had to work after you graduated, either in a government establishment, a defence research establishment, or for a company which was working on defence. That was very sensible. Now, when I graduated it was clear that the situation in Europe was changing, and fairly obviously the war was going to stop in Europe before very long. So, UCL wanted me to stay on and become the first PhD student they had after the war. But when I put this to the board that was making the decision, they weren't very thrilled with this idea, and said that, I would definitely have to work for a government defence establishment. And originally, I was supposed to be working at the Admiralty Signal Establishment at Haslemere, but I got a letter later on saying that I had been reassigned, and instead I would be working at Fanum House in infrared administration.

Where was Fanum House?

That was in Leicester Square in the centre of London, which was quite convenient, and in fact it was extremely convenient for me, because, my fiancée I had met, Betty, my future wife, who was studying French at Bangor, and we knew we were a couple by then, and she was on a three-year degree for arts. It was different then for female students, they weren't called up at all. So they could stay on doing arts. Men weren't allowed to stay on and do arts subjects if they were medically fit. So my being in London meant I could actually see Betty quite often, and actually I could also attend some courses at UCL, which I did, I did a course in crystallography I remember. It was very interesting, and I did some advanced maths courses as well. So I didn't actually waste my time in London. So there I was at Fanum House.

[12:13]

And, when did you become interested in infrared?

Well that was my job basically. Essentially the group at Fanum House was the administrative centre of several government research laboratories which were doing work on infrared devices for the forces. We were doing some, in Britain we were doing some quite advanced infrared research to make devices, particularly in the near-infrared. It was my job to actually read all the reports that were coming out from the USA, who had an immense infrared programme, and to communicate anything that I thought interesting to our scientific labs working on infrared. I soon appreciated that that would probably not be appreciated. So I did the reading but not so much of the communication. But I did get to know the people at the two main establishments, Haslemere where there was an infrared group, and at Teddington. And, being in a position where I was reading all these reports, I did learn a great deal about infrared, and in fact, I got very interested in infrared transmission through the atmosphere, which came in useful later on. And I wrote a very long report while I was there, 100 pages or more, on the infrared transmission of the atmosphere.

Do you still have the report?

No unfortunately I don't. I remember the report. I think it was called ARL/R2/E-300. But I, not very sensibly, loaned it to, loaned the only copy I had to somebody, and I discovered a few years ago he had never returned it. But I still remember some of what was in it.

[14:13]

And when did you move to Teddington?

Well, of course, I was in London, and circulating, and, the senior officer noticed that I was spending a fair amount of time in some of the back streets of London, studying the life there as you might say. And he thought that wasn't a good influence on a young graduate. So, he suggested that I actually moved permanently to Teddington instead of just going there occasionally. This was reasonably convenient. We had just got married in ’47, so, we were living in Wimbledon and it was relatively easy to do a commute to Teddington. So I joined the infrared group in Teddington. Now most people won’t appreciate there was an Admiralty Research Laboratory in Teddington, but, there was for many years; there no longer is, as you will hear later on. But it meant that, I also was able to take advantage of another laboratory which was just across the fence, the National Physical Laboratory, which was doing some interesting research. So I got to know some of the people there quite well. And our sports facilities were also united, we were one of the divisions that took part in their sports day.

[15:39]
And did you meet Alan Turing there?

Yes I did. I didn’t really regard it with any great significance then, but I realised that it was significant later on. I played him at chess. He was in the chess team of the physics department I think, it may have been a computing department. No, I have a feeling it was the physics department. And I was on the board number one in the ARL team. And I played him, and I don’t think his mind could have been completely on chess at the time because I was able to beat him. So it’s one of my claims to fame, that I beat Alan Turing at chess. I may say that I didn’t learn a great deal about what they were doing, but I did learn a bit from him and the head of the section, whose name was Fox, who later became a professor. But they were doing some pretty advanced devices, particularly in memories and things, which has not always been fully appreciated. And I never thought that NPL makes enough of the fact that Alan Turing did work there on computing. You know a lot about him being in Manchester, but not so much about him being at Teddington. And, NPL was doing some pretty advanced work on computing for the, at least for the ten years after 1945.

[17:05]
And then, your group moved to Baldock. Why did you do that?

Well, in one of the moves that the Civil Service often does, and probably still does as far as I know, was simplifying administrative arrangements. And the Admiralty discovered that they had two laboratories working on infrared. They had a team at Teddington, an infrared group, and a group, not, more or less the same size working at the electronics laboratory in Baldock, which was an inter-service laboratory working on devices, electronic devices generally. And they had an infrared imaging group there. And it was thought it would be more sensible if the two groups were combined, and they weren't very sure what they were going to do long-term with ARL Teddington, because again, this was a pretty expensive site, and, there were various reasons why they didn't want to continue there. So the whole group moved to Baldock in 1950, and I went along with them. Now that was interesting time, and I obviously was happy to work on infrared photocells, we were working on lead sulphide, and lead selenide photocells. But then there came quite a change in my life, because, first, the group head was promoted to a London post, and then his deputy was in line for a PhD course, and he left too. And the director of SERL then realised that I was now the senior scientist working there, apart from two Polish scientists, who were quite advanced and quite, well they were in their fifties, were there, and he didn't want them to run the group, so, he asked me to run the group. So at the advanced age of 25 I found I was not just running the group but also in principle was running the infrared programme for the Admiralty, which involved contract research at various establishments. And, unusually I suppose these days, nobody trained me in management at all, I just had to learn by making mistakes, and hopefully somebody pointing them out to me, because I never realised them myself. So that's where I was in about 1952, '53.

[20:05]

During this time you visited the EDSAC and Maurice Wilkes in Cambridge?

