ORAL HISTORY OF EUROPE IN SPACE

INTERVIEW WITH
HARRY HINDMARSH ATKINSON

Conducted by Matthew Godwin

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Revised Transcript

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Matthew Godwin: If we could just start off with the chronology of your career, that would be a good place to start.

Harry Atkinson: OK. It's a fairly long and complicated one so if I start to digress, or spend too long, kick me under the table.

I was born in Wellington, New Zealand, in 1929, and spent the first 11 years of my life there. My father was a lawyer. He was head of the Patent Office in New Zealand and I used to meet some of the inventors and people who were applying for a patent, a trade mark or design – and that was, I suppose, one of the things that made me interested in science. I was also particularly interested in chemistry. This was at the age of 10, I suppose, and somebody had given me a chemistry set, a toy chemistry set, which had iron filings and sulphur and things like that in it. Also, for a birthday, my parents gave me a dictionary of science – which I kept beside my bed and would read every night. I soon decided that I needed some chemicals which were rather more professional, and I found a supplier in Wellington called Wiltons; and they would sell me anything! I continued to buy things from them until I was about 12 or older. I remember that on one occasion they sold me two ounces of potassium cyanide – enough to kill the whole population of Wellington or Nelson![1] I was at primary school at this stage, Wellesley College, in Wellington, which was quite an inspiring place.

In about 1941/2 my father retired and we moved to Nelson, which was just across Cook Strait. There we had half of the large house of my widowed grandmother. I went to Nelson College, a very good boys' school, where I finished my primary education, and then went on to the secondary part. Nelson College was – I suppose you'd almost say now – a "comprehensive" school because it did everything from practical agriculture to university entrance. One of its famous old boys was Ernest Rutherford. The school had a large boarding department, but I was a dayboy. One of the boarding houses was called after Rutherford; another boarding house, previously called Fairfield, had been originally owned by my great-uncle (Arthur Samuel Atkinson) who was, in fact, an amateur astronomer (though by profession, a lawyer). When there he bought a telescope, imported it from England, and observed the Transit of Venus for the Royal Society on 7 December 1882; he was a real polymath. In order to do this, the timing of the observations had to be very accurate. This was done by extending to the small observatory he'd built near his house, the telegraph cable which connected Nelson and New Zealand to Australia and thence to England. Through signals on this cable, the precise timing of the transit could be measured. I suppose that this family interest in science and astronomy also influenced me eventually to go into science. On 21 February 1876, a sub-marine cable was opened between La Perouse (Sydney) and Cable Bay in Nelson. I guess that it was this cable through which the time signals came to my great-uncle's observatory.

But particularly important in encouraging me towards science was a man called Sir Thomas Easterfield, who lived about 500 yards from us in Nelson (New Zealand). Easterfield had been one of the foundation professors at Victoria College, now Victoria University, in Wellington – the professor of chemistry – and he had come to Nelson to set up the Cawthron Institute – which was devoted to biological and agricultural research. When I knew him, Easterfield was about 80, and well and truly retired, but he was still extraordinarily interested in teaching people about science. He encouraged me to set up a laboratory in the washhouse of our house and would suggest experiments, which we would do. He gave me his own set of balance weights (by Oertling), and other instruments and books. He was really the main – although you can see he was not the sole – influence on me to become a scientist. Incidentally, my mother first wanted me to do architecture and my father to be a lawyer.

[1] Actually, only enough to kill 300 people!

Having finished school, I went to Canterbury University College in Christchurch, which was then one of the four main university centres in NZ. This was indeed the university where Rutherford had been a student. It still contains his "den", as he called it, where he did early experiments; some of his equipment is still there as a sort of museum.

MG: What year was that?

HA: I started there in 1948. The New Zealand university system was based more on the Scottish system than the traditional British one, so one began reading four science subjects – for me, physics, chemistry, mathematics and applied mathematics (the last two counting as separate subjects). After the first year, I became rather bored with chemistry, particularly organic chemistry (which in those days seemed to have no rhyme or reason about it) and so I decided to specialise in physics – which I did (for a BSc) and then stayed on to do a Masters degree. This was an Honours degree, including a thesis, and taking 2 years. My thesis, supervised by Professor Chalkland, was on soft x-rays; these were so "soft" that they would (essentially) only go through a vacuum.

After that I had to decide what to do next. All New Zealanders, perhaps because the population in New Zealand was only 1.5 million at that stage, felt they had to go away from the country for a bit because it was just too small and confining (and they needed wider experience). The aim was always to come back eventually. Almost all the physicists and other scientists went to England, or to Scotland – so I decided that I would do something different. I applied to universities in three countries to do a PhD: Canada (McGraw at Montreal), the States (Cornell) and Australia. I rapidly received offers from Cornell University, which is in upstate New York, and Sydney University (where a remarkable chap, Harry Messel, had recently come as a professor). Regarding Australia I thought, no, that's too close. So I ended up, because I hadn't heard from Montreal, going to Cornell and I was a year and a half there.

MG: That was in?

HA: That was in 1954 to the middle of 1955. It was a fascinating time to be there because Senator McCarthy was just coming into power, "hunting for reds under the bed" and particularly "red" academics, and running the "Army/McCarthy" hearings. Cornell was a so-called "land-grant" university, which gave it independence; but most of the other universities were "state universities" (I mean universities set up by individual states) – and every member of staff of the latter had to take an oath of allegiance to the American flag to show they weren't communists. Cornell had a remarkable group of independent people in the physics department: Hans Bethe who had, of course, been one of the crucial figures in the development of the atomic bomb; Ed Salpeter a mathematician; Dick Dalitz and, in particular, an extraordinary man called Philip Morrison who should have won the Nobel Prize in about five different areas but never did. Morrison had been the first US scientist into Hiroshima after the bomb had exploded, and that turned him completely off the bomb. Oppenheimer, the "father of the bomb", was of course still around at that time – but he had had his security clearance removed as a result of McCarthy's investigations. Oppenheimer was a hero to most physicists and there was a great feeling at Cornell that, you know, we all had to fight for academic freedom in a way. Morrison was called before McCarthy, but won. There was a political feeling at Cornell that I'd never felt subsequently in England – because here it didn't *really* matter whether the Conservatives or the Labour or the Liberals were in power.

At Cornell, I soon discovered that it was going to take me five years to complete my PhD. I thought that that was too long – and I would become an American, then, almost by default. I wrote to my parents in NZ and my father replied saying: why didn't I have one shot at going on to a university in England? I looked to see what area would be interesting and I thought,

well, if you're going to go, you'd better do it properly. So I wrote a letter to Neville Mott who had just become Cavendish professor at Cambridge, and I got a reply within, really within perhaps a month, which seemed very quick. He said, yes, come, and that he'd arrange for me to be granted a small scholarship. So I went. But I learned an enormous amount at Cornell, not just about America and Americans, but also because there were a lot of foreign students there, numbers of whom I got to know, from many countries – including the "Brits", who seemed to me very much like foreigners.

MG: This was slowly entering into the Brain Drain period, so were there quite a few Brits?

HA: Yes, there were a lot of people from Great Britain; those who were fellow graduate students tended to keep to themselves. My research supervisor there (Lyman Parratt) was an extraordinary man who was professor of physics; he was a Mormon and incredibly straight. He introduced me to a "fraternity" (it's got a terrible name, fraternity) called Gamma Alpha; but it was a graduate science fraternity, and quickly changed its name to Society because it couldn't bear to be associated with all that the word fraternity had come to stand for at that time. Gamma Alpha had a fraternity house where I lived for a while and made some very good friends, all Americans or Canadians. After that, four of us (all the others were Americans) decided to rent a house; they were an amazing and interesting group. One (Gale Dick) had been a Rhodes Scholar at Oxford until he came back to do postgraduate work. One (Norman Baker) was doing his thesis on the collision of a galaxy of matter with a galaxy of antimatter (for Philip Morrison), and so on.

I had an old car there; I never wanted to be without a car, which was also essential for going around the country – but that's nothing really to do with this interview (though I covered 20,000 miles in it in two summers, covering nearly 40 States). In the end, I booked a passage on the Queen Elizabeth to go to England – for the first time. I packed up my old Nash car with all my worldly goods, and drove to New York City where I stayed with a family whom I'd met in New Zealand; he [Frank Martin] was a director of the Texaco Oil Company. I drove from Cornell and parked my car in a car park at Newark. Martin took me to lunch in the Rainbow Room at Rockefeller Centre, and Rockefeller was actually sitting near by. And then I went off to get my car – but it had been stolen! I won't go into that (a whole story in itself) but I ended up getting it back (thanks to Martin and an ex-captain of the New Jersey police who worked for him) and driving on to the Queen Elizabeth. I was the only steerage passenger who had a car. We had an interesting crossing of the Atlantic, but I won't go into all the social things that happened on the voyage. We arrived in Southampton and I drove to London where I had friends, and then on to Cambridge. I thought Cambridge looked a very funny place with lots of old buildings and things like that. I became a member of Corpus Christi – which was a good college (and one of the oldest in Cambridge).

MG: Did you transfer your PhD?

HA: No, no, I started from scratch.

MG: Oh, OK.

HA: Yes, there was nothing really to transfer (that was relevant to my new work). When I arrived, Neville Mott asked me to come up to his office to introduce myself. He asked what courses I'd been doing at Cornell and I told him I'd been doing maths courses covering spaces of infinite dimensions, and attending very good lectures on solid state physics and so on. He said: "I can understand now why the people we get who have been through US graduate education really had learned so much more than our UK students had done". The research supervisor to whom I was assigned in the crystallography part of the Physics department (the Cavendish Laboratory), was Peter Hirsch. Hirsch was a young man who was working on a

Coal Board Fellowship; he had no official position in the University, but nevertheless had a number of research students. He asked me to look at low-angle scattering of x-rays from fatigued metals. There was a theory that metal fatigue was caused by cavities forming in metals when they were repeatedly strained or stressed.

In 1954, just before this time, the Comet, a very advanced type of passenger jet airliner, began having accidents, and some planes simply disappeared over the sea. This was put down to metal fatigue – so my research topic was a very fashionable subject. I worked initially with x-rays (to probe possible defects) but it turned out that they gave spurious results and that it was necessary to go to longer wavelengths. The only way to do that was to make use of "cold" neutrons, very cold neutrons, which could investigate the crystallography without suffering multiple Bragg reflections. I'm getting a bit technical, but the only place where this could be done was at Harwell (the Atomic Energy Research Establishment).

MG: Oh, OK.

HA: Coincidentally, a chap from Harwell visited the Cavendish at about this time (Ray Lowde). He had previously tried to set up better external research relations with Oxford, but he'd got nowhere there so decided to try Cambridge. It turned out that he was personally much concerned with neutron diffraction (the technique I was interested in) and so the door was opened to my going to Harwell to make use of the nuclear reactor, BEPO, which had what was called a "cold hole", containing liquid hydrogen, parked inside the reactor, which cooled the neutrons down. That was an interesting time and I got the results I wanted to and, in due course, completed my PhD.

MG: Which year did you graduate?

HA: I think, yes, I actually graduated in 1959 so I hadn't quite finished my PhD by the time I left Cambridge after three years – and so I'll just say what happened to me then. I had to decide whether to apply for an academic post or not. I applied for jobs lecturing in physics and I was offered a post at Nottingham University, which had a leading group in magnetism – at about £800 a year. I also applied for a Fellowship at Harwell, the Atomic energy research establishment. There I went before a panel chaired by Brian Flowers who, of course, eventually became Rector of Imperial College and finally VC of the University of London. We got on very well. The main reason, I think, that I was offered the Fellowship was I had them all laughing – and they didn't ask me questions that were too difficult! The pay was good and I accepted.

So the question then was where to go within Harwell. At this time, in the Controlled Thermonuclear Reactions (CTR) Division, was a toroidal plasma device called "Zeta" which had just appeared to work, producing neutrons from apparently thermo-nuclear interactions (perhaps as a first step towards a new energy source). So I though, aha, well, I'll go into that Division – and perhaps help to solve the World's power problems. And that's where I went. But this work was just beginning to unravel; in fact, the Zeta neutrons were not thermo-nuclear but were caused by the acceleration of particles by the magnetic fields involved. The original work was reported in a famous press conference given by Sir John Cockroft, then the head of Harwell, which turned out to be a bit of a disaster and was, perhaps, almost the beginning of scientists not being trusted by the public. After his report, Cockcroft was asked: was he sure that these were *thermo-nuclear* neutrons? He said, no, that wasn't at all clear – they might be or they might not be. Then one of the journalists – I don't know whether he was from *The Times* or *The Daily Express* – said, well, could you put a probability on it? Cockroft said he wouldn't like to do that. But he was pushed and pushed and finally said, well, probably 90% chance that they were: and of course the newspaper headlines reported that thermo-nuclear neutrons had definitely been produced. Cockcroft was a very famous

scientist and the creator of Harwell; but in current terms he could be seen to be naïve in this reply.

So, I went into the CTR division. The director of the division was Peter Thoneman (an Australian), and the deputy director was Sebastian "Bas" Pease.[2] Well, I'd only been there for about a few weeks when an envelope arrived on my desk that said, in a handwritten note, could I come and talk to the writer in the metallurgy division. The person who sent me that was Alan Cottrell, who was then deputy head of the metallurgy division at Harwell. (The head of the division was Monty Finneston who later became chairman of British Steel; and, of course, Alan Cottrell went on to become chief scientific advisor to the British Government. I subsequently got to know Cottrell well.) He said, would I like to come and work in the metallurgy division on the Windscale affair, which had recently occurred after an attempt to anneal the fuel rods on the nuclear reactor at Windscale [in October 1957]. The reactor had got out of hand because of the so called "Wigner release": which occurred when you raised the temperature too much, so that all the stresses that had been built up in the fuel rods by the bombardment with neutrons and so on, were released, causing extra heating – which caused more and more energy to be released – so the process ran away and the rods became red hot.

Naturally, they were terribly keen for me to study how this Wigner release was occurring. I could see that was a very useful thing to do but, I somehow thought, no, I'm going to save the world in another way – to make power from seawater (as the Press said at the time). So I continued in the controlled thermonuclear field. Incidentally, the head of the theoretical physics division at that time was a man called Walter Marshall. Walter Marshall, of course, became eventually Lord Marshall and was again, I don't know what, chairman of the Atomic Energy Authority and then Chairman of the Electricity Generating board. Perhaps I should stop at that point.

MG: OK. Right, OK.

HA: We were still at Harwell?

MG: Yes, that's right.

HA: At about halfway through the three-year period of my Fellowship, Zeta was increasingly seen not to be working as expected, so new types of devices were being thought about [to contain plasma] and I worked on a thing called a "mirror machine". But then came along to the CTR Division a marvellous man, an engineer, who'd been at the Rutherford Laboratory, called John Lawson. He rapidly proposed, theoretically, the "Lawson Criterion" – which said that you could only have sustained thermo-nuclear reactions (and a net power output) if the product of the density of (say) a deuterium-tritium plasma multiplied by the containment time exceeded a certain value [of order ten to the 14^{th} power]. This was almost impossible to achieve in practice, and with it "endless power from seawater". Also, at that time, it was discovered that if you had plasma on one side and a vacuum on the other, there was always an instability. So Lawson just left after three weeks! And I thought I'd better look somewhere else too, in the longer term.

