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Professor Maurice Wilkins CBE FRS in interview with Max Blythe
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Part One

MB Professor Wilkins, your life began a good way from here in New Zealand. Can you tell me about early years, the first decade, parents, background?

MW Yes, my parents both came from Dublin and my father did his medical training there. And he was brought up in a rather unusual atmosphere, which derived largely from his grandmother who had a very wide perspective of rather advanced ideas and had been, for example, one year at Girton and was interested in Buddhism and all sorts of religious and philosophical ideas, and holistic health and so forth. And also on the other side of the family there had been a very real interest in advanced ideas of education for women, and my great aunt was one of the first nine women to get a university degree in the British Isles. So that this environment my father was brought up therein - preventive medicine, holistic thinking, natural foods, exercise, all this sort of thing... He went out to New Zealand initially as a GP. My mother, incidentally, came from rather a conventional family with none of these sort of rather unconventional ideas, but she had a lot of basic common sense which I think was a very good thing to balance a certain unworldliness on the other side of the family. My father was a kind of dreamer, rather, and not a very worldly man. So, he after a while became director of the school medical services in New Zealand. It’s only a small country so you could go straight from the job of a young GP into what appeared to be an important position like that, and he did a lot of propagandising for preventive medicine: whole wheat bread and healthy lifestyles. But he antagonised the millers and I think also the brewers and other people and he got the push.

MB He was really into the politics of prevention.

MW Yes, but he was naive politically and I don’t think he knew properly the way to handle these things. And a friend of his came out from Dublin - some of them were a bit wild - and this friend was a doctor and he went round giving lectures saying white bread is death. Well, the millers didn’t like that for as you know millers have to make money and they like white bread because it stores better; I think that’s one reason they prefer it. And so at the age of six, we came back from New Zealand and he did a DPH [Diploma in Public Health] - strangely enough at King’s College [London], where I have spent so much of my time - then went as an assistant school medical officer to Birmingham, where he worked in the slums of Birmingham and was very impressed by the dreadful poverty of the people there and developed a lot of work on poverty and health and poverty and nutrition.

MB This is the early 1930s we're talking about

MW Yes, that would be early '30s and throughout the '30s. He then was in touch with [Robert] McCarrison and (?) McGonogall and [John] Boyd Orr…

MB A classical time for child nutrition.

MW Yes it was. He was very much into this type of thing. Well, now we... you'd like me to start discussing my own life.

MB Yes. You went to school in Birmingham and have memories of education in Birmingham.

MW My first... I think as a result of this general environment coming from the grandparents… I had a workshop there and my father encouraged me to use tools and to get stuck into woodwork and craft generally. And we were encouraged to study nature generally, walking in the country, the fresh air and healthy lifestyle, and avoid sort of dogmatic conventional ideas. And my first major interest was in aeroplanes and flying. I was interested in all sorts of mechanical things and I made model flying aeroplanes, the ordinary sort of things that some boys made, the rubber band type of thing. I think what excited me there was the idea of the thing… I got this exciting vision of this thing soaring under its own power up there into the sky. This somehow really did something to me. But then later on I became rather more sophisticated and got interested in studying astronomy, that was my big hobby and I made telescopes and I made contacts with people who were amateur telescope makers. And I learnt about this and made some quite big... a 9½ inch reflector was the biggest one I made.

MB Maurice, this was in teens, we're moving into teens?

MW Teens, yes. And so I think the telescopes were exciting. It was like H G Wells and the science fantasies I found so interesting. The telescope gave you a view of other worlds, I think this was the thing. Even if you were looking at the neighbours' chimney pots you were getting a different view of it through testing a telescope that way. And I think that all this business of what the 19th century people used to call the glory of the heavens… what was it? Something about that this illustrates… no the splendour of the heavens illustrates the glory of God... I think there was this kind of spiritual uplift in it. But on the other hand I also became interested in physics through working with clocks and watches, and carpentry and things like this - actual craft around one. And in the physics, in working with telescopes, you use light and you get on to physical things like light interference, interference colours, all sorts of exciting things. So you begin to see through these studies a level of order in nature which is not normally visible sort of underlying things; you've got a new kind of world underlying the somewhat superficial world of everyday life. And so I got interested in the idea of atoms and electricity and... but I was also interested in meteorology, clouds and lightning - all these.

MB Was this assisted by school science?

MW Yes. School science was fairly good because we went to King Edward's School that was in the middle of Birmingham and we had some good teachers there and they were encouraging in the main.

MB Anybody in particular who stands out from that period?

MW Well, there were two physics teachers and I remember one of the physics teachers... that sometimes when we did practicals… I had a friend there and we developed philosophical literary ideas, generally, this other friend, and funnily enough, that after we had done say half an hour of physics practical and it was a bit of a bore, the physics teacher used to come to the pair of us and we would have talks about Buddhism and philosophy and all sorts of things instead of getting on with the physics. But I think what it shows is that in that school I was meeting other boys with wider interests, or extending the interest in science into the dimensions of philosophy and a little politics and things like that, so I was getting a much wider perspective for science generally.

MB So what was happening seems very prophetic, looking at what was to come later.

MW Yes, I think that this was where my main sort of direction was sorted out. Now, that school was well geared into sending its good boys to Cambridge. And I remember one of the headmasters of one of my father's schools visiting us once and saying... my father was talking about, you know, Maurice does this and Maurice does that... and this man said he must go to Cambridge, to the Cavendish Laboratory where [Ernest] Rutherford[1] is. And I thought well I've never heard of the Cavendish Laboratory. Mind you, the school would have done this anyway. And so I went up to Cambridge finally to do physics because I found that you couldn't do physical astronomy at Cambridge, you could only do it through a mathematics degree, which didn't interest me much. But it was interesting that I got so much into the astronomy and telescope making - I'm not quite clear how it came up – that as a school boy I already had visited not only in their observatories but had tea in their homes of [R O] Redman, who was director of the solar physics laboratory there, and I knew Birch at Bristol, a leading research worker who was developing new techniques for testing optical surfaces. So I was right into a lot of this even before I was an undergraduate and... but I think probably what happened was I may have gone in for a scholarship exam at Cambridge, I may have just walked out to the solar physics place and pushed the doorbell or something and said, 'Look you know, I do astronomy,' but anyway I made contact. I don't think I had any special introduction to those people through the school. So, in Cambridge… well I had to broaden my interests and actually there's an interesting point, I had to broaden my interests rather before going to Cambridge because when I went to get a Warwickshire County Council scholarship and went to an interview in Warwick, the first year I applied they apparently thought I was a bit stuck up and terribly interested in physics and they said, 'No, we won't give you a scholarship now, you must spend another year at school,' which is very usual, two years in the sixth form after higher school certificate, 'and you must broaden your

[1] Ernest Rutherford, first Baron Rutherford of Nelson (1871-1937). New Zealand physicist. In 1919 Rutherford was appointed to the Cavendish chair of physics and the directorship of the Cavendish Laboratory at Cambridge University. He received the Nobel Prize for chemistry in 1908.

interests.' And so I was a bit irritated by this. It was very good because during that year I went back to Birmingham and did all sorts of reading in Birmingham Public Library on architecture and philosophy. I read Schopenhauer - I didn't understand a lot of this stuff at all – and Le Corbusier, all kinds of things. And I went to the Arts Gallery and discovered the Pre-Raphaelites, which were relatively unknown at that time. There was a very good collection in Birmingham. So I broadened my interests a lot and with this friend of mine that we used to have philosophical discussions with the physics teacher, he was very into these things too, and so a whole lot of breadth of background…

MB So this was as enormous bonus.

MW Yes, it was very, very good. And there was another friend who was into politics. He was a year before me. He told me about Shaw and socialism and H G Wells' *[A Short] History of the World*, and left-wing philosophical political ideas. So that sort of gave me...

MB A good preparation for Cambridge years.

MW Yes, and then when I got up to Cambridge...

MB This was in 1936?

MW I went up in 1935. And so I wrote a little article – I'll give you a copy - on how failure can be useful. My failure to get up to Cambridge, you see, was useful, because I went up there a year later and was much better able to take advantage of it than if I had gone a year earlier. And it gave me a broader perspective which led me in Cambridge into going into the Cambridge Scientists' Anti-War Group, and I think I was the only undergraduate member in that group because as usual as an undergraduate I somehow got in touch with research workers in the Cavendish and I think one contact there was the brother of a friend of my sister's at Oxford, because she had gone there - she was a literary person on languages on the arts side, rather distinct from my interests. So these were very exciting times in Cambridge with all these political notions about science and planning for a better world and J D Bernal's[2] ideas – he was a great inspiration at that time.

