History of Modern Biomedicine Research Group
School of History, Queen Mary University of London
Mile End Road, London E1 4NS
website: www.histmodbiomed.org

AUDIO INTERVIEW TRANSCRIPT

Norris, Keith: transcript of an audio interview (15-Dec-2015)

Interviewers: Tilli Tansey, Bob Maynard

Transcriber: Debra Gee

Editors: Tilli Tansey, Bob Maynard

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Norris, Keith: transcript of an audio interview (15-Dec-2015)*

Biography: Dr Keith Norris BSc PhD (b. 1928) trained as a biophysicist at King's College London under M F H Wilkins (later FRS and Nobel Prize winner for his contributions to the study of the structure of DNA), where he developed reflecting microscopes for the study of DNA with ultraviolet and infrared radiation. He joined the Microbiological Research Department, Porton, to apply physical methods, including infrared spectroscopy for the detection and identification of bacteria in the atmosphere, becoming Head of Aerobiology and Field Trials, before moving to the Chemical Defence Establishment (CDE), Porton, as Deputy Director responsible for the development of chemical defensive equipment, and then to the Ministry of Defence in Whitehall as Director for Chemical and Biological Research and Director for Internal Security Research, at the time when the United Kingdom was taking the lead in establishing the 1972 Biological Weapons Convention. He then spent two years as Scientific Adviser to the General Officer Commanding, Northern Ireland. Retiring in 1983, he became a Consultant to the Director of the CDE and a part-time Regional Scientific Adviser to the Home Office, and served on the Home Office Home Defence Scientific Advisory Council until 1993, when Civil Defence was abandoned by the Government.

TT: Tilli Tansey

BM: Bob Maynard

KN: Keith Norris

TT: Can you just say a little bit about your background, where you come from, Keith?

KN: Yes, I came from a very simple background. My father served in the trenches during the First World War. My father, basically he learnt to shave in the trenches, he learnt to drink rum in the trenches, but when he came out he would not talk, he would not talk about it to me at all. In fact he only talked about it once in the house. But I had a friend from Northern Ireland who was an historian, an army historian, and came, and they hit it off and they talked about World War I just like this. And that was the only time my father ever talked about it. My father was basically self-taught; he worked as a local government officer. He was more or less a secretary to the Medical Office of Health for the North Riding of Yorkshire when tuberculosis was the scourge. And in those days the county ran about three or four sanatoriums, and so the top medical man went around these almost, not weekly but regularly, but my father went with him to pick up all the bits and pieces that needed doing. And that was his job; he was quite remarkable. He'd been a signaller during the war and he was remarkably good at Morse code. I mean things like Morse codes, typing, shorthand. And in fact he taught shorthand for years. He tried to teach me, and I regret to this day that I never learnt it.

TT: Where did you spent your childhood?

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* Interview conducted by Professor Tilli Tansey and Professor Bob Maynard, for the History of Modern Biomedicine Research Group, 15 December 2015, in Salisbury, Wiltshire. Transcribed by Mrs Debra Gee, and edited by Professor Tilli Tansey and Professor Bob Maynard.

KN: Northallerton in Yorkshire. It was the county town, and I went to the junior school and to the secondary school there. But my mother was a staunch Roman Catholic, and so I had to go to a Catholic school. Well, that meant travelling to Darlington 15 miles away, or Middlesbrough 25 miles away. And in the end, one of my father's old friends from World War I, who lived in Middlesbrough, said, 'Well, why doesn't he come and stay with us?' So I went to Middlesbrough. But of course that was 1939. I moved to Middlesbrough just at the time when people were moving the other way: they were being evacuated to Northallerton, to avoid the bombing. But I missed two terms, the first two terms at Grammar school, because the school hadn't got adequate air raid shelters. And so I commuted to Middlesbrough; I was a weekend commuter to Middlesbrough for the first four years of Grammar school, and I did my school certificate and did quite well. That was during the war where we lived next door to the fire station in Middlesbrough and we were bombed out one night. Teesside was a target for the Germans, so we spent many hours in air raid shelters. But very happy years; I remember skating in the park on the ice in the winter, when we had hard winters when we were skating. Lovely.

TT: Did you get interested in science at school? Was this influenced by your father?

KN: No, it was in the Sixth form there that I became interested, where I ran into a good physics master, one physics master, and he suggested to me that I needed to go to university, and he was remarkable. He was one of three people who've really moulded my career. He took time off to go and see my parents and persuade them that I had to go to university. So in 1945 I went off to London for an interview at King's and then I had - I was telling Bob [Maynard] earlier - I had pure maths and applied maths as one subject at High School, and I had chemistry and physics. And the interview at King's was very good. They asked me why I was interested in physics, and it was largely because I'd picked up one of the Christmas lectures by Bragg and this was the inspiration. The fact that I mentioned this in my interview they said, 'Yes, we want you as a student but go away and do your maths as two separate subjects.' Your maths won't be good enough for the physics course. So, in the end, that meant that I changed schools and went to Northallerton Grammar School. But it had one good advantage: it was the first time I met my future wife, so it did some good. But there it was very difficult, because they didn't have a good physics master and then the chemistry teacher was even worse.

But I did the pure maths and applied maths as two subjects, as separate subjects, and got the grades required, and so I went off to King's in 1946. Well, being a schoolboy I was faced with a group of about, I think we were 18, probably 20, I don't know now. There were three of us straight from school and all the others were ex-servicemen. One navy Wren, and two from the Royal Air Force [RAF], another couple of navy bods, but many from the army - all of them had been involved during the War in radar communication and so on, technical things. Physics was a very interesting subject. Randall came and, of course, I attended his inaugural lecture, which was absolutely brilliant because he was demonstrating modern radar techniques on the bench in front of us, and cooking things with, what we do now - a microwave; that was in 1946. So it was all very good.

BM: Could I just ask? Randall, he came to King's from…

KN: St Andrews. He came to King's, he came with Wilkins from St Andrews.

TT: They came together?

KN: Randall and Wilkins worked at Birmingham before the War. They were at Mark Oliphant's Department. They worked on phosphorescence. During the War, Wilkins was sent to work on the Manhattan Project in the United States. Randall went off and developed the cavity magnetron, which made airborne radar possible. It reduced the wavelength of radar from metres to centimetres. And really, he invented the method of generating high powered microwaves. His inaugural lecture was absolutely brilliant at demonstrating on the bench.

Basically, at the end of the War, Randall went to St Andrews to do physics, because he wanted to create a biophysics unit. Wilkins joined him to do biophysics, because he didn't want anything more to do with classified work - particularly on weapons - but they discovered quite quickly that there wasn't space or there wasn't funding available in Scotland, and so Randall looked for somewhere where he could develop a unit, and he got the Wheatstone Chair at King's.

BM: Did you ever hear why Randall showed interest in biophysics, because at that time I would have thought that Randall was a straight up and down physicist without any interest in biology or biophysics at all. I've always been surprised that he should have shown an interest in biophysics. Now I wonder whether he had met and had known people like Alan Hodgkin? Hodgkin won the Nobel Prize with Andrew Huxley for their work on the nerve action potential. And Hodgkin was at Cambridge before the Second World War. He was just getting off the ground. I wonder what it was that triggered Randall?

TT: A V Hill?

KN: Somewhere in the back of my mind I have got something about this. Randall did work with one person who was interested in biology. I can't remember the name at the moment, it will come to me afterwards, but there was one person that Randall had worked with, and I think this was probably dealing with X-rays more than anything else. But, I think the wartime experience of so many people - so many scientists have been pitchforked into subjects, operational research, all sorts of subjects like this - and, I think there was benefit of a multi-disciplinary approach to problems. You need to bring a range of disciplines to bear.

TT: Randall's Unit was funded by the MRC [Medical Research Council] wasn't it?

KN: Yes, it was.

TT: So perhaps pressure/direction came from the MRC as well?

KN: Well, the thing was nucleic acid, people had X-rayed nucleic acid before the War. Avery had done some work on nucleic acid and had got some very poor pictures, because his specimen was not crystalline. I think Randall was more interested in proteins than nucleic acid actually. In fact, for many years he was working on collagen, or he had the biologists working on collagen. He thought he was going, you know, collagen and muscle. Jean Hanson was a great muscle woman working in the Department, but he was primarily interested in protein. And in fact, yeah, I don't know. I suspect it was things like penicillin, the development of penicillin had to have a multidisciplinary approach. In fact one of my colleagues, when I went to Porton, one of my first colleagues who lived next door to me, Frank Belton, he was working with anthrax, and he actually infected himself. And, fortunately, at Salisbury there was a hospital, the Odstock Hospital, and there was a base built for the Americans. And because they wanted to isolate him, they sent him to Odstock Hospital. The Americans had penicillin and I suspect he was one of the first persons to be treated with penicillin in this county, and he made a very good recovery. So, you know, it was, I think it was multidisciplinary things that were in the air.