I've always tended to oscillate between, if you like, being an engineer and being a scientist; I like the theoretical side although I'm not in any way a theoretician. But I also like practical things: perhaps all New Zealanders think back to their forbears who had to cut the trees and "pit-saw" all the timbers by hand, and so on, to build their houses [as my grandfather did]. So, having gone into the CTR world and seen that the prospects weren't very good, I thought I'd do something else. So I moved to the pure science side, going to the Rutherford High Energy Laboratory, which was outside the security fence at Harwell. The Laboratory had been

[2] Pease was the great-great-great-great-grandson of the potter, Josiah Wedgwood.

built within a new organisation to support university research in big science: NIRNS [the National Institute for Research in Nuclear Science], formed in 1957 – the first main function of which was to build an accelerator, a big proton synchrotron (Nimrod). My main remit was *not* to do anything that had anything to do with building the accelerator (on which essentially all other staff members were employed)![3]

MG: OK.

HA: And that was enjoyable.

MG: Which year was that?

HA: That was 1961, I suppose. The UK Science and Technology Act [1965] reorganising Government research hadn't happened by then, and NIRNS had a very privileged position. Its Chairman was Lord Bridges (previously Cabinet Secretary, 1938-46) who knew his way around Whitehall to put it mildly; so it really went well. NIRNS had an office in London, but that was simply a sort of *pied-à-terre*, and was never called a headquarters; that was very important to us – it wasn't a headquarters and so we ruled the roost at NIRNS. NIRNS received its funding direct from the Treasury, I seem to remember.

MG: What happened to that model of institute, though, because Harrie Massey was very keen on a similar structure for space research, um, but it was always refused?

HA: I don't know. When was he talking about that?

MG: Early 60s, 1962, 1963, something like that.

HA: I didn't know much about Whitehall at that stage. Certainly the Rutherford Lab worked well and, later, there was a considerable debate internally as to whether it should expand into other areas of science. I always argued that it should – and other people argued that it shouldn't. [In fact it did, in the end.] We went through a financial crisis (in the mid-60s I suppose) because, really, the financial control of science generally was not particularly good at that stage.[4]

In March 1967 [not 1968, as I said], there was a disaster. The oil tanker Torrey Canyon went aground on the west coast of Cornwall, and started spilling oil. Solly Zuckerman (who was then the Government's chief scientific adviser, and who had come in with Harold Wilson at the beginning of the new Labour administration) – was called upon to advise on what should be done. So Solly flew, in a Royal Air Force plane, over the tanker; his recommendation was that it should be bombed and, he hoped, the oil would then all go up in flames [rather than pollute the sea and the nearby coast]. Thus, there was an attempt made to bomb it – which was completely ineffective! Subsequently, a cabinet committee was formed for "hazardous cargoes", to try to make longer-term recommendations for that sort of disaster.

In 1968, a letter came around to various government establishments to see whether they wanted to second somebody to work on the staff of the chief scientific adviser (Zuckerman). My director said that I was always complaining about London and how they didn't understand us, so he thought I might be interested in going to try to change this. I agreed that he should put me forward – on the understanding that I could come back at any time. My first question was: where's the Cabinet Office, what's it for? Anyway, I went to Whitehall and was

[3] Note that the Daresbury Laboratory, founded in 1962, was also part of NIRNS, and began by building an electron synchrotron to provide x-rays for research in solids and liquids.

[4] HM Treasury always tended to be against special organisational arrangements. Thus, I believe that they were against the Atomic Energy Authority's being set up after the War [in 1954], outside the classical Civil Service. And, after a period of "freedom" under NIRNS, the elements of NIRNS, the Rutherford and Daresbury Laboratories, were taken into the new research council system set up in1965 – as was the UK space science programme which Harrie Massey had in essence run from the Royal Society, a most unusual arrangement.

interviewed, sort of interviewed, and seemed to click. But then for quite a long time I had to wait while I was "positively vetted" because I'd been all over the world. My friends told me afterwards they'd been seen in America and New Zealand by mackintoshed chaps asking what I thought about all sorts of things.

Anyway, in due course I went to the Cabinet Office [in January, 1969] and had a fascinating time. In the end I stayed for three and a half years, although two years was the normal secondment. One of the nice things about the Office was that essentially everyone else was on secondment, too. The only person who wasn't was Sir Burke Trend, the cabinet secretary. So nobody was jockeying for promotion and so on while they were there. It was a position, in a way, with of lots of power to influence, but no executive responsibility – which is quite a good position in some ways to be in. I cannot talk generally about what went on there, but I suppose I could mention two things. One was that in the chief scientific adviser's office, there were really no purely scientific decisions to be made – because if there were a simple scientific answer to any problem, then it would have been made previously. So it really was about the politics (or policies) of areas which were concerned with science including, for example, what should be done about the future of Concorde; and hazardous cargos. Concorde was interesting in part because the industrial firm responsible on the British side (the British Aircraft Corporation, later BAE) was in the Bristol constituency represented by "Wedgie" Benn MP [Antony Wedgwood Benn] – who was also the minister responsible for the Ministry of Technology. He fought very hard for Concorde. I could go on for a long time about Concorde but, fortunately, I won't.

In the years about 1970, one could say that "the environment" was invented. Before that the word environment didn't have the same meaning as it does now. There was then a junior minister (in the Ministry of Housing and Local Government) in the Labour Government of Harold Wilson, called Wayland Young, or Lord Kennet, who was very interested in the environment. He was a good friend of Solly's, and together they decided that there should be a Cabinet Office report on all activities in the government relating to the environment. Solly asked me to deal with this for him; it was quite a wonderful thing to be asked to do because nobody knew the answers, and I had to go round talking to practically every Government department. I think Inland Revenue was probably the only department which didn't, then at least, have anything to do with the environment.

I brought all this material together and wrote a draft of the report for Solly. The report went through; of course it was considered by cabinet committees. I remember saying in the draft that the countryside was becoming "destroyed" by hedgerows being ripped out: but the Ministry of Agriculture said, no, you must say the landscape was "modified", not destroyed. The (unpublished) report went down very well and then became called the Zuckerman Report – which was high praise. As a scientist, I'd always wanted my name on scientific papers, but when somebody like Solly's name appeared then that was great (because the paper would then be more influential).

And then the question was, you know, what should be done about it? All sorts of things actually flowed from the report. Firstly, there was created a new post of Director, Central Unit on Environmental Pollution. In fact I applied for the job, but came second to a biologist, Martin Holdgate who got it, and whom I subsequently got to know extremely well – and we became good friends.

In addition was the idea of setting up a department for the environment and also a standing Royal Commission on Environmental Pollution [established 1970]. These were both done in due course. The Department for the Environment was a considerable innovation: this was done in the time of Ted Heath as Prime Minister, following the general election in 1970

which Labour and Harold Wilson lost. I saw from the point of view of the Cabinet Office how the whole system worked when there was a change of government. Again, I'd like to go into a lot more detail including on the fascinating system of briefing possible new administrations and so on. But I won't.

Solly had the best office in Whitehall. It was in the old Whitehall Palace, in the Kent [Treasury] Building, number 70 Whitehall; but on the back of this was a wonderful, I suppose (early) 18th century, building, which overlooked the garden of number 10, and also Horse Guards Parade. [The remains of Henry VIII's tennis courts can be seen within 70 Whitehall; and part of the site was originally the Cockpit, used for cock fighting in the Tudor period.] I'd just mention that lots were drawn when Trooping the Colour occurred once a year, so that CO staff could bring their families to watch it through the office windows.

After this election, I then realised how rapidly the administration changed. Heath was a very different character from Wilson. Heath set up [in 1971] a central group in the Cabinet Office to review government policy generally, and he appointed Victor Rothschild [Lord Rothschild] as the head of this "think-tank", which was formally called the Central Policy Review Staff (CPRS); and that became a remarkable institution. Rothschild was completely independent in the sense that he could essentially do whatever he liked. If the government didn't like his advice, if the prime minister didn't like it, well, he could just say, well, that to you, and walk off. This gave him considerable power.

I got to know Rothschild at this stage: one day he and I shared the lift from the ground floor of the Cabinet Office to the second floor where his office was. We spoke, and he asked me to come to see him. Shortly afterwards I started working quite informally for him over what turned out to be the Rothschild Report – which introduced the "customer-contractor principle". This tried to make government funded research (research council research) much more responsive to practical needs. And there were other changes proposed, for example, that there shouldn't be Departmental "scientific *advisers*" any more – rather, there should be *executive* chief scientists. This was a very important conceptual change: the chief scientist in a department should be at deputy-secretary level and should be part of the board of directors, so to speak, of that department, and should sit in on *all* the decisions that were being made, even though they had nothing to do with science – rather than just sitting as sort of adviser on the outside knocking on the Permanent Secretary's door occasionally.

All this was, to a large extent, implemented, particularly as far as the research councils were concerned. Money was taken away from those research councils which did some applied work – like the Medical Research Council and the Agricultural Research Council – and given to the relevant Government Department – to fund contracts back with the Research Council to do useful research. For some research councils, something like half of their money was removed in this way, without assurance that it would all come back in the longer term. The Natural Environment Research Council (NERC) was the most difficult case because it didn't have any one department that really felt responsible for it. People in the NERC – and in the Medical Research Council – had a really a very difficult time.

I left the Cabinet Office when the Rothschild Report was published (in mid-1972). By that stage Solly had retired and Alan Cottrell was the chief scientific adviser. Just before I leave the subject of Solly Zuckerman, I would mention that he did have a most extraordinarily wide range of contacts and friends. He knew just everyone, everywhere. On one occasion I went with him to Washington, when there happened to be a new chief scientific adviser to the US administration. Solly just told him exactly how to be a good chief scientific adviser to the President; and this man, whose name I forget perhaps conveniently, was more or less nodding and taking notes.

So the question was, what should I do next? I asked Rothschild and he said: you've got to become deputy chief scientist at the Ministry of Agriculture [Fisheries and Food]. I could see that he wanted me to ensure that the message which both of us believed in should be taken and implemented in departments. I said, no, that's a two grade promotion jump. Oh, he said, "that's all right – I've already spoken to the head of the Civil Service and that's OK". So I had quite a lot of talks with the Ministry. There, under the (departmental) chief scientist, the work was divided into two parts: plants and animals. I would be dealing with the animal side. But I just couldn't see myself talking to pig farmers – so I decided in the end I wouldn't do that. Another suggestion was I should become deputy chief scientist at the Department of Health and Social Security. I talked to the then deputy chief medical officer, whom I knew, and eventually I decided that because I was not medically qualified – which was equivalent to being unqualified in the eyes of the medical doctors – I wouldn't carry much weight in the department, so I decided against that, too.

But then I thought, what about going back to the Science Research Council in their (I won't say headquarters) London Office? Brian Flowers was chairman then and he welcomed me back. I was offered any of three division headships: one was Engineering, one was Science (which meant "small" science), and the third was Astronomy and Space. And I'm afraid that, for perhaps rather frivolous reasons, I thought that the last sounded fine. So I chose the astronomy and space division – and that really was the most wonderful job.

MG: That was?

HA: That was in August, 1972. An interesting period. Fred Hoyle was chairman of one of our committees. Key areas and people included radio astronomy, of course – the two ruling kings or barons being Martin Ryle at Cambridge and Bernard Lovell at Manchester, Jodrell Bank. They fought each other for SRC money, tooth and nail. They had completely complementary techniques. Manchester had a big, steerable dish of 72m diameter (250 ft), and looked directly at the sky; Cambridge had an interferometer, which was much more sophisticated with a much higher resolution. Bernard Lovell had totally overspent on his dish and was only saved by Sputnik which he observed just after launch in 1957. Sputnik broadcast signals which Lovell found to be in teletext form [I believe]: so he sent one of his people down to London to somebody he knew in the *Daily Express* (or some other national) newspaper – who lent him a teletext machine which he took back to Manchester. They fed the satellite signal into the machine and, lo and behold, out came a message in Russian about the satellite's performance. That was a tremendous boost, because the Russians were not releasing any results; so Britain stole a march on everyone else. After that the Treasury, who'd been threatening to prosecute Bernard Lovell for overspending his budget, became much more amenable! I got to know Lovell quite well from 1972 and, of course, Martin Ryle. [Sir Bernard Lovell is still alive – and kicking.]

On the ground-based optical astronomy side, there were the two big observatories: the Royal Greenwich Observatory (founded in 1765), which had been moved from Greenwich in about 1960 to Herstmonceux Castle to the south of the London, where the air was more clear. And the Royal Observatory at Edinburgh, ROE, on Blackford Hill, not far from the centre of the city. The ROE also had problems: the telescopes were frequently inoperable during the winter because of the bad weather, and in the summer because even at midnight the sky was still not really dark. The observing conditions were so bad at ROE that the astronomers started looking for better sites overseas sooner than did those at RGO.

What was needed was a big ground-based telescope that really would be world-class. So, in the end, the Science Research Council, after much study and discussion, decided to build such a telescope in collaboration with other countries, on La Palma in the Canary Islands. But we

also built in Hawaii, on the top of the Big Island, on the 14,000 foot [c. 4,200m] peak called Mauna Kea, a large infra-red telescope (UKIRT), which was above most of the water vapour in the atmosphere. This has proved extraordinarily successful and has done wonderful work in these areas. We also built on Mauna Kea a 10m dish operating at sub-millimetre radio wavelengths to look at molecules in space. The Netherlands collaborated on both projects.[5] That was a most exciting period for British ground-based astronomy.

On the space side [in the UK], most of the scientists were physicists and pragmatists who knew how to get their way. They were good "politicians". But I must say that there was a tendency, then, for the ground-based astronomers frequently to argue amongst themselves; they'd argue literally about a bicycle to ride to an observatory, and who should pay for it. Key figures on the space side were [Sir] Harrie Massey (a physicist) who was, of course, very much involved from the beginning of space science; and Robert Boyd, also at University College, London and, subsequently, director of the Mullard Space Science Laboratory (MSSL) – partly funded by the Mullard company – to the south of London (at Holmbury St. Mary). Boyd had a good number of research students, and these included Ken Pounds and Peter Wilmore (who later went to Birmingham). Pounds subsequently went to Leicester University and really got space astronomy going there. He has had an extraordinarily successful career, particularly in x-ray astronomy. [And Peter Wilmore was also most successful.]

The MSSL was also concerned with the ionosphere and solar-system astronomy. There was a big division between the solar system people who were interested in more classical physics and the Earth's immediate environment, and the extra-solar; and the galactic astronomy people, involved in the discovery of distant galaxies and the expansion of the universe, the Big Bang and so on. They were surprisingly different in their outlook, and in the techniques used. To begin with, space science was very much concerned with the Earth's environment and the Sun – because the subject started off with sounding rockets like Skylark, using the V2 rockets that were captured after the war. Werner von Braun, of course, the German rocket expert, was taken to the States very soon after the War ended. Sounding rockets were much used to investigate the upper atmosphere and the ionosphere.[6] That was a very profitable field.