MB He must have been a remarkable character to have had around.

MW Yes, I have to write an article in some fiftieth anniversary of the Social Function of Science. It is very difficult to... He had an enormous impact then, but in some ways some of his things do not wear all that well and of course he had his weaknesses, but he was a great inspiration. Now, I think that I had to broaden my scientific interests and had to in the part one of the natural sciences tripos – you'd got to do four subjects so I had to do mineralogy, geology. And that was okay up to a point because it put science, geology... it coupled together the walking in the mountains - you see, the old idea from my grandparents in Dublin in the mountains of

[2] John Desmond Bernal (1901-1971). British crystallographer. In 1927 Bernal moved from the Royal Institution to Cambridge to a lectureship in crystallography. A committed socialist, he joined the Communist Party in 1923.

Wicklow, all this sort of fresh air - it linked that up with a sort of scientific analysis of what was happening in the rocks underneath, what underlay the whole thing. But on the whole I didn't get very much out of geology, the physics was my main scientific interest and in part two I was doing physics only, for one year. But, partly because I was spending my time on these political things with the Scientists' Anti-War Group and I was given a job actually of doing an experimental study with incendiary bombs to check up on a story from Spain that incendiary bombs dropped on a tall building would burn their way right through from the roof right down. And I couldn't get that to work and [W A 'Peter'] Wooster, in whose back garden I did these things - a crystallography lecturer there – said - we were all downcast that the experiment wouldn't work; the thing would burn but it wouldn't burn through floorboards - he took me on one side and said, 'Whatever you do after you've graduated, don't go into experimental research.' I took no notice fortunately, I think. And I was keen enough, but I only got a 2.2 degree, and I think my interests in astronomy were fading out rather. I was getting more interested in science and the real world. I mean, the Cambridge Scientists' Anti-War Group was you see bringing that out. So when I was thinking about a research area I got interested in solid-state physics. I remember [John] Cockcroft[3]… I didn't want to do nuclear physics because this was done in big teams of people and I wasn't the only one in the Cavendish then. Rutherford had gone one year already by the time I… no, he'd gone two years then because he was only there for my first year – I heard him lecture once. So the thing was what kind of physics would I want to do. I took it for granted that I wanted to do research, it was my homing interest. I remember Cockcroft once said to me, 'Go into the library,' the Mond Library I think it was 'and read through some of these journals and see what you think is interesting.' A rather weird approach!

MB But it worked.

MW I found various things from the Phillips Lab in a Dutch journal *Acta physica*, and electrons running around in crystals, you see, in semiconductors and luminescent things and so on and this interested me. I think it was the special type of movement of electrons in the solid-state that somehow excited me, that they were kind of almost alive and this switched me on rather. Solid-state physics was not an area that was receiving much attention.... It was beginning, but I mean compared with after the war it was a totally different subject. It was very conventional...

MB But there was great excitement in this vitality of electrons...

MW This I think was it! I mean rather similarly I found an ex-student of mine the other day saying that when she first read that DNA molecules could breathe – I think it's a term they use for expanding and contracting - she said she'd never forget how excited she felt, that in a way the DNA molecule is a chemical structure but in a sense it's alive. Well, I think I had the same feeling about... all though I wouldn't say I could express it consciously. All that I knew then was that these things were exciting. You had these symmetrical arrangements. I'd done, you see, quite a lot of crystallography - three dimensional arrangements of atoms, which was all sort of physics, sort of rather stuck and regular and not moving, but then you had these things

[3] Sir John Douglas Cockcraft (1897-1967). British physicist. Cockcroft was a member of Ernest Rutherford's team at the Cavendish Laboratory. He received the Nobel Prize for physics in 1951.

sort of running along in sort of channels, in energy bands and so on. [Marcus] Oliphant[4] used to say that the gaps between the atoms all line up and the electrons just go down through the gaps, which of course… he's a great physicist, but it's not actually quite true. It is an intuitive notion... and so that you had a special structure there which was the basis for a special type of movement and special properties, you see, emerge from these structures. But then with a 2.2 degree I couldn't... I looked into various possibilities at Cambridge but no one would touch me. They couldn't get me a research grant with a 2.2 degree. But I got a good reference from one of the people there… Who was it? I think I remember he gave it to me in an open envelope and I think I actually looked at it. I don't know what one should do in such a case, and I was rather astonished that this chap was saying very favourable things about me. Then I realised that there were other universities than Cambridge in Britain. Normally, this was… you know the whole world was Cambridge, and I was ignorant in a way. So I was very cast down initially that I couldn't stay on to do research. But I went up to Newcastle - that was a depressing environment there. I went to Leeds - that was depressing environment. There was luminescence work in those two places. And I didn't like this. Haworth suggested doing something on the coal utilisation thing... it was dreadful dreary work, I mean… God, coal dust coming down surfaces or something. I was in Birmingham and Oliphant, who was the deputy director, or had been under Rutherford [at Cambridge] had gone to Birmingham, so I was at home in Birmingham, feeling a bit frustrated so I picked up the telephone and rang up Oliphant and he remembered me from my first year there, which was nice. And I said, 'Look, you know, anything interesting?' He said 'Yes, there's a chap called [John] Randall[5] here who's looking for a research assistant.' So I went over and I started that link up with Randall on luminescence of solids. Now Randall was really on the ball in luminescence. He was bringing all the new approaches in, whereas these people in Leeds and Newcastle were a fuddy-duddy lot; they would never get anywhere. But Randall was really going places, so that started me up very well. Randall gave me a very good problem to work on and I got a PhD thesis in two years. So that started me up as a scientist very well and gave me some confidence.

MB You had also published two impressive papers as a part of that PhD.

MW Well, I wrote up my thesis and.... No, I think it was the other way. I wrote the two papers, part one and part two...

MB For the Royal Society.

MW Yes. And then the thesis rules said that you couldn't use papers, but all I did was to take those two papers and put in a sentence or two to link them together and that made my thesis. I mean I wrote it all myself and did all the diagrams myself, so there was no problem. I mean it wasn't using Randall's stuff. So that set me up well

[4]Sir Marcus Oliphant (1901-2000). Australian physicist. He worked at the Cavendish Laboratory and in 1937 was appointed to the Poynting Professorship of Physics at Birmingham University. In 1950 he returned to Australia.

[5] Sir John Randall (1905-1984). British physicist. In 1937 Randall was appointed to a Royal Society fellowship at Birmingham University. He worked on radar during the war and (with Henry Boot) he invented the cavity magnetron. He was appointed professor of natural philosophy at the University of St Andrews in 1944, and in 1946 he became head of a new MRC department of biophysics at King's College, London University.

and I was then working…well, initially, with the war, I was greatly relieved when the war began to go for interview and be told that I was reserved for research in a civilian capacity only – magic words. I thought thank God, you see, I won't need to go around in the armed forces blowing people up. It wouldn't be... it didn't appeal to me. I didn't think I'd be any good at it. So Oliphant had very good contracts for radar so he built up the physics department in a very big way. Before the war began he was in on the radar - a man of considerable foresight. But in the luminescence work, this was a bit on one side, which gave me more freedom so I could push on and get this PhD done. And initially we were working on rather trivial applications of luminescent materials in the black-out. There was some memorandum from the Ministry of Home Security in which there was one paragraph entitled 'luminescent dog leads and dog collars,' and so we made jokes about the needs of the clergy and so on. But this was stupid, stupid, but soon I was able to get on to radar screens.

MB Right.

MW So that was more interesting, and radar was really a key thing in the war and so I got in on that special end of radar, although Randall and the others were doing magnetrons and all the big stuff in the main radar laboratory.

MB Birmingham was really the centre, wasn't it? You had gone to the right place.

MW Exactly. And you see it was choosing to go to Cambridge and then having a connection with Oliphant, who was a very go ahead man with a real vision - he's still alive, I've been in touch with him recently. And so I was very fortunate there and, also, Randall you see was a man with great energy and vision, too. And so this is the way if you want to get on in the world, you have to find out the right channels to work in. If you get stuck in the wrong corner, I mean, you can do nothing very often. So, let me see, that gets me on to the end of the radar work. But once the high power generators of centimetre waves - that problem was solved by Oliphant's lot and Randall - Oliphant said out of this he got in on the fission bomb. And in fact at Birmingham [Rudolf] Peierls, [Otto] Frisch and others were doing extremely important calculations on the possibility of a fission bomb and I'm told that this was critical in the relation of the whole American project as the Americans thought it might be possible.