BM: I think you may well be right, because certainly people were taken, you know, they were asked rather than just taken. People like Alan Hodgkin.

KN: Was that Dorothy's [Hodgkin] husband?

TT: No.

BM: What was Wilkins' background?

KN: Wilkins was working with Randall at Birmingham on phosphorescence, luminescence and things like this. He was a physicist, yes. He was a physicist and, in fact, phosphorescence and luminescence were all

properties of radioactive material, and what did they want on the Manhattan Project but people who knew about radiation; so he was a natural for that. But, I think he came away, when he came away, he was determined to have nothing to do with it, and hence his becoming Chairman of the British Society for Social Responsibility in Science and so on.

TT: Can you say something about being an undergraduate at King's?

KN: I was an undergraduate at King's and I made the mistake - I think it was a mistake - of going on and working in the same Department, because when you were in that Department as a post-graduate you were still one of the, you were treated the same as you were as an undergraduate almost. I think I would have done much better, except I wouldn't have had Wilkins as a tutor. And so when I finished my degree I had another year to do before my degree was awarded, whereas the ex-servicemen got their degrees immediately.

TT: Why was that?

KN: Well, I don't know. It was a peculiar arrangement at the end of the war that if they came back and did two years, it was a three year degree course, we did a three year course in two years. It was crammed; it was crammed into two years and so we had lectures, we had six hours lectures on Monday, six hours lectures on Tuesday, three hours lectures on Wednesday morning, Wednesday afternoon was tutorial, Thursday and Friday were practicals. It was tough, there wasn't an hour off.

BM: Almost as bad as medical school.

KN: Well, it was a school, it was a school. It was run as a school.

BM: You did the same course, but you had to add on another year at the end of it, because you weren't an ex-serviceman. I think that was the reason.

KN: That's right, that's how it worked.

BM: But tell us now, what you did in that third year? You didn't do any more physics?

KN: No. I didn't do any more physics.

BM: What did you do in the third year?

KN: Well, in the third year I was given the choice. In the third year I started doing research. I started the general honours biology course. I did the first year of that. I also became a demonstrator for medical students.

TT: So why did you do the biology? What intrigued you about the biology?

KN: I only did a year. I think Bruce Frazer, who did it with me, he's also tied up in the DNA story. I think we upset the lecturer, I think. She was talking about how fluid got to the top of the tree, you know, and she was talking about suction pressure and, I think Bruce got up and said, 'Excuse me, but isn't a suction a [sucking sound] and a pressure's a [blowing sound]?' So we were not very popular because we were asking awkward questions because the biology in those days was very descriptive; well, we did dissection and we did all sorts of things as well, but it was very low grade.

BM: The difference was that it was a descriptive subject, not an analytical subject.

KN: That's right, it was descriptive. And, anyway, one did that just to, I started doing research and I went off to lectures occasionally in biology. I did statistics as well.

TT: And who did you do the research with and why?

KN: Wilkins. September 1948 I sat down to talk to Wilkins as I'm talking to you now, and he told me what we were going to do.

BM: So had you applied to do a PhD, Keith?

KN: No, no, no, I was only finishing off my approved education. So he told me what we were going to do. And that was it. So I started working in the lab and I was allotted a little bit of space in the corner of the laboratory, and told to get on with it.

BM: And what were you doing?

KN: In those days I was looking for immersion liquids for ultraviolet microscope objectives. So I was looking for liquids which had the same refractive index in the ultraviolet band as quartz or something like that. Even looking for liquids that transmitted ultraviolet. And, so, I must have measured the refractive indexes of function of wavelength for 20 or 30 liquids and found one or two that looked promising and then looked at the chemistry of them, and then looked at similar compounds. But that was to try and make a really good immersion lens with fused quartz that we were aiming for. We were going to add, Wilkins and Seeds had already made one simple reflecting microscope, and they had used this for studying mitosis in living cells and all sorts of things. And they'd started up a tissue culture collection, and they were looking at dividing tissue culture cells. This is where the DNA started. It was, the emphasis was always, with Wilkins, on DNA in cells. Their aim was to, they were looking at the way absorbing materials collected during mitosis and split and so on. But they'd done quite a bit of work, they wanted better reflecting microscopes and I was given the job of seeing how they could reduce the central obstruction because of the mirror, and we tried to increase the numerical aperture as far as we could go. But in the first year we had plans to put a special, we called an "Amici prism lens" in front of the reflector which would have increased the numerical aperture not by a factor of the refractive index of the lens (ca. 1.5), but by the square of the refractive index (i.e. 1.52 or 2.25).

TT: And this was going to be your PhD Thesis?

KN: It became it but it was, well in those days PhD-students were slave labour. You know that. And basically it was, 'This is what you're going to do.' And it was interesting and I got my teeth into it, but then of course very early on we needed to, we needed to build an interferometer to study the wave fronts coming out of these objectives, and so I got onto the development of ultraviolet reflectors more than anything else. And I was just left to get on with it. When I say left to get on with it, I suppose I spoke to Wilkins not every day, but nearly every day.

TT: And was Wilkins your main advisor or did you have anybody else advising you?

KN: Maurice [Wilkins] would sit in the chair and say, you know, 'How are you getting on?' and so on, and he'd say, 'Well, let's have a look at it. Yes, I think, what about this?' And he used to sit there, he used to sit there with a small piece of paper and a very fine pen and he'd scribble away and he was always picturing what you'd got to do next, or working things out in his mind and saying, 'Well…' When we were designing the interferometer, you know, 'Why don't you do this?' And I'd say, 'Okay.' And I'd go away and do it. But then of course we had to make things in the workshop. We had literally a workshop alongside the lab with milling machines, lathes, and welding and all that. You just had to use these things correctly and get on with it.

BM: Was Wilkins already regarded as very distinguished, Keith?

KN: Well, yes, he was Randall's deputy, but he wasn't a member of staff of the college, you see.

BM: But he was, for example, a Fellow of the Royal Society?

KN: He wasn't then.

BM: How old was he?

KN: Oh, that's a good question. I would have said thirty something.

BM: Quite young. A young man. Obviously very, very talented.

KN: Yes. Well, Randall had known him since about 1937, I suppose, so Randall had known him for quite a long time.

BM: Right. Presumably he'd done his undergraduate work with Randall?

KN: He'd done it with Oliphant I think.

TT: This was in Birmingham?

KN: This was in Birmingham, yes.

BM: But he'd worked on the Manhattan Project during the War, so he'd been abroad for a spell.

KN: He'd been abroad, he'd married and the marriage had broken up and he came back, I think that had hit him hard. So it was a new life for him.

BM: So is it fair to say that he was restarting his career as a biophysicist?

KN: Well, I think everyone was starting their life as a biophysicist, yeah. And he decided to go in for ultraviolet microscopy as a means of studying cells and nucleic acid, and that was the way he went. But from the word "go", from the first moment I sat down with him, I mean, he was interested in nucleic acid in cells and their role, and they'd already done, they'd got quite, some very good film of mitosis in cells and within the ultraviolet. But at that time Professor Robin Barer your great friend…

TT: And my Professor. He was my Professor of Anatomy.

KN: He was your Professor as well?

TT: Yes.

KN: Well, he had liaised with Professor Burch from Bristol, Professor C R Burch who was a brilliant physicist. I've got to be careful what I say. He was married to a child psychologist and I once spent a night in their house in Bristol and it was frightening. I mean their daughter, their daughter was showing me how she'd used a reflecting telescope to take pictures of the sun and oh dear, you know. I was woken at five o'clock in the morning with bang, bang, bang, bang, bang. And Birch was up in the loft, I don't know why, he had a workshop up in the loft and I don't know why he was there, but he was making a V groove in metal. And there he was banging away. He was brilliant but he made, his mirrors were about, his concave mirror was about that big.

TT: For the tape recorder can we just say six inches diameter?