There was at about this time a young physicist, Mac Robins, who worked at Farnborough, and he was sent off as soon as the war had ended to try to get V2 rockets and equipment for the UK. The Americans were doing the same. I'm not sure of the exact order of things, but he was in touch with Massey and so they really went all out to get some of the rockets. Mac Robins, you know, has written a book with Harrie Massey called *History of British Space Science*. And so the British space programme began, very early, through those sounding rockets. I think the first one was launched from Woomera in 1958, was it? 1957, yes.

There was a remarkable feature about this period involving the Royal Society (which, of course, is purely an academy and is not supposed actually to *do* research – rather it's supposed to discuss science, influence people and appoint fellows and so on). Because there was no existing UK institution to look after this new subject, space science, the remarkable Harrie Massey persuaded the government to give a grant to the Royal to develop a space science rocket programme under his (Massey's) leadership. Mac Robins was seconded from

[5] I might note that this was under an agreement between the SRC and ZWO, its Dutch equivalent, to cooperate generally in ground-based astronomy. This agreement was initiated between Harry van der Laan and me – between "the two Harrys".

[6] I said: "I guess they discovered the Van Allen Radiation Belts". In fact they were discovered in 1958 by NASA's Explorer 1 and 3 satellite missions.

Farnborough to help Massey. Massey was then also head of the physics department at University College London, not far across London from the Royal Society. So the Royal Society and UCL became the nucleus of UK space science.

As I've already said, Harrie Massey was one of the most remarkable people I've ever met and I got to know him very well after 1972. Massey had come from Australia to England in 1929, years before the war, and had worked with Ernest Rutherford at Cambridge. During the war he worked on anti-mine detection and, I think, invented ways of detecting and dealing with magnetic mines. For Harrie Massey it was an interesting war as indeed it was for a lot of scientists.

Now, after the war, Britain felt it had in a sense, won the war – with a little help from across the Atlantic – and Britain was well advanced in most areas of technological warfare: radar, the bomb and nuclear energy, aircraft and so on. At the same time, continental countries were just ruined, particularly Germany. The French and the Germans, you know, immediately after the war, were, or their leaders were, absolutely determined (particularly the French, I suppose) that German boots would never march through Paris again. The recovery of Germany had been greatly helped by the Americans through the remarkable Marshall Plan. (The allies had done entirely the wrong things, of course, after the First World War, driving Germany onto its knees and so creating the conditions, including hyper-inflation, under which Hitler could arise.)

So, they were absolutely determined, under de Gaulle and Adenauer, to work together essentially to make the two countries mutually dependent. They quickly decided to work together (amongst other things) in science, particularly, in big science. John Krige in his history of CERN, covered this very well; in fact, the whole first volume is concerned with the time leading up to the signing of the CERN convention, because this was such an important event, politically, for Europe.[7]

Britain was initially very sniffy about the idea of CERN and almost didn't join the organisation at all. Indeed, in due course, the UK also didn't initially join ESRO, either. We thought we could do it ourselves. For example for space, the UK had excellent relations with the Americans and they were launching anything we wanted them to: why should we, we spoke English, bother? But Harrie Massey had a much wider view and had very good friends both in the States and on the Continent. He knew all the people who counted scientifically in Europe: together they steered this political desire between France and Germany to do something together in science. Of course cooperation began with the iron and steel communities, and the other things that preceded the Common Market. But, as I've already said, France and Germany wanted to cooperate in science. I have the feeling that to begin with, it didn't matter actually what the science was (though nuclear physics and space must have been seen as very important in winning the War): it was just working together to achieve *something* big that was beyond, in a way, the capacity of either of them. Perhaps it was almost by chance that the first effort was in nuclear physics and not something completely different. Harrie Massey was much involved with the beginnings of both CERN and of ESRO. [He was the first Chairman of the ESRO Council]. So those were the ideas behind the beginning of ESRO. And, finally, of course, Britain did join both CERN and ESRO.

There was, of course, a corresponding organisation to ESRO called the European Launcher Development Organisation (ELDO) in which Britain played a major role to begin with, in part

[7] A. Hermann, J. Krige, U. Mersits, D. Pestre, *History of CERN, Volume I. Launching the European Organisation for Nuclear Research* (Amsterdam: North Holland, 1987); *Volume II. Building and Running the Laboratory 1954-1965* (1990); J. Krige (ed), *History of CERN. Volume III. The Years of Consolidation 1966-1980* (1996).

because it had already developed a successful ballistic missile called Blue Streak. The idea was to build a European launcher by sharing responsibility between different countries. Britain was to provide the first stage based on Blue Streak, France the second stage and Germany the third. But the launcher never worked as a whole. [8] Note that Blue Streak always worked correctly in the various launcher tests.

Britain at this time was keen to have the technology to do almost anything. At Farnborough, in particular, was a remarkable man called Desmond King-Hele, who did a lot of the theoretical work on orbits – and geostationary orbits in particular. Based on these ideas, it was decided to launch a UK satellite using a UK launcher [Black Arrow] in a low-Earth orbit from Woomera in Australia. The programme was eventually cancelled – just before the first launch, which was successful [on 28 October 1971]! This was fairly typical. The cancellation set a pattern (or continued a pattern) in that we would depend on the Americans for launchers. The Americans had beautiful launchers, and we assumed that they would always do what we wanted. We didn't feel they would ever turn us off, turn off the supply. They were trusted as colleagues in the war – but it was really quite different, of course, for the French and the Germans.

The French have always had a grand approach to everything, really. They believe in grand projects; and they believe in words like autonomy. De Gaulle particularly believed in this concept, from near the beginning of the French space programme. Autonomy and cooperation, were the two things – and autonomy meant not having to rely on the Americans or anyone else for launchers or for approval for launching satellites. For example, if one wished to have a remote sensing satellite and to use it for military purposes, then the Americans probably wouldn't agree. The Germans, of course, had invented the V2 rocket, but they were just dead scared of doing anything independent in space because it might look as if they were preparing for future world domination, as they had in the past. Their economy had, after all, been saved by the Marshall Plan, and so their orientation was very much towards America. All these elements – cooperation with the Americans, the development of a European launcher, and a satellite programme – came together eventually in the formation of the European Space Agency. Of course, before that was ESRO, but ESRO was a purely scientific organisation: although I'm sure that in supporting ESRO, the participating countries had well in mind the other interesting things that they could do in space – just as for CERN, although it was basic science, people knew that the bomb had won the war and thought that the technologies involved in nuclear physics might be useful in important applied areas.

ESRO was set up with funding on the basis of GDP, gross domestic product, or the National Income.[9] Thus each country paid according to its wealth. There was one proviso, however, that no country should pay more than 25% [of the total] because then it might dominate the organisation. As Britain was the only country at that point that would have paid more than 25%, [I suppose that] Britain thought that that was a good idea! ESRO worked quite well, although there was always a certain argument as to whether its science should concentrate on geophysics or shift more to astronomy. But also it gradually became apparent that there were other interesting things that could be done of a more applied nature. Thus, ESRO started to develop programmes for meteorological satellites and communication satellites, so it gradually changed its nature – but the funding was still on a GDP [strictly, National Income] basis. This became increasingly clumsy because different countries had different objectives and were prepared to pay accordingly.

[8] All the stages were "active" in the ninth and last, firing, but the satellite failed to orbit – 12 June 1970.

[9] In fact the ESA Convention says that the scale of contributions should be based on "the average national income" for each Member State…

Thus was developed the brilliant concept of the *core programme* (which was mandatory and GDP, National Income, related) plus *optional programmes* for applications, where countries could put in whatever money they wanted; and that became a tremendously successful concept for the European Space Agency.

These concepts were discussed in meetings of the European Space Conference (ESC) in which ministers from different European countries interested in space (through ESRO, ELDO and CETS for telecommunications) got together to discuss broader matters from time to time. It was beginning to get increasingly clear that ESRO didn't have the right machinery to march successfully into the future and yet it wasn't clear what should be done. I should mention that the ESC Ministers were supported by a Committee of Alternates, in which delegates from the different countries prepared future plans. I often attended this committee for the UK.

The thing that, in a way, crystallised a new approach to European space was a series of meetings in *Palais d'Egmont* in Brussels; some remarkable individual ministers were there. I started to get involved in all this from late in 1972, because of my (new) job as head of the Science Research Council's astronomy and space division. The SRC was at that stage the main user of space in the public sector apart from the Ministry of Defence – and the Ministry of Defence had a completely separate programme, by definition outside of ESRO or ESA. The applications side came under the Department of Industry (or Trade and Industry, DTI) so the UK delegation to these European discussions became an almost equal partnership of science and applications. *The two sides within the UK were completely separately funded.* The SRC got its money direct through the Department of Education and Science, and it was absolutely clear that that was for science; and nothing else. The Department of Industry was rather interventionist at that point – it still had a bit of the old Ministry of Technology in it – so we formed, actually, a rather interesting delegation with these two parts.

MG: Did the Post Office have an interest in satellites at that time?

HA: I don't think they did. I mean obviously the Post Office did a lot with a big satellite dish [opened in 1962] at Goonhilly and so on, but I don't remember any substantial direct interest in their developing satellites. The British National Space Centre, came later. When was that?

MG: That was the 1980s.[10]

HA: Yes, so I mean there were briefing meetings, for the delegation, but I'm slightly jumping ahead. I don't remember the Post Office ever having much to do with it.

The discussions in the ESC went on for several years but a major step forward was made in 1973 or 1974. The first moves to change ESRO, and to bring in applications in a more sustainable way, became called the First Package Deal (1971). The concept of *package deal* finds its way through as a wonderful way of reaching a compromise: the combination of having a package deal *and* having *à-la-carte* or optional programmes, was that you could build a package which had something in it for everyone irrespective, almost, of the amount of money each country had. That was all very well, but it was difficult to decide on a new programme, a new applications programme, which other countries were promoting, unless you had your own favourite project to trade with it.

MG: Hmm, OK.

[10] The BNSC was set up in 1985, and announced at ESA's Council meeting at ministerial Level held in Rome in January of that year.

HA: This meant, from time to time, a somewhat artificial position where one had to wait for a whole collection of different things to come together of sufficient interest to engage everyone, more or less; and then to say, right, we'll do this if you do that before finally marching forward together. Of course, that means that having got one package deal going, you have to wait for another for five or six years, before you could form a new package deal. So the process had its pros and cons. To begin with, for a few years after 1971, almost all the discussion was about *what the programmes should be*, not *what the organisation should be* to implement them.

MG: OK.

HA: The French, slightly to reiterate, the French wanted at that stage an independent launcher and they were pretty determined to do that, alone or not; but they preferred to get in some other partners with money to contribute. The Germans were determined, as I said, to build up a good relationship with America, with NASA; and Britain, as usual, mainly knew what it did not want! But it really did want to specialise on building (or being the prime contractor for) satellites which were at the heart of all really successful applications. We had no interest at all in manned space flight or in building launchers. For satellites, this particularly involved the British Aircraft Corporation (later British Aerospace) and Marconi.

At this stage, in about 1973, our friend Michael Heseltine was appointed a junior minister in the Department of Trade and Industry. This was his first ministerial appointment. I understand that he called his deputy secretary – a scientist called Iuean Maddock (who had been director of the Atomic Weapons Research Establishment, AWRE, Aldermaston) to ask what he, Michael, should concentrate on. He was told, I mean presented with the fact, that European space was in a bit of a mess – but that this was a problem which was capable of a satisfactory solution. Michael Heseltine, not surprisingly, as a young minister, really wanted to make his mark quickly. Another of the options was, I think, to try to solve the problems of the British Steel industry; but that was an intractable problem which could take many years, and he didn't see that there would be many real accolades for that.

So he decided (I understand) that space was indeed an area of manageable size – that in a finite time he could really make his mark on – and he jolly well did. I went with him on at least two occasions to ministerial meetings in Brussels, each time in a small plane. The first occasion was in a nice little jet. This was quite cosy really, and he asked, and I told him, how the Cabinet Office worked. Later, we went in a bigger plane of the Civil Aviation Authority, which was normally used to test landing navigational systems: it was full of special electronic equipment. Each time, Heseltine was really remarkably effective. There was one occasion (in fact, it must have been the last occasion??), which was held in the *Palais d'Egmont*. Because fog was predicted, the DTI man, Harold Robinson, and I went across by train to Brussels and we were there bright and early next morning, as all the ministers were gathering before the meeting in the amazing hall of the *Palais*. (I believe that some called it Belgium's Concorde, for its grand design – and cost.) The press came in as the Ministers took their seats – but the front seats for the British delegation, where the minister was to sit, were empty. So my DTI colleague and I moved into the two front seats to cover his absence (he'd been delayed in Heathrow by the predicted fog). As the press cameras went past, they just looked at us, quickly realised that we weren't the real thing, and went on their way.

The meeting could not begin without Heseltine, because he was to make key, important proposals. The ministers were not too pleased; but Michael Heseltine finally arrived, several hours late, and his performance was tremendous! During the meeting I was sitting immediately behind him: a number of ministers of other countries were really trying to work him up and contradict his proposals. I could see the hair standing up on the back of his neck,

but he remained absolutely in control, and played a beautiful hand. He described (in his talk to the ESA history meeting in 1998 at the Science Museum in London – you must read it) how the final session went on all night and how, as we all emerged, blinking, into the early morning sunshine, he was ready to tell the Press of his triumph – but instead they descended on a UK official, believing that he was the minister… (I am more or less quoting…).[11]

The key to what Michael Heseltine did was as follows. Firstly, he had a very poor hand. He had little money and so had looked around before these crucial meetings to see whether he could pull together all the UK civil space expenditure into one envelope to form, if you like, what might constitute essentially an executive, independent British National Space Centre. The first thing he wanted was the Science Research Council's money – both that spent nationally *and* the science part of the ESRO subscription. In my meetings diary of that time, I'd noted: "Hes. wants our money". A few weeks later (I'm looking now at my diary), on 4 December 1972 (three days after a meeting of the Committee of Alternates, which I'd attended) I see that I had a meeting with the chairman of the SRC about "his (Heseltine's) plot to take our money". On 12th December, there was a meeting between Margaret Thatcher, Heseltine, Cottrell and Flowers. Margaret Thatcher was then Secretary of State for Education and Science, and Alan Cottrell was Chief Scientific Adviser to the government (having recently succeeded Solly Zuckerman). Brian Flowers was chairman of the Science Research Council. The meeting was to discuss the SRC's retaining its space money, the phasing of national programmes, and the proposal for UK to cooperate with its counterparts in Europe to avoid duplication of domestic space programmes. The result was that Heseltine was told that he couldn't have our money. On 20 December, there was a meeting in Brussels at the *Palais d'Egmont*, with Lefèvre in the chair but, I believe, I didn't attend because I was committed to other meetings.

I think that that decision (on the SRC's space money) was right, because it's a fundamental principle of the UK government that, broadly speaking, government departments have functions – and HMG doesn't generally have a department responsible for the *means* for doing something, rather than the end. So the Science Research Council retained its money, including the UK share of the ESRO budget, for space science. And that's a pretty important principle, though it's got its good sides and its bad sides. (Perhaps one can find exceptions?) But undoubtedly that was a key meeting.

And then, oh, discussions continued; and there was already a drafting committee for the new ESA convention; I was on that committee.

MG: Who had put forward the plan? Was that Heseltine?