MB Yes this really sparked the American interest, didn't it?

MW I gather so, and I think the Americans don't like to accept this, but I've never checked up on just how true that is. Certainly it was very important. I knew Peierls and Klaus Fuchs was there too. He solved an integral for me for one of my phosphorescent things. So, let me see whether I have left anything out of that…

MB That's the Birmingham phase?

MW Yes. So here I was, on the atom bomb whereas before the war I had been in the Cambridge Scientists' Anti-War Group, opposed to the idea of science being used for war. But you see the background was Birmingham was full of refugees from Hitler. There had been one man from Austria had lived in our home for a year or two. The somewhat left-wing intellectuals that I associated with in Birmingham filled with excellent characters from Europe: Jews, trade unionists, all kinds of people. It was very inspiring to see what good people these were and this strengthens one's conviction that you couldn't afford to take any risks of Hitler being the first one to get the bomb. Now in retrospect, of course, looking back now you may say that although this was understandable, yet, it still does raise question marks now about whether it was the right thing to do. But I think it was almost impossible, anyone who was in with that sort of thing unless you were an absolute pacifist – there were a few of them about, Christian pacifists - not to go on with the bomb. And so after a while Oliphant got more or less the whole group there moved to Berkeley, California.

MB And you went there with that group.

MW So this was very exciting, getting into the Californian city lights and sun and everything, out of the black-out of Birmingham. It was just a different world.

MB Culture shock.

MW And it was a very exciting place, culturally, generally in a wider sense with all the arts and cultural developments beginning there which led on to the whole sort of beat thing and hippies, later on. And the way in which scientific research was done with big machines under E O Lawrence[6] was most impressive and exciting. The enormous input in funding and engineering skills that went into that thing was overwhelming and what we were doing, you know, we were just sort of just piddling around back in England. Britain could not provide those facilities. The other thing was people like E O Lawrence making big machines had built up a new type of engineering in physics which went far beyond what any of the British, like Oliphant, although Oliphant was always thinking big, the Americans had got ahead in advance and that was the centre of the world.

MB This was the beginning there of a new era.

MW Exactly, exactly. You go from the Rutherford thing where the physicist was supposed to make everything with his own hands – you completely moved out of that to where the physicist had be part of a very big group of professional electrical engineers and all kinds of specialists to build these immense machines and to operate them.

MB Again you are at the right place.

MW Yes, so that was very interesting.

MB How about work on the bomb? Did that get you more and more deeply involved in this aspect or did you still stay on the surface? Were you deeply involved at the centre of that work?

[6] Ernest Lawrence (1901-1958). American physicist. Lawrence was appointed professor at the University of California at Berkeley in 1930, and director of the radiation laboratory in 1936. He was awarded the Nobel Prize for physics in 1939 for the invention of the cyclotron.

MW Well, we were on very much the fringe of the bomb, because before we got to Berkeley the separation of… the electromagnetic spectrograph method of separation of the uranium isotopes, which was the critical thing if you wanted to use uranium, which Peierls and Frisch had said if you can separate uranium-235 present there in only a small amount in natural uranium, if you could separate that you could make a bomb. But this had already been done. They had set up an enormous plant with hundreds – I don't know how many it was - of immense mass spectrographs the other side of the States - I think it was Oak Ridge [Tennessee]- and this was operating. But all that we were doing was helping to improve the design a bit, the problem had been basically solved, and so in one sense I didn't do anything useful to help the atom bomb. And the particular… which you might say eased my conscience in hindsight appears – it's a silly way of looking at it, but I mean that's the way it was – but I was also... again I was lucky but I was also put on a special problem in Birmingham of trying to evaporate uranium metal and not use the hexafluoride, which was the big thing everyone was using - a very difficult problem on which I failed completely, in Birmingham. But going to Berkeley we had much better facilities and we did make some progress on this, but in the end it didn't come to anything. We had a very exciting time for a while working three shifts, that was the usual thing there. Our shift was the worst, from midnight to eight o'clock in the morning. These big machines had to be run around the clock. Normally a lot of my work was done as an individual. You see, I had my own equipment, I had my own sort of problem there and I was told to get on with it. Lawrence, he switched me from evaporation to using sputtering. It was very impressive having contact with him. But sputtering then started working by evaporating it by ion bombardment and we had a little team built up and it was very nice working with a little team then. You see, one had such a nice sort of comradely feeling with about half a dozen people, and then you take over from the group before, the shift before, and you compare notes and then you go on your shift of eight hours and then you hand over to the other people. So I saw both things, individual research and also the very nice spirit of communion in a group enterprise.

MB You were in America when the war came to an end?

MW Yes. The first we heard about the actual bomb was the news in the newspapers about Hiroshima because we would not be… except the higher ups like Oliphant, they wouldn't know what was going on at Los Alamos and wouldn't have known about the test in the desert, and so I think that must have been it. Then the whole question was what one's attitude was.

MB It must have been a big shock.

MW Well, it was and it wasn't. My memory… I've been trying to get this a bit clearer. There was a man there who was a philosophy don, who during the war was put on, in the Berkeley laboratory, on designing big vacuum pumps - a very good chap and we used to talk a lot over lunch sandwiches, and I remember that evening going down to see him at his home and being a bit struck by how downcast he was. The bomb had gone off on the Monday and he said 'It's Black Monday,' and he and his wife were very low. I'm pretty sure that I wasn't very low, I may have been a bit low, but I think he had a better understanding of just what that meant. And so I took the cue from him. I thought well, you know, I suppose he's right. And I think this illustrates the extent to which you get caught up in these research enterprises and you have difficulty in viewing them in wider perspective. This is a bit shocking in a way because many of us… there were several of us on the project: there was Eric Burhop[7], a friend of mine, he came from Australia and he was also Cambridge Scientists' Anti-War Group or had a best friend or some connection like that. So you had these people who had been thinking about the wider problems of science in the future and science and war and peace, there. And yet some of us, I think we hadn't quite ... we were a bit sort of confined in this community and I think this is one of the major problems of trying to get clear to what extent the scientists were trapped in their own little conventional world, or to what extent they really did think about the wider implications. People have gone on record making rather contradictory statements about this. But undoubtedly a lot of the Los Alamos people had terrific parties and were very elated after the bomb - certainly some of them. I think it was only the outsiders like Niels Bohr[8], who came over, who were able to see it in a more detached way and immediately threw himself into the whole business of trying to get international control of nuclear energy, all round, bombs and peaceful application, and of course, he failed. But, I mean, I soon fell in with the trends there and saw what had to be done and when I came back to England, I remember being brought to see [Patrick] Blackett[9], who was President of the Royal Society, and putting him in the picture about the Federation of Atomic Scientists and their work in the education of the public and the politicians about what these bombs could really do. They weren't just a somewhat bigger bomb from what people had before. There was the need for what Einstein called a fundamentally new type of thinking.

MB You were in America from about 1943 to 1947. Is that right?

MW No, it was earlier than that. I think it was more like '42 to '45. I think it was about two and a bit years.

MB Looking aside from some of the world events that followed that bomb making process, returning to your domestic life, you became married at that time, to an American.

MW Yes. I got married in Berkeley to an arts student there with somewhat left-wing views, who was into all sorts of things. But their kinds of political interest were rather different from those in Europe because they had the problem, the big problem of anti-black racism – which we didn't have – and ideas about socialism were in many ways much less developed in the United States. But I was sort of into the whole art world there and through her knew art teachers on the campus – some of whom were driven out by the McCarthy business – and these people weren't specially left-wing at all; it was horrible. And David Bohm[10], he was there and I met him. And David Bohm and I and Eric Burhop all went on one summer holiday up to the Lakes and lived in a log cabin. And he was into left-wing things and as he's explained in some

[7] Eric Burhop (1911-1980). Australian physicist and mathematician. He worked on the Manhattan Project (development of the atomic bomb) in the USA from 1945-50.
[8] Niels Bohr (1885-1962). Danish physicist. In 1943 Bohr became a consultant on the Manhattan Project. Bohr was awarded the Nobel Prize for physics in 1922.
[9] Baron Patrick Blackett (1897-1974). British physicist. Blackett was awarded the 1948 Nobel Prize for physics for his development of the Wilson cloud chamber.
[10] David Bohm (1917-1992). American physicist. Bohm worked for a period on the development of the atomic bomb at Berkeley, California.

interviews I've recorded with him for a possible intellectual autobiography, he says well, it was the American depression – the fact that capitalism had not worked – this made an enormous impact on people in the United States and so they started casting around for alternative approaches. That led them towards Marxist ideas and socialism, and I think the weaknesses of the Soviet model had not properly become apparent at that time.