Well that comes a little bit later. Because, I began to work, as you will hear, on various semiconductors. I continued working on infrared, and then I began to work on new infrared photocells, which got me interested in semiconductors like indium antimonide, there aren't many semiconductors like indium antimonide. And, that's how I got interested in, in semiconductors. And, then it so happened that we, one of the people in the group had come up with an idea for a new converter of infrared radiation, invisible radiation, based on the absorption edge shifts in a material called amorphous selenium. And I got interested in that, and started doing a fundamental study of how thermal image converters worked from the transmission through the atmosphere. I didn't say that one of the things I had done at Teddington was join the group working on infrared atmospheric transmission, and we did quite a lot of fundamental research on that, and established it and published a paper in the *Proceedings of the Royal Society,* which still is occasionally quoted, many many years later. So I was still interested in infrared transmission and how it all worked. And I wrote a series of papers on how a device like this worked, including the absorption in metal films, and, transmission through the atmosphere as I said with absorption edge shifts, what makes an absorption edge. And all of this was reasonably good, fundamental material, so I was able to publish it.

[22:06]
Where and when did you get your PhD?

Well that was one of the results of doing this fundamental research, that, essentially I had a series of papers, and in those days you could take an external PhD. You still had to register with the university. And, I had no idea whether my papers were good enough for a PhD, but I thought this was a good idea. So one day I went to see my old tutor, who, Dudley Orson Wood, who had been very helpful to me in my degree time at Bangor, UCL at Bangor, and, I took my papers to him, and said, 'I'm thinking of doing a PhD. This is my basis. Do you think this is the basis?' And he looked at the papers, and he said, 'It's more than a basis. All you need do is bind these together, and you've got a PhD thesis, with perhaps a linking thread. And, I will tutor you for your PhD.' So that's how it happened. And, essentially, I was able to show that this was probably not going to be a sensitive enough device to actually be used. And I had migrated to work then on photocells generally, and indium antimonide in particular. But by the late Fifties I had had to change what I was doing, well the whole group had to change what they were doing, because the infrared imaging device which that group at Baldock had originally been working on had clearly no future, we had to stop it, and instead, it had been decided that the whole group would move over to semiconductors generally. And part of the group, not my part originally, would work on a material called gallium arsenide, which of course is very common these days, and you will all know about that, but in those days it was pretty unusual. And, two of the people who had been working on it, and it was a really difficult material to actually work on, but by 1960 they had decided that they wanted a change, and I was asked if I would take over the leadership of the research on gallium arsenide. Which was quite lucky for me, because in fact the two people who had been responsible had actually broken most of the problems and more or less had the basis for progress. So I was able to benefit greatly by what they had done, and actually start making progress on the manufacture and use of gallium arsenide.

[24:57]

Now one of the things I had done, which had helped me considerably later on, was to write a book on semiconductor III-V compounds with one of my colleagues, Christopher Rose-Innes, who is still a friend of mine. He later on went to Manchester and became a well-known professor in physics. His chosen topic was superconductivity, which we had a small group working on at Baldock. But he agreed that he would collaborate with me on writing this book, and, we wrote this book, which was in a series from Pergamon Press, which was organised by Captain Maxwell, whose name will be familiar to most people. So I met him, and, that was quite interesting. And, began to, we began to get known as really the home of high temperature superconductors. And we called our programme the Red Hot Transistor, which went down pretty well, though there was never actually any red hot transistor or anything other than a pretty warm transistor, it took a long time before we actually had any kind of transistor, and it certainly wasn't that well known.

[26:13]

And during that period I had a contact with Maurice Wilkes, who wanted me to come and look at what they were doing at Cambridge on EDSAC, and I remember going there and seeing a rather small room filled with systems that all were working off valves of course in those days, and it was extremely hot. And Maurice explained to me that it was a good computer while it was working, but essentially, one of the valves actually would break every three days. So it meant it was a considerable interruption, and was there any possibility that we could actually make transistors that would actually solve his problems? Well, I pointed out that there were people working on silicon who would make a great contribution to him in time, but we were the wrong people. We parted friends, and I saw him now and again in places, and, convinced him that gallium arsenide was not going to be his way forward.

[27:17]

So there were we, working quite happily on gallium arsenide, and, I was able to organise quite a few of our industrial labs to put two or three people on various aspects of gallium arsenide research, and I did originate the idea of having exploitation in the hands of the common good, and formed the Gallium Arsenide Consortium, which actually had the roots of the European systems later on. And although these were competing companies, they were actually willing, because the research was so difficult, to have a free discussion between them on the subject, though we had obviously to have some rules about the thing. And originally it was thought that the managers of the laboratories would not be very keen on people sharing their results, but indeed, they were quite happy with it. They said as long as it wasn't publicised, that it was my way of organising the things. And it wasn't called the Gallium Arsenide Consortium there, it was called Hilsum's Consortium. So that when it went wrong, I would take the blame and nobody else would be there. It wasn't, later, until it actually began to clearly show results that it began being called the Gallium Arsenide Consortium with its members. But everyone was happy.