KN: Well, a good six inches, nearly six inches diameter, and they were made from speculum so that he could use them as a mirror even as he was working them. And so basically he'd get an image, he'd get an image off them and then he'd try and correct the spherical aberration by grinding, hand grinding here and there. But as a result, what happened was, his surfaces finished up as a large number of facets, and they worked very nicely in the visible, but in the ultraviolet where the wavelength's halved - they just scattered light all over

the place. They acted more like diffraction gratings. And so, this was the battle we had with Barer, because Barer had acquired one of Burch's microscopes and he was using it in the ultraviolet of course and he was about the only person in the country who could do that at the time. And Wilkins and Seeds had come along with a cheap, shoddy little thing that got images in the ultraviolet, and so Barer had to have a go at them, didn't he? The problem with the spherical mirror, if you just put two spherical mirrors together, the problem with them was that the field of view was quite small. In the middle they were perfect, but the central obstruction caused the diffraction from a pin hole to throw quite a bit of light into the second rings. But by reducing the central obstruction you could reduce that amount and the field of view was restricted by the aberration coma to about 50 microns. Which was satisfactory for examining single cells of a specimen.

TT: So it didn't matter?

KN: It didn't matter. So Barer attacked us because these weren't microscope objectives.

TT: And when was this disagreement?

KN: It was in Cambridge in about 1940, no 1950, I think. We had a real ding dong. It's in the Faraday Society. I still got it. It makes fascinating reading 60 years after the event. But that was the first time I'd presented a paper to a distinguished society and to have somebody jump up in the middle of it and argue...

BM: He must have been fairly young himself then, because he had three degrees. Barer had a degree in physics, a degree in physiology and a degree in medicine. A very clever man.

KN: A very clever man.

BM: But he must have been a young man then, about 30, because when he was teaching you (meaning TT), that would have been 20 years later, 1970?

TT: Don't forget he'd been in the War. Perhaps early thirties, mid-thirties. He had been one of the first people to liberate Sachsenhausen.

KN: Well, anyway, it was the first time I'd given a paper at the Faraday Society, and somebody gets up and says, 'If there's somebody in the hall, if there's somebody here who knows more about this I'd like them to explain it to me.'

TT: That's a nightmare.

BM: Sounds like Physiological Society at its best [laughter].

KN: Slowly Maurice Wilkins got up and said, 'Professor Barer, I do.'

TT: Wonderful.

KN: And sat down [laughter].

BM: When you then, Keith, started your PhD, where did the money come from to do it? Did you have a grant?

KN: Well, my first year, my third year there, which was first year research, was covered by a county scholarship. As an undergraduate. Then I was awarded my degree and then it was that year, at the end of that year, by about the end of July I said to the Professor at tea time one day, 'I'm sorry, Prof, but my grant's up now, and I really, I can't continue this work. I'll have to go. I'll have to go and do my National Service now, you see.' So he said, 'Don't do anything rash, don't do anything rash.' So I went home to Yorkshire and about two weeks later I get a letter from him saying, 'I'm delighted to tell you that I've found some money for

you.' He twisted the Rockefeller Foundation's arm and the Rockefeller paid me. But when I came back I applied, I also applied for a job and got it as a, I then became an Assistant Lecturer. I'd done a year as a demonstrator for first MB students and I found that interesting because it wasn't practical work, it was tutorials more than anything else. People brought problems, you know. 'I don't understand this.' So it was tuition more than demonstrating that I found interesting. So the Professor then said to me, oh I think even before I'd been appointed, 'Well, I'd like you to give some lectures, you see.' So what does he do? Of course he allocates general honours physics, he'd allocated all his mates to the lectures that he wanted and then when he got down to the one that he couldn't fill, he said would I do it? So I got electricity and magnetism. I could have strangled him. I could have strangled him. One of my first students was A C Clark, the science fiction man. He'd been a corporal in the RAF doing radar and most of the people, most of these guys had come back from the armed services having done radar communications and here's me, a young sprog, doing magnetism and electricity. So it was a very hard year. It was a very hard year.

TT: Did you do that throughout your PhD or just one year?

KN: I think by then, do you know, I simply slipped into doing a PhD. I think about that time the Prof said, 'You know I think you ought to,' and I must have filled in some papers, but I've no recollection of life changing in any way at all. It just went on. So my research became much more, well of course, directed at the development of reflecting microscopes then. But there were all sorts of diversions, because there were no cover slips for ultraviolet light, nobody made them. We had two girls grinding them on a thick piece of glass from a ship's port hole onto which the quartz glass blanks were stuck. They measured the thickness with a micrometer until they were the correct thickness and then polished the surfaces. In those days a quartz cover slip was worth a fortune.

TT: I remember rewashing cover slips when I was a student.

KN: Well, rewashing them, these were guarded. I've got some somewhere, and they were like gold.

TT: And these girls were employed in your Department?

KN: Oh yes. Well, one corner of one laboratory was given up to this activity. I mean it had to be done. In the same way later on when Wilkins got into, when he got into X-ray work, one of the problems with the X-ray tubes in those days, well first of all the exposures were very long, and to get high intensity X-rays you had to have a very, very fine focus of electrons hitting the anode. And, of course, after a very short time it eroded it, burnt a little pit in the copper anode and the intensity of the X-rays went down very significantly. So one was forever polishing anodes of the damned X-ray tubes as well. And so, you know, I'd come in and find Maurice beavering away polishing one of them and I'd say, 'Come on, I'll do that.' That was it, that was the way things got done. And when Rosie came into the Department, well, all these problems had been sorted out.

BM: When were you first aware, Keith, of work on DNA?

KN: Day one, day one.

BM: They were talking about it from the very start?

KN: Yes. Day one, Wilkins said, 'The object of the exercise is the study DNA in living cells.'

BM: He used that abbreviation?

KN: No, nucleic acid. It was "nucleic acid", because there was a lot of, thymonucleic acid, the nomenclature associated with nucleic acid was very confused. And in fact when I got my microscope working and I worked with J Cheyen - who was one of the biologists in the group. He was a good microscopist, but he was doing histology and of course Feulgen was the standard one. But one of the problems was when you looked, we

treated cells, he stained cells and I looked at them in the ultraviolet and the trouble was that the method of fixing for Feulgen determined what you saw. And the other problem we had, of course, was that in the ultraviolet, you were seeing nucleic acid in the ribosomes as well. And so depending upon your fixation method, if you broke up a ribosome you got lots of, you got ultraviolet absorption in the cytoplasm. So comparing histological techniques with ultraviolet images of cells, which had undergone the same fixation and so on, it was very interesting indeed to compare them, because the acetic acid used in the Feulgen stain preparations really caused the nucleic acid to bunch up in the nucleus. Whereas if you, if you looked at that cell in the ultraviolet, it wasn't like that at all.

BM: So the work on DNA was central to the work that Wilkins was involved with. So can we ask, when did Rosalind Franklin appear in the Department?

KN: Well, she came from France where she'd been working, she'd come from France, I think the Professor recruited her because Wilkins and Gosling had got good pictures, they'd got very good pictures of nucleic acid only because Maurice had drawn them out into tiny fibres and checked under the microscope. And then he'd mounted them, I don't know whether you've seen pictures of his frame? I mean it was a paper clip to start with. He mounted, he mounted about 20 of these fibres; also it was crystalline DNA, he got good pictures from. He'd already done this, and he'd realised the importance of humidity in controlling that crystalline structure. And it was, that was with DNA that he'd been given by, oh, it will come to me. But what he was concerned to do was to show that that DNA that had been isolated from the cell was the same as the DNA that he could find in living specimens. He'd lined up sperm of various fishes in the same way and got identical X-ray pictures. So he was convinced that what had been done in isolating the DNA hadn't destroyed it too much. And in fact, Maurice was always going back, always going back to nature as it were, rather than the test tube. Now Rosalind came in with the specimens that Maurice had been working with, and also some other DNA that had been isolated and prepared by somebody else, and in the Department there was Bruce Frazer and his wife who was a biochemist, who was also, they also isolated DNA from fish roe. They used to go down Billingsgate fish market and come back reeking to high heaven, you know, and isolate DNA from this stuff, and they were working with some quite highly purified DNA. This was going on before Rosalind came.

BM: What did Rosalind come to do? X-ray crystallography was her subject.

KN: My memory tells me that she was initially employed to work on the Professor's project, the initial contact came through Coulson, I think. But when she came I think Randall wanted her to work on collagen. Maurice was doing X-ray on nucleic acid, she was going to do X-ray on collagen. But Maurice had made so much progress with DNA that he said to the Professor, 'Wouldn't it be better to concentrate and do this?' So somewhere in the process when she came she was going to work on purified specimens of DNA which Maurice had been working on. She was going to do that.

TT: And did she think she was going to be in charge of the nucleic acid work?

KN: I don't know. I don't know how she could have been given that impression because Maurice was; she was employed by the MRC, she wasn't employed by the College, it was an MRC employment. Randall was the Director and Maurice was the Deputy-Director, so I don't see how she could think that she was going to take over the whole work of DNA at all.

BM: No, no, well that's clear. She was brought in as a specialist in a technique.