MB Maurice, when you'd got through this fascinating American phase of your life, what actually brought you back? Did the whole team come back to Birmingham?

MW No. They split up. What happened was that Harry Massey, one of the British party, a very good man, I think he was aware of the fact that I was not quite clear what I was going to do after the war. Mind you, this was a general problem. But anyway, he brought me Schrödinger's little book *What Is Life?*[11] and said, 'Look, you may be interested in reading this. So I owe that to him. Now, Randall, funnily enough, had, post-war, got the headship of the physics department at St Andrews [University] and had written inviting me to a lectureship there – and he was doing biophysics. Now, Randall had had some interest in that at Birmingham and it didn't move me at all. I wasn't interested in the biological sciences really. Also, I hadn't been interested in Cambridge by Bernal's work on the structure of viruses because these things struck me as static structures and I thought what is the interest of just finding a static structure of some damn thing here like another crystal structure? I failed to see that these static structures could have dynamic properties and I think at that time this was not an unreasonable lack of vision because none of these… one had not got the foggiest idea of what the structures were like. And so, in spite of the charisma of Bernal, I was not led in that direction at all, and when I got the invitation from Randall I wrote back and said 'Thank you very much, but I don't want to do biophysics' – and I was still casting around. I even had some vague idea that I might go to Paris and be a painter because I was very much into amateur painting and art. And I thought well, why do I have to stay in science. I was developing a very ambivalent attitude towards science and art. They appeared somewhat polarised. Most of my friends and my girlfriends, they were always on the non-science side: they were dancers and artists and so on… nearly all dancers – a long list of them. I don't mean that I had a long list of girlfriends but that the ones I knew, they were nearly always dancers – although Ruth, this American girl, she wasn't a dancer but she was into music in a big way, and art, and somewhat left-wing liberal thinking. Now, I read this little book of Schrödinger's and I was very struck by what he said about the structure of the gene being a defined three-dimensional structure of atoms which would encode the genetic message. And as a solid-state physicist, this was very interesting because it was the same sort of language: he was a physicist. Now he also referred to this as an aperiodic crystal. Now, I didn't know… it was not at all clear to me what I really made about this, but here was someone talking about a gene could be an aperiodic crystal – just what it meant I didn't really know – but this helped to switch me on as a physicist who had worked on crystalline electronic properties in solids which were almost entirely crystalline structures, also with aperiodicities in them. Now I think this got me interested. In actual fact, I think what he meant was it was just like a big irregular structure – a defined irregularity in it. I don't think that he meant that it was like a regular crystal with occasional lattice

[11] Schrodinger, Erwin., 1945. *What is Life?* Cambridge: Cambridge University Press.

defects or something. But anyway, that switched me on and so I decided I'll have to go into this sort of physical thinking applied to biology. And it's interesting that Schrödinger makes the remark in that book, he said, you know, if physicists go into this area they make fools of themselves and have no success. And I remember very clearly discussing this with some people there and saying, 'Look, you know, this is going to be a complete failure. If I were to try to become professional golfer or an actor or something I might be an utter failure – but I want to have a go, and jump in.' I also thought that I'd been a success as a physicist, I had really achieved something right from the beginning with that PhD work and I felt that what I'd done afterwards was reasonably successful – although nothing much came out of that – but there was no reason to think it was a failure. And so I thought well, in one branch of science I seem to be able to do it okay, I'll try another one. But I think the other thing was, you might say, I started up in the clouds - meteorology somewhat, but astronomy in particular, remote from human life, not connected with human beings, and I then came down to physics, solid-state physics, which is very much a thing you have on the laboratory bench – you don't look through telescopes at something millions of miles away. And I think you might say that this was the next stage in a progression: to come down from the isolation from the human world which physics has, more into the human world by going into biophysics, where you are concerned with subjects like medicine and agriculture. And so I felt that this was interesting. It was an intellectual challenge and also it got one away from physics with its bomb making and so on. I certainly didn't want to stay in that lot of stuff. Some people did and went back to Aldermarston and so on and stayed in the field. But there was no question in my mind. It wasn't just the revulsion about the bomb – I think it would be quite wrong to suggest that was my only motivation. It was the intellectual challenge as well.

MB How did you get the link back with Randall then?

MW Well, Randall wrote to me a second time saying 'I've still got this post,' and I said okay.

MB So you went to St Andrews.

MW Yes, I went there. But, you know, looking around Berkeley then, I didn't know [Wendell] Stanley was working there on viruses – but I think I can't have known that - and it was not apparent to me that there was anything in the United States which would have suited me as a physicist working in biology. I mean there were immense developments later on, but I think it is understandable that I didn't see the opportunity there. There was incidentally very little x-ray diffraction work in the whole of the United States then compared with Britain, which was outstanding. Mind you at that time I didn't necessarily want to do x-ray diffraction work. But I couldn't see interesting new types of applications of physical techniques and thought to biological problems there in the United States. So I knew Randall was a live wire – I didn't always get on with him very well, I knew that.

MB It was a difficult relationship.

MW Yes. But he had vision, he had push, and he was keen, steamed up about the biology and now I was getting steamed up about the biology, so I said okay I've got a free trip home, I might as well take it. My marriage had broken up.

MB Yes. This was a sad time as well.

MW Yes, I suppose so. But that had blown up six months or a year before I left and so I really had another sort of very interesting type of life post the brief marriage and made a lot of social and cultural contacts there and thoroughly enjoyed myself in California at that time. So really in many ways I was very sad at leaving California, but, you know, I thought well, I'd like to go back and see what's happening in Britain. And maybe this was a mistake, although on the other hand, the British scene scientifically was very much in the lead for at least ten years up to the sixties, when it began to fall behind the States, where all types of new science began to grow, and grow in a very impressive way in the States. So I suppose it was a sensible thing to do. But when I got back to St Andrews of course I had to choose a topic to work on and so I was thinking about the structure of the gene. And I had noticed about [Hermann] Muller's work about x-ray mutations and so I thought, well, what do I do and so finally I decided to see what ultrasonics might do in the way of mutations and chromosome breaks and so on. I thought, well, maybe there's a way into the structure of the gene by trying to break it up. I mean in retrospect it probably was not a very sensible idea, but there again, on the other hand, the whole approach of genetics is to look for small differences in structure and use small differences to illuminate the structure – and the structure as a whole. I remember J B S Haldane once stopping me somewhere and saying, 'What is the essence of genetics, you see? It doesn't tell you about what things that are like themselves, it tells you about the differences between them.' So in that respect that type of approach... let's make changes, small changes in the thing and try to understand the whole thing, you know, had a sensible, philosophical base. So, I started on that sort of work in St Andrews, but after the very exciting cultural and social environment of Berkeley I could see that St Andrews was absolutely the end. If I'd had a wife and young children and we lived in a house near the beach or something, one might have settled down, but after a few months the whole thing was simply to get out. I was not the only one. The more lively staff all wanted to get out. The other thing was that one could see that for a new type of inter-disciplinary science it was a very unsuitable place. You had no proper biologists there. I mean D'Arcy Thompson[12] was a very entertaining man. We used to go to the circus with him; we had a wonderful time but he had nothing to contribute to us except general encouragement - nothing more I think. His sort of mathematical interests I don't think were any use to us.

MB So very quickly you were looking for a way out of there, you were looking for a ticket away.