[28:57]

So, exploitation was actually a very live subject in those days. And that led to a series of conferences on this. The Americans had always had a device research conference, which was by invitation, and there were no proceedings of this. The original American exploitation was done through the Device Research Conferences, which moved about. And if you were thought to be able to contribute, you got an invitation. But there were no proceedings, there was no publicity, you were not allowed to photograph everything, you just had to take your own notes, and you certainly weren't allowed to write a report on what you had learnt. So, this was the way in which the Americans progressed, and it was extremely successful, as a result of which, getting on for 1970 and the late 1960s, we organised our own conferences. And this proved a slight problem, because we had a, a conference, a Solid-State Device Research Conference, annually, held through the Institute of Physics, in, the conference was in Manchester in January. Yes, not very good, but still. And, the Germans also had their Semiconductor Device Research Conference, which was held usually in Munich in March or April. And at one stage, a gentleman from Philips in Eindhoven, and Philips was a centre of good research in Europe at the time, a lot of devices came out of there, and still do of course, they were at Eindhoven. And one of their senior people called a meeting with me, who had got to be a reasonable position, I think I was secretary of the solid state device research group of the Institute of Physics. And the head of research at Siemens, Walter Heywang, who became a very good friend, and this man Leo Tummers, called a meeting that we would hold, I think it was in London, and said, 'You are embarrassing us.' And we both looked at each other, and said, 'How are we embarrassing you?' He said, 'We cannot put our research in both of your conferences. We don't know which one to choose. So why don't you get your heads together and have one conference.' Well I was quite keen on this, because I tried to actually move our conference from January in Manchester, and I wanted it to be at Easter or something like that, when the German conference was. So I thought this was a good idea. Heywang thought it was a good idea. We had discussions obviously, and it was agreed that we would form the European Solid State Device Research Conference, which held its first conference in 1970. Now you may appreciate, that is 50 years ago. So that conference still exists, and I'm quite proud of the fact that 50 years later it's still run. I'm a bit disappointed that the UK participation appears to be limited.

[32:30]

Now, those among you who have a European basis will note that, this was the UK, the Netherlands, and Germany. And there's one notable exception, which is France, which certainly had a good programme. But the Germans weren't all that keen on collaborating with France then, this is long before, thing, more recent things have happened. But I thought that that pressure could not be resisted, and we would have to bring the French in on an equal basis with the other three countries. So it was more or less agreed, and I think it was about 1972 or '73, could have been '72, it had been agreed, a man called Maurice Bernard at the French Post Office was the main person involved, and, he came to our conference and was going to make a, a presentation, and in fact put out leaflets on the new French conference that was going to be the change. And, there was obviously our normal business, and I attended on, I think it was the second morning, and, suddenly was a bit surprised when a pretty notable German professor, Heinz Beneking from the technical university in Aachen, stood up and went to the front and said, 'Chairman, I insist that I make an announcement.' So everyone… And the chairman was obviously taken aback, didn't know quite what to do, but he couldn't see what to do, because, Heinz was standing in front of him. And he said, 'By all means.' So Heinz went up on the stage, and waved this piece of paper. He said, 'We have had a very good relationship in Europe around this conference, and one of the things that we have agreed on,' and I have to say that this was rather a surprise to me at our meeting with Heywang and Tummers, that one of the conditions that Heywang had made was that the conference language had to be English. And I said, 'Well, but all your people speak German.' He said, 'That's the problem. They must all learn to speak English, because of all the American work in this field. And so they must learn to understand English, and to present their papers in English.' So that is why it's got to be English. So the first conferences had as their publicity that the official language of the conference will be English. Well this piece of paper that Heinz Beneking was brandishing around said that the official languages of the conference to be held in Grenoble will be English and French. And it said on the back, that in French, '*les langues officielles seront…*' in French. And, Beneking said, 'This is a change, and I insist that the chairman of the organising committee tells us now what the organising committee is going to do to make sure that the fraternity we have built up in Europe over these conferences continues. I insist that he tells us what we are going to do.' And everybody started clapping, and I started clapping too, until I realised everybody was looking at me, and the man next to me nudged me. And I suddenly realised that I was the chairman of the organising committee. And this had all come as a great surprise to me. But anyway, I got up and made a normal Civil Service speech as you might say, trained in this, and which I sounded as though I had the whole thing in hand, and everything was going to be OK, and they should rely on the organising committee to come up with something, and they could be sure that we were discussing this. And I went to sit down, and my neighbour said, 'What are you doing? Do you know what you're doing?' I said, 'I haven't a clue.' But, then I decided that, we should think about this, and I came up with a solution, which I think was OK at the time, that there should be no official language for the conference, that anybody should come and give their paper in any language they liked, Farsi, or German, or Swedish. But, there would be no official translation. And everyone thought this was OK. And after that, the first conference, the Grenoble conference, some of the French came and gave their papers in French, and they soon realised that nobody was attending. And after that, there was no problem. Everybody learnt to give their papers in English, and I think you will find that, except in Russia where there may be some exceptions, that nearly always there is no official language.

[37:30]

Can you say something about the patents you filed?

Yes. Now I haven't said very much about the details of the research we did. But this was quite an interesting time, because, we had conquered much of the problems of gallium arsenide, we had indium antimonide. So we had a lot of devices that we were making. And, it was natural then that we should think of patenting them. So I learnt a bit about patents.

We are talking about, which years?

We are talking now about the years from 1955 to the early Sixties. And we had a series of patents coming out from the group, some of which were quite advanced, particularly in semiconductor lasers and LEDs. I remember we made some quite nice devices which showed that you could make tape readers which could be immersed in oil and still worked well, which went down pretty well with the Foreign Office. And, these taught me quite a bit about patenting. I also began to realise that the Admiralty was not terribly interested in exploitation of patents, which was a change when I had to move, as we will come to in a bit. But we had a whole series of patents, and we were very conscious of the potential commercial applications for what we were doing.

[39:06]

Why did you move to Malvern in 1964?

