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Sir Rex Richards FRS in interview with Max Blythe
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Interview 1

MB Sir Rex, I'm going to ask you to turn your mind back to early years, to Devon, to Colyton and to rather interesting parents.

RR Well, I was born in this little village in Devonshire where my father was a small builder. He'd inherited the business from his father and grandfather, and my recollections of that time are somewhat patchy, I'm afraid, but I went to a local elementary school and then got some sort of scholarship to the local grammar school, which was a very old school.

MB This is Colyton Grammar.

RR This was Colyton Grammar School, yes. And my father was a very gentle, not very ambitious man, very good with his hands, a natural ball player, and I think I learnt a great deal from him because he had very high standards of craftsmanship and he taught me to use my hands and always insisted, as he put it, on a thing being done properly. He was a great gadgeteer and I remember my earliest recollections of my home are of primitive radio sets being built.

MB And you taking part?

RR No, I didn't understand anything about radio. This is back in my primary school days, but I did manage to pick up all sorts of bits and pieces, switches and wires and rheostats and resistors and so on, with which I played. My mother was quite a different character. She was a very strong character and she provided the business brains for the building business. She had been the secretary of the Bishop of Exeter as a young woman and was I think quite emancipated at the time. I hope I inherited something from each of them. And so then I went on to the local grammar school where I was extremely lucky to have really very good teachers, particularly on the science side. I had a good chemistry teacher and a very good physics teacher and physics was my great love at that time, and I learnt a lot from them and became interested. I have a very happy recollection of my childhood, of being a country boy, of a lot of outdoor sports; I was very keen on ball games. My father played almost every ball game one could play, not to excel because he didn't have that kind of ambition, but he did it very well. It's rather nice to have very happy recollections of one's childhood.

MB And you can also remember, while at Colyton Grammar School, views of a science career beginning to form.

RR Yes, I never had any doubts in my mind that I wanted to do some science, and it had never crossed my mind that I was ever going to go into the building business, although I was the only child, and to my mother and father's credit they never tried to

influence me to do that. It's very hard, casting one's mind back. I can't really say how this interest came about.

MB But quite early it was there?

RR Oh yes, certainly, I never had any doubts, even from the time I went to the grammar school, which must have been when I was about eleven.

MB Your early rapport with laboratories was intuitively right.

RR Yes, I had one of the bedrooms in our house which I used as a kind of workshop and did all kinds of experiments, which absolutely horrifies me now, the things that I did, and I don't think I had any doubts that that's what I wanted to do. Then as school went on, I didn't excel particularly at school, although I was good on the science side, until when we got to school certificate I started to take life more seriously and then worked quite hard in the sixth form.

MB This was in chemistry and physics?

RR I was doing chemistry, physics and mathematics, the usual combination. I did virtually no biology. Then when the time came to consider going to a university, of course, I knew nothing whatsoever about how to set about it, and the headmaster put me down for Oxford and for London University and for Exeter and they were all taking their scholarship and admissions at about the same time. The headmaster was called Mackay-Ohm, and he was a Cambridge mathematician. I remember him telling me that he'd come up to Oxford and had walked up and down Museum Road asking anyone he saw where he was best to send a chemistry student, and he actually met and talked at some length to Freddie Brewer, who was in those days a fellow at ... I've forgotten which college he was at, but he was in the Inorganic Chemistry Department. And he told Mackay-Ohm 'Send him to St John's where Tommy Thompson is the tutor.' So that's how I came to be entered for St John's, and that was at Eastertime and I came up to Oxford and, of course, it was an extraordinary place to me, a country boy. I knew nothing about it and it was all very strange. I had a frightful nose bleed in the exams, I remember, and covered all my papers with blood, but nevertheless got through it alright, went home. And having been to Exeter and sat some papers and had an interview, and also been to London a week or two before, I had only been home a few days when we got a letter from Oxford saying I could go to St John's as an exhibitioner, and so I said 'yes'. Then, about a week later, offers came from London and Exeter which I of course then turned down and caused a lot of irritation at the time. I can't quite understand why. I think at Exeter they offered me some kind of closed scholarship for which I was eligible, but they were rather offended. But I remember my father saying well I must just write and tell them that they should have written to us more quickly. But anyhow, that's how it was. Then we discovered that one couldn't get into Oxford without having done some Latin. I had already done some French.

MB But no Latin?

RR No. I'd never done any Latin and we only discovered it then. It was panic stations in Colyton Grammar School, and Mrs Mackay-Ohm, who was quite an

educated woman, started coaching me in Latin in the evenings. So I just learnt enough Latin to take Responsions in September.

MB So the Latin lessons went well?