MW Yes. So I did a lot of exploring of American labs. Again, I didn't come up with anything. I couldn't find any American lab - I wanted to get back to the States - which seemed to be into anything interesting. I was interested in cell division and things like that, you know, the replication of genes. I couldn't find anything there so I was a bit stuck. Then meanwhile, Randall had been offered the Wheatstone Chair as

[12] Sir D'Arcy Thompson (1860-1948). British biologist.

head of the physics department at King's College in London. And I'd already been up to London a lot making the acquaintance of lively young people like Mick [H G] Callan, for instance, the cytogeneticist in Darlington's lab. And at one stage I wrote to A V Hill and he said he would give me a job, but then being a perfect gentleman, he said to Randall, I can't be taking one of your people. He was a very upright person. Probably just as well because I don't think that sort of work probably would have suited me. I remember once saying to Mick Callan, I said, 'Look, why do we have to find some senior person who we've got to work under? Why don't the two of us just go into it and set up our own enterprise?' And we were thinking about this a bit. Now, how the hell you did it, you see, where did you get the laboratory space, you had to fit into some laboratory? But we didn't get very far on this. Now he had some very exciting… all the right ideas about the importance of nucleic acids, DNA on chromosomes. And he knew [H N] Barber - who had in 1940 written to Randall – who had published a book on x-ray diffraction of amorphous and semi-crystalline solids and he wrote to Randall saying, why don't you do x-ray diffraction of sperm heads. Randall was always trotting this out but he never did anything about it. I'll come back to that later. He couldn't during the war, of course. Anyway Barber was there [in London], Callan was there and so in the middle of this Randall said that he was moving to London because the MRC [Medical Research Council] and other organisations wouldn't give him a grant for building up a biophysics school in an isolated place like St Andrews. They said it's got to be Cambridge or London. And here he was at the centre of things in London and so he said, well, you know, you'd better come to London with me, so I said okay. And Randall has said apparently that I went to London without a job, but I suppose he was right. I was unmarried anyway… I suppose I knew that something was going to come out in the wash and I got a post there. I think I may have started as a lecturer initially, I forget, on the MRC staff. And so that was the beginning of the Randall biophysics lab at King's in London, and that grew up very successfully. It was rather a sort of weird mixture and a bit sort of crazy atmosphere of people, inexperienced, going into biology. But we had a solid core of people who really did know some biology in the lab most of them were women, incidentally, like Jean Hanson[13] and Honor Fell[14], the senior biological adviser, who was director of the Strangeways lab in Cambridge, and came down one day a week. But we also had lots of other biologists and biochemists, all round London, and one had all these international meetings and so on taking place on one's doorstep. For example, down at the Royal Institution there was [Rudolf] Signer who came over with the new DNA and offered it to everybody, and I said yes, I'll have a gram of that too and so we all went off with Signer DNA and of course that was what enabled us to make the breakthrough in good x-ray diffraction patterns for the first time. But let me see I'm jumping the gun a bit there.

MB You're beginning to approach the DNA story but we ought to fit in... Randall had this great tie with your career for so long, this was building into Randall's department and really beginning to get associated with a new network of colleagues. This was a very important time and I'm trying to pin-point this. This was late forties, early fifties.

[13] (Emmeline) Jean Hanson (1919-1973). British biologist. Hanson was a member of the scientific staff of the MRC Biophysics Research Unit, Kings's College, London, 1948-70.

[14] Dame Honor Fell (1900-1986). British biologist. Director of Strangeways Research Laboratory, Cambridge, 1929-70, and senior biological adviser to the MRC Biophysics Unit at King's College, London, 1970-80.

MW Yes, we started there in.... I went there at the end of ’46 and I went on with the ultrasonic work for a little while and I got a research student and we got a *Drosophila* geneticist, who was very helpful, who looked at chromosome breaks and things. And after a bit I saw I wasn’t getting anywhere, but I was also interested in making an ultrasonic [ultraviolet?] microscope and I did some experiments on that, but this was big technical thing and we wanted to get very short wavelengths so you’d get a resolving power roughly equivalent to the light microscope, or approaching it, and I dropped that.

MB Maurice, these were years in the late forties when you were looking for something quite significant, trying a number of areas, building new contacts...

MW Well, yes, but I think the main thing was that one was getting stuck into something biophysical and so this first thing didn’t get very far, although Sellman, my research student did go to Edinburgh and went on with it a bit. I don’t think this ever contributed... it contributed very little I would say.

MB I’d like to come in at this point and say that the next years at King’s I’d like to leave to a second part of the interview and we’ll just close down on the first part at this stage.

The Royal College of Physicians and Oxford Brookes University

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Professor Maurice Wilkins CBE FRS in interview with Max Blythe
Oxford, March 1988

Part Two

MB In this second part of our talk today, Professor Wilkins, I'd like to take you towards the build up of your interest in DNA and the work that took place at King's that we began to talk about at the end of our earlier conversation.

MW Yes. As I said, I began biophysical work with ultrasonics trying to make changes in the genes and chromosomes and I was not satisfied with the way this was going, and the next step was that Randall had a few workers making reflecting microscopes to study nucleic acids, mainly nucleic acids in cells. This wasn't going well, and he knew that I had an optical background with the telescopes so he said would I like to take this work over and be in charge of this small group. So I thought, yes, certainly this could be interesting. Now, the interests in this work, scientifically, were a bit undefined. I think the idea of reflecting microscopes was to use ultraviolet light; they hadn't been used much. Quartz microscopes had been used before by [Torbjörn] Caspersson, and Caspersson had built up in Stockholm a school for studying nucleic acids and the way they moved about the cells during cell division and growth, using ultraviolet microscopes. And there had been a Society of Experimental Biology meeting on nucleic acids which Randall had attended. Actually, there was a little bit of tension between me and him – he didn't really suggest that I went to these things with him. I remember that he went to NewYork and saw [Alfred] Mirsky there who was working on nuclear protein. Incidentally, he missed out there because he saw the wrong person at the Rockefeller Institute. He should have seen [Oswald] Avery[15] who was showing that DNA was genetic material. He saw quite the wrong man because Mirsky had a sort of - some people claimed - virulent campaign against the validity of Avery's work. But that particular thing came out in the wash alright, because in our biochemistry department we had a man called Taylor who had worked for a year in the Rockefeller Institute and on enzymes and knew about Avery's work and how that Avery's work was sound because it was based upon the specificity of purified DNAase. And so Taylor told Geoffrey Brown, one of our research students, a physicist who was learning biochemistry by going up to work part-time with Taylor, about Avery's work and how important it was.

[15] Oswald Avery (1877-1955). Canadian born, American bacteriologist. Avery worked at the Rockefeller Institute Hospital (1913-48). In 1932 he began a study of transformation in bacteria, and in 1944 Avery, Maclyn McCarty and Colin MacLeod extracted and purified the transforming substance and showed it to be deoxyribonucleic acid (DNA).

Because Avery's work was often rather passed over by British scientists, but not entirely because I think he got some reward from the Royal Society, but it didn't get much attention. There was a very big school of opposition to the DNA genes thing in Britain. There was a group in Edinburgh, two people there; there was a chemist in Birmingham who said it's all polysaccharides, nothing to do with… that bacterial genetic transformation was polysaccharides – Stacey, that was him. And so there was a lot of general pooh-poohing. In fact the idea that DNA was gene material didn't really come up much at all in Britain. It tended to be dismissed in textbooks and so forth. But we had this connection with Avery and I remember Geoffrey Brown telling me very clearly, you know, that there is this real evidence that DNA is gene material and that was very helpful. I think Randall's interests were somewhat vague. I think that he had various programmes, items on programmes of research for the MRC - he had a MRC unit there and he was the director of it - about cell division and all the mechanics of cell division and to what extent... Well, he didn't seem to be pin-pointing nucleic acids as a primary interest... Well, maybe, I should modify that. It was a fairly important interest, but it was not the only one in relation to cell division, and certainly not DNA.

MB So he was pointing in a strong direction?

MW Not.... It's a bit difficult... to some extent yes, to some extent... he was in the general picture about nucleic acids being interesting and important and you see many people thought they weren't.

MB So he'd gone that far.

MW And Francis Crick[16], you see, working with the protein people in Cambridge in [Max] Perutz's[17] lab, Geoffrey Brown swears, and I certainly remember it too, Francis Crick - because I was friendly with him - coming down saying, 'Why do you spend your time Maurice working on nucleic acid, you should get a nice haem protein like us and do an x-ray structure study.' Of course, Francis denies this completely and says he was always very interested in DNA, but you know…Certainly, what he says may be quite true but there was this other side of it. So we did in general see the light there and... but initially I was working on nucleic acids in cells with microscopy and I'd been doing this for some time before I began to see this whole kind of Casperrson approach was not really getting me anywhere important. I didn't quite see the way nucleic acids move about cells, picking it up by ultraviolet absorption… I mean what precisely was one trying to get at? Well, clearly there were certain things one wanted to demolish like [C D] Darlington's idea that the DNA was only on the chromosomes during mitosis and then came off the chromosomes, so that the genes were in fact proteins and all that DNA was doing was something in the replication of the protein gene. We wanted to get rid of that idea. In fact, Walker - and others in our lab - was one of the people who showed that. So that it is very difficult now for people who have learnt from early days in school that DNA is gene material to picture

[16] Francis Crick (1916-).British molecular biologist. Crick and his colleague James Watson worked on the molecular structure of DNA at the MRC unit at the Cavendish Laboratory, Cambridge. In 1953 Crick and Watson published a molecular structure of DNA.