That is a, almost a philosophical question. The director of SERL was a very very clever inventor. He had invented things during the war on vacuum tubes. But his basis was vacuum tubes, and he had never felt really comfortable with the group that had been set up largely through headquarters on semiconductors. He didn't understand semiconductors. He was a world authority on vacuum tubes, particularly magnetrons, and travelling wave tubes, but semiconductors was a foreign country to him. So he was never terribly happy with what we were doing, and not entirely happy with the time it was taking to correct these materials and make sure that they could actually work. And his confidence in me actually also got to a low level. So in the end he said, look, we couldn't together work in the Establishment, and he had no intention of leaving, so would I kindly find somewhere else to go? I wasn't the only person he told to go, in fact he more or less folded up the solid state device research being done, and, oh, half a dozen of the senior people all left and went to, various people I mentioned, Christopher Rose-Innes went off to Manchester, and various other people also went. In fact most of the people of unranked, lower than me, went, and, I had to find another home. And I was lucky in that Malvern knew about me. We had worked together on photocells for infrared missiles for some years. So the people knew me. And their leading light in semiconductors, Alan Gibson, had decided to move to Essex, the University of Essex, as a senior professor, so they were feeling that, they were actually losing people at the top of the field. So asked if I would come and work there, on my own subjects, not replacing Alan in any way, but more or less changing what Malvern had been doing, because they had been doing largely silicon, and this was proving a problem in keeping up with industry, because chips were now being made, and that involved great expense in the thing. So they really wanted to get on more exotic materials, and they were quite happy to actually see if we could do something on gallium arsenide together.

[42:06]

Now it so happened that I had come up with a new property of gallium arsenide which was going to prove extremely useful, and this was a negative differential resistance, which is something people had wanted for some time, and had had various ideas for doing it with large magnetic fields in germanium. But it clearly was impossible. And through my work on the book on semiconductors, which I had mentioned earlier, I realised that there were some strange things that went on in III-V compounds that didn't happen in the elements. And that was due to the details of the crystallography. And in fact the, there's what is called a Brillouin zone in semiconductors. Well there is in all materials, but in semiconductors. And in several of the III-V compounds, the electrons are working at a minimum in crystal momentum which they aren't in germanium and silicon. And this is why they are different, why they have a higher mobility. And I was intrigued by what happened in gallium antimonide, where in fact all of the levels are pretty level, and as you change the temperature, so you change the behaviour. And I thought, why can't that happen in gallium arsenide? And when I did a calculation, which I did in 1961, and was published, I showed that there was actually a negative differential resistance that occurred at about 3,000 volts per centimetre. Now that seems like quite a high voltage, but if you think that at the same time you were starting to work on epitaxial growth of semiconductors, it means that if you actually took a piece of the semiconductor, just straightforward gallium arsenide, ohmic, with no n or p junctions in it, and applied three and a half volts to it, then it would become unstable. Now the father, one of the fathers of the transistor, William Shockley, had shown many years earlier that a negative resistance, a negative differential resistance in semiconductors, played havoc with the stability, and wrote some fundamental papers, which is why people have been interested in negative differential resistance in germanium. But here was I saying that you could do this just with three and a half volts applied to a piece of material. Which was rather surprising. And, the surprising thing is that it actually happens. And that is what works. If you put three and a half volts across ten microns of gallium arsenide, it becomes unstable, the current starts oscillating, and if you put it in a resonant cavity, you have a microwave source. And, though I didn't have the basis for doing that in Baldock, once I went to Malvern, they were very interested in the prospect of a microwave oscillator that was so simple, and indeed, before long, using the Gallium Arsenide Consortium I'll say, using material made at Plessey. And, it's interesting that we made the first microwave oscillators based on material that was made at Plessey, in the consortium, using contacts that had developed at STL Harlow, through the consortium, and a complete arrangement that was jointly done between Plessey and Philips at Redhill. A real example of exploitation through the consortium. And indeed, both Plessey and Mullard's brought out the first microwave oscillator based on the negative resistance in gallium arsenide in, in, I think it was '66. And STL followed one month later. And all of them were making the same device, all done through the consortium. So a real triumph of collaboration.

[46:39]

The thing that's not so great is that the first actual exploitation – experimental, sorry, not the exploitation, the first experimental realisation of this, came from IBM, in 1964. And they, the group there, refused to accept that it was a negative differential resistance, which caused havoc in the American community, because Bell Labs and Stanford also said that it certainly was negative differential resistance, and IBM should accept this and give us acknowledgement. And IBM never did for many years. But we didn't care, because, we knew what it was, we knew how to do it, and immediately made mini-radars which gave us burglar alarms and speed meters, and all kinds of early devices. They are called Gunn diodes, though my friends always call it the Gunn-Hilsum Effect. I do have some friends still you see.

[47:43]

How did you come to develop the technology that forms the basis of modern LCD technology?

Well of course, I was pretty happy working on microwave oscillators, and also continuing some of the work we did on LEDs and semiconductor lasers and things, and, everything was going well. And then, the Establishment changed. Whereas previously we had been I think the Ministry of Aviation, or, maybe… Oh no, we were Radar Research Establishment. That's right. And, then, Harold Wilson had been talking about the white hot technology that was going to come in once the Labour government came in. And, as a result of this, the Department of Industry, a Department of Technology had been formed within the Government, and our establishment, and some others, were moved under this. And, there was a Minister for Technology whose name was John Stonehouse, who became famous later on for other things, and he came down to the establishments and, had a meeting with our director, George McFarlane, a distinguished mathematician, extremely nice person, who was running the place, and had a meeting with him, and, I gather that the next morning he rang George and said, he was convinced by the meeting they had had, and would he start a programme on flat-panel displays? Well George was taken aback by this, he couldn't remember this, and called me and the director of physics. I was by now a reasonably senior person and a certain rank in the organisation. Though I still was allowed to work on my own topics as what you might call an individual research scientist, which was a title that you got then in the Civil Service. And David Parkinson was Head of Physics, he called us down, and said, 'I think I've got us in a bit of a problem here.' I said, 'Why is that George, how has it happened?' He said, 'Well, I think I said something to him which you had told me.' I said, 'What was that?' Which was that we were paying more to the Radio Corporation of America in Princeton as royalties on the colour television tube than we were spending on Concorde. I said, 'Did I tell you that?' He said, 'I think so.' I said, 'Well it sounds like the kind of thing I might have said, but I don't really know whether it's based on actual facts.' He said, 'Well, it convinced John Stonehouse, so what can we do on flat-panel displays? Can we make a flat-panel display?' I said, 'We can put together a few LEDs.' He said, 'I don't think that would actually do.' And David Parkinson, who was a very learned gentleman, said, 'I think we should start a working party to actually look at the topic. George has a great idea.' So we reported back to John Stonehouse that, yes, we were going to do this, and the first thing we were going to do was to start a working party on this. So we started a working party with David running it, and me obviously on it, and people from the Malvern group who were working on various aspects of physics that they thought would actually be relevant on this.