RR Oh it was absolutely ridiculous really. I just learnt it up sufficiently to get through, came up in September, passed the Responsions, that was late September I suppose, and was almost immediately struck down with an infective jaundice which was sweeping round the country. So in fact I didn't come up to St John's at Michaelmas term at all. I was pretty laid out.

MB That was in '41, going into '42?

RR That's right, That was in the Autumn of '41. So I went up as an undergraduate in January '42. That was pretty traumatic, because not only did I find it very strange, but, of course, I was on my own, all the others having been there a term, and I clearly remember my first tutorial with Tommy Thompson. He'd told me that I should go away and read about the monosaccharides, a set of sugars which have very similar formulae but different configurations in space and I was pretty fascinated by this, reading about how the classical chemists had worked out the different configurations of these sugars, but it hadn't crossed my mind that I was supposed to learn it all, have it all at my fingertips, and so when I went to my first tutorial with Tommy Thompson he said, 'Well Richards, just write down the formula of galactose, will you?' and I thought God which is that? With a great struggle I managed to dredge that up out of my memory. 'Oh yes, what about mannose,' he said. And at that I was floored and said, 'Well look I can explain to you what the logic was of the sequence of events that showed what the structure was, but I doubt if I can remember which is which.' 'Oh well, Richards, I think you've got a lot to learn. You'd better do this again next week,' and that was the end of the tutorial. It only took about ten minutes and I was absolutely flummoxed by this. It was an appalling experience for a young man. I think it was a bit unkind, but it did have a tremendous effect on me. I went away realising what I was up against. You know this is a problem which I've noticed in my career that people who come up having been a big fish in a very small pool do find it rather traumatic to find themselves a very small fish in a very big pool, and that certainly hit me. I think Tommy Thompson may have done it on purpose. It set me on the right lines. I knew what the standards were then. But he turned out to be a very good tutor indeed, I think. He had a very clear mind. He didn't waste time. It was not often I had my whole hour, which was the usual Oxford allocation, but he packed an enormous amount into it and he knew just what was necessary. I mean, he didn't take a great deal of time in teaching me individual things. He assumed that I could go and find those out but what he did was to make sure that I properly understood it and made it quite clear what standards there were and what was expected. I think a lot of teachers who may be better at conveying the information don't always make those standards absolutely clear. I've felt that that has been a very great asset to me all through my life. So I owe a lot to Tommy Thompson in that way.

MB These were war years.

RR These were during the war years, yes.

MB An exciting time but an anxious one as well?

RR Oh very anxious, yes. The system at Oxford was to take in promising young men - there were no women, of course, at St John's in those days - and weed them out each year. So we started off with about sixty in the first year and ended with about fifteen taking finals. And at the end of my first year I had assumed that I would be called up because I was of the age then, and I had been very much involved with the Home Guard in Devonshire in my last year at school and in a kind of underground organisation which was being organised by a chap who was called Major Stukely, so I was interested and was all ready to go into the Army. In fact, I went to the barracks at Exeter for the whole of the vacation at the end of my first year. I had a very interesting time actually. Then I had a letter saying well you've got to come back to Oxford. So back I went, did my second year, and at the end of my second year got my call up papers. Then, just as I was about to leave, I got another letter or telephone call saying 'Don't go, it's been changed. You've got to go back to Oxford.' There was a great deal of confusion and it's all rather dim in my memory, but in the end I went back again and did my third year, and we had at the end of each year what were called special examinations, the examinations on which all this weeding was done. So evidently I just scraped through those and then took my Part I finals at the end of the third year and started research with Tommy Thompson in my fourth year, doing infra-red work, of course, because of what he was doing. All the work there was very much bent towards the war effort. It was concerned with the analysis of aviation fuels; the analysis of fragments of German rubbers to see what sort of synthetic rubbers they were using and what the raw materials were. We looked at all sorts of insecticides because of the war in the tropics. The hexachlorobenzenes had just been discovered and they were very baffled because some preparations seemed to work and others didn't, and it turned out that it was only one of the isomers of hexachlorobenzene that was actually active and we could analyse for these by infra-red methods, which chemically was jolly difficult. There were all those things going on.

MB From a war purpose point of view, all very interesting.

RR Well it was very interesting and in the primary assignment in my Part II it was very lucky because penicillin was just beginning to be isolated at that time. The preliminary trials had been done, and they were working very hard on what the structure of penicillin was - the benzyl penicillin. Anyway, by the time I was doing my Part II it had been narrowed down to one of two structures : one was called the oxazolone structure and the other was the beta lactam structure, and there was very big emphasis on trying to find which it was. Dorothy was working on the crystal structure.

MB Dorothy Hodgkin who was here only a few weeks ago?