[17] Max Perutz (1914-) Austrian-British biochemist. Perutz was director of the MRC Molecular Biology Unit, Cambridge (1947-62). In 1962 he was awarded (jointly) the Nobel Prize for chemistry for his work on the structure of haemoglobin.

the amount of confusion and the extent to which leading scientists were not at all clear that DNA was gene material at that time. But we on the whole were in a fairly favourable position. We were new to the subject, Randall was new to the subject, he had gone to this S E B [Society for Experimental Biology] nucleic acids meeting and he'd heard there [William] Astbury[18] talk about his pre-war x-ray diffraction work on DNA fibres, which was suggestive but still not terribly encouraging in the definition he got from his patterns. But Randall was always saying, '[H N] Barber in 1940…' and so on - I got a bit bored with 'Barber in 1940'. He went in totally different directions with fine structure of ram's sperm in the electron microscope and he set up his research student [R G] Gosling on some other problem - I must ask Gosling what the other problem was. And it was only when I went along to Gosling with some of [Rudolf] Signer's[19], the oriented fibres of DNA which I had made and said, 'Look these things might be very good for x-ray diffraction,' that Gosling… we then took x-ray diffraction patterns with the very rough equipment which Gosling had got. But I think afterwards there was some ill-feeling developed because I think Randall felt that I had rather snatched away this plum which was something he had always intended to work on. May be I was not sufficiently sympathetic towards his feelings about this because I felt, well, I'd had to push this ahead myself although obviously my background was influenced by his background and none of us were separate there, but in the main he certainly hadn't been pushing DNA.

MB Can I just pin-point… what was the first date on which crystallography began to be applied in your laboratory to DNA material?

MW Well, it's a funny thing, Randall had applied to the Rockefeller Foundation for special expensive physics equipment for biophysical research and ultra-centrifuge, x-ray diffraction and I think something else, an electron microscope, I think, but he never did anything about getting some good x-ray diffraction equipment in; I don't know why. I never discussed this with him, but it is a bit of a puzzle. This sort of got lost sight of and as a x-ray worker at the GEC [General Electric Company] before he went to Birmingham, this is a bit odd that he was interested in doing x-ray work and on the other hand he wasn't. But I had moved off the ultraviolet work, microscopy on cells, a bit to crystals and virus particles and so on and things like orientated sheets of DNA using ultraviolet dichroism to study the orientation of chemical groups say in tobacco mosaic virus. I was getting on to this molecular structure side, partly as a result of an American, Gerry Oster (?), who was over working in Bernal's laboratory for a year or two, and Gerry Oster was saying to me, 'Look Maurice, as a physicist you ought to be working on the molecular front and not doing these optical studies of dividing cells. I was influenced by that, but I still had some reluctance to get into pure molecular studies because I had this bad memory of Bernal before the war, all this static structure x-ray diffraction work, and it was not going ahead well in Bernal's laboratory post-war. And it was a very long painful slog in Cambridge before those people made any progress up there. So I was not ready to go into that area. But somewhat accidentally we got this very good preparation of undegraded DNA from Signer with his generosity. Various workers used it with different techniques, infrared absorption and so on, and I had this ultraviolet dichroism and then I thought

[18] William Astbury. British x-ray crystallographer and molecular biologist. Astbury worked at the Royal Institution, London (1923-45) and in 1935 began a study of nucleic acids by x-ray crystallography.
[19] Rudolf Signer. Swiss chemist.

well this looks good for x-ray diffraction. Gosling and I got immediately much better patterns than Astbury, which really made one sit up - clearly defined crystalline patterns. Astbury's were good but the main difficulty with Astbury's… well, there were two things: the DNA wasn't much good, and the second thing was that for some reason being a protein fibre man he hadn't realised the importance of humidity in the whole thing. You have to hydrate the fibre. Now, I remembered what Bernal had said about this when I was an undergraduate, the key to make this discovery… if you want to get sharp diffraction patterns from protein crystals then you have to keep them in the mother liquor, you have to keep the water content as an essential part of the regularity of the crystal. So, right from the beginning we were putting water vapour round the fibres, and Randall from his GEC experience said 'You should get rid of the air because this will fog up the diffract. You want to use hydrogen.' Helium wasn't around then, so that we were using… Gosling and I were using hydrated hydrogen around the thing. This gave a big advantage compared with anything that Astbury was doing. He was a marvellous man, Astbury, but in many ways experimentally backward. Well, we got the best patterns on a very unsuitable x-ray equipment in the chemistry department.

MB This was in about 1949?

MW 1950. But after we'd got these good patterns we didn't know what to do with them and I had no experience, apart from a little undergraduate training in crystallography in Cambridge about x-ray difraction. [Alex] Stokes was there; very brilliant mind but he wasn't much on the experimental side, he was very good on the theoretical side, on interpretation, and Gosling was an inexperienced research student. So, I remember we all went with Randall to see [Charles] Bunn who was an expert on fibre diffraction – it worked well in ICI [Imperial Chemical Industries]. We showed him the patterns and said what do we do next? And he said something about getting double orientation and trying to index the spots. But I think if we had spent more time with Bunn and been able to pour over it more, we might have been able to do more, but we had a very exciting pattern basically and were not in the field enough to know quite what to do. Mind you, the thing was to some extent there wasn't a field. What had been done with fibre x-ray diffraction before was not adequate, was not a suitable model for… For DNA work, you had to work out some methods from scratch, which we had already worked out on the experimental side, but we had a highly unsuitable x-ray set and x-ray cameras. So it was at that stage that Rosalind Franklin[20] got a fellowship to come to our laboratory and Randall had arranged with her to do some work on x-ray scattering of protein solutions. So I was very ready to agree that this was just what we wanted; we wanted an experienced x-ray diffractionist to help with the DNA work. Now, when I say the word help, you see, this pin-points part of the trouble, that Randall wrote to her and gave a very clear idea that she would solely be in charge of the work, and so....

MB This is not a picture that you were given, though?

[20] Rosalind Franklin. (1920-1958). British x-ray crytallographer. Franklin worked in the Biophysics Unit at King's College from 1951 to 1953, and then at Birkbeck College, London University until her death from cancer at the age of 37.

MW I knew nothing about Randall's letter. Randall and I discussed this. I personally think that I suggested that she be moved off this silly, ill-defined protein solution work – God knows what it was – on to DNA. Here you had a really exciting thing. Randall in his notes says he suggested it. It doesn't really matter. It was just damned obvious that you were having a new person who was said to be good coming in, so this was the problem. It would be crazy to leave this other thing sitting around there without somebody full-time on it, because I was still involved in the microscopy work. You can't always shut down other lines of work straight away. She arrived in January '51 and I was off in Wales with a German girl in the mountains and I remember, up there one day I was looking at the snow on the mountains and thinking a bit about the research and it suddenly struck me, all this working on the cells with the ultraviolet microscopy....

MB Had got to go....?

MW I'd got to this give up and really I've got to get… my research must be this molecular structure of DNA with the x-ray diffraction. Many people would say well, what an idiot I was I didn't think of this much earlier in the summer. But you know... people, all of us are idiots like that and most people wouldn't have even possibly got as far as we had, you see. Most people are even bigger idiots and that is why they don't get on to things which are important. Anyway, I'd overcome and got set upon that and for the first three months Rosalind Franklin was writing-up her Paris work on carbons and she did nothing about DNA. I remember up to Easter or something I went to her and said, 'Look, when are you going to start doing something on DNA?' She said, 'Don't worry, I'll be starting soon, I'm a quick worker.' But I had gone on doing more work on fibres at different humidities and measuring their lengths and swelling and so on. But it is interesting that I missed a particular point, although the fibres increased in length with humidity I was misled by the fact that the diameter increase was roughly the same and so I thought it was an overall swelling. I missed the important point, which she got later, was that the increased length was to be associated with a change in the extension of the helix. Mind you, there was more to it than that. And so, in the autumn before she came, we decided we had to get some equipment which was suitable for working on thin fibres of DNA. In one letter to Roy Markham, I said we should have a microcamera that would work on one fibre of DNA, because we'd been using bundles of possibly thirty or something fibres, very laborious to assemble these things. And we knew about microcamera work and so we had to get properly set up with the right sort of x-ray equipment and the right sort of x-ray camera and the right x-ray generator - but we decided not to go in for rotating anode x-ray equipment. We went into microfocus, which was being done by [Werner] Ehrenberg at Birkbeck, and so we got one of these things in. Stokes was setting it up, but when Rosalind Franklin came she wasn't satisfied with the way Stokes had set it up so she set it up again and now she... it must have been she who decided to get a Phillips microcamera because I was getting the workshop to make a Huxley type, with Hugh Huxley in Cambridge - but she bought this equipment and it worked very well. And as soon as they got a single fibre into that camera with the Ehrenberg fine-focus tube - an enormous improvement in the patterns. But meanwhile, before that was set up, before they got the better patterns, I was impatient and wanted to do a bit more, so I went back to the old equipment. I took some diffraction patterns, various specimens of DNA from different sources and also sperm heads, or the sperm head was the main diffracting part, as Barber had suggested in