[51:40]

And then, yes, history repeats itself. I told you how I came to the Baldock group by people getting promoted and coming out. David was promoted to a job at headquarters. He announced to me by coming in and throwing a folder on my desk, and said, 'Cyril, this is yours now.' I said, 'What is it David?' He said, 'I'm going to headquarters. This is the working party.' So I was saddled with a working party on flat-panel displays, and I knew damn all about flat-panel displays. But anyway, the working party worked pretty well, and there are some stories which have been published as to why we chose liquid crystals, but we did choose liquid crystals. And we got the blessing of the headquarters group that was, that placed contracts. The arrangement was that, thought the establishments looked after the actual research done in industry on their own topics, it was all run in solid state devices: well not just solid state devices. Any kind of electronic device was run by an organisation called CVD, which stood for various things, but was always known as CVD, coordination of valve devices, or, things like that. And, they were going to run the flat-panel display programme. And, they called a meeting in London where I had arranged that we would be given lectures by all of the people, particularly from academia. Not much serious research on devices was done in academia then. We're talking about 1970, '71. And, people in industry were interested in devices; people in academia were interested in fundamental research. And that is the way we went. So naturally we called quite a few people to this meeting, including representatives from industry who claimed they knew something about liquid crystals. There had been some work done in Marconi actually, quite early, one of the first people to actually work on liquid crystal devices. And, there were various people from academia at this meeting. I won't go into the details of what actually happened, but at the end, I realised, there was only one person in the room who understood anything about liquid crystals, and this was a gentleman from Hull called George Gray. He wasn't a professor at the time, I will say, but he obviously knew something about liquid crystals. So, when Leslie Large, the man who ran the CVD organisation, said, 'Have you come to any conclusions Cyril?' I said, 'Yes. Put the gentleman from Hull on a contract.' I did not use the word gentleman, but I will here.' But, I said, 'Put him on contract.' Which came as a great relief to George, who had just had a contract proposal refused from ERC or whatever it was called then, on the grounds it didn't have enough chemistry research in. And I offered him a contract. And I think it was for £1100 a year, which may not seem a lot now but he was very happy with it. And, he started his research on liquid crystals. And after a very harassing two years, came up with the answer. And the world had been trying hard to find a stable material, and suddenly we had it. It wasn't perfect as came from George. In fact I had recruited quite a brilliant physicist from Cambridge, Peter Raynes, who later became a famous professor at Oxford and a Fellow of the Royal Society and all that kind of thing. But he came to me, and, I realised he was the right kind of person when he came for an interview. I said, 'Have you any idea what you want to work on?' And he said, 'I don't care, as long as it's not superconductivity.' I said, 'What's wrong with superconductivity?' He said, 'I've done three years on that, and that's enough for anyone.' Which I thought was pretty forward-looking. And Peter worked extremely well with George, and came up with a modification of what George had done on biphenyls, and came up with a stable material.

[56:19]

We then had the problem of how to exploit it. And here again I was lucky. I'm using this term quite a lot, because I think, I have been extremely lucky, and sometimes luck is better than brains. But, previously, I had been trying to exploit the microwave oscillator, remember, and though Plessey had done good research on making the epitaxial layers, their research lab wasn't noted for successful exploitation. And I had wanted a chemistry-based organisation to actually make the material. And I had hit on what was once known as British Drug Houses that later just became BTH, I think it was owned by Glaxo at the time, down at Poole. And I persuaded them to actually make layers of gallium arsenide, with some reluctance, but they were prepared to accept a government contract. So they were making our epitaxial layers, quite successfully. And I thought, well I'll have a go at seeing what they can do. So I asked Ben Surgeon, who was then their director of research, to come along. And I explained to him what I wanted him to do, and I had no idea what he would be, want to do. And he looked at me at the end of my long sales pretext. I wanted to push this over, so I had used all of my, powers to convince him. He looked at me and he said, 'Cyril, do you know, for the first time you've asked me to do something which I understand.' I said, 'You understand it?' And he said, 'Yes, I'm an organic chemist. All the other stuff, I've done just to help you. But here, this is something I am interested in.'

[58:11]

So, he took over the manufacture, they did this. They were bought out by Merck later on, and this established Merck in liquid crystals, and they now sell tons. They don't use exactly the same thing, but the material that George made was the foundation of their work on liquid crystals. And it came basically from George Gray and Peter Raynes. So, that was it. Now of course, that brings in patents, as to what to do. And now I was skilled in patents and exploitation, I had made sure I understood them. So I set out to, us exploit them, and actually get some money back for the taxpayer. And we succeeded, we, me, and later on people who took over from me, working with our patents group, which unlike the Admiralty group was really interested in exploiting patents. And they exploited the thing, so they are sold worldwide. And altogether made a bit over £100 million for the Government. So, I was quite popular. And, you can say that, everyone lived happily ever after, but most of the people who, did, and out of that liquid crystal group were, George got an FRS, two people at Dundee working on the display programme in a different way, on amorphous silicon thin film transistors, got FRSes. Ian Shanks, who has later become famous for a patents dispute with Unilever, that was based on his liquid crystal experience, he got an FRS too. So, all in all, though we actually were quite popular because of the money, the people who took part in it all had their careers extended and made good because of it. So, it was a, a pretty good exercise that John Stonehouse started. It's a pity he never got the credit for it. He actually had a, a different life after that, which I won't go into, but it is in my publications if you're interested.