HA: Oh, sorry. Yes, yes. This was crucial. I beg your pardon; I've jumped ahead too far. What happened was that the UK government agreed that Heseltine should make the following suggestion: that the UK would join a new agency provided that the new agency would include the means of space transport (which ESRO didn't) as well as the satellite side, and that – as well as a mandatory programme – there would be optional programmes, as I said before, which countries could join on an *à-la-carte basis*. Above all, Heseltine would say that the UK (DTI) would fund half the cost of a telecommunications satellite, MAROTS, for a communications system between ships at sea – which couldn't then be otherwise done (except through unreliable hf radio). This was an important application for Britain as a maritime nation. Also, this proposal would not tread on the feet of Intelsat and the other existing communications organisations. The other thing, of course, was that the proposal was for peaceful purposes. The latter had been taken for granted for ESRO, as a purely science

[11] M. Heseltine, "Britain and ESA", *Proceedings of an International Symposium, Science Museum, London, 11-13 November 1998* (Noordwijk: ESA SP 436, June 1999), 25-29.

organisation – but in the new agency it was very important to put in this condition explicitly – if only because the Swiss and the Swedes simply wouldn't have signed up otherwise.

The other element that Heseltine put in was *an organisation*, but an organisation which was to cover everything: to have these obligatory programmes and also the optional ones. *But it also had an industrial policy*, and that was extremely important (I believe that ESRO did not have such a policy). Thus for ESRO there had been no requirement to share the contracts between different countries in any formal way.

MG: But they had a sort of loose…

HA: Perhaps, but I think that on the whole they just went to whichever contractor would do the job best. I believe that Britain did quite well out of this, because it had a good aerospace industry – and so did France. I guess that Germany probably didn't at this stage, because it was only beginning to recover from the War. [I may be wrong about Germany.] So those were the main elements. I'm probably exaggerating a bit, but I don't think that the French could have cared less about having a new organisation. They just wanted to get going with their rocket launcher – which was initially called LIIIS (*Lanceur à Trois Etages de Substitution*). This wasn't a very sexy name, so there was a competition and eventually it became called Ariane, which was a bit more romantic. [Peter Creola was, I think, involved in the naming process.]

The Germans, and I'm still talking about the development of the Package, the Germans wanted to join what was called the "Post-Apollo Program". The Americans had got to the moon, NASA had got to the moon, following Kennedy – and NASA was moving to what they called Post-Apollo (Apollo being the rocket that took everything to the moon). NASA had built up to an extraordinary annual level of funding during the Apollo programme: if one looks at the change of funding over time, then it is quite astonishing really: the US civil space expenditure – mostly for the moon flights – rose rapidly with a peak in 1965. The US defence expenditure, which was the other part, really only started developing heavily a bit later than that. Of course both were bigger by an order of magnitude than anything in Europe. But, after the Moon landing, the Americans were desperately looking for a way to get more space money to keep up annual expenditure, I mean NASA's future depended on it.

Thus, NASA proposed the Post-Apollo Program. This programme was, essentially, a space shuttle: a new way of launching satellites, and big satellites, from a reusable vehicle. And the space shuttle was going to be very cheap. I remember, I mean at the time probably even of my period at the Cabinet Office, you know, the cost per kilogram was going to be very low, far cheaper than for an ordinary rocket, and they'd reuse the shuttle and so on. Of course, the shuttle was "man-rated": it carried people and so it had to be man-rated. And as it gradually became apparent, man-rating requires a completely new, totally different order of magnitude of reliability. If you lose one automatic satellite in five, so be it, but if you lose one shuttle, especially if it's got a female schoolteacher on board, then, you know, you just cannot do it.

In the light of the Post-Apollo programme, the Germans, as part of the European package for ESA – and obviously having consulted the Americans – proposed Spacelab, which would fit exactly into the shuttle cargo bay and be entirely dependent on the shuttle's services. This was how Germany proposed that Europe would contribute to the American manned space programme. At that stage, the French, I think, were not really interested in man in space because they were much more concerned with having an independent launcher on which they could put automatic satellites for military communications, for earth observations, and for meteorology – and even perhaps for spying? So they really were more practical in their objectives, although this required a grand concept – Ariane.

At the ministers' meeting, the principle as expounded by Heseltine for a European space agency with all the factors I mentioned before, was accepted. There was still a long way to go to work out the details, but the principles were agreed. The other elements of the package began to fall into place. Germany said it would come into Ariane with a relatively small percentage. For Spacelab, Germany was to contribute 60% (I don't remember the exact figures), the French a smaller percentage, and Britain offered to put in, I was going to say 5% or 2.5% – but in fact they said so many millions of pounds, effectively as a cash limit.[12] For Ariane, Britain's contribution, partly symbolic, was to be twofold. Firstly, Britain was to provide the "claws" for the Ariane launch pad at Kourou (the French launch centre in French Guiana): the launcher had to be held down with great claws that hinged up and grabbed it at the bottom. It was only when the engines were fired, and really pulling, that the jaws would be released. So British jaws were our one contribution. The only other contribution was to provide the inertial guidance system of the rocket – which was, of course, extremely important. Britain had (Ferranti had), built inertial guidance systems for missiles and so on. Of course, at that stage there was no GPS to guide things, so an inertial guidance system was very important. There's the whole other question of the politics of Kourou, and launch sites in general, but I won't go into that here.

Regarding Spacelab, the next question was, *what was it going to do*? How was it to be used? Everyone wondered about this. The Germans said, oh, there are lots of things that it can do. And, of course, their scientists said, yes, we've got good things to do in zero gravity; they were talking, for example, about metallurgy in space, and about growing beans in space. But in the UK, because all these ideas looked like science, the Science Research Council was consulted. At that stage we, the SRC, had a biological sciences committee, one for material science, and also committees for the whole range of science which we called "space science" (essentially geophysics and astrophysics). We put these proposals to our various committees, but none of them would even give them a gamma, for quality. The committees of scientists were just completely against spending their hard-pressed funds in this way.

We did manage to find that there were one or two people in Britain who were quite keen on the idea of using microgravity. One was a wonderful man called Heinz Wolff (then at Brunel University) who was an absolute character and really, you know, he'd done an awful lot in popularising science. I remember he invented "the Egg Race", for children's television. It was wonderful. I think he also had connections with Kodak in the UK. This general opposition by UK scientists was all very difficult, but it seemed a pity if Britain wasn't going to have *any* experiment on this thing (Spacelab) – and could see no reason for justification for it. There was also a problem about how this microgravity work should fit into the structure of the ESA programme. The ESA science programme committee certainly did not want to pay for it.

I'll leave that in suspended animation for a moment, and go back to the key meetings in Brussels in December, 1972, and in 1975, when Ministers finally agreed to proceed with ESA. The ESA Convention was in draft, but it was decided at a relatively early stage that ESRO would henceforth "trade" under the name ESA, though it was still legally under the old ESRO Convention – to give it a legal personality. A provisional, draft, convention was being developed during this period.

One of the interesting things about the draft convention was that to begin with *the position of the science programme hadn't been very well defined*. It was generally agreed that there should be a science programme, but in the drafting (I attended quite a lot of those meetings)

[12] I believe that the main contributors were: *West Germany (53.3%), Italy (18%), France (10%) and the United Kingdom (6.3%).* The UK contribution may have been expressed as a cash sum.

the French, particularly, said that the science programme should be controlled directly by the ESA Council and by implication could be, in the sense, subject to political whims. (Of course they, the French, wouldn't have put it this way.) The SRC was, of course, completely against this because we wanted to have an independent, quote, unquote, *independent* science programme committee which would be run by scientists, and would make decisions on scientific grounds, not on political or industrial ones. The SRC would, of course, recognise that a science programme committee would receive a budget from the ESA Council itself; and that that budget would be agreed from time to time as part of an agreement for the overall level of resources. But once that envelope had been agreed, then all the scientific decisions should be made by the science programme committee. As I said, the French didn't like this idea at all and there was a meeting of the Committee of Alternates, probably in February, it may have been on 25 February, 1975, which I attended. "Alternates" meant the officials who were standing in for their ministers, and that was under the European Space Conference. At that meeting there was discussion of where science should fit in; the French said that there could be a science *advisory* committee which would advise the Council, but that that committee would have no direct responsibility for defining the actual programme. It would be the Council itself which took responsibility for that.

I voted on behalf of the UK against the French proposal; but it was either 13 to 1 or 14 to 1, countries against me: the French had everyone except me on board. I went back to the UK that night, I suppose, or the following morning, and immediately picked up the phone and called Harrie Massey and Bob (Robert) Boyd, proposing that they should get in touch with the leading space scientists in all the relevant European countries to alert them to what was happening.

Harrie Massey then chaired a group called the Provisional Space Sciences Board for Europe. It was entirely non-governmental, and consisted of the key people who were really concerned with, and doing space research in the different European countries. There was an American equivalent group [probably the Space Studies Board of the National Academy of Sciences, established in 1958] and Massey had a beautiful relationship with them. His committee and the US one, often talked completely independently of ESA and NASA, who were sometimes seen as the big bureaucracies. Following my talk with him, Massey contacted every single one, every single person, of the key people in the different European countries (except one), and on 6 March – realise this was already 25 February – by 6 March, Harrie Massey sent a telex to ESA. This made it absolutely clear that there should be a mandatory science programme and that that should be controlled – once the level of resources had been determined – by an independent science programme committee. It was signed by: Amaldi, Boyd, Elliot, Falthammar, Geiss, Herlofson, de Jager, Lüst, Massey, Occhialini, Peters, Stromgren, Swings and van de Hulst. Only Professor Blamont could not be contacted to sign in the five or six days we [Massey et al] had.

I was absolutely amazed at the way, and the speed, with which this "Mafia" operated. I mean they'd got well-oiled procedures, in a way, because I guess that they'd been doing the same sort of thing regarding the formation of ESRO, and many of them had been concerned with the formation of CERN.

And that meant that the next meeting we had of the Committee of Alternates (on 3 April) – that's in less than a month – there was another vote and it was unanimously agreed that there should be exactly the sort of arrangements that the UK had originally called for. The French grudgingly joined in, but they said they really thought it was not a good idea. That was quite a crucial point in the development of ESA. This decision was taken very close to the date of the final agreement of the substance of the ESA Convention – which was 15 April. It was

also very important that the science programme was to be mandatory, and that each country's contribution would be in proportion to their National Income. If it hadn't been mandatory – if every science project had had to come up as a separate item with its own funding – I think after a while the science programme would have just disappeared completely, because different countries have different priorities.

Let's just pause again.

The different European countries had very different internal arrangements for managing and funding their science and applications programmes. The UK, as I said, was very much separately funded for each, whereas in some other countries, all space activities came from one budget: that's partly a good thing and partly a bad. If all the space work comes from one budget, then you can trade off between the amount spent on science and the amount spent on applications. This was almost impossible in the UK.

Of course the amount spent on applications could be enormous – for example when it came to a developing rocket launcher – so the proportion of ESA's budget going to the science programme would be a small proportion of the total activity.

However, to join the "club" of the European Space Agency, you had to join what was called the Mandatory Programme. This, in fact, consisted not just of the Science Programme but also included a technology programme which was based on ESTEC and which developed techniques and so on, and provided facilities for testing spacecraft. The cost of the technology programme, together with the administrative costs, was called the General Budget, and this was a substantial amount.

As the plans for the new ESA unfolded, with this Package deal all these different programmes (including that for science) could proceed. However, the means for controlling the optional programmes were very different from those for the mandatory science programme, in that each of the former had its management arrangements and its own project manager.

In the case of Ariane, the programme was not surprisingly dominated by France – and this meant by the centre at Toulouse, which was part of the French space agency, CNES, a very powerful organisation. Toulouse already had equipment for doing essentially all of the testing required on satellites and components involving vibration as well as space-environment work. So most of the technical work on Ariane was done at Toulouse with French equipment; they did not really need (or want?) to use the facilities at ESTEC. The same applied in varying degrees to Meteosat and to other remote sensing satellites.

At the same time, a full range of testing facilities was developed at ESTEC in the Netherlands, under the control of the ESA member states collectively. Projects of the mandatory science programme were tested at ESTEC. In contrast, as I've said, each applications programme was much more directly under the control of the individual states which were financing it, and it was difficult to require them always to use the ESTEC facilities.

There was another, important, difference between the mandatory and optional programmes: the latter each had a built-in contingency allowance (perhaps of about 20%), budgeted from the beginning – and if the overall budget (including the contingency) was exceeded, then individual States could legally withdraw. This imposed a powerful internal discipline to keep within the budget. However, for the Science Programme, if one project overran, then that could often be accommodated relatively easily, usually by delaying new projects in the pipeline, which was not satisfactory.

And so, from the science programme committee's point of view, a feeling of dissatisfaction arose both because the programme essentially had to use, *and pay for*, the testing facilities at ESTEC; and also because the project control at ESTEC sometimes seemed insufficiently rigorous.

In particular, the science programme was essentially forced to use a European launcher if an appropriate European launcher was available, except as follows. At a meeting at Bad Godesberg, some time before, it was agreed that a US launcher could be used if the cost of an equivalent European launcher was more than 125% of the cost of the US one. (I think that this was the figure.)[13]

I was one of the people who, in 1976, was unhappy with the arrangements for the science programme. The chairman of science programme committee at that point was Professor Hubert Curien (a very eminent French scientist who subsequently became Council chairman and then Minister for research in the French government). He and I got on very well. (In fact, subsequently he proposed me for the chairmanship of the Council, but we haven't got to that point yet.) I would talk to him, and we would often meet together before meetings. He shared my views on this matter, as did others, even though it was Toulouse which was causing part of the problem; Curien was a wise and open-minded person. So we brought this up at a meeting of the science programme committee. As a result, the committee decided to set up a small *ad hoc* panel to look into the structural problems of the science programme – which I was asked to chair.

This was the beginning of the so-called "Atkinson Report". [Specifically, the aim of the panel was "to maximise the cost-effectiveness of the mandatory science programme budget for the European scientific community".] The panel membership was: Livio Scarsi, a distinguished Italian astrophysicist; Peter Creola, the Swiss delegate to ESA (who was also qualified as a space lawyer); and Gérard Brachet, a young French space scientist, who later became head of Spot Image, and was subsequently DG of the French space agency, CNES – he was excellent. The panel was ably assisted by Dr Massimo Trella, then ESA's Technical Inspector. The report was submitted to the SPC on 10 May 1977.

So it was a good group; we had the various elements which we needed. We started meeting and fairly early on decided that we would have to go to ESTEC to see how all these things worked in practice. We couldn't of course go to ESTEC without getting the permission of the director-general of ESA (Roy Gibson), and after some discussion, he of course agreed that we should go; indeed he, as far as I can recollect, was civil enough actually to come with us, at least at the beginning of our visit. We had a really thorough look at ESTEC. We saw all the facilities and talked to many people there. [We were also accompanied on our visit by the Director of ESTEC, J Berghuis.]

There was a very important element of ESTEC, the Space Science Department; this had two functions really. One was to help to manage the science programme's projects (and possibly other projects), usually satellite projects; but also, in order to get good people, the Department's staff members were allowed to spend up to 50% of their time working on their own research projects. This was essentially the SSD's second function. [Note that, regarding its programme, the SSD reported to the Director, Science in Paris.]