KN: Yes. Well, Gosling had been brought in as a post-graduate student to work with Maurice on X-ray, and it was realised that they wanted somebody to work on X-ray crystallography. That was it, and Franklin was as part of the team.

BM: And she was an expert X-ray crystallographer.

KN: But she wasn't a team worker at all. She was a person who just worked on her own, and that was it.

TT: Can I just ask you about her name. When you first mentioned her you called "Rosie", and it has always been disputed. Jim Watson called her that in his book, and there was a big objection.

KN: Well, she was always known as Rosie in the Department, but she didn't like it. 'You must call me Rosalind,' or it was 'Oh call me Dr Franklin.' She didn't have time for undergraduates, or for post-graduates at all, but she did for Raymond. Raymond was a post-graduate, but he was her tame post-graduate. But of course he was only continuing working for Maurice.

BM: There's such an interest of course in Rosalind Franklin that to hear anybody who actually knew Rosalind Franklin first hand nowadays is extremely rare. There can be comparatively few people alive who actually knew her well, and of those the number that actually worked in the same laboratory as she did or the same Department as she did, that must be vanishingly small. It's only people from King's and possibly people from Bernal's Department, where she ended her career. So there can't be anyone else, I guess, now. The Paris people will have gone, they were older again.

TT: And her sister of course.

KN: I can remember when I first met her. I had been given the job to make this X-ray camera for collagen work, which was a highly mechanical gadget. Well, basically, to line up three slits which are only a few microns apart. First of all I'd got to grind them, they were one centimetre slits, and I'd got to grind them and mount them and get them the right widths under the microscope and then align and orientate them and so it demanded a fair amount of machinery to do it, to get pictures out of it. That's as far as I got. I got pictures out of it with collagen. And then it was handed over to A C T North, who subsequently became Professor North, and he was at the Royal Institution for some time.

TT: So this was never used for the nucleic acid work?

KN: Well, that, I got it to the point of working, but it was never used in DNA because we weren't interested in those sorts of spacings, we knew those spacings. Basically, when Rosalind came to King's the unit cell of the structure was known. The number of bases in a complete turn the slope of the helix and the dimensions of the helix were known. It was given to her on a plate. She came in March 1951 and left in March 1953, so she was at King's only two years.

BM: But when you met her what was your first impression of her?

KN: A very attractive looking lady, very aloof. Very dark hair, dark eyes. Jewish. She stared at you and it was almost like being interviewed by the head mistress.

BM: Shy? Would you have said she was a shy person?

KN: I think she was basically shy. I say, I thought when she came to King's, she had no time for anything that went on at King's, so I don't know why she came. Well, first of all I suspect, I suspect the atmosphere in the place, it had been bombed, they were building the new laboratory in the quadrangle. The common room was for the senior staff who were very structured on male lines. Although, having said that, there were more women working in the Biophysics Department than men.

BM: But I think there was no common room at that time for ladies, was there?

KN: Oh no, no. As I mentioned, in the new laboratories there weren't even any lavatories for women. But that was part of the time, wasn't it?

BM: And was she returning from Paris where she had been successful? Was she returning to an environment that she wasn't familiar with? Was that part of the difficulty?

KN: Well I don't know. She got on very well with, the strange thing is she got on very well with several members of the staff like the photographer. She got on very well with Sylvia Jackson who eventually went on and got her degree. I think she didn't get on with Maria Friedlander, who was our electron microscopist; she didn't get on very well with Angela Martin, who was a very extrovert biologist who was very good. I don't know if she didn't seem to think anything good came out of King's and the fact that Stokes - who was a theoretical physicist - had the audacity to suggest what the DNA structure was, was absolutely outrageous.

I met her in a corner of her laboratory. I went to see her to seek advice on the camera I was working on and what I should do, what would she do if she was designing a camera for that sort of specification. And I didn't get very far because I think she was the sort of person who took a piece of equipment that was made and used it. I don't think she made any contribution to changing the design to get a better picture. She just took what was available, what was there.

BM: So where was her skill, Keith? Because everybody said she took superlative X-ray, crystallography photographs, that they were very high quality, very high standard. Everybody says that.

KN: I think it's attention to minute detail. Yes, I think it was that more than anything. No, it was getting, it really was improving the techniques that Maurice had developed for getting new material, which was different. There was a bit of a battle, well I say "battle", I mean she originally told Maurice to go back to his microscopes thinking that she was going to do everything connected with X-rays. But Maurice had already ordered the high focus X-ray machine that she, I mean she effectively took it on, the bits and pieces that he'd ordered. So she was presented with everything.

TT: And what about Raymond Gosling? To go back to Bob's point about the skills.

KN: Well, Raymond came in as, he'd been doing X-ray, the odd hospital X-ray work - he was doing radiography - and he wanted to do a higher degree and Randall took him on as one of the MRC's students, and originally he spent a good year and a bit working with Maurice on X-rays. Well, Maurice was working on X-rays, they were working together for a good year or 18 months before Rosalind came.

BM: Rosalind liked Gosling and Gosling got on well with her?

KN: Yes. So that was good you see, but Gosling was also communing with Maurice, and so the problem was being discussed around and about.

BM: So when did you meet Watson and Crick?

KN: Crick was a friend of Maurice's and they came into the Department. Crick came quite a few times on his own, but then he started coming with Watson, and Watson was, at one time, sufficiently often in the lab that he met Edna several times, and she only came in occasionally.

BM: This is your wife?

KN: Yes, my wife, and she formed a very poor opinion of Watson at that time. He was a very strange, brash man, it was just his general attitude, almost flippant. He was a strange mix. He was an uncouth, untidy.

BM: Unimpressed by what other people were doing?

KN: Oh, yes, he could, he thought he could see right through what everybody else was doing. Yes, a very strange man, very strange.

BM: And Crick? Very tall.

KN: Very tall and bright. Well, he worked during the War, he'd worked on codes, breaking codes, so he was the brains there, there's no doubt about it. He was the brains, what I met of him. But Maurice was the person they came to talk to; they were quite regular visitors to the laboratory. I'd knock on the door and walk into Maurice's lab and find the pair of them sitting there talking, just like people do, and then Maurice would take them off for lunch and they would disappear.

TT: So you couldn't say that they were competitors at that stage? I mean they were just scientific colleagues and friends?

KN: No, I don't think they were competitors, they were, it was, it was three or three different minds looking at a common problem.

BM: But Franklin was not much involved with those three?

KN: I don't think so, well she wasn't involved, no I don't think so. She kept herself very much to herself. I can see from reading her biography that she was a loner. She seemed to like loneliness. That was her. But she did play hockey at school, and subsequently, and I snuck off most Wednesday afternoons to play hockey when I could, and so we did have that in common. But when I mentioned it she didn't want to know. I couldn't connect at all. I found it very difficult. It was like being interviewed, you know - it was like being interviewed for a job almost. Sitting there, and I was trying to get information out of her on I said, 'Well, with X-rays generated from a copper anode, and you're talking about collagen, you know, massive spacings. What sort of resolution should I be looking for?' You know, 'How narrow should the beam be?' And I didn't get very far at all. I didn't get very far at all with her. I didn't find her very helpful. Edna met her quite a few times and usually in the ladies loo in the College and well, there was some conversation but not a lot. She was a loner and she was a thinker, a thinker.

TT: So we're talking about the early 1950s.

KN: Well, she didn't come to King's until 1951 and she left in 1953. So I left in 1952, so basically, I had about a year.

TT: That's what I just wanted to clarify.

KN: The number of times I could say I was in the same room with her and talking to her would be counted on the fingers of one hand just about.

BM: It's very difficult to form an impression about how clever people are. I find it difficult. I've known people who are obviously clever, and I've known people that I've only discovered were clever after I'd known them for a long time, that at first glance didn't appear to be particularly unusual at all, but as the years have gone by you realise that actually they're very talented. Would you have said she was very clever?

KN: Well, at that stage I hadn't read any of her papers. She just struck me as being aloof. And arrogant. Aloof and arrogant. I got no opportunity to find out how clever she was or not.

BM: Well, you knew Crick was clever.

KN: We used to have our colloquia in the small lecture theatre, with a blackboard. When she was present, I'm struggling to think of any contribution she made, if you know what I mean?

BM: But you had no difficulty in recognising that Crick was clever, and you didn't meet him very often?

KN: I didn't meet him very often. But, yes, he was very clever; well, first of all he took an interest in what I was doing, you know.

BM: That would help.

KN: Well, it helps because then you can communicate. But Rosalind never took any interest in what I was doing. John Coupland was building his million Volt electron microscope in the corner, and she never took any interest in him.