RR Well she was working away on that, of course in those days with no computing machines, using appallingly laborious methods for doing the Fourier transformations. We were looking at the infra-red spectra, and I, in my Part II years, I had developed a method of looking at the infra-red spectra of small samples of material by powdering the material and grinding the material in liquid paraffin. And liquid paraffin and most organic compounds have refractive indices which are very similar, so you can make this kind of goo of the material, only a tiny speck and the

infra-red light would go through it without being scattered by the crystals because there was no change in refractive index. So I started using that to look at samples of penicillin and I had a frightfully traumatic experience. The penicillin was being isolated by Norman Heatley.

MB Who also was here a week or two ago.

RR Dear Norman. And he came over with a little ampoule with, I suppose just two or three tiny crystals at the bottom and I had to weigh this out and then put it into my apparatus.

MB This was invaluable material.

RR Oh yes, it was not a question of value, this was priceless. You know they were growing mould in bedpans in the path lab. I was rather nervous of course and it was an extremely thin tube and in trying to get the lid off the tube I crushed the tube in my fingers and these little crystals just flashed out onto the bench. Fortunately, I had been well trained. I had laid out shiny paper and so it was all there. So without doing anything more I rang up Norman and said, 'Look, I've had a terrible disaster,' and I was sweating like anything, you can imagine. And Norman came over with a camel hair brush and we swept it all up, dust and everything, and a few hours later he came back with it all recrystallised, so I was able to do the experiment. But in the end, I backed the wrong formula because it turned out that there are curious distortions of the vibrational spectra which take place in crystals which, of course, we knew nothing about at the time. But our experiments seemed to support the oxazolone structure and I well remember we'd been working away and always talking to Dorothy, and Dorothy came over to the lab one day and she said, 'Rex, I'm quite sure it is the beta lactam structure.' I was pretty confident at that time that it wasn't, and I said to Dorothy, 'Are you sure that the x-rays haven't caused it to isomerise?' And she said, 'Ohh....' and I saw her hesitate just for a moment, and then she said, 'Oh no no, it couldn't be.' And, of course, she was right. It was a wonderfully exciting time and a tremendous opportunity for a young man. It seemed to be so important. As it turned out it wasn't all that important because it turned out that it was very expensive to synthesise and so it was being grown by the bugs. It became important later because they varied the structures of the penicillins and found there is a great family of penicillins. That was what I did for my Part II, and as well as that I got involved in designing and building infra-red spectrographs, because in those days there were no commercial instruments, they were all made in the lab.

MB Here's the apparatus building part of you coming out again.

RR Yes and I was very interested in that and it all went back to my father's day and so I felt that anything that... if we had an idea... I could make it. And that was a very great asset, and David Wiffen and I built an extremely primitive instrument, but which worked beautifully, worked for years and years, five or six years, and we churned out enormous quantities of very good scientific work using very very simple equipment. I remember all the optics, infra-red optics, before they went into the spectrograph, we made by just taking clock glasses, aluminising the surface, then we glued a cork on the back with picein wax, and just held it in a retort clamp and if you wanted to move it you could bend it about. It was perfectly adequate and cost nothing

really. To go to Hilgers and buy a silver aluminised mirror would have cost a fortune to us in those day. So then I went on and did a PhD with Tommy and built more equipment and started building electronics and did a lot of infra-red studies of all kinds.

MB You wrote up on what theme?