Interestingly enough, there was a very similar arrangement at the UK Science Research Council's Rutherford Laboratory – where there were also major facilities provided for use by university scientists, for example the proton synchrotron, NIMROD. This type of

[13] Resolution No. 5, *Production and use of European launchers*, adopted on 14 November 1968, CSE/CM(November 68)22 (Final).

arrangement was almost certainly necessary to get good people. But there was often a certain tension between the special interests of the in-house people, often for a particular science instrument on the satellite (in the case of SSD), and the broad objectives of the mission as a whole. In reviewing all aspects of the SSD, we considered this possible clash of interests. We felt that the in-house people sometimes had a bit of a stranglehold (that's going too far), but they had a strong (undue?) influence on the way a particular satellite developed under the SSD project management.

At ESTEC we also looked into the cost per hour of using the various facilities. For example, there were a number of big vibrators, big enough to vibrate a whole prototype satellite – which could weigh many tons. This was to simulate the vibration that the satellite (or its instruments) would experience from the rocket launcher at launch. There was one vibrator, I think, called the 14 ton vibrator, because it could take a satellite of that weight. The satellite would be mounted on a platform and shaken by devices like huge loud speakers; these actually had played through them a record of the real spectrum of frequencies of sound, at intensities which were experienced in practice.[14]

However, we found that the Centre's 7 ton vibrator cost 50% per hour *more* to use than the 14 ton one – because it was used much less often. And, of course, for the science programme, it was often *smaller* things that needed to be vibrated. These services were costed by ESTEC by taking the total cost of providing that vibrator during the year, and then dividing by the number of users and the time each took. If there were very few users, then each paid a large amount. *We said that it was quite wrong that the science programme alone should essentially be keeping the whole infrastructure going – when the application programmes were not required to use them.* That was a particular grumble.

Anyway, we made our recommendations; that was, I think, in 1977 [10 May 1977]. The recommendations[15] covered, in addition to the work of the SSD, and the charging for the use of Centre's facilities:

- the choice of candidate missions and preliminary studies;
- phase A/B studies and approval of Projects;
- the responsibilities for executing the science programme;
- technology, design philosophy and choice of programmes;
- residues and under-used capacity.

We also considered that the future planning for the Programme should be the responsibility of the Director, Science – rather than of the Director of planning and future programmes.

I left my close connections with ESA in about 1987, but the consequences of the review were still rumbling around in 1990 and beyond. It clearly had a deep influence and was a deep irritation on a system which had perhaps got a little bit too easy. In any case, ESA always seemed to be enormously expensive compared to any other way of doing something. [However, ESA missions almost always actually worked!]

MG: OK.

[14] Above, and in what follows, I have corrected the figures I gave for the large vibrator and for the smaller one, but the message is exactly the same.

[15] In my interview I did not go through all the recommendations of the Panel, which are set out in ESA/SPC(77)17. I would urge anyone interested in the effectiveness of ESA's science programme to read this paper, even now! Perhaps the paper should be put to the Science Programme Committee every year or two.

HA: I think on the whole it [the ESA way] was probably not more expensive than if one had had to do the same project entirely by oneself. [For example, if we'd gone it alone, we, the UK, would have had to pay 100% of the costs, rather than 25%. ESA could not have been four times more expensive!] But to see better the UK frame of mind about ESA, I'd now like to drop back a bit in time.

The British space scientists had always worked with NASA from almost before the time of ESRO, and as ESRO was in operation. The British scientists were very good; the sort of people I'm talking about were, of course, Harrie Massey and particularly Robert Boyd and the people at the Mullard Space Science Laboratory, which was part of UCL (University College, London). Boyd, a professor at UCL, was also head of that Laboratory. Also very important were his students, for example Peter Wilmore and Ken Pounds – who in due course went out to form their own space research departments at Birmingham University and Leicester University, respectively. They were very competent and could build beautiful instruments. This ability had come, I suppose, partly from testing their ideas on Skylight rockets – which also, of course, vibrated when they took off! The UK groups were thus welcomed by the US space scientists, in part because of their expertise in x-ray astronomy or whatever. A satellite usually included a number of experiments; and NASA would provide (and pay for) the basic satellites with mostly American experiments and, of course, the launcher – and the UK would provide some instruments in the package.

The formal arrangement by which the UK provided the instruments for NASA missions was through a very important device called an "MOU". MOU stood for *Memorandum of Understanding*, which really meant a gentleman's agreement. The MOU would say that NASA would endeavour to launch a satellite mission which had certain characteristics [at NASA's cost]; and the UK, *at its own expense and with its own management*, would endeavour to build one or more appropriate scientific instruments. But these were not legal obligations, which was most important. The American legal system was not well orientated towards international collaboration. For a legally binding contract, a *treaty* between nations was required – and a treaty is a very, very heavy-handed thing. But what it meant was that the UK did extremely well from the NASA programme – and (in turn) NASA benefited from the UK involvement.

When I came to be head of the SRC's Astronomy and Space Division, I found that there was an annual, bi-annual or even more frequent, mission that went to NASA from the UK to discuss future programmes and progress on existing ones. I would join each visit, though the emphasis was on the space scientists who were directly involved – people like Ken Pounds, Peter Wilmore, Bob Wilson and so on. We would have most productive meetings. Everyone got on well together and the US side trusted the ability of the UK experimenters. [I should also mention the International Ultra-violet Explorer mission, IUE, in which the UK (particularly Bob Wilson, who conceived it, and the SRC's Appleton Laboratory) worked with NASA. This mission was outstandingly successful. The seeds of this were originally proposed by the UK to ESRO in the LAS project, but this was not in the end accepted. [However, ESRO/ESA was responsible for IUE operation, which went on remarkably for many years, from 1978 to 1996.]

A good example of how the UK/US cooperation worked was the launch of the UK-5 satellite (renamed Ariel V, after launch) in October 1974 (see the photograph that I showed you earlier on). This was a NASA satellite including some American experiments, but it primarily comprised UK experiments and, in particular, an experiment of Ken Pounds on x-ray astronomy. X-ray astronomy was just beginning to become an extremely important field, an entirely new field, of astronomy. The satellite was launched on a US Scout launcher by the

Italian government – at their launch pad off the coast of Kenya, near Mombasa and Malindi. I attended the launch. It was a remarkable experience. The rocket was planned for launch on a particular day, but failed to pass all the pre-launch tests; these faults were corrected and it should have then gone up a few days later on a Friday – but Friday was considered to bring bad luck by the Italians, and they delayed it until the following week – while the gods were appeased. It worked beautifully in the end; we had an excellent party which included an Italian defence Minister and the Italian ambassador to Uganda, and everyone was happy.[16]

So the UK's space science programme, which was paid for by the Science Research Council, was extraordinarily cost-efficient. And then, gradually, at the time that ESRO and then ESA were developing, there were more and more pressures on the UK Science Budget; I suppose that these pressures became particularly strong by about 1977, with the oil crisis. I think there was then even talk of petrol rationing in the UK. The SRC realised that it needed to demonstrate that it was increasingly doing something even more useful, or more practical. It already supported engineering research (in universities) – but it was very difficult to get the engineers in university actually to apply for the grants and take them up! It was quite extraordinary, in fact, that the Council had to appoint special directors, one for each specific programme, such as its marine technology programme, to take the initiative in going round to universities and shaking them up so that they would then make use of the money which the SRC had available. At that time, every year the Engineering Board failed to spend its allocated money. (Eventually, in 1981, the letter "E" was introduced into the SRC's initials, so becoming the Science and Engineering Research Council. This was when there was perceived to be a threat of the creation of a separate engineering research council. That wouldn't have done at all!)

MG: OK.

HA: So, gradually the pressure increased for UK space scientists to use the ESA programme more, and NASA less. The former was vastly more expensive – partly because the launcher had to be paid for, and partly because ESA was much more bureaucratic (as already said). Also, of course, as time went on, the sort of missions required to do front-line science themselves inevitably became bigger and more sophisticated. If you needed a telescope, it had to be of bigger diameter – and so what you could once do with a Skylight rocket, for example, to look at the Sun, or from UK-5 to do x-ray astronomy, now required much more sophisticated (and bigger) instruments. [So this factor alone made wider partnerships more and more important.] However, the UK space scientists were not without resourcefulness, and so on one occasion we went to Japan to seek further opportunities there. Japan had an interesting space science programme which was completely independent of its applications programmes. The latter were run by NASDA, but the former at the University of Tokyo by Professor Minoru Oda, a remarkable man and scientist. Thus, in July 1980, I went to Japan with Ken Pounds (University of Leicester) and John Houghton (who was subsequently director-general of the UK meteorological office). The latter was particularly interested in the earth's atmosphere, of course, and in measuring the temperature of the oceans and so on. I'd love to go on about this visit but I won't. Suffice it to say that we formed a very good relationship with Japan in space science and they flew instruments from Ken Pounds' group in their subsequent x-ray astronomy missions.

There were yet other things going on, on the side. Nobody in UK space science wanted to be completely in the hands of ESA – beautiful organisation though it was, and is. For example,

[16] The Italian San Marco platform had been created by General Luigi Broglio (who was also professor at the University of Rome); his deputy was Major General Carlo Buongiorno – who was, incidentally, at the celebrations of the 200th ESA council meeting in Paris in 2008).

the Dutch decided with the Americans (with the UK joining in subsequently), to build an infrared astronomy satellite called IRAS. That would be the first time that the skies had been surveyed in the (far) infrared. IRAS consisted essentially of a big tub of liquid helium and the telescope was mounted in that – because it had to be able to go to very long wavelengths in the far infrared. An interesting feature of the (new) ESA Convention was that any proposed national mission should be offered to the Agency for "Europeanization". I'm sorry to say that I remember well helping to offer IRAS to the ESA science programme committee, but I'm afraid that we painted a rather unattractive picture of the new mission. Regrettably, I guess, the committee decided it would be a waste of ESA money to participate, so IRAS went on as it was. Launched in January, 1983, it became an extraordinarily successful mission. IRAS was subsequently followed by an ESA mission called ISO, the Infrared Space Observatory, which was also astonishingly successful, building on IRAS. So I think that worked out very well for IR astronomy in the end.

The pressure to move everything to ESA was very strong. It would be interesting to follow this up with others still in the space science area in the UK, to see what they think now. There was, in fact, a committee set up [in the late 1980s] by the Science Research Council under Mark Richmond, who at that time was vice-chancellor of Manchester University, to form a new policy on what should be done regarding ESA. The result was to apply even more pressure for UK space science to depend almost exclusively on ESA.

ESA has had to work pretty much on a fixed budget for science, but there have been attempts made to increase it a bit, from time to time. There was such a move before the ministerial meeting of ESA in Rome in 1985 – to increase the science envelope through a programme called Horizon 2000 – trying to look forward to the next century. This caused an enormous problem in terms of [overall space] package deals for the UK, because it would require a 5% increase in funds for space science – and the Science Research Council had other priorities and was not prepared to put in this additional contribution. This really became almost an international issue, because we were running quickly towards the Rome meeting, and we still hadn't agreed within the UK how we could swing the package. Finally, there was actually a meeting with Mrs Thatcher, then prime minister: she took money away from other government departments to fund this increase for ESA's science. It was relatively a minute amount of money but, in the end, the 5% was produced. It proved a very costly business in terms of blood within the UK.

MG: So by that stage you'd become Chairman of the [Council]?

HA: Yes. I'll explain. It was usual for the Council to have two deputy chairmen. By the way, the ESA Council normally meets at what might be called "official" level, but the Convention makes it possible for the Council also to meet at ministerial level on occasion, when there were really big issues. The ESA Council at official level (and I was a member of that from the beginning of my time in this area in 1972, when it was still formally the ESRO Council) prided itself on being able to solve almost all problems without recourse to ministers or, if you like, without recourse to extreme politics. But, occasionally, a ministerial meeting was clearly necessary, perhaps every few years or so. This has been particularly the case in the period of "package deals", in which major decisions were required involving large sums of money and "political" compromises. (I've already explained how the system can work through such deals.) In particular, some time before one package deal has been implemented, you have to start working towards a new package deal – for the next wedge of money.

The package deal in the mid-1970s, which lead to the creation of ESA and the programmes which went with it, certainly required meetings at ministerial level. However, when there were no major issues, bringing ministers together might not be in the interests of the agency.

This was, in my view, the case for the ministerial meeting in Paris in 1977. It was not really needed, and didn't do much at all. I'll leave it at that.

However, it soon became clear in the early 1980s that there would have to be another ministerial meeting because by then a number of major future issues had to be decided in essentially a new package deal. This meeting took place in 1985 – when I was Chairman of Council. Here is the background.

Having been a Vice Chairman (one of two) from July 1981 to June 1984, I was nominated by Curien to be Chairman following him [and this was approved by the Council] starting on 1 July 1984, and leaving the position in June 1987. [John Kingman, then chairman of the SRC, agreed that I should do this.]

Being chairman was a most interesting experience. I had seen at first-hand the Ministerial meeting of 1977, and it really was a bit of a disaster (as indicated above); it had not been very well prepared (probably because there weren't any real problems), and so I was determined that the next such meeting would be a success. The meeting was agreed to be in January, 1985 and so, for the previous six months, I really worked quite hard with delegations and through ESA staff: my objective was to try to decide everything possible beforehand! Ideally, we'd bring the ministers together at the meeting and they would be presented with draft documents which they would understand and could approve easily. I knew, of course, it wouldn't be like that – although for a while it really began to look like that in practice – and a number of delegations said to me: why are you worrying about trying to get essential decisions made, provisionally, in advance: why not leave it to the ministers? Of course it turned out that there was plenty left for the ministers to decide – and, because the meeting had been so well prepared, it meant that they could really concentrate on the important things. So it was the most wonderful meeting.

One of the things that I (I say "I" but I mean "I" representing the system), had to do was to choose the minister to chair the meeting. And this didn't have to follow the nationality of the official chair (me), which was of course British. So Reimar Lüst – who was the director-general who had recently been appointed – and I worked together in this task. Reimar Lüst was very experienced not only as a space scientist, but as the head of the Max Planck Gesellschaft – that's the society which runs all the Max Planck Institutes – and, indeed, also as a science advisor to the Federal government. He was senior to me in terms of the German system, but we played it as a team and he was completely straight.

We understood that Signor Luigi Granelli – then the Italian Minister for Scientific and Technological Research (who was responsible for space) – very much hoped that he could have the chair: but that did not seem appropriate as [an Italian, Mario Pedini] had chaired the previous ministerial meeting, in 1977. But Granelli was so keen and Italy was so important, that we wanted to keep him entirely on board. Thus, I soon agreed with him that the meeting should be held in Rome. We then decided to ask a man called van Aardenne, the Dutch minister for technology, whether he would chair it. The Netherlands, was a small country which wasn't involved with any big project, so he could be a more neutral chairman. I (with Reimar Lüst) visited him in The Hague, and I essentially interviewed him for the chairmanship of the meeting; and he played the interview very well. I mean he knew that we weren't promising it to him, but he took it in very good form and we concluded that he would indeed be the right man. So he was soon appointed chairman.

I might mention that there were sometimes certain divisions within the Italian delegation, just occasionally of course, and we wondered whether these would cause difficulties. But in the end Minister Granelli was excellent in helping to ensure the success of the meeting, and so were the Italian delegates and the Italian system as a whole.