BM: She seems to have been an introverted person.

KN: She had a few friends, she had a very small circle of people who she communicated with. Outside that I know she used to come into the common room for tea, but not very often. And if she did she would pick up a journal and get her head into it.

BM: Yes, it's interesting because when I gave you Brenda Maddox's book on Rosalind Franklin, which you've now read, the account of her in Paris paints a much happier picture.

KN: Oh, going out to dinners, enjoying. Outgoing, outgoing. But Maddox doesn't throw up anything that might have changed her between Paris and King's. But she came to King's, to say she was surly would be wrong, but I got that impression. She was gay and happy in Paris, comes to King's, something had happened, I don't know what.

BM: Maybe she'd been unhappy, maybe she'd had a love affair or whatever.

KN: But she was a long time getting going at King's, because she spent the first, I think, six months finishing her work in Paris, writing up her work in Paris.

BM: It does appear that she was happier afterwards when she was with Bernal, after she left King's and she went to Birkbeck. She seems to be happier there. Where were you, Keith, when the DNA story broke?

TT: It's the 1953 *Nature* paper.

KN: I was at Porton. I joined Porton in November 1952.

BM: And were you following the story from King's?

KN: I was following the story. In fact I had been back to King's several times, I brought my ultraviolet microscope to Porton because nobody else was using it. I brought it to Porton in the hope that I would be able to do some work with it. In fact I'd used it at King's for work for Porton before I joined, before I actually went there. When I went to Porton I was working with a chap by the name of Dr Powell, and his wife, Joan Powell, was a very good biochemist who was working with the spores of anthrax and the spores of *Bacillus megaterium* and things like that. She wanted me to take some ultraviolet pictures of sporulation, which I did for her at King's. So when I went to Porton, I took my equipment, but I never got it working at Porton, it was too difficult. I'd built the thing, but you turn a page, don't you, and you've got to put it behind it and move on. At one time I went back to King's - it was probably when I went to collect the microscope, I was talking to Maurice and it was then that I got the impression that in fact it was the Editor of *Nature* that had put him up to the fact that Crick and Watson had submitted their article, and that he was busy trying to get his paper ready because Rosalind and Gosling had already got their paper ready. And that was how they came to be published together. But that's not the story as told by Maddox at all.

BM: No, because she implies it's Randall.

KN: Well, Randall might at the very end, when Wilkins and Stokes were having, not difficulty, but it was taking time for them to get their bits and pieces together. But for some reason Rosie never, I don't think, I'd be surprised if she ever talked to Stokes. She had no time for modelling. Well, I'm not going to say simple things, but basically things that could be resolved uniquely by using X-rays. And really were only an extension of what Bragg had done years before. True crystallography, where things could be worked out. Well of course the structure of penicillin had been worked out before then, and the structure of haemoglobin was nearly worked out. But Rosalind was a dyed in the wool X-ray crystallographer, and anybody trespassing on her ground, was living dangerously. That's basically how I saw it. But nucleic acid was something different. It was much more complex and you've got to bear in mind that when that paper was put forward, it was a hypothesis. Took about another 10 years or more to prove it.

BM: And that at least is what Rosalind said. She thought the model was, you know, interesting, obviously she thought it was clever but she wasn't very generous about when she spoke about it, but then probably she wasn't very generous about anybody's model.

KN: There's a paper by Hamilton written 10 years after the event now. I don't know whether you've read it, but it's a very good article.

TT: Bill Hamilton wrote extremely well, didn't he?

KN: He did. It's extremely well written and reading between the lines, admittedly he was a King's man so you might say he's biased. I think he tells the real story, and I think Watson did when he said that Rosie never talked to Wilkins and never talked to Stokes about DNA. All she wanted was a crystal, get away, leave me to it now, I'll sort it out, that sort of attitude. And yes, she could from the crystal, but the crystal wasn't the be all and end all of it all. And that was the big problem. I think Maurice, this is where Maurice, and to a certain extent Crick, come in, because, I think, they could see a much bigger picture. They could see a much bigger picture. And it was this; it was, Maurice was not only a brilliant experimentalist, but he was a thinker as well. He always had some tobacco plants growing on the window sill, and one afternoon when I was in his laboratory I sat down with him at his microscope as he was examining a crystal of Tobacco Mosaic Virus. He asked me if he could use my monochromator for an experiment. He then proceeded to illuminate the crystals with the yellow sodium light emitted by a mercury arc, and he used the crystal as a diffraction grating, and within minutes he was able to calculate the length of the virus rods in the crystal. An experiment he was to repeat under more controlled conditions later for a scientific paper.

BM: That's the real thing, isn't it?

KN: It was his ability, it was ability to handle things, do a simple - well, not simple - experimentation, but he was very good at experimenting. Plasticine was never far away, for example. A bit of plasticine for holding things.

I can remember Maurice on one occasion, oh yes, on one occasion he had some squid sperm which he stroked onto a microscope slide and then let it dry and then he used a piece of polaroid on it, and this squid had made a perfect polariser. He could see things that other people couldn't see, and he could do simple experiments, and I just found him incredible that he was able to take a problem, throw all the detail away and expose the problem. Get rid of all the detail, forget that, and get to the heart of the problem.

TT: Can we just get you to Porton Down?

KN: I went to Porton Down in 1952, November, and…

TT: And this is because of your National Service?

KN: Yes, basically. I'd been interviewed by the man himself, the Civil Service Commissioner, C P Snow. He'd just come back from the United States - they'd just done a trawl through the United States - and I was summoned to Shell Mex House for interview, so I trotted along the Strand to Shell Mex House and after

the interview and he said, 'Right, go and see David Henderson at Porton.' And that was it. And that's how it happened.

BM: You told me once you were going to be a Bevan boy?

KN: Oh yes. My national service number ended in nine, and so I had to do national service before I was 26. So when I was 24 I volunteered for the air force, they said no; volunteered for the navy, they said no.

BM: The Bevan boys worked in collieries.

KN: Collieries. Way down the pits. So I did go to Wakefield to a pit head where I was instructed that I would, that that was my final destination and I started, I was there one week in the initial training when I got a telegram saying, 'Report to Porton.' And that was it. I reported, I went to the manager, pit manager, and said, 'I want a rail warrant.' 'What's a rail warrant?' he said. I said, 'Well, I can't afford, I have no money. I can't afford the train fare to Porton. Where's Porton?' So I said, so in the end he produced a railway warrant from Wakefield to Porton, and I went and saw David Henderson.

TT: We've just got you to Porton Down and you're still doing your national service. So what are you doing for your national service as? You're not in the armed forces, you're working for the Ministry of Defence?

KN: Instead of doing two years down the pits, I did three years at Porton. That was the deal. And basically we'd had a grotty flat, we tried to get a flat in London in the year of the Festival of Britain and accommodation was really difficult. We got a flat in Streatham, which wasn't bad because it was on one of the main routes into London. I could get into King's quite easily. And then Edna was teaching in Stockwell, near, so it was a good route in for both of us. She was teaching "domestic paralysis" as I called it. But as she said, she was only qualified to teach it, not to practice it [laughter]. But having said that, entertaining colleagues and American visitors was a full-time job really.

I came to Porton as a young lad and the lowest scientific grade for a scientific officer, but I was put in a nice laboratory, I was given a technician, a senior technician, a very good laboratory technician, medical laboratory technician, and a young assistant. And our aim was to find rapid methods, physical methods of identifying bacteria, basically. And they'd already tried some work in CDE, and it was suggested that infrared would differentiate between bacteria, quite rightly. Because of the state of finance of the country I couldn't buy an American Perkin Elmer infrared spectrometer, I had to buy an English one. And so my first job was to sit down and select which infrared spectrometer I wanted. I didn't have much choice but in the end I sat down and I just took a piece of paper and wrote on it: 'To the Director, at long last I've made my decision, this is what we need,' and I sent it off.

And I think the cost in those days was £4,000 which was eight times my annual salary. It was a lot of money. And we'd also got to build a stable platform, mess the laboratory up and do all sorts. Anyway, I wrote this down on a small piece of paper and sent it off. About a day later I get telephone call from the Director's secretary. 'Dr Henderson wants to see you in his office.' So I said, 'Oh dear, what's going on now?' So I went all the way upstairs to the office and he picks up this piece of paper and says, 'What's this?' I said, 'It's a note to say…' 'Don't you ever do that to me again.' He tore it up. 'Now tell me, tell me what you want and why you want it.' So I did. 'Right, go and tell the Administrative Officer to order it.' That was the way decisions were made. There wasn't any paper, no paper.

BM: What a good way to run a Department.