[1:00:32]

And, what brought you to be appointed Chief Scientist at GEC Hirst Research Centre in 1983?

Well, I've said, of course I ran the Gallium Arsenide Consortium, I ran a lot of contracts through CVD with industry. And, when I ran contracts, I didn't sit back and just wait for results to come in. I would have progress meetings, and would actually make sure that I knew the science that was behind what they were doing. And, some of them welcomed this; others, were not too happy about the amount of examination they had to do to justify their research. But of course, it was part of my job. But, when I was 57, one of the people who I had worked with closely, including close questioning on what they were doing at Plessey, he had now become Deputy Manging Director of GEC, which was our biggest electronics company. And, he saw me, or wrote to me, I forget which, and said, 'How about you leaving the system and joining GEC?' And I was intrigued. Because, you haven't asked me how I came to my decisions, but if you've been following carefully, you will know, I have never made any decision in my life as to what I should do. I've been called up, to work in the system; I've been sent off to Teddington; I then managed to qualify for the Civil Service with a minimum possible mark; and then I've been thrown out, I've been transferred from one establishment to another, without my having any say in it; I have been moved from another establishment, because I disagreed with the director. And any time I got an offer, or thought of leaving Malvern, I would go and see the senior person, the deputy director and somebody, and said, 'Look, I, I've had this offer and I'm intrigued,' they said, 'Oh you can't do that, we're just about to promote you.' So, I had been promoted several times, until I had reached the top, I was a Chief Scientific Officer by then, and the senior working scientist in, certainly the Ministry of Defence, and actually for a few months in the whole Civil Service, nobody else had the rank of Chief Scientific Officer. So there wasn't much I had to complain about.

[1:03:14]

But, I now had this offer. And, our two daughters had both moved to London by now, so, there really was no domestic tie keeping us. Betty, my wife, had a good job at Malvern, but she was quite willing to now move. And Derek Roberts, who later became Sir Derek, was very persuasive in saying, 'It's about time you saw what life is like from the other side. You've been telling us for a long time what we should be doing; now you're going to be in a position where someone else tells you what you should be doing.' Actually it didn't work out like that, but still. So I said to the system, 'I've got this offer and I want to do it.' Now it's not easy for senior people to leave the Civil Service like that. They always think they've got some special knowledge. So there has to be some interaction with the other companies to check that they don't object. And they also said, 'Well we don't want you to leave.' I said, 'Well, your retiring age is 60, I'm 57.' They said, 'Oh no no. An individual merit scientist doesn't need to leave at the retiring age. You can go on. In fact there's somebody,' Sayce, actually who I knew, 'who is 85, and he's still working for us.' I said, 'Oh, well what happens?' He said, 'No, you do what you like. You can work on what you choose, and, do this, and we'll be happy.' I said, 'Good.' And he said, 'Of course, you have to write a report each year on what you have done.' I said, 'Oh, well that's interesting, and, who would read it?' And they said, 'Well somebody at headquarters would read it.' And they said… I said, 'Well how would he judge its quality?' And they said, 'Oh well, he shall have the background.' And I said, 'I'm not sure. I mean, if he didn't like the report, then, I wouldn't be able to continue.' They said, 'No, I suppose not.' So I said, 'No, I'm not too happy with that.' So I decided I would go. And most of the companies didn't mind. Mullard said that they didn't want me working on infrared devices. And I wrote back saying, 'I'm extremely flattered by this, because I haven't worked on infrared devices for ten years. If somebody thinks I can make contributions now, I'm really very flattered.' So they decided that that didn't matter, so, I did join GEC, and moved to London. So I have made a decision.

[1:05:49]
What did you do at GEC?

What did I do?

Briefly.

Mm?

Briefly.

Well briefly, I worked on a great many subjects. One of the things I had to do of course was to answer things that came up from Lord Weinstock's correspondence.

People would complain about GEC, or actually make a suggestion as to what we should be doing, and it wasn't sufficient just to answer and say, 'This is rubbish,' you had to say why it was rubbish. So I had to do a lot of that. I had to run a research laboratory, the Hirst Research Laboratory, I was essentially responsible for the research there. I had to set up a chief scientist unit, and I can say that again I was pretty progressive, because I insisted they should be, gender neutral, that I could take on 20 people and ten of them were going to be female. And in fact, ten of them were female, and in fact one of them went up and is now running quite a lot of research there. And I had recruited some women at Malvern, and one of those became director of the Defence laboratories. So, I've done quite well in diversification. So that's one of the things I did. I also started a programme on high temperature superconductivity, which was my introduction to the field, and, the people in materials there were very good, and we made some contributions. We also did some good work on olfaction. Various topics that I contributed to. As well as, there was of course a gallium arsenide group which I led, and I would poke my fingers in there, and I was responsible for the long-term research laboratory, which did nanotechnology and came out with two people, one of whom became a senior professor in Cambridge, and the other one's now a, a dean at Surrey University. So again, people generally got on fairly well. So that's what I was doing there.

[1:08:02]
What did you do after leaving GEC?