Thus, Granelli invited us, the Council, to meet in the Villa Madama in Rome. This was a most beautiful palace which had just been refurbished, and Italy really pulled out the stops for us. The atmosphere was good, the preparations had been well done – and each delegation had something that they wanted to get out of the meeting, so they had real desires for success. For example, the French wanted to proceed to manned space flights and they proposed *Hermes*, a "Navette" – which was their name for a sort of mini-shuttle to be carried up on an Ariane 5 launcher. The latter was the second thing, the main thing they wanted: Ariane 5 was a new, much bigger, rocket launcher, and if it were to carry up the Navette with people on board, then it obviously had to be man-rated. So there was an enormous amount of discussion about the problems and costs of man-rating.

The US Space Station was very much in the offing and, of course, the Germans were very keen on participating in that. In addition, the ESA science programme was planned to increase (as I've said); and there were other, new, satellite programmes being proposed. The ministers, apart from Granelli, included: Geoffrey Pattie, then the British minister responsible for space (who was very good); Hubert Curien, who was by then the French minister; and the German minister Heinz Riesenhuber. They were all excellent and presented their cases in the most forceful way – but there was a lot of joking.

Thus, I remember Curien saying (he had to speak in French, you know, because he was talking officially): Ah, here are the English who were always so much against developing rocket launchers, proposing HOTOL, a launcher which would take off horizontally – while we, the French, are proposing a new rocket (Ariane 5) which takes off vertically... (The British proposal for HOTOL, which stands for "Horizontal Take Off and Landing", would take off from a regular airfield, go up and come back for a horizontal landing.)

And we had parties and other events and visits, so the meeting in Rome really was a very good occasion. [We spoke of "the Spirit of Rome".]

In the end the Ministers effectively agreed in principle to an ambitious programme which would require a considerable increase in expenditure. Following this there was a ministerial meeting in The Hague in 1987 (chaired by Dr Riesenhuber). But I had left my role regarding ESA well before that, and so didn't go to that meeting. However, it was clearly a difficult occasion, partly because the pressure on finance was increasing for everyone. I mean even for the French and even for the Germans. Germany was working very well industrially and economically, but both countries realised that they couldn't go on at that level without a complete change of outlook. So The Hague meeting was indeed difficult. The UK was represented by a different British minister, Kenneth Clarke, and he spoke his mind very clearly, for example, about the *Navette*, which the French didn't altogether like, I believe…

So that was near the end of my period of involvement with ESA. It was a very good period and I made many good friends. Something that struck me very clearly was that although the delegations would come with briefings to do this or that, they also felt that they were part of an important engagement, a *European* activity, that was very successful; and so their loyalties, in a way, were divided (and quite rightly) between their country's narrower interests and the interests of the programme as a whole. And that, I think, was the most important thing that made everything work – that, plus the fact that there were the optional programmes which gave much flexibility.

You've probably got some questions at this stage…

One of the major successes of the period during which I was the chairman of the Council, was the launch of the Giotto, a spacecraft to intercept Halley's Comet, in 1986. Of course, Halley's Comet only returns every 76 years, and there was a rather limited window of time

for the interception. If it didn't work then, there would be a long time to wait before the next encounter. Harrie Massey, just as an aside, Harrie Massey told me that he had seen Halley's Comet the last time it was around – in April 1910 – when he was still in Australia, his birthplace. He was, I think, aged just two, very young, and he remembered being carried into the garden near Melbourne, on his father's shoulder to see Halley's Comet. (Most people don't remember anything from when they are two; but for Harrie...)

I might just add that Harrie Massey became ill a couple of years before this [the Rome meeting], and I called to see him. He had a house near London. He was essentially dying, and I think my visit turned out to be only weeks before his death [on 27 November 1983]; but we had a jolly good conversation. He asked me what was going on in the European Space Agency, and so on. Then, as I was about to go, he said to me: "Harry, what we really have to do is to find the Higgs Particle".

Higgs: it's an unlikely name for a particle [sometimes called "the God particle"]. It comes from the name of Peter Higgs a physicist (now in Edinburgh) who proposed it on theoretical grounds, in 1964. It hasn't been observed yet – but may be discovered this year, 2008, using the Large Hadron Collider, LHC, at CERN. But it was remarkable that *"we really have to… find the Higgs Particle"*, were almost Massey's last words… [The LHC did not, in the event, operate in 2008.]

Now back to Giotto… There was a lot of argument before it was approved, in 1980, about whether it was appropriate for the ESA science programme at all. This was partly because missions such as this take many, many years to prepare and are very expensive – inevitably precluding a number of smaller missions. The other thing was that the chances of success seemed relatively small. To win a Nobel prize for one individual, or to satisfy the scientific needs of many different groups and producing many good publications? I can't remember whether the UK voted for Giotto or not, but the decision for Giotto turned out to be absolutely right. [Giotto was ESA's first deep space mission.]

In due course the Giotto mission was developed and the actual encounter was in 1986. It was during my chairmanship that the spacecraft was launched (on 2 July 1985) and I was there. It was the first time I'd ever been to Kourou. The industry concerned in the preparation of the spacecraft and launch – there were many countries and many industries involved – paid for a Concorde to take us to Kourou and bring us back. That was quite an interesting experience in itself. Very few of us had actually ridden in Concorde before. The plane could only just make it across the Atlantic; it's a long way to French Guiana, so we had to call in at Dakar, in French West Africa, to refuel. But then we went straight across. It's rather nice having a plane to oneself with the ministers and officials and so on, and we had a very good time. When we got to Kourou, we had time to acclimatise ourselves before the launch: that was a totally nerve-racking business. This particular launch required, in fact, four stages to the Ariane rocket. The first three stages (the normal configuration) put the spacecraft into a high transfer orbit around the earth – and then the fourth stage had to ignite to carry the spacecraft out of the Earth's gravitational field. (This four stage configuration had not been tested before.) So the rocket was launched, successfully, but we had to wait two days before the firing of the fourth stage. The launching *per se* couldn't be declared a success until that had happened.

I don't know if you're very interested in this, but two days after the launch we went to Devil's Island by boat, and had a nice lunch there. I found the Island to be rather a touching place. It was more or less what you'd expect from a tropical island – palm trees, sandy beaches and things – but at the top were the old prison buildings. Also, there was a big artificial lake, which apparently had had crocodiles in it; if you tried to escape you could find yourself

thrown into this pool as a punishment. There was a church, a sweet little church, which had been decorated inside by the prisoners – with murals of scenes of French life, you know, vineyards and so on. While we were having lunch, the message came through that the fourth stage had fired successfully and that Giotto was on its way to its rendezvous with Halley's Comet. That, of course, was the most extraordinarily exciting news.

The spacecraft went on, and about 8 months later was due to be at its nearest approach to the Comet (on 13/14 March 1986). For this event, a number of us went to Darmstadt, to ESOC – the European Space Operations Centre – which controls the operation of spacecraft in space, the communications and so on. Here were also the control-rooms for the many instruments on [Giotto] operated by numerous laboratories, university and other laboratories, particularly from Europe, but also, I guess, from America too. We arrived there in good time to talk to the scientists and ask them more about their experiments.

Those present from other space agencies included: Prof Hirotake Matsuo, from the Institute of Space and Astronautical Science, Japan; Academician Roald Sagdeev, the Space Research Institute, Moscow; and Dr William Graham, Acting Administrator, NASA. Also present, were Jan Oort and Fred Whipple (who had worked on cometry physics), Mr van Aardenne, Senior Granelli, Geoffrey Pattie, and Mr Collins, the Irish minister. [Sagdeev was a remarkable man; he'd been science advisor to Gorbachev, had married Eisenhower's granddaughter, and was very pro western, despite the cold war.]

Professor Lüst, in his opening address as DG, referred to the success of the Soviet's Vega-1 (7 March 1986) and Vega-2 (9 March 1986) encounters with Halley's Comet; and to the Japanese spacecraft Suisei (encounter on 8 March 1986) and Sakigake (11 March 1986), which had provided important information which helped to guide Giotto to its rendezvous. There was an extraordinary feeling of closeness amongst the worldwide community who were there.

There were a number of other speeches before the encounter was due. I gave a piece (as the Chairman of Council) and chose, not surprisingly, to talk about Halley himself as a British scientist. Halley was a remarkable man, a contemporary of Newton, and I see that I followed something of his history. Of course, that was the time when the Royal Society was beginning – a most extraordinary time in England. Galileo had been working just a little before. In fact, I believe that Newton was born [on 4 January 1643], only about 12 months after Galileo had died [8 January 1642]. Galileo didn't understand gravity fully so, for example, he couldn't predict the tides – which was a problem! But Newton did. Halley urged Newton to publish his work, which he finally did in his *Principia Mathematica*. Halley was remarkable, too... And so, of course, are comets. As I said in my talk, Halley "turned the comet from superstition into science".

Incidentally, many years later, in 2000, I found myself chairing a Task Force for the UK government on the hazards of asteroids and comets hitting the Earth.

Comets have always been seen as sort of omens – not just Halley's Comet, but any comet that came along – as being signs from the gods that something awful was going to happen. The Bayeux tapestry of course depicts Halley's Comet (in 1066): there it was called "the star", and people believed then that it predicted (King) Harold's death. Oh, yes, for your recorder, we're looking now at a part of the Bayeux tapestry and it shows Halley's Comet going along the top, somebody talking to Harold, and, to his left, six men pointing to the comet. Above them are the words – ISTI MIRANT STELLA[M] ("they are in awe of the star"). So comets have got an extraordinary place in the human psyche – they always have had, since the beginning of time. (Asteroids, which can be equally or more damaging or more damaging to the Earth on impact, can't easily be seen and so have not collected the same mystical reputations.)

To return to ESOC and the Giotto encounter: after the various talks had been given, and all present were in good spirit, the time approached predicted for the closest encounter. But, for this, nobody could be quite sure of what was going to happen. Because Giotto was approaching not so far from the tail, it was quite possible that the electronics in the spacecraft could be taken out by bombardment from the tail's dust.

Then, suddenly, the communications stopped and there was a terrible silence in the room. Nobody knew whether that meant that the spacecraft had failed entirely – or what. I can't remember how long that was for. It may have been ten minutes, but it seemed like an absolute eternity; and to think, you know, in that time – oh, this beautiful mission! But in fact it *had* worked, and the data soon came through. The mission had been very successful. I have on the wall behind me (in our dining room at home), where I'm dictating, a big false-colour picture of Halley's Comet, about 80cm square, which was taken from the Giotto spacecraft, and given to me very kindly by ESA. So that was really a most exciting event and I must say in many ways the crowning achievement of that first phase of the ESA science programme. I just happened to be there at the time when everything was working well. So that's quite good! The Giotto spacecraft went on to other targets, the mission finally ending in July 1992. Do you want to ask any questions?

MG: OK. If we go back a bit – this is possibly a little bit before your time – but I've seen from government records that the SRC was changing its mind about membership of ESRO in the late 1960s. In the late 1960s it was rather against staying in ESRO but it decided to keep in on foreign policy grounds. I just want to ask about how, I mean given all the sort of European policy implications of ESRO and so on, how much the Foreign Office was involved in any of the sort of ESRO policy discussions.

HA: I don't know, I was not, of course, around at that time directly. But this was in the period when British space science was getting outstanding results by working with the Americans, including through the UK/Ariel series of missions. So I guess that ESRO was not seen as a particular priority, as I've already described. I don't know what role the Foreign Office played then (if any) because the SRC liked to make decisions based primarily on the science. In my time, the Foreign Office would often come to ESA Council meetings in Paris; I mean somebody from the Embassy might come along. (I'm talking about meetings at official level, not ministerial.) It was perhaps unfortunate that, when ESRO HQ was at Neuilly, there were usually only two seats at the Council table and one microphone per delegation. And so, in fact, it often happened that the Foreign Office, if they came, would have to sit behind – without a microphone; indeed I can't really recall when they did sit with a microphone. That was a bit different from some other delegations, [for example the French].

MG: Did you have briefs written for you or you prepared your own briefs?

HA: Oh, no. As far as the Science Research Council was concerned, we would discuss it at, I mean the general policy, at what was called the Astronomy and Space Board, which was chaired by an outside scientist, for example Professor Harry Elliot (of Imperial College). No, there was no external briefing. But before an ESA Council meeting in which there was likely to be discussion not on science but on completely different things with foreign policy overtones, then the Foreign Office would always come to the overall briefing in London.

MG: OK. And what about the sort of ongoing debate about the European programme versus the national programme? That was sort of a very prominent discussion in the early 1970s, do you know much about that?

HA: Well, I think I've probably said as much as I know about that, namely that the gradual pressure, *for financial reasons within the SRC's financial envelopes*, was to shift the emphasis towards the European programme; and that was, to some extent also, I suppose, a policy.

Also, communications programmes and those concerned with remote sensing would often have an international policy element.

The same sort of thing – the pressure to use international facilities – has occurred in nuclear and high energy physics, in response to financial pressure. I said before that in the 1960s I worked for NIRNS – which eventually became part of the Science Research Council when the Council was formed in 1965. The UK research council system was always designed – following the Haldane Report of 1918 – to be independent of outside political influence (including from, say, the Foreign Office or any other government department). But also it's natural, I think, that scientists want to do what they can near home, in a familiar environment, rather than having to work abroad [away, say, from their university]. So the equivalent thing was happening in high energy physics: CERN was being created while at the same time there was a completely independent effort in the UK, at both the Rutherford Laboratory (using the proton synchrotron called Nimrod), and at Daresbury (with its synchrotron radiation facility). The particle physicists and other scientists who were working with these facilities really didn't generally want to go to CERN (in Switzerland).

MG: Yes, OK.

HA: At this time there were meetings, which became called "Parish Pump" meetings, where the university nuclear physicists gathered together (for example in London at the Royal Society or somewhere), and really expressed themselves as being quite unhappy with CERN – which was also seen, to some extent, as being bureaucratic and expensive and so on, a bit like ESRO! But, again, because you had to build bigger and bigger machines to do really front-line particle physics, it really became essential to move to the big international facilities. And people gradually accepted it. The government pressure was – and I already saw this within the Cabinet Office – the government pressure was to constrain science budgets: so if one thing was increasing in size, then you jolly well had to cut the smaller things, whether you liked it or not.

As time has gone on, the organisational arrangements in the UK to manage science and technology have changed, bringing different parts of the science programme into direct competition – that is, "the Machinery of Government" has changed. Thus, since 1 April 2007, a new research council has been set up called the Science and Technology Facilities Council, STFC, which includes all the big sciences – astronomy, space and major facilities for solid (and liquid) state science (including synchrotron radiation facilities, major lasers and so on. The STFC's budget has to include the ESA subscription, I guess, and the subscription for CERN. With the new system, the scientists are complaining loudly that after paying for all the capital facilities [and international subscriptions], the amount of money left over actually to do experiments – to build experiments, and to participate just at the margin of these things – is getting increasingly small.

MG: OK. One recent issue that's being discussed is having an ESA organisation in the UK and there was obviously a discussion in the 1960s about having a site at Bracknell. I just wonder whether that was ever revisited in the 1970s, 1980s.