KN: That's the way it was run. It was his Department and it was going to be run the way he wanted it. He was a vet, he was a scientist. He'd worked with Sir Paul Fildes during the War. A small group had been set up in the CDE, it was known as the "Black Huts". I don't know whether you remember them?

BM: I do indeed. They were on the end of the Pathology Department?

KN: Yes, that's right. They were given the task of deciding whether biological warfare was possible and in the space of three years they wrote about 50 reports, most of them handwritten by Fildes himself. They had developed a means of blowing up an anthrax bomb, which was effective and tested in an island off the coast of Scotland. And they had also developed a method of spreading infected cattle cake, and they made that up at the black huts. They actually grew the anthrax and made the biscuits up there during the war. Now that they did starting from scratch in three years. The trouble is that they designed a weapon that worked, it was dropped from an aircraft once and it worked. And when the Americans came into the War all this information was given to them; but, whereas we had grown anthrax, the spores of anthrax, they'd been grown on trays, agar plain trays, about 18 inches long. And then harvested with a mini vacuum cleaner to get large quantities - this way you got spores. But of course when the Americans took it over they only ever used corn steep liquor - or something like this - for growing bacteria, so they chose to grow anthrax that way. Well getting it to produce spores was extremely difficult.

During the war, Fildes and colleagues decided that they couldn't have a bomb made to their own specification, and so they got to take an existing one. They took an incendiary bomb, a four pound bomb, and converted that into a biological bomb that worked. And they gave it to the Americans. But at the end of the War, the Americans still hadn't made enough anthrax, hadn't got around to making any because they tried the wrong methods of doing everything. The Canadians were also involved in this. Tizard, when he went to the United States, taking radar, the biological bomb, he took all those, jet engine, all went with Tizard to the United States, and of course the Americans were able to develop them. The reason I think we went the biological way was, the anthrax was grown on a casein-based medium. There wasn't enough casein in this country during the War to do it. It was as simple as that. So the Americans tried to get round it by using different methods of growing cultures, but of course growing large quantities of spores of things like anthrax in a liquid medium, then concentrating it down, is very difficult, very difficult.

BM: But the chemical establishment got out of biological work at that time, didn't it? It was handed over to MRE, I suppose? When did MRE start?

KN: The Microbiological Research Establishment. Well, it was decided in 1946 to build a new establishment at Porton, but separate the biological from chemical. And so it was started in 1948. The new building was built and it was the largest brick built building in the country at the time, largely because there was a shortage of steel. It was originally designed for a steel structure, but because it was brick, with brick then the walls had got to be very substantial and so they decided to, all the walls inside, inside walls were very thick. And all the services came down from the services floor into the laboratories. So if you wanted anything changing it was all up in the loft. You didn't have to break into the laboratories. It was a very good design, it was well thought out. And in fact the laboratories were all identical and they built a block, they just built a block, a section through the building, testing everything, all the plumbing; so once they got a design they could just mass produce it to produce it all the way through the building. That building is now said to be not fit for purpose.

TT: And when was it built?

KN: It was started in 1948 and they moved in in 1951, in June 1951, I think, they moved in, and I joined in November 1952; so it was a brand new building basically. And they were recruiting madly. The Korean war was going and the Ministry of Supply at the end of the War had got rid of all their scientists, they had all gone back to university. Then when they wanted to expand again they had to go out recruiting, and that's how I was scooped up, because I think at the end of the War the Ministry formed a scientific register. So anybody with scientific qualifications was on this register, and they just trawled through it and said, 'Right, you're going to Porton.' And of course I finished up you know, I finished up a round peg in a round hole, basically. So I was able to start work the day I joined just about. And it was, I think I described King's when I went, the Department was electric because it was new, and they were looking forward to the new laboratory. The MRC Unit was new, everything was. And there was a real purpose behind it. In the same

way, Henderson who created MRE, he was very crafty. He said, 'It's going to take 15 years basic research before we can think of any application, you see.' Well, anybody with a wit would have said, '15 onto his age takes him just to retirement age,' if you see what I mean. He was no fool. But in the event, after seven years he took a sabbatical and instead of going somewhere else, he took a sabbatical to work in his own laboratory. And he adopted a white coat and went to work in his own laboratory for a year. He was quite a remarkable man. He had the first, what I call "digital brain". He would sit and question you and break everything down until he got a 'Yes' or a 'No' answer, and he would remember that. And if you ever changed your mind and he found you out, and he came back to you, or you happened to say 'No' one time and 'Yes' the next time, he remembered and you could pack your bags and go.

BM: So was that a rather rigid mind then, Keith?

KN: Oh indeed. All he did was question you into a corner and say 'Yes' or 'No'. If you changed your mind subsequently and you went and sort of said, 'Things have changed,' well that was fine. But once again he'd sit and question you. Very analytical, but very practical. I can remember one day, oh in those days because you couldn't take your notebook out of the laboratory, your notebook stayed in the laboratory, and so on. I got in at nine o'clock one morning and he's sitting at my desk reading my notebook. He was reading what had been done the day before, and he was capable of doing so. And he sat there, my microscope was beside him, and he kept doing this, then he got a lens out, got a lens out of his pocket and started studying his finger, you see. Didn't say anything. Didn't say a word. But he was there about half an hour, and he did this once or twice, got this lens out, didn't say anything. But basically he was saying, 'Your microscope is dusty and it should have been washed down every day at the end of the day with a chloros wipe.' But he didn't say anything. He was like that.

BM: Where did he come from, Keith?

KN: He was a vet, I don't know what he'd done before. He was recruited by Fildes during the War; he was Fildes' right hand man during the War. He was just capable of getting things done and he got this laboratory built, he based it on the MRC Unit in London.

BM: Mill Hill?

KN: He based it on that, but they couldn't reproduce it on this patch.

TT: It couldn't be at Mill Hill at that period, that would be Hampstead.

BM: I don't know. When Medawar went there as Director it was Mill Hill, wasn't it?

TT: That was 1964.

BM: It might have been the Hampstead one in that case. MRE was a big schloss-like building rather like the Mill Hill Institute.

KN: Major workers had a laboratory, a hot room, and a cold room. Just like that. There was a central supply for all materials - you didn't do any washing up, it all just went and that was it. So there was nothing to prevent you working, it was an ideal environment. And because of our isolation, he encouraged speakers to come from outside. He took advantage of things like the Ministry of Supply which had a Biological Research Advisory Board and at one time the President at the Royal Society, Sir Charles Dodds, was the Chairman of this committee. They were nearly all FRSs. And each one of the members took an interest in one of the workers in the establishment. I think I'd been there about six months, we'd just got the spectrophotometer going for a short time, and Henderson came back from a meeting in London and the following day calls me up and says, 'I was talking to Sir Charles. He's very interested in what you're doing. Next time you're in London go and see him.' Now, this is to a young scientist. Go and meet the President of the Royal Society and tell him what you're doing. Well, it was new to me, I thought that's how young scientists got on, but

that's not true. That's not true at all. It was just my luck to have Henderson as a Director and people like Pat Lawther for example. Pat Lawther was the Chairman of one of the committees. I got on very well with him, because he had a son who had a kidney problem, and I had a son with kidney problems.

BM: I think you and I had an advantage. And that was that when we were young it was recognised that we were doing work of national importance, and that it was in the interest of the defence of the realm. We weren't doing science for our own personal research interests, we were doing work of national importance. And because of that great members of our profession - members of the Royal Society, the most distinguished scientists - showed a kindly interest in what we were actually doing. Professor R H S Thompson was the adviser to my group.

KN: Ah, Robert Thompson was marvellous.

BM: Well, he was allocated to me. And he looked after me, in the sense that he came to see what we were doing. He used to like to talk about the experiments and then when the committee met he would give a short report. And we always went down to the pub at lunchtime, to The Pheasant. With the best will in the world, if I had stayed in Cardiff as a Lecturer in Physiology, I would not have been meeting Professor R H S Thompson once every six months. I would never have met him in my life.

KN: I've got a very soft spot for him because later in my career, when I moved over to CDE in 1970, I became Secretary of the Chemical Defence Advisory Board as well and Robert Thompson was the Chairman. So I had, before a meeting I used to go and meet Robert and brief him on the subject and get the minutes approved by him, and this sort of thing. So I was very fortunate.

BM: Would you agree, it's because the work was seen as important? And so there was no shortage of first class advice?

KN: Oh yes. Well, when I went to CDE I had trouble with people like Rex Watson. He used to face a portrait of the First Commandant of Porton, who was a full Colonel, Colonel Crossley, who was a distinguished chemist and a Fellow of the Royal Society. And so when Watson was getting difficult, I used to glance up at this portrait of Crossley and the message got through quite quickly.