1940 to Randall, and I was able to show that the diffraction was off the length of the main axis and at [Max] Perutz's meeting on proteins in Cambridge I reported on this and I said here for the first time is experimental evidence that DNA is a helical molecule - as a hypothesis. Mind you, Stokes had first come out with this suggestion, looking at the crystalline pattern, and pointing out that the absence of reflections along the meridian... he must get the credit for that, but on the other hand, these other patterns I got, which were poor patterns, reinforced the picture on the helix. And so this made quite an impression. I heard some comments in the tea-queue - favourable comments about my talk and the result was that Rosalind Franklin came up to me afterwards and the first time we had any differences at all. She said, 'Look, you know, you must stop this and get back to your ultramicroscopy. Go back to your microscopes, this is my work.' So I thought, really, what's she talking about, I mean, I can't take this seriously, because I knew nothing about the Randall letter. This was a shame. Mind you, whether we could have sorted it out I don't know. And so this whole business about Stokes and me saying its helical was a very difficult position for her because when she got the transition from the crystalline A to the less crystalline B thing, which was more obviously a helical diffraction pattern it was strikingly like the calculations which Stokes had made of diffraction of a helical structure. Because in the Cambridge talk I had been using the idea of a helix was just a sort of series of zig-zags - that was good enough for my purposes then - but I said to Stokes, you know, there's [Linus] Pauling[21] working on alpha helix diffraction; he hasn't got a proper theory of helical diffraction. I said, you know, 'This has got to be worked out, why can't you do it?' And so, I've always said, and I think it is more or less true, that he worked it out going home to Welwyn Garden on the train. He was a very clear thinker and a very economical thinker. And so after Rosalind Franklin got this B pattern, I mean, I remember Stokes and me, we went down the corridor to her room and said, 'Isn't this marvellous, you see this pattern is just like this helical diffraction.' And then she blew up, you see, and said 'How dare you interpret my diffraction patterns.' So we retired in confusion. And this sort of set the scene. She was caught in the trap that if she came out openly and said it's helical then she'd have to work with us, and she didn't want to work with us, she wanted to work on her own. Okay, people and scientists are entitled to work on their own. Why shouldn't they? Some people don't like working in groups for all kinds of reasons. But I don't think there was any personal animosity. We'd been quite friendly for four or five months. We used to go out for lunch together on a Saturday when we were both working there - two unmarried people in the lab, you know, some other colleague often. There was no trouble at all. She was a bit abrasive sometimes, but, you know, no harm in that. But I think the main trouble was that she wanted to do her work her way and she didn't want Stokes and me barging in, as she saw it, with simplifying hypotheses. Of course, you couldn't get anywhere with a problem like that without a simplifying hypothesis.

MB The more exciting this work became the more difficult that situation must have become?

[21] Linus Pauling (1901-1994). American chemist. Pauling worked on a variety of problems in chemistry and biology, and from 1934 he started working on complex biochemical compounds. He received the Nobel Prize for chemistry in 1954 and the Nobel Peace Prize in 1962.

MW Well, I... You see I thought something has got to be done about this, we've got to get on, we've got to get some working thing. So I have a very clear memory that I decided that she must work on the Signer DNA, and I think this was after I had been to New York and [Erwin] Chargaff[22] had given me a lot of stuff that made fibres. And so I was really a bit foolish, I thought the fibres looked good, they'll give good diffraction patterns and so I handed over all the Signer stuff to her and said I'll work on this other stuff. But it was a bad bargain really because the Chargaff stuff didn't... well, it gave tolerable patterns but nothing like the Signer and I hadn't got the courage to go back again and say, 'Look, sorry Rosalind, you know, we made an agreement but I want to break it now because this stuff is no good and I want to use some of the Signer as well.' I wasn't prepared to do that because I was probably rather frightened of the utter scorn I would get about how can you behave in such a despicable manner.

MB And so you stuck in.

MW And so I didn't get very far, but I made some progress on getting B patterns from other material. I made my own x-ray camera or got it made in the workshop. I'm not very clear which x-ray set I was using then, but in the main certainly the Ehrenberg thing was handed over to her, but maybe I got some time on that, I don't know. Then my work tended to fritter out somewhat, then. I was isolated. I found it difficult to work under... I'd never had this situation before of working in a laboratory where previously people had been friendly and co-operative.... and I find it difficult to work in an atmosphere like that. Then...

MB Was there by that time a feeling that there were glittering prizes ahead.

MW Oh, I don't think there were thoughts of a prize at all.

MB It wasn't that kind of… just closed professional…

MW We were very interested in making some progress on this problem which undoubtedly was very important, and there's no question that I saw this as being very important because I had written to Geoffrey Brown, who had gone to Stockholm for a year. He remembers that I wrote that we were finding the structure of the gene, exclamation mark. But I don't think Rosalind Franklin realised this. She hadn't got any biological background. It wasn't to her detriment. We'd been there several years working with… as biologists, reading biology, and so...

MB I'm just trying to account more for Rosie Franklin's kind of tightness and possessive...

MW No, I think she'd been engaged to do this problem; here was a nice pattern and she wanted peace and quiet to get on and do it. The other thing was that she liked to keep to professional procedures and conventional procedures, like this three-dimensional Patterson analysis which was a fairly automatic thing. You just number-crunch data in. And she did very good work in setting up the procedures for

[22] Erwin Chargaff (1905-). Austrian-American biochemist. Emeritus professor of biochemistry at Colombia University. Chargaff established the one-to-one ratio between the purine and pyrimidine bases in DNA.

measuring the x-ray intensities, you know, got a proper professional approach to the whole thing, which we as amateurs hadn't done. So she contributed a great deal to it, but we then came to the time in the summer of '52, when she called a meeting, and Stokes and I went to a discussion with her. I don't know whether Randall was there. And she delivered this new evidence that the molecule was asymmetric and couldn't be a helix. Stokes and I like idiots didn't ask can we take the evidence away and study it. I mean, I think we thought she would have been very offended by suggesting she didn't know how to do her job properly, and we didn't; we were fools. After that I didn't work on DNA for more than six months because I thought it's useless, leave them to this procedure which she says must just go on and on and on. Maybe I'm wrong, maybe something will come out of it, leave her to it, you know, what's the hurry. I mean all this thing about races and so on, I mean, it's not the way I look at things. I want to enjoy the work in a lab and if you miss some race, I mean, who cares. The important thing is enjoying your work doing the research. Of course, one's disappointed if one doesn't score a bull's eye one's self, but this isn't the primary thing in life. It's the question about what is success, you see. I think the real thing about success is making something meaningful of your life and not hitting jackpots. You should try and hit jackpots, of course, but the jackpot shouldn't dominate your existence so that you are at the mercy of the jackpot. You should enter into the work with enthusiasm, yes, but the real thing you might say is in the travelling and not in the arrival, I would say.

MB But when Crick and [James] Watson[23] eventually became involved, then I did feel, all the impressions I get are that people started to step up to what amounted to a race of some importance.

MW Yes, that's true. According to Watson's story[24] he seemed to be thinking in terms of Nobel Prizes from the word beginning and presumably... My impression is that what he's set down in his book is an accurate account from his point of view, which is a big qualification. I don't know. There is very little in there which was in error, which is in striking contrast to some accounts.

MB I may be outstripping the flow of the story, but when did Crick and Watson become involved... was it when you returned to the story in early 1952?

MW This is a bit complicated. No. You see, I knew Crick and what I don't understand is that when I gave my talk in Cambridge in July '51 that Crick doesn't seem to have heard this talk. Other people were… I mean it was Perutz in the tea queue saying to somebody else, saying, 'That was interesting what Wilkins was saying about DNA being helical,' and everything. And I was a bit embarrassed because I thought he's going to turn round and see me there. So I remember very distinctly that that talk went down well. Crick seems to have known nothing about it. I don't know. Maybe he wasn't in the lecture room at that time, which is odd too. Anyway, I can't explain that. But it was when Watson came to Cambridge that he got interested. And Watson you see had seen the diffraction patterns and heard me speak briefly in Naples, as he says in his book - I think it was May. And so I couldn't make

[23] James Watson (1928-).American biochemist. Watson worked on the molecular structure of DNA with Francis Crick at the Cavendish Laboratory, Cambridge.
[24] Watson, J. D., 1968. *The Double Helix: a personal account of the discovery of the structure of DNA.* London: Weidenfeld and Nicolson.