HA: Yes. That's a very interesting question because it was really quite extraordinary that the UK did not bid more strongly in the past for an ESRO or ESA establishment to be based here. I've been involved in quite a number of international or European organisations – for example the European Synchrotron Radiation Facility (at Grenoble), the ILL research reactor (also at Grenoble), astronomy facilities and so on, but on the whole Britain didn't seem to be interested in going for the sort of perceived goodies which were associated, for example, with ESA. So when ESRO and then ESA were formed, because of our sort of half-heartedness, Paris got the headquarters. I'm sure that France just really went out for it. Often it's a matter

of making a contribution to the initial capital costs of buildings or site. I wouldn't have been surprised if the French government hadn't said it would pay towards the building of the ESRO HQ. So France got the headquarters and therefore controlled all the strings.

Germany got the operations place (ESOC at Darmstadt) we've been talking about; the Netherlands – possibly as a sort of semi-neutral country but also very good at astronomy and science – got ESTEC. The Spaniards got a dish for space communications near Madrid. The Italians got ESRIN, the information centre, at Frascati; they, well, the Italians, have always realised the importance of people. I think the Italians recognised – perhaps because their governments were often not so stable, very often in the past – they realised that what mattered most was having actual people in the system. I remember that when there was a threat from ESA to withdraw from Frascati, the Italian government more or less said that they would leave ESA if that happened. ESA relented and so ESRIN was retained in Italy and indeed the information centre was extended. [As I saw it, the UK also did not always press as hard as did some other member states to obtain senior posts in ESA; nor did we always keep politically in close contact with the senior people we did have there. This seemed to be in contrast to the French, for example…].

Britain didn't seem to be so interested; but then at about the time when Ted Heath came in as prime minister (1970), there was a move in Europe to set up a European Patent Office. Now, if any country should have the European Patent Office it would be Britain, because Britain invented patents, pretty well. I can say that because, as I mentioned to you earlier, my father ran the Patent Office in New Zealand, and Britain really did invent the whole concept. But nobody in the UK seemed to go after it (the European Patent Office) seriously: perhaps this was because it might have cost the UK a little more, initially; or because there might have been a housing shortage at the time or something like that. But Germany went for it; suddenly people in the UK woke up one day and found that it was to be in Munich and of course everything that went with the Patent Office – all the legal system, the attorneys, all the assessment of the patents, went there too. Incredible! So when Heath was told this, I believe, he said: "we must get the next thing that comes along, whatever it is; we must jolly well get it."

The next thing that came along was the European Medium-term Weather Forecasting Centre. And so he went – and good for him – he went all out for it. There was only one slight inhibition, and that was that the Meteorological Office in the UK was extremely powerful and, I mean, a remarkable organisation. It's almost a university and the Office had a very powerful director-general in John Mason – who was also Treasurer of the Royal Society and so on, and a most interesting character. I think that the other meteorological organisations felt that he was such a dominant figure that they weren't so sure they wanted to be next to him at Bracknell. However, it did happen – the Centre came to the UK – and that was indeed a Good Thing. But it was seen as (at the time, at least) nothing like the economic prize that the European Patent Office would have been.

So, now, at last, it seems as if miraculously there's a move to try and get something from ESA! I don't think it's actually finally been agreed, but I've heard the minister in this new department (DIUS, the Department of Innovation, Universities and Skills), Ian Pearson, speak positively about it. I don't think the terms of reference of the ESA centre have been entirely worked out yet. However, I would like to see it, amongst other things, being concerned with asteroids and comets. I'd be delighted if it happens.

After all, however, one thing we *did* get, years ago (in the early 1970s), was JET, the Joint European Torus for fusion research. We got JET, at Culham, in Oxfordshire [to which site Harwell's old CTR Division had moved] and, as a result, an international school was set up

nearby. I think it's the only international school in England, apart probably from one in London. [There may well be more international schools than this.] If you drive around Abingdon, you'll find quite a lot of French number plates on cars, which is very satisfactory. And, of course, if the ESA centre goes to Harwell, it will be quite near that complex – helping to make the case for the centre. [We did work hard to get CERN's new "300Gev" accelerator to England (at Grimes Graves), but did not succeed…].

MG: OK. Moving on to the Heseltine delegation and that period, I had a look at some of the public record office documents for that and there's a sense that comes from those that Heseltine and the DTI were out on a limb to an extent with these space proposals that they were putting together. I mean I've seen a memo from Mrs Thatcher that she sent to the Treasury, I can't remember who it was at the time, but…

HA: Not Maurice Macmillan, then Chief Secretary? No. I mean Harold Macmillan's son.

MG: I can't remember. But they weren't too keen on it, I don't think. Thatcher's main aim was to safeguard the SRC budget.

HA: Oh, you're talking now about the period when she was Secretary of State for Education and Science?

MG: Yes.

HA: Oh, yes.

MG: So I just wondered about that.

HA: Sorry, could you just say that to me again.

MG: I just wondered; the Heseltine plans seemed to be slightly out on a limb within Whitehall.

HA: Oh, yes, absolutely, and I mean I touched on that a good deal earlier [in this interview], when the Science Research Council was worried that it might actually lose its space money. But we should also perhaps have a word about the British National Space Centre, which came along much later? But, I mean in the end, Heseltine's intervention went ahead, of course, *without* taking the SRC's money. The SRC didn't object at all to the final resolution, so I think that while he may have gone out on a limb in some ways, the overall result was a good thing; and I think that probably ESA is better because of it.

I mean Heseltine is a very interesting character and, of course, went on to become Secretary of State for Defence – when he, in fact, appointed John Houghton as director-general of the Met Office in due course.

MG: Oh, right. OK. I suppose the next thing is actually the BNSC in the 1980s. Were you involved with the setting up of the BNSC? [in 1985]

HA: No, not really… BNSC brought together the interests of this rather dispersed group of UK interests in space. [But that was about 10 years after Heseltine was directly involved in space matters.]

In his address to the Symposium on the History of the European Space Agency, at the Science Museum, in November, 1998, Heseltine gives a very personal and very entertaining account of the position as he saw it in about 1972. His view was almost entirely that of what was best for British industry (he was, after all, in the industry department). He referred in his talk to: "space responsibility littered across Whitehall – every government department had its finger in some particular piece of the pie that had to do with space. There was *telecom* space,

defence space, space *science*, *industry* space, *agriculture* space – they all had their activities."[17]

But he did not seem to understand that the expenditure on science using space techniques was very substantial – and crucial to the Science Research Council's pursuit of its remit to support university scientists to understand the nature of the universe and of the solar system.

I can understand his frustration, but he should take considerable pride in his achievement in helping to create the new European Space Agency.

The BNSC – which, as I've said, came years later – I think proved a reasonable sort of, well, a British compromise. First of all, it provided a sort of focus, a centre for discussion of space matters and, of course, for the briefing of the delegation to ESA for which the Centre's staff increasingly provided members. Of course the BNSC was not able to provide the large increase in space funding which many hoped for, but I doubt whether that could have happened through any other machinery, either.

I mentioned before that the delegation to the ESA Council, in the old days, simply comprised the Department of Trade and Industry, DTI, and the Science Research Council. (For the DTI there had always been an under-secretary – who came and went to some extent – but John Steele was amongst a number of good ones and, I might also mention, Harold Robinson, Ted Mallett and Jack Leeming.)

The DTI money was considerably greater than ours because they were concerned with the applications programmes but that, I think, worked out quite well. There were a number of directors-general of the BNSC who were generally successful. Also on occasion there was somebody in the Centre, sort of seconded from the SERC; thus, Paul Murdin was there at one stage. He's a Cambridge astronomer and had been at the Royal Greenwich Observatory. And so I think it [the Centre] worked not too badly. The BNSC provided the secretariat for our study of NEOs (asteroids and comets) – by the task force set up by HMG in 2000 under my chairmanship. But then Roy Gibson came in, under rather different circumstances. Now, when was he director-general?

MG: '85.

HA: '85, yes. [1985 – 87]

MG: Something like that.

HA: So he, I think, I don't know whether you could actually say he'd been misled. I mean I don't think one can say he was deliberately misled, but perhaps, I don't know, whether he read a lot into some hints. [I was not directly concerned in this.] But I think he expected that all the budgets would come together and would be increased. I don't think that that was a realistic assessment. I mean to be director-general of ESA, is a very powerful position and the resources are huge; you have very good staff and so on – but the BNSC was just not like that. It can't be like that under the present financial arrangements, so I'm sorry that he was disappointed. I think he would have, you know, done well. I don't know how long he was there, was it about a year?

MG: A couple of years.

HA: A couple of years, yes.

[17] This Symposium also has a concluding paper from me, Harry Atkinson, which includes a chronology of the development of space, including through NASA and ESA: "Concluding Remarks", *Proceedings of an International Symposium, Science Museum, London, 11-13 November 1998* (Noordwijk: ESA SP 436, June 1999), 219-244.

MG: Because speaking to him, he gives the impression that Margaret Thatcher sort of changed her mind about it as a kind of good idea.

HA: I think that, I mean, it might have been so. I don't know whether she changed her mind or whether he… Margaret Thatcher was a very interesting [person]. I got to know her a little bit when she was Secretary of State for Education and Science, but also when I was in the Cabinet Office [1969-72] – because I was at one time secretary to the Science and Technology Ministerial Committee and she was on that. And she played that brilliantly; but that's something for another occasion. I mean she was most astute and excellent, although her officials didn't see her, at the time, as being a potential prime minister. I don't know what went wrong with Roy and the BNSC. It might be interesting for me to talk to Roy on some occasion about it. The 200th meeting of the ESA Council is occurring in June [2008], and I sent you the piece that I wrote for that which actually sort of hints at quite a lot of the things I've already said. They've invited me, and they're going to publish a booklet. And they've invited the previous chairmen if they're still alive, to have dinner with them the night before, and I think Roy Gibson is going, so I'll ask him then. [In fact Roy did not attend.]

MG: OK, because he also came up with the space plan, the mid-80s space plan, which were his proposals for what Britain should be doing in space. He sort of briefly discussed what it included but I think the actual report is still…the space plan I mentioned. Did you have any part in drafting that?

HA: Oh, the space plan and, no, I didn't; when was that?

MG: Mid-80s, I think. Yes.

HA: Well, since it was mid-80s, I should know about that but it doesn't trigger an immediate reaction.

MG: OK. Just slightly peripheral questions, but how did you find the Treasury, how did your interactions go with the Treasury?

HA: [As far as the purely science side is concerned,] our only interactions with the Treasury were indirect, through the Science Budget – which in turn came through the Department of Education and Science. The essence of the Research Council system was being given a block of money, and having a "Forward Look", so that we made our own decisions within those totals. I mean I encountered the Treasury when I was in the Cabinet Office, but that was in quite a different context. Of course, some of the brightest people go into the Treasury or the Foreign Office; but, no. [As I've said, I sometimes saw Treasury people at the briefing meetings for the ESA Council. But in the UK I was not of course directly responsible for the space applications side.]

MG: OK. Fast-forwarding, the 90s when you came to be appointed to the Near Earth Objects Working Group…

HA: Oh, yes.

MG: How did that come to be set up?

HA: [Laughs.] Let me recollect. I think one could say that the question of asteroids and comets and impacts on the earth really came into focus in the 80s – to some extent in the context of the event 65 million years ago which wiped out the dinosaurs. And so people who were trying to – the Americans on the whole – trying to understand how this happened came to the conclusion that it was probably through the impact of an asteroid or comet, and a very large one indeed. It subsequently has been pretty well proved to have been caused by a comet landing near the Gulf of Mexico, and studies of the seabed and so on showed that it certainly was a comet.

The processes were considered which must have gone with that: first of all the shock wave in the atmosphere from the entering comet and then the emission of dust and acid rain and so on which followed. The first time that there'd been a quantitative study, I guess, of that sort of global catastrophe was in the context, after the war (WWII), of an all-out nuclear war. And so a lot theoretical work was done which led to the concept of the "nuclear winter". So it was realised that an asteroid or comet impact could set up such a nuclear winter.

At Tunguska, in Siberia, was an event, in 1908, which certainly was within recorded history – and could be seen to worry people. One of the things that had put people off [the scent] was not realising just how many impacts there were in the solar system. When people looked at the Moon, on the whole the craters were thought to be of *volcanic* origin, *not* caused by impacts. And, of course, on the Earth the craters are usually under the sea (because there's more sea than land area) – and the signs of most of the others, on the land, have eroded. There were some relatively clear ones like Meteor Crater in Arizona, and with the development of remote sensing, the images showed a remarkable number of huge craters, for example, in the North of Canada.

But you ask how it really started off: the first thing that really shook people, I think, was the Shoemaker-Levy 9 comet impact with Jupiter. That was in, I think, in 1995, [actually, May 1994] and that really got people worried because it was seen that the comet broke up into a number of pieces (at least 21) – and each piece made an explosion bigger than the size of the whole Earth; and that really worried people. That was something that people actually observed [on their own TV sets]; and all the World's telescopes were trained on it.

The Americans had got going before that, and, by the end of the 80s, such impacts had already interested Congress; and NASA were ordered to study this effect. Then came along Schumacher-Levy 9. In the UK, Lembit Öpik appeared on the scene soon after this. Actually it was Tate, Jay Tate, who runs a thing called Spaceguard UK, who first triggered interest here. He was an army/ex-army officer, a very interesting character, and he inspired Öpik.

But it was really Lembit who, as a member of parliament, could take it up [at a high level]. His grandfather [Ernst Öpik] had been an eminent astronomer and had been director of the Armagh Observatory and was very interested in comets; so it all fits together. Then Lembit asked a Question about the dangers of asteroids and comets in Parliament in 1999, I think, or just before, which is in Hansard [the official record, for 3 March 1999]. The question was addressed to John Battle, then the relevant DTI minister (as the Minister for Energy and Industry). Lembit apparently got up and said, "I have a problem with asteroids", and the Minister said, "What?"[18]

So pressure was put on DTI, on the BNSC, really. Would they do something about it? Would they study it? Lord Sainsbury, David Sainsbury, had by then been appointed minister for research, including responsibility for BNSC. He took the dangers of NEOs seriously and decided to set up a working group to look at the subject. I don't know whether another minister would have dared to do so, because of the "giggle factor"; but Sainsbury, you know (a bit like Rothschild) was not drawing his salary and perhaps could tend to be a bit more independent than an average career politician.[19]

I knew nothing of all this until towards the end of December, 1999, when I was rung up to say, would I chair this panel? I (too) said, What?

[18] *Hansard* continues: "Mr. Öpik: Asteroids. And so has the rest of the human race. Unless we do something to stop it, sooner or later an impact with an asteroid or a comet will lead to the end of most life on Earth." It's all in *Hansard* on-line.

[19] I mentioned Victor Rothschild, because he could be similarly independent when he was running the CPRS, the Government think-tank, in the 1970s; and, I believe, also did not draw a salary.

I was very dubious about this and I knew nothing about the subject. Of course that's exactly what they wanted – I would not be biased! And so I rang Martin Rees whom I knew very well from all sorts of places in the past. He had been Chairman of ESA's Scientific Advisory Committee (1976-78) and other relevant things at various times; and was currently Astronomer Royal (for England) etc, etc. I said: what about it? Is it stupid? He said, no, no, not at all. So, really, on that basis, I said yes. The other two members of the group [Task Force] were David Williams, who was professor of astronomy and physics at UCL, and very good though that wasn't his particular area either; and Sir Crispin Tickell, a most interesting person whom I subsequently got to know well. Of course he'd been British ambassador to the United Nations and so on, Warden of Green College, Oxford, and was currently Chancellor of Kent University. In the end we, the Task Force, actually wrote, I think, a very good report, and it immediately sparked lots of cartoons in the newspapers [as well as world-wide interest of a more serious sort].