TT: At what stage did you decide to stay at Porton Down?

KN: Well, I started there as a young Scientific Officer and I had to do three years. I had to do three years to satisfy my National Service. It was agreed when I left King's I would return for some reason or other. Both Randall and Wilkins made it clear that I should come back. And I kept that option open really for some time, but first of all I moved into a brand new four bedroom house at Porton and I got a challenging job, which was going well. Edna got buried in teaching here, we started a family and the thought of going back to London looking for accommodation after living in Salisbury, frankly it didn't appeal, even at the salary I was on. The other thing about London and teaching in those days was that as an Assistant Lecturer I was paid three times a year at the end of term. Well, trying to run a house on being paid three times a year, it's grim. Is it still like that?

So at the end of three years, first of all I got promoted, which increased my salary by about 50%, which was quite a good promotion. We had Helen [KN's daughter], and life in Salisbury was good, the job was good, but by then, I came in 1952, so that's 1955, yeah, I continued doing a lot of microscopy. The thought was that I started air sampling and looking at the large amount of proteinaceous material in the air and said, 'Well, if bacteria are released in it, how many are there going to be?' And the idea is 'not a lot.' I mean with things like tularaemia, well I think the lethal dose is about 20 organisms inhaled in the right particle size. So when you're talking about looking for concentrations of microorganisms at that level in among what's already there, it's extraordinarily difficult. Anyway, I spent a lot of time air sampling and looking, using cascade impactor samples to examine things. The Americans actually did some trials in San Francisco where

they released *Bacillus globigii* (a sporulating bacterium also known, then, as *Bacillus subtilis*) spores, and sampled it downwind. And I worked out the concentrations. I analysed the data and it became clear to me that the concentrations we were looking for were about a hundredth of what the Americans thought they could detect. Well, having a machine that misses isn't much good, and so we started looking at different methods. First of all we went to fluorescent antibodies. Well, in those days generating antibodies was a very primitive business, labelling them with fluorescein worked quite well. And then we moved over to, well I suggested we move to radioactive iodine, and that worked quite well. So we spent a lot of time improving air sampling methods.

And in fact we developed cyclone air samplers. Ken May, working at MRE, developed the three stage sampler. You [BM] probably know that.

BM: I know that.

KN: In the process, well first of all, when we started spraying organisms a lot of work was done because theoretically you couldn't work in the open air, people constructed rotating drums. And you'd spray an aerosol into there, and the drum rotated, and you could keep an aerosol in there for 24 hours and sample it. And if you measured the viability of the organisms as a function of time they'd survive quite well. They'd survive for 24 hours, no problem at all. And people developed bigger facilities, where at Porton we had a large facility, massive spheres that could be used - CDE had them.

BM: Barratt's balls.

KN: Barratt's balls, named after the Canadian superintendent of chemical warfare in Canada who came over here for a year to work, and he designed them, and they were always known as "Barratt's balls". They've been demolished.

BM: There were right next to the old ballistics building where I worked.

KN: Yes, I know. They were a remarkable facility. But in those days people thought that they represented what would happen with a realistic attack with bacteria. The bomb bursting at Gruinard Island had shown that you could explode them, you could get a fine cloud released from a bomb.

TT: When was that?

KN: During the War, during the War. And then the meteorologists got very interested in using zinc cadmium sulphide particles for plotting where air movements go. And CDE carried out a lot of experiments, in one they released particles along a 100 km track over the North Sea. And this cloud could be collected all over England. It drifted on the wind. Under ideal conditions they were travelling 200 miles. The Americans conducted experiments where they had clouds travelling thousands of miles. And everybody became very concerned that if in fact with bacteria, where you can produce in very vast quantities, if you release the aerosol at, say, 1,000 feet, under a temperature inversion which limits upwards dispersion of the aerosol, you could fill the whole of that space, it would travel long distances and it would be a very effective weapon. Well I started spraying bugs outside simply; we tried to find them in among what's there, and I stained some with methylene blue. Now I had a laboratory in MRE where I could spray them into a lab, seal it up and just use the lab as a chamber and sample that for 24 hours without any problems. I tried that outside, after a hundred yards travel outside I couldn't find them. No methylene blue at all.

Couldn't understand it. Couldn't understand it. Then I went to one of my technicians, who we'd trained, and I said, 'Sit down, Ken, and we'll do this using a phase contrast microscope and look for them (unstained organisms can be seen using phase contrast). They're there.' And the bacteria were there, but the stain had disappeared in the best part of 100 yards travelling in a bit of daylight. So things outside were very different from what they were inside. So while this was going on, Henderson said, 'Well while you're doing that, you might as well do some trials with some live bugs. Look at their viability.' In the meantime at MRE, in the

1970s, they carried out trials off Scotland and in the Bahamas where they had released clouds from bombs over the sea and sampled them downwind, and they'd carried out the trials which showed that organisms did travel, but their survival was very dependent on particle size. If the bugs travelled in large particles it appeared that they would survive quite well. The bugs travelling in small particles - which were potentially the most effective from an inhalation exposure point of view - were dying off very rapidly.

BM: Was that ever explained?

KN: Viability was very particle size-dependent. Not really explained, except that all the bugs, they weren't highly purified bugs, they were surrounded with the media. Basically, what happens is if you produce a droplet at 10 microns from your spray, the water on it evaporates quite rapidly, it comes down to a dry particle about three microns, say. But the viability was so dependent upon particle size, it concerned us. And in fact it was then discovered that instead of doing actual experiments in the open air like that, it was much easier to expose them on microthreads, spiders' webs. And we used this technique to study them where you could spray on any particle size you wanted and hold them on the spiders' web, and expose them outside and measure the viability. I left MRE in about 1970; we were still struggling with what was known as the 'open air factor'. When we exposed organisms upwind of Southampton and downwind of Southampton, we got very different results. Something was being generated in the city that caused the organisms to die off quite rapidly.

And so the problem became, to carry out an assessment of a microorganism sprayed in a certain way, would it survive in downwind travel. It was extraordinarily difficult to assess how to improve viability. But then one discovered that one could add some long chain alcohols, carbon 20 alcohols, which surrounded the bugs and protected them against these open air factors. So to make an assessment of whether bugs would survive in downward travel in England, was very difficult. But that's where we got to and then of course the Biological Weapons Convention (1972), proposed by the UK, arrived and put an end to it all. Well, I did some large scale trials spraying bugs off Portland harbour. We used a ship to spray, well first of all we had to generate large quantities. We had a pilot plant at MRE which was capable of growing large quantities.

TT: And when you say large quantities, of what?

KN: Oh, of *E. coli* which is a small bacterium which was readily recognised, because it produced big colonies, you could count them on a plate very easily. So we did trials to see whether clouds would travel. We collected from a ship spraying it off the Dorset coast; we were able to collect samples 40 miles downwind that were viable. And even the small particles could be made viable. But the trouble with that was that when we started growing large quantities they were grown in large batches, controlled, and everything depended upon the conditions of growth. This organism that we used was *E. coli* that was perfectly safe and the pilot plant could grow it so that it was almost like a coccus, round and tiny, or they could grow it so that it was quite large, depending on whether in the growth conditions carbon was limited or nitrogen was limited. Depending upon which you wanted you could grow them into wizened old men like me, or you could grow them into great big hulking cells. Now the little wizened things survived. The big ones that had better bulk were still metabolising presumably, they died quite rapidly. But towards the end, the pilot plant could grow *E. coli* to any specification I wanted.

So to talk about an organism surviving airborne travel, well, what organism, how grown *etc. etc.* There are many questions you've got to ask. The trouble is that the Americans had even exposed volunteers to aerosols of *tularensis* [*Francisella tularensis*]. The trouble with microorganisms is that instead of having a probit curve with a high gradient they have one with only a low gradient, which makes it very difficult to define the risk from an airborne infection even if the dose received is known accurately.

And people who stand up and say, 'Oh dear, this is serious, you're at serious risk,' I shake my head and say, 'No, you've got to look at all the factors that are involved.' It's very difficult once you spray, it's alright doing it in the laboratory in clean air, but doing it in the open air, you don't know what's in that open air.

BM: Microbiological particles are an order of magnitude more complex to work with than chemical particles.

KN: In those days we had one chap, I can't even think of his name now. Oh, he used to live with…; oh, the scientist who talks about the earth being an organism.

TT: Jim Lovelock.