Watson out in his conversations with me. I didn't know what he was going to contribute. What I didn't appreciate was that Watson had a very extensive background of ideas about genetics and gene structure and so on, which I was relatively unacquainted with. So I think he had these contracts and experience in various research lines in the United States which placed him very well to see the immense importance of this DNA work. And so he linked up with Crick in Cambridge in September or something...

MB This is '51?

MW Yes, '51. And they started some work off. Their first model was all wrong, which was due to Watson mishearing what the water content was, which was partly true. But it was also, I think, that their model was based on the very reasonable hypothesis that the regular parts of the molecule, the phosphate-sugar, have to be at the centre to determine the regularity. That's very reasonable, so it wasn't just water content, I think, anyway. [Linus] Pauling had made similar models for the same sort of reasons, but it obviously wasn't right. And so there was all this argy-bargy about were Crick and Watson in one MRC unit treading on the feet of another MRC unit. This point often isn't brought out. There were two MRC units and it would look to head office, putting money into these things, they'd say what's going on, these people are working on the same thing. So some of it was a matter of gentlemanly agreements between [Lawrence] Bragg[25] and Randall, but there was also the business about MRC head-office asking what these programmes were. So that as the work had started in King's the natural thing would have been to let King's go on, or have a collaboration. But collaboration certainly was not on after Franklin had arrived because she would not have agreed to collaborate with them, because they in many ways were written off in Cambridge, described as butterflies, superficial people, terribly clever, but they'd never do anything solid. This kind of attitude prevailed amongst biochemists, at least. So she - like many women - the approach was to keep to conventional approaches because if you're a woman in scientific research you want to play safe and you don't want to stick your neck out on crazy things, unconventional things; you cannot afford to. And so I think that it is not to her discredit that she had this inclination. But conventional methods were not suitable. You had to develop new methods for this kind of thing. So the newcomers like Watson… and Crick was quite new to DNA and fibre diffraction, he had done some crystallography in protein, three-dimensional crystals, which was different. These people could bring new angles to it. We could bring new angles to it in our lab. I think one of the sad things was that Rosalind Franklin, at the time she gave us the evidence it couldn't be helical had a month of two before got a much improved B helical type diffraction pattern which she didn't show to us. And I think it indicates the very destructive atmosphere which had built up in the place, but I think basically she was an honest and very decent person that I could respect, but that it didn't occur to her to show this to Stokes and me. Because if that had come out of the drawer then, I mean, Stokes and I would have gone through the ceiling saying, 'Look we could see good evidence of it being helical before, but this is so much clearer, you've got the same type of pattern much clearer.' I think

[25] (William) Lawrence Bragg (1890-1971). British physicist. In 1938 Bragg succeeded Ernest Rutherford as head of the Cavendish Laboratory and Cavendish professor at Cambridge. In 1953 he became director of the Royal Institution, London. He shared the Nobel Prize for physics with his father, William Henry Bragg, in 1915.

[Aaron] Klug[26] said something about that it wasn't obvious to her that it was of any importance or something. Well, she wasn't that stupid. Anyway, it wasn't shown and I was handed that pattern shortly before she was leaving the laboratory for Birkbeck at the beginning of '53, and it was at that stage that I, possibly rather indiscreetly, showed it to Jim Watson on a visit to the laboratory. I thought it had been handed over to me and that I was entitled to do what I liked with it at that stage. She was going to Birkbeck. But you might argue that I should have discussed it with her first, but on the other hand it had been handed to me without any qualification. She was leaving, so I don't think it was anything very dreadful I did, but it was very foolish in a way because it stimulated Watson to get very excited. But I had thought he was quite convinced that it was helical in any case. In his book he rather astounded me by saying this really set him in motion again. I didn't think he needed any encouragement because when I told him in Paris after the anti-helix evidence, at some meeting, I remember talking in a café and I said, 'Look, you know, experimental evidence it's not helical.' And he just said 'Oh, I don't believe that. It must be helical.' So I thought he was a 100% switched on helix man. Evidently he wasn't . So this really stimulated them and they had another go. But they also had got the research report from the MRC unit and that contained critical information, some of which Crick was able to interpret the significance of: the monoclinic unit cell and the evidence on the lengthening of the fibre with humidity that Rosalind Franklin put down there, which… well, I had got data like that earlier but I hadn't linked it up with the A-B transition which she did. That was the critical thing. Various things like that which were very important were there. And of course years later it comes out that after her final talk before leaving the laboratory, in the last few weeks before she left the laboratory, her notebook showed very clearly that she started helical interpretation of that pattern herself and made great progress towards it being a double helix. Now I'm not quite... there's a bit of argument about just how far it was, but it was a long way towards that, no one can argue about that. And so she had become a convert at the very last minute, which I think one can explain on the basis that she had done nothing for nine months on that pattern, but I think I would say it is very reasonable, she was leaving DNA work, going to another laboratory, Randall had put some pressure on her and said, 'Look, when you go can you give up DNA work?' - which wasn't totally unreasonable I think in the circumstances. Some people have said that it was a dreadful thing to say but she apparently was ready to agree to this, and I suppose she felt well, before I go, let's just assume it is a helix and see what happens. And she sat down there and did the sort of things which Stokes and I had been saying for a long time and made remarkable progress. Another little thing was that I had become a convert to base-pairing - one to one ratios being very important - and I was very slow in wakening up to this, but at the beginning of '51, I said to Jim Watson once, 'I think this is the key to DNA structure,' and he said 'Yes, I do too.' So, this idea was in our laboratory. I was doing it actually the wrong way. I was trying to use base pairing to explain the fact it was helical but gave non-helical diffraction, to explain her results, which after she left I asked Gosling for those results, worked through them and saw really there was no clear evidence there to the anti-helix. It was, I think partly, you know, as scientists do, wishful thinking. So after the

[26] Sir Aaron Klug (1926-) South African biophysicist. Klug worked at the Cavendish Laboratory, Cambridge from 1949 until 1954, when he moved to Birkbeck College, London. In 1962 he returned to Cambridge.

Watson/Crick model[27], the first thing we had to do was check up on the anti-helix data. Then we got better data then with improved cameras. And then the Signer DNA, rather to my surprise when I asked Gosling, he said it had all gone, but we got some new DNA from other people like Hamilton in New York and we got better diffraction patterns then and were able to check up, and the anti-helix evidence just faded away.

MB And the Crick/Watson model became more solidly established...

MW Yes, well it took a very long time to adjust that type of model to give exact agreement with diffraction data. It took us years and some people tend to deride us for being so slow, saying Watson and Crick built a model in a few days and Wilkins and his people took seven years. They just don't understand, it wasn't just building a model. We were building up all the procedures with calculations, diffraction patterns and computer programmes and all new approaches, which hadn't existed before. So a lot had to be done. So all that rather boring confirmatory research, it went on for about seven years, but we had several different patterns. I forget whether we had two, or we had three eventually: A B and C, and we had to measure all these very accurately, much more accurately than they had been done initially by Rosalind Franklin, which is again not devaluing what she did. What she did was very good but we needed to get much more precise results and it was possible to do that and that was where some of the time went in over the years. We got lithium DNA, lithium salt, and the sodium salt which crystallised in the B structure, which enabled much more accurate diffraction intensities to be got and we could build three structures: A, B and C with models and get a good agreement with the diffraction data for all three. It was important we get it tied up because after we had done that there were still other people coming forwards saying this wasn't right and trying to push alternative schemes. I don't regret the time that was spent. In a way it was a bore and I remember Honor Fell coming to me and saying, 'You've got in a rut, why do you go on year after year on this stuff? Get on to something new, do something more exciting.' And I thought, well, I've started on this and I'm going to finish it because I think this is very important and it's got to be tied up.

MB Maurice, it is at that point, right on the bell of lunchtime, I'm going to leave it at that classical moment, if I may, and we'll talk later, at some length on what was to follow.

In 1962 Maurice Wilkins, James Watson and Francis Crick were awarded the Nobel Prize for physiology or medicine for their work on the molecular structure of DNA.

[27] Watson, J.D., and Crick, F.H.C. 1953. Molecular Structure of Nucleic Acids: A Structure for Deoxyribose Nucleic Acid. *Nature*, 171(4356), 737-738.