I've never done anything before that sparked cartoons. But this one [showing a cartoon], I think, was quite a good one.[20] In another cartoon, you see there's Gordon Brown in the middle and, of course, Jack Straw, and this is the head of William Hague as an asteroid. Brown's looking through a telescope the wrong way round. The cartoon is over petrol prices, but he [Gordon Brown] said of Hague's head: "… statistically, its likely to burn out before hitting anything". So that was technically correct, for an asteroid! I thought that was interesting regarding Gordon Brown, subsequently. I mean the guy (the cartoonist David Brown) knows what he's talking about.

The Report caused an enormous amount of interest. Partly, perhaps, because the day the formation of the Task Force was announced was the first working day of 2000 – it was 4 January. The DTI press office said they'd never ever had any announcement that'd caused so much interest; and I don't know, I did about 20 interviews or more in the following two days.

Perhaps one or two other points?

MG: Yes, OK.

HA: I'm not interrupting your points?

MG: No, those are the main ones, yes. It's OK.

HA: One is that *ESA is a research and development organisation*. I don't know whether I emphasised that before, but it's not a routine operations place. It's designed to, it's supposed to, test out technologies but not to operate routine systems… For science that's immaterial because it's not then an operational system; but with things like meteorology and remote sensing, [after the development phase] the activity then hopefully moves to the stage where it can become an operational system. So, having tested the concepts and done the development work, for example, on Meteosat, then it is not legitimate for it to remain within ESA and has to go to another organisation: here called EUMETSAT. That was an enormously successful programme.

The MAROTS (MARECS) satellite, which was very much the UK's creation for marine communications, eventually came under a new organisation for its operation called Inmarsat. Actually that *does* have its office in England; that was one thing that we got! Ariane, the launcher, of course, you know, came under Arianespace [the world's first commercial launch services provider] – although the development of Ariane 5, I guess, has again been under the auspices of the European Space Agency. So there is always a tricky period when things were

[20] It was by Austin of the *Guardian*, showing two ETs on an asteroid approaching Earth: one is saying to the other "They'll never stop us. It would mean a 7% tax rise".

moved away from ESA for the operational phase. Of course the more successful ESA is at R&D in a particular area, you know, the more the product is then moved away, so the Agency then has to think of something new to do. That's healthy.

Remote sensing was always a sensitive area, I think, partly because of its possible use for military surveillance and so on [as well as for commercial intelligence]. The French were very interested in remote sensing. I really don't know the full scope of the British defence programme. Clearly, surveillance from satellites is absolutely crucial in defence terms. The French proposed to develop a thing called SPOT, a satellite for Earth observation. I'm sorry to say that some people said that SPOT really stood for the *Satellite pour occuper Toulouse.* But SPOT was (and is) really important for all sorts of applications.

Not surprisingly, ESA also wanted to get into remote sensing, but this field was already well stocked not only with SPOT but, of course, particularly through the US remote sensing satellites such as Landsat. However, as both the French and the Americans tended to concentrate on observing the land, the first move of ESA was to concentrate on satellites to observe the seas and oceans. Thus, ESA began the ERS satellite series with ERS-1 – and that was, I would say, one of the many really successful things that ESA did. Having decided to look at the oceans, the question was: and what aspects, to begin with? Then, the question was, is this "science"? Does it, should it, come under ESA's Science programme? I'll come back in a moment to what "science" means – but anyway it was decided that this activity would have a programme of its own, funded separately outside the mandatory science programme. Nevertheless, it was actually looked after by Roger Bonnet as ESA's Director of Science, adding earth observation to his science management portfolio.

The package of experiments for ERS-1 that were first proposed didn't seem to fit well together. But then ESA called in Professor (now Sir) John Houghton, who had done some early remote sensing experiments of the atmosphere. I have mentioned him before, but I may not have said that he ran the department of atmospheric physics at Oxford, and I got to know him well at that early stage (in 1972-3). His group had provided instruments for American NIMBUS satellites, measuring the Earth's upper atmosphere. These were funded by the SRC.

[I shall return to the ERS programme later…].

To digress for a moment – at this time the SRC had an establishment at Slough called the Radio & Space Research Station, RSRS, which later became called the Appleton Laboratory. Appleton, of course, was one of the early scientists who looked at the ionosphere. (The establishment was responsible for IUE, NASA's International Ultra-violet Explorer, by the way, as mentioned elsewhere; but that's another story.) In due course, it was decided that the site at Slough would close and the Laboratory be moved to the Rutherford Laboratory, at Harwell, then renamed the Rutherford Appleton Laboratory (RAL). John Houghton was brought in as Director of the Appleton Laboratory and then became also deputy-director of RAL. Houghton lived at Oxford so it wasn't a difficult thing to do; and I got to know him even better at that stage.

I should mention that there was an interesting line of demarcation in the Research Councils between the Natural Environment Research Council, NERC, and the SERC (SRC plus E for Engineering) because the NERC, of course, had a primary interest in the atmosphere and the land and the oceans.

However, historically, because of the SERC's support for space science, the Council had for years *de facto* been supporting university groups looking at the Earth's upper atmosphere (and indeed oceans) from space. And this technique became more and more important as climate models came into being. Thus, historically, John Houghton was funded at Oxford University

through an *SRC* grant – but in principle he could equally have been funded by the NERC. However the NERC was not really used to spending big sums of money for a facility – except for boats for ocean research. The NERC was set up in 1965, from a miscellaneous collection of activities including the Nature Conservancy (which eventually bailed out), ecology at Monks Wood and so on. [It also included the marine science laboratory in Southampton, the British Geological Survey, the British Antarctic Survey.]

Continuing this digression, I should like to mention an affair [in the early 80s] when the SRC tried to work more closely with the NERC on climate matters. [As background, all the research councils, except the Medical Research Council, had moved from London to Swindon during the 70s. Thus, the SRC found itself next door to the NERC.] Sir Herman Bondi had become Chairman of NERC [1980-84]. He had been, of course, director-general of the European Space Research Organisation, ESRO [and before that chief scientist in the Ministry of Defence (1971-77) and then at the Department of Energy (1977–1980)]. He'd also of course been at Cambridge. I got to know him at various stages, but I'd like to talk about the time when Bondi was running NERC and (Sir) John Kingman was the chairman of the SRC. John Houghton (by then at the RAL) was also part of the band, and we decided (and there were also meetings in Whitehall about these things), we decided that we really would like to have a sort of coordinated programme with NERC. So John Houghton and I went to a NERC Council meeting and we presented the case for this. I suppose that Herman Bondi chaired the meeting. But we just didn't get any reaction at all. Bondi is no longer alive, sadly, but we felt that this was an enormous opportunity to work together on this new area – but it really didn't come to anything much at all [as a *joint* activity]. [Bondi seemed to be against it, perhaps because some small extra expenditure would have been needed…].

To return to ERS-1, as I said, John Houghton was finally called in to run a small group to decide finally what the instrument payload should be. He did an outstanding job and produced a satellite that did great, new things: it measured [synoptically, over all the Earth's oceans, and frequently] sea surface temperatures; and also, it measured wave heights and wind speeds and directions. The wave heights were measured with radar; and the wind speeds by observing the structure of the surface ripples.

MG: OK, yes, OK.

HA: And so ERS-1 really produced the most outstanding results. It must be said that John Houghton deserves a lot of credit for that. John, of course, then moved on to be director-general of the Metrological Office – incidentally, he was appointed by Michael Heseltine. Having retired from that (it could have been a very frustrating job, I think, in some ways) he then became the co-chairman of the UN's Intergovernmental Panel on Climate Change, IPPC, and did that very successfully. He became knighted (partly, I guess, because of that work) and he's still going strong. So that was a most important area.

The question of what "science" means in the ESA context, of course, was really a difficult one. This was in part because the old stagers said that science meant what the science programme committee was about – that it meant astronomy, the solar system, and geophysics (insofar as it does the upper atmosphere), and the ionosphere, but it probably stops somewhere around where the ionosphere stops. It was said, well, perhaps it should include meteorology. Or even, some said, perhaps it should include, in particular, zero gravity! The science programme committee wanted to have absolutely nothing to do with zero gravity for reasons I've already said – for exactly the same sort of reasons as did the SRC's committees.

MG: OK.

HA: I don't know, I could just mention very briefly some of the other things I did which touched on the science activities of ESA? I tended to run international affairs at the Science Research Council. I eventually became Director of Science, so I looked after the Science Board as well as the nuclear physics board, and astronomy and space board. I had quite a lot to do with CERN, but the astronomy and space always held a particular interest, and I've really described most of that. I've not told you about the SERC's big observatories. First, I'll mention the Royal Greenwich Observatory, RGO, which eventually we moved, under great protest from some, to Cambridge. We also had bids to house the RGO from Edinburgh and Manchester. But in the end we moved it to Cambridge, to be close to the strong university astronomy groups there. Now, the RGO's more or less disappeared into this new organisation (the STFC).

In addition, because the southern skies were very important, we had good astronomy programmes with the South Africans (through the SAAO and going back to the early 19th century); and with the Australians, through the 3.9m Anglo-Australian Telescope and the 1 metre Schmidt. This was the biggest telescope in the Southern Hemisphere for a long time. With these existing facilities in the South, we didn't join the European Southern Observatory, ESO, for many years largely because we couldn't possibly afford to do everything. However, as the facilities at ESO developed, we increasingly worked together and, eventually (after my time) decided to join ESO formally concentrating our southern work there.

In the Northern Hemisphere, we developed excellent, international, facilities, on La Palma in the Canary Islands, (including the Isaac Newton Group); and in Hawaii, though the UK Infra-red Telescope (UKIRT) and the James Clarke Maxwell millimetre telescope. I was much concerned with setting up the observatories at both places.

On the other "big science" fronts, I got much involved in the creation of the European Synchrotron Radiation Facility, ESRF, at Grenoble. I was on the committee that set it up originally, and that was a fascinating affair. Also at Grenoble, we were partners in the high-flux beam reactor at the *Institute Laue Langevin*, ILL. I also helped to set up EISCAT, the European Incoherent Scatter Facility, which is a collaboration between the Nordic countries and France, Germany and the UK – to look at the ionosphere and magnetosphere. It is a powerful ground-based radar within the Arctic Circle. So all these things were quite interesting.

I think that's pretty well all. But let me mention the following:

One of the most remarkable events I went to was the opening of the Observatory on La Palma in the Canary Islands, by the King of Spain in 1985. Normally, if something big is opened, it is usually by government ministers – and the ministers are the top dogs. But on this occasion, most of the countries involved in the observatory were, in fact, *monarchies:* Spain, the UK, the Netherlands, Belgium, Sweden, Denmark and Norway. Italy, Germany and France are also members.[21] Spain poured enormous resources into the ceremony, which was quite remarkable. I should say that the *Roque de los Muchachos Observatory*, on the peak of La Palma, was established under a series of international agreements in 1979. The Observatory is closely associated with the *Instituto de Astrofísica de Canarias*, the original director of which, Francisco Sanchez, is still in post. [Note that ESA's optical telescope in the Canary Islands, on Tenerife, at the *Observatorio del Teide*, also comes under the *Instituto de Astrofísica*.]

This must have been the biggest event ever in which so many monarchs came together to celebrate the beginning of a major scientific facility. And I doubt if it will ever be repeated!

[21] The list of countries needs checking.

For the ceremony, Spain covered the whole of the top of the island of La Palma with giant banners, specially created. And there the ministers had a relatively low profile [yes! laughs]. The King of Spain at a separate ceremony on this occasion made his son the Royal Astronomer. [Perhaps he couldn't make him "Astronomer Royal", because that title was used in England and Scotland.]

I'll just say that in about 1990, when I began to leave my work with the Science & Engineering Research Council, I worked half-time for a while, and became chief scientist for the Loss Prevention Council – the science and technology arm of the British Insurance Industry. This came about because I'd kept in touch with Solly Zuckerman [by then Lord Zuckerman]; and one day he phoned up and said: I've just been looking in Who's Who, you must be just about nearing retirement, what about a job in the insurance industry? It turned out that he was already involved with that organisation. It was quite fun: I got to know about general insurance: and we commissioned studies in universities on all sorts of things – from global climate change through Employers' Liability (and work related) illnesses, to the stealing of wheelie-bins.

I got to know Ron Oxburgh well when he was at Imperial College. When I was leaving the SERC, I asked him if he had any odd jobs for me; and he suggested that I might help the Universities Grants Committee of Hong Kong, of which he was a member, in a study of a new funding system for research and teaching in all the universities (and polytechnics). And so I used to go out four or so times a year, and devised a new system of funding for them. That was also quite fun.

Later, he [Oxburgh] was on a committee, an Oxford committee, called the North Commission, which was looking at the organisation of Oxford to try to ensure that the University would remain competitive for decades ahead in an increasing competitive world scene. There is an increasing tendency for there to be only a few universities in each country which carry weight world-wide. For the UK, these included Oxford, Cambridge and Imperial College and a very few others. In particular I was asked to report on the governances of five other universities: Cambridge, of course, perhaps seen as the real competitor; Manchester, as a top civic university; Warwick, as a new research university; and De Montfort, then still a Polytechnic. Finally we chose Imperial College [Oxburgh's university]. Then I went round and interviewed the vice-chancellors and the senior people in each of these. In some universities, which will remain nameless, I sometimes found myself becoming a sort of psychiatrist – because when the door would close, this senior member of staff would pour his or her heart out, and say how terrible everything was. I had to be very careful when I wrote the report.

Finally, I'd just say, regarding asteroids and comets, that although the Government Task Force which I chaired in 2000 is no longer formally in existence, we do meet informally from time to time and try to jazz up the BNSC and make a nuisance of ourselves.

That's the end of the story, I think.

MG: OK, thank you very much.

[*Postscript: I find that I did not mention that I attended the launch of the maiden flight of the Space Shuttle, on 12 April 1981 (at Cape Kennedy) – and went (following a chance encounter at the launch with a NASA man from Washington) in a NASA plane to its landing on 14 April at Edwards Air Force base in California. I was there in part with the UK junior minister concerned with space, Michael Marshall MP; though for the launch joined up also with an ESA group which included Roy Gibson. The launch was delayed by an apparent computer error about 10 minutes from launch, which should have been on 10 April. I filled in part of*

the time by going to Disney World at Orlando, not far from Cape Canaveral, where I visited Space Mountain. Later, in London, at the Royal Society, I met an American woman astronaut and asked her if she'd ridden on Space Mountain, and how did that compare with the real thing. She said she had – and Space Mountain had been far more terrifying!

The first shuttle flight was an astonishing occasion – not only the launch but even more so, the landing. Suddenly, when the shuttle appeared from the sky and glided in to a perfect landing – alongside all the world's press with their cameras – the feeling was suddenly that the US stood tall again; was at last technologically on top again for the first time since the States had been shocked by being beaten into space by the Soviet Sputnik. I may have been the only Brit at the landing. I should write more about this.]