KN: Well, he came to Porton and he worked on sulphur dioxide first. Tom Nash, that was his name. We could find no correlation between sulphur dioxide content in the air and survival. Then we went for ozone and we looked at ozone, but we couldn't find a real correlation between ozone, not under the sort of levels that existed in the open air that we were measuring. But then there wasn't much in most of the industrial complexes that we looked at. So we didn't get any correlation.

BM: Tom Nash, was the man.

KN: Tom Nash. Oh well done, you. Tom Nash. A lovely man but so strange and quiet. He was, you know, kept himself to himself. Very quiet, but very bright.

BM: Oh yes, oh yes. One of Pat Lawther's original recruits, I think, to the Air Pollution Research Unit. Waller, Commins, Nash, Lawther. That was the team I think.

KN: Yes, well Pat exported Nash. Well, in those days, why didn't I leave Porton? Well, I became a PSO [Principal Scientific Officer] quite quickly, which was quite good. I was quite young when I became a PSO. And then I became head of aerobiology which was at SPSO [Senior PSO] grade, so there was no way I was going to leave. Then I moved to CDE as a Deputy Chief Scientific Officer [DCSO]. I was Deputy Director there in 1970. It was very interesting. One of the things we did was we did quite a bit of the survival of microorganisms in Her Majesty's ships. The concept was then that you had a convoy travelling across the ocean and that's an ideal target for biological warfare. You could spray bugs 40 miles in and let them steam through it. They put the aerosol all around the ships and everybody gets their dose and about the time they come to dock they're all going down with something or other. In fact in 1958 I did some trials with the Americans where they were doing that in the Pacific, and seeing how bugs penetrated ships. And we did it too, seeing how bugs got into the various quarters of ships and survived. It was interesting work and it never came to a conclusion. Well, I'm sorry I left MRE when I did, because we'd developed all the techniques which were up and running. There's a difficulty with things like this - you can't do it, you can't do it with a man and a boy.

BM: And how long were you at the MRE?

KN: I was at MRE from 1952 to 1970. It was 18 years.

BM: So Henderson was quite right when he said it will take 15 years to do the basic science.

KN: Yes, well in fact we did have a new Deputy, we did have a new Director. Gordon Smith came. We needed to have a new Director. And of course people became very interested in viruses. I didn't get involved in the virus side. We did do some phage work. Phages survive very well actually, as you would expect them to.

BM: When did Harry Smith come to MRE? He became a Fellow of the Royal Society.

KN: I don't know when Harry Smith came to MRE. He was there when I got there, that's all I know about Harry Smith. He was an anthrax man, and a smallpox man.

BM: And he was Professor of Microbiology at Birmingham. That's what I know about him. And I remember meeting him on a high level trip.

KN: I did a lot of work with John Postgate. John Postgate was at Porton and he went into continuous culture of microorganisms a lot. And of course it's fine saying, 'I've grown strains of so and so for six weeks,' but I'm quite sure if he'd looked at the organism when he started, and looked at the organism when he finished, he'd have found he had something quite different.

BM: Did you have a series of young medical officers coming through MRE who were doing their national service? The reason I ask you is because at CDE there was a very effective series of young medically qualified physiologists. All came to CDE and Dick Adrian was one, Lord Adrian's son, Richard Adrian, David Band was another one. Bob Torrance and Arthur Buller were others. All very distinguished, distinguished later. At that time they were all young men, they all had medical degrees, they all were wanting to do physiology and so they did their national service at Porton Down, mainly working on organophosphorus compounds. That's what they came there for. I wondered whether you had the same arrangement at the MRE?

KN: No. When the Department got involved with viruses, a small number of young doctors were recruited, but they worked primarily as microbiologists.

BM: Did you have people coming to do their national service?

KN: I had one appointed to me. On one occasion we selected four potential pathogens: *Brucella*, *Pasteurella pestis*, anthrax and *tularensis*. And we tried to find quick methods of identifying them. I tried infrared first and then we tried fluorescent antibodies, but this was microscopy. Then we went onto iodine-labelled antibodies, but developing antibodies against these things was not easy. And in fact for a period of time I had a medical half-Colonel attached to me who spent his time growing nothing else but those four organisms for us to play with. But of course in those days we were handling them on the open bench. Microbiology was still on the open bench. One relied entirely on skill. Technique and skill. MRE had one fatality, Jeffrey Bacon. He was working with plague, but by the time he was diagnosed it was too late to treat him. But the safety was run by a naval Lieutenant Commander, well he was a Commander medic, Royal Naval medico and he ran the safety department. He's still alive, aged 99, in Porton village. But he was a very flamboyant character in the days of his youth but by God when it came to safety he ran the place well [KN refers to Dr Mark Darlow, Surgeon Commander, Royal Navy]. We were filled up with vaccines of this, that and the other regularly, and it worked. It worked.

BM: Very different from university work.

KN: When I was doing aerobiology, first of all it was, well first of all you are very concerned with aerosol physics. You've got, the sampling, any form of air sampling. We tried everything that we could. We developed cyclones, for example, where you are dripping media into them long before Mr What Not [Dyson] came along with his modern vacuum cleaner. And then in order to find out, meteorology, I knew very little about the Meteorology Department at Porton, we'd had two previous Directors of the Met Office at Porton.

BM: Yes, but that's because the work was of national importance.

KN: Yes, in 1963 when I did the first trial down in Weymouth bay, away from Porton, I had a tele printer working at the end of the office producing the raw meteorological data from the stations, and I had a meteorologist with me, and we were drawing the synoptic chart ourselves. None of this business of it just being printed out instantly! We were starting with the individual observations in order to work out where we were going to position people and whether we could in fact work that night. I was lucky that I had a very good forecaster and you'd say, 'Well, that's a funny observation, what do you think?' He'd say, 'Ignore that station, ignore that station,' he said, 'They're a joke.'

If you ignored that station everything was fine, you see. But once again I wanted to sample at various levels. So I got a balloon license. I went to be trained at Market Lavington on flying barrage balloons so as I could

hoist balloons, so we could get samplers up and down in the atmosphere. We also wanted the temperature structure of the atmosphere in the first thousand feet. The easiest way was to let a barrage balloon up, bring it down and measure the temperature. But in the end, I did use the Germans' lovely meteorological sond. The trouble is it sent all the measurements in Morse.

BM: If only your father had been alive [laughs].

KN: He taught me Morse, but he didn't teach me short-hand. I had to relearn Morse in order to get the data from the sond. I was naughty in that I only wanted the temperature, so I listened for the bits I wanted. But in order to do a simple experiment like that first of all we'd got to get a large electrical generator, onto one of Her Majesty's ships. Then you've got to get a compressor to produce compressed air for the sprays. And then you've got to get the pilot. Making meteorological measurements to do this sort of work was a full-time job. We even instrumented a Devon light aircraft at Boscombe Down to fly and take measurements and actually to take samples, but of course on the conditions when it was ideal for us to do the trials [laughs] they were flying on another mission. We never got the two together. Never got the two together. You can have bright ideas on how to do it, but the conditions never come right to it at all. But I think what we did learn was that any experiments conducted in the laboratory on aerosols, microbiological aerosols, could be forgotten. Unless they're done out there under controlled conditions, because in the laboratory it's simple. Things survive. Outside they pop their clogs with rapidity, great rapidity. I was fortunate in being able to sit and plan, in fact one of the first things I had to do was to sit down and work out the probability of being able to do the trials. One required certain conditions and in order to do them I had to take a team of say 40 people away for a week or 10 days and they were devoted to that and nothing else. Couldn't do it here. I tried to do it at Porton and say, 'Right, tonight we're going to set experiment. Set up, we're going to do it tonight.' But people had so many other commitments, they couldn't do it. So they had to be taken away physically for 10 days and be ready to do the trials when the conditions came right. And so we usually managed to get one or two trials in a 10 day period but that was. But we were handling 4,000 Petri dishes in a mobile laboratory, they all had to be counted. Oh, and I ran the slit samplers - the Porton slit sampler which just rotated our Petri dishes.

TT: Perhaps we'd better finish there Keith, and move on to filming a few short video sections with you. Is that all right?

KN: Yes, that would be a good idea.

TT: Thank you so much for your time and patience in sharing your memories.

[END OF TRANSCRIPT]

Further related resources:

1. Jones E M, Overy C, Tansey E M (eds) (2016) *Air Pollution Research in Britain c.1955-c.2000*. Wellcome Witnesses to Contemporary Medicine, vol. 58. London: Queen Mary, University of London.
2. Tansey E M, Maynard R (intvrs); Tansey E M, Maynard R (eds) (2017) *Norris, Keith: transcript of a video interview (15-Dec-2015)*. History of Modern Biomedicine Interviews (Digital Collection), item e2017024. London: Queen Mary University of London.