Well I stayed with GEC for an indecently long time, indeed the tax people got a bit irritated because I was at one stage getting a pension from the Civil Service, a national pension, a pension from GEC, and a salary from GEC. It didn't seem to worry me, it didn't seem to worry GEC. I had given up the post of Director of Research when I was 70, but I carried on as a consultant for them for a couple of more years, [coughs], when they complained that the Jaguar was now becoming too expensive to keep going. So, I agreed, and we parted as friends. And I had to think about what I was doing. But of course I had built up quite a bit of goodwill. One of the things you didn't mention is, I had done quite a bit for the Institute of Physics, and actually I was President for a short time, and managed to do some things for them, which has established them, so they were pretty happy with me. And, I had of course had people who had left and they wanted to continue working with me. So through various things. I went to work for Unilever as a consultant, for Cambridge Display Technology. And, I also was doing some work at UCL. I had become a Visiting Professor there, so I was doing some work with them. So, my time was taken up OK.

[1:09:38]

And then of course, I, I got into a bit of venture capital, through, again a friend I had met who said they were forming a venture capital group and would I actually join them. Because, one of the things that venture capital groups always lacked was somebody who knew some technology, and when they got some technology applications, they didn't know what questions to ask them. So I got into that, and worked with them. And one of the companies that we were asked to put some money into was a company in Yorkshire, well, I think it's Yorkshire, yes, Darlington. I'm not sure if it's Yorkshire or Durham, but it's in northern England. They, it was called Peratech. And, we put some money in. And they asked if I would like to join their board as a non-executive director, and head of their science. This was a smallish company, 30 or 40 people, and they were working on force sensors. And, this was interesting to me, because it was quite different to what I had been doing. And essentially it was taking inorganic metal oxide, that, at that stage was nickel oxide really, and, it's, if you put this in a binder and use some of the science that had been learnt from conducting adhesives, you actually can make something that varied in its resistance with force. And this could be extremely large, the change. It could go from, almost an insulator, we're talking about tens of megohms, down to a few ohms with a pressure of a few neutrons – not… Pressure of newtons, not neutrons, newtons. And, these they were selling, and selling quite well. So I joined them and spent some happy times with them, until unfortunately they got into financial problems, which I will not go into, but in the end they went into administration, which actually was very sad.

[1:12:05]

But one of the things that came out of it, they had funded a small company through the auspices of the Wool Research Agency of New Zealand, which was interested in applications of wool other than clothing. And naturally that was diversification, and they had wondered if Peratech could do anything. And what Peratech decided to do was to set up a company called Softswitch, together with the Wool Agency, which would actually investigate these materials that they were developing, based on nickel and nickel oxide, alternatively on titanium oxide, in clothing. And they had a good start. They made some key pads for ski jackets that could work, and you see how far back we're going, for MP3 players, that people could use when going skiing. I personally said, I said, 'I don't understand why anyone should want to work an MP3 player while they're skiing. They should want to stay upright.' They said, 'No no. Most of the time you're skiing, you are going up or down in a ski traveller to get to the heights, or back again to your hotel, and it's then you want to be able to do it. So we put these things in a ski jacket.' Which they did. And they sold them in an American ski resort for 200 quid, which was more than twice the normal price of the ski jacket, and they put 200 on sale and sold them in one morning. So clearly it was something happening. But, Softswitch couldn't continue, but nothing to do with the product. Actually two of the people working for it fell in love, and, the other two key people, other than the inventor of the material, but the two key people on the manufacture, fell in love, and they decided to marry and emigrate to New Zealand where they are very happy. Which was great of them. They set up a small company there, and lovely it was too. I mean, as far as I know they've started a family and everything. But it put Softswitch in a problem. But, what Peratech had done was just put Softswitch on the side, but they had got some patents, and again, when they went into administration, the administrators separated out the patents from Peratech into the textile ones and the others. And a friend of mine actually got interested. She was a shareholder, she also was one of the people in venture capital. Actually as her who introduced me to the venture capital group. And she asked me if it was worthwhile taking on these patents. Well, I was a bit uncertain, but I thought, yeah, this is OK, this is worth doing. I mean, she has a fair amount of money. And, so, she decided to buy the patents, and I would help her. Actually the inventor, David Lussey, also would help her at the time. He has left since then, but, he agreed. So we started, she started basically, this company called Infi-Tex, which still is running six years later, and, its progress is slow. But I do the science, and one of the things I'm interested in is, improving the materials that we are using, and in fact, I was able to come up with some ideas, and I took out a patent last year.

[1:15:48]

So I am still patenting things, at the advanced age, it was then, I think I was still 93, but I might have been 94 by then. But, anyway, so, I have a patent application, and everyone is still happy, and I'm talking hard with companies about the exploitation of patents and trying to use the knowledge I have gained after many years working. And I still actually lecture on intellectual properties at Imperial College, to the MSc course, on intellectual property, because this isn't something which is generally covered by people, but is a very interesting problem for people, for students, what you do, your inventions. And hopefully you are going to say, it is possible that you will make inventions and want to know what to do about them. It shouldn't be something that you leave entirely to someone else.

[1:16:48]

The British Liquid Crystal Society awards a Cyril Hilsum Medal each year. Would you like to talk about it?

Not particularly, but it was quite interesting when I was approached, would I have any objection, and I couldn't think of any objection. They did a, a reasonable picture of me for the medal. And it does go to young people, people in the early stages of their career, and, sometimes they even contact me and say how pleased they are to be, have their research recognised. A young American lady actually got it this year, and we are in email contact. So, yes, I think it's good. And of course quite a lot of the awards I've made, I did get a few awards earlier for my semiconductor work, but I've had quite a few since then on the liquid crystal work.

[1:17:42]

What were the key decisions, positive and negative, you made, and what difference did they make?

[pause] As I have mentioned, I didn't make any key decisions. The decisions were always made for me. I was called up, and then when I thought of making changes, clearly, when there were alternatives, one way was obvious, and I chose that. And the only decision I really made was when I was 57, when I decided together with my wife Betty that we should move to London to GEC. And what difference did that make? It obviously put me in a different context and enabled me to stay active for longer. Because, Malvern's a lovely place to be in, but it's not the centre of activity, and you will find that the people who have stayed there are mostly interested in who actually has died during the past month, and, what kind of memorial service they're going to

have. It's not a centre of activity any more. Whereas coming to London, it's very easy to still take part in science, and to come and be interviewed by very charming ladies.

[1:19:05]
What are the proudest achievements of your career?

Oh, the negative differential resistance is obviously the thing that I did that had a pronounced effect on a number of inventions. Though it got replaced by laser devices and ultrasonic devices later on, but for a period of some 20 years or more, the speed camera was based on negative, on our diodes, negative differential effect. And of course it meant that a lot of military equipment could be based on them, on the oscillators, because they were so simple compared with the vacuum tubes. So yes. And also, I didn't deal with indium phosphide, but we did move on to indium phosphide in the early Seven-, well, the late Sixties, early Seventies, and showed that that had advantages over gallium arsenide, and did some fundamental research. And that is still being exploited. So I can say that the things I did have really been the roots of quite a lot of the devices that are used in electronics, including computers, for many years. And of course the fact that all computers use a flat screen. I don't know whether it would be our flat screen, but I'm pretty certain liquid crystals would not have got going if it hadn't been for my initiative with George Gray and Peter Raynes. And… So, we have flat-panel displays on so many things. People are trying to replace them, but, as far as I know, more than half of the displays are still the same.

[1:21:01]
What is the Karen Burt Memorial Award?

Oh, that brings in some rather sad times in my life. My wife Betty, who I've mentioned, died when we were 62, and, an even, well you could argue a bigger tragedy, my elder daughter Karen, who was a scientist, working at one stage for British Aerospace on satellites, but later on moved to UCL. She died ten years later. And, I didn't know how I could best show signs of what I felt about them, but, I did start a scholarship at Malvern Girls' College, I don't know if it still exists, in the memory of Betty. And one thing that certainly still exists, I founded the Karen Burt, Karen was my daughter, Memorial Award with the Women's Engineering Society. And, this we discussed for some time as to what it would be, and I wanted to make it different from other awards. And this is an award for the woman, we were allowed to make it for a woman, for the Women's Engineering Society, the woman who had reached chartered status that year, and it should be a competition between all of the scientific institutions, including the Institute of Physics, and I think the British Computer Society, on the woman who has actually, proved that. And they have a committee which decides, and each of the institutions chooses their best woman candidate that year. And it does vary a lot. But it's still going. I think it must be, almost 20 years now, and, I attend the ceremonies each time, and, some remarkable women came up with this. Well obviously it's extremely competitive. It's, getting on quite well, and in fact, a number of my friends, I put some money in, but a number of my friends have also contributed at various times through events in their life, and when they want to, say, when I have helped somebody, or done something, and they ask how they can reward me, I say, 'Well put some money into the Women's Engineering Society fund for Karen.' And they're all happy to do that. So, it's still going, and it's likely to go, well for the next 20 years. And we keep on putting up the actual amount that they get. So it's now £1,000 they get, which is not trivial for the people at that stage in their career.

[1:23:57]
What do you think are the materials that will change our lives in the next 20 years?

Mm. Well, I think that it's inescapable that we're going to be doing things in the actual clothing. I think… One of the changes that's come over research in physics exploitation in the last few years has been the liaison with medical physics, and with medicine. And many more instruments are being developed that are based on physics research. For instance, when something went wrong with my eyes, I was able to look at a picture of my retina done by optical coherence tomography, and I had learnt about optical coherence tomography at NPL, the National Physical Laboratory. And that's something which is exploited and shows you things. And they do quite a lot of measurements on blood and stuff that's done by using sensible physics. And that makes a lot of progress, and I think that will continue. And of course it means that sometimes some exotic materials start being used, and there is a, a lot of research on different materials. I'm astonished sometimes, and I still do a bit of reviewing, through some organisations I'm linked with, and, I'm amazed at some of the materials that come up, quite often semiconductors with unusual properties, and I think, I'm supposed to know a lot about semiconductors, but I knew nothing about this. And, that's one way in which things are going to be developed. And of course, we will be doing, I hope, much more in textiles, and putting new materials in textiles for measuring a lot of things that people use in their everyday life and in their health, things like that.

[1:25:57]
What would you do differently if you had your time again, and why?

I think I would work on life sciences. I think that could enable me to go on still longer than this has done. Might actually be progressing new, or… Yes, life sciences is a very intriguing thing. I know a lot about life sciences. My partner… I said Betty died some years ago, well, 30-odd years ago, and, I was lucky enough to find another partner, Anne Dell, who is a professor, Head of Life Sciences at Imperial College, and, though I can't follow everything she does, I'm pretty certain that what she does is very interesting, and particularly in the mass spectroscopy, curiously enough, which is something I did work on many years ago. But, nothing like the scale on which people do now, and that's what's amazing, the detail they can work with, and the instruments they use.

[1:27:04]
What advice would you give to someone willing to pursue your career today?

Oh. I've given up giving advice to people, because I always give good advice to people and they ignore it. Though, indeed, I am still astonished occasionally at meetings I go to when somebody comes up and says, 'You won't remember seeing me,' or talking to me, or me coming to see you, many years ago, and I usually make some excuse and say, 'Well, your face is familiar but I've forgotten your name.' And they say, 'Oh, you gave me such good advice, it's helped me no end in my career since then, and I'm really grateful.' And they say, 'You must be pleased at that.' I say, 'Yes, I'm always pleased when somebody comes up and thanks me for the advice they're given.' But of course I remember all the people I have given advice to who don't come to see me, because it hasn't worked. So I've given up giving advice to people. They don't necessarily benefit from it.

Fair enough. Thank you Cyril, it's been a real pleasure talking to you.

And a pleasure talking to you Elisabetta.

Thank you.

[End of Interview]