RR Well, what did I do in my DPhil? Lots and lots of structural studies really. You see, by that time we had realised that the infra-red spectrum of a molecule tells you what its vibration frequencies are and these often could tell you straight away what kind of organic structures there were in an organic molecule. We studied a whole range of organic materials: silicones which were just beginning to be produced at that time, plastics and all kinds of polymers. We studied, for example, when you polymerise butadiene to make rubbers you can have all sorts of different methods by which it polymerises and you could do all that kind of thing. It was all pretty applied, when I look back on it now. And then I did a lot of thermodynamic studies. I did a lot of calculations of thermodynamic properties of simple molecules that were involved in the chemical industry, simply by measuring the vibration frequencies and, you know, you can compute from them what the thermodynamic properties will be, heat capacity, specific heat, heat content, free energy changes and so on in chemical reactions. So, I did a variety of things involving vibrational spectroscopy. We designed and built the first double beam infra-red spectrometer which compensates for the absorption of atmospheric water vapour and CO_2. It didn't work frightfully well, but it worked. So by that time I was six years from going up to Oxford and settling down, and I had imagined that I would go into ICI. That was what all the chemists did in those days. ICI was a very enlightened chemical company using enormous numbers of graduates and lots of good Oxford graduates went into ICI in those days. Almost every week there was someone from ICI prowling around the lab. I rather took it for granted they'd offer me a good job and that's where I would go. In fact I had been up to Billingham and to Northwich and various ICI Divisions, and to Manchester, really advising them, because they were beginning to use vibrational spectroscopy. So I went up as a young man helping them with that and it seemed a perfectly natural thing to do. And one day I had been playing squash and came back from the squash courts to my digs in Walton Street and found Keith Murray sitting on the sofa and he said, 'Look, would you like to come to Lincoln as a Fellow?' I said 'Well of course,' and that's how I started my academic life. I succeeded Nevil Sidgwick at Lincoln who was by that time quite an old gentleman. He must have been a Fellow of Lincoln since before the first war and he stayed on when the second war came because they needed people to continue with the teaching. So there were quite a lot of people who stayed on long after retirement age and Nevil Sidgwick was one. He was a bachelor, a distinguished man with an extraordinary reputation and I thought, 'Gosh this is a wonderful privilege to be able to go and succeed this man.' All sorts of people said, 'You're going to have a very difficult time,' because Sidgwick was well known to have an extremely acid and sharp tongue, but he couldn't have been nicer, he was frightfully nice to me and I discovered that he used his wit and critical strength the more distinguished the person, and was always very nice to the young. And so it was a tremendous privilege for me to go and follow him, for in those days the bachelors stayed on living in college. He occupied the rooms in Lincoln and never interfered with me as a tutor, but of course, he travelled a lot in the States, had lots and lots of friends, all over the world and they all came to visit him

and he always invited me and my wife to join them. I didn't realise at the time what a wonderful opportunity it was, and now I think it is such a pity I didn't keep a diary because I met all sorts of great figures from the past who came to visit Sidgwick.
R W Wood, who is a very great experimental physicist came to spend a couple of weeks in Lincoln, visiting Sidgwick. I met him and saw a lot of him. An amazing, extraordinary character, full of the most astonishing stories. It was a great privilege really.

MB You mention that your wife was with you by this time. Can we say something about her?

RR Well, my wife was born in Hungary and her family were all murdered by the Nazis, except her mother and father - and they escaped in 1938, late '38 or early '39, intending to go to America, and they came to England, and my wife's mother went off to the States where they had family living and almost immediately war broke out, and so her mother was stranded in the States and her father, who was an architect, and my wife were here. Her father took the English architectural exams and was used by the Government, although he was an alien, to do a lot of work on bomb damage. My wife went to Westfield College which was evacuated to St Peter's Hall in Oxford and she did a London degree and then did some research work for her PhD with Richard Barrow in the Physical Chemistry Laboratory on ultra violet spectroscopy and that's when we met. And we got married about a year after I had been elected to my fellowship at Lincoln. In those days, in Lincoln, I wasn't actually allowed to be married in my first year! As soon as I had been appointed as a fellow of Lincoln, of course, one is then really on one's own. You've got to think of doing research. I had by that time become very interested in the intensities of infra-red absorption, which in those days was thought to be of no interest by most people. And I had a very good research student called Ted Hartwell.

MB Who I met years later at Charterhouse.

RR Yes, you probably did, he went to Charterhouse, but Ted was a very good experimentalist and he worked with me and we published some quite interesting papers on intensities which, years later, were all taken up and proved quite interesting. Well Ted did that, so that just kept me going for a year, but I realised that Tommy Thompson was a very powerful personality and I didn't really want to feel myself tied to his apron strings for much longer, so I said to Tommy, 'Look I'd better stop doing infra-red spectroscopy altogether because there is inevitably a conflict of interests.' Tommy felt that I was using the equipment perhaps too much, so I just gave it up altogether and started to cast around for something else to do and I thought I might do some work on magnetic susceptibility and so I borrowed or begged an old magnet from the Inorganic Chemistry lab and set up some susceptibility measuring equipment. It wasn't very interesting as it turned out, but anyway it was a start, something to do. As an Oxford tutor your're expected to take students for Part II and you've got to think of things for them to do, so I started doing some calorimetric, thermodynamic measurements on clathrate compounds. These are crystalline materials that crystallise in such a way that they have large cavities within them and if you crystallise in the appropriate manner you can trap small molecules that are otherwise gaseous. For example, you can make a compound of quinol with oxygen

molecules trapped in the holes and at a pressure equivalent to many many atmospheres, and you can put lots of small molecules into those.

MB So a very complex matrix effect.

RR Yes, yes, and these compounds had been discovered in the 19th century and totally overlooked until Tiny Powell, who was a crystallographer here in Oxford, started to do crystal structures on them. It was through conversations with Tiny that I thought 'Well, it would be very interesting to study the thermodynamic properties of these molecules within the cavity. Are they banging about in the cavity; are they rotating or are they just jammed in? What's going on?' So, once again all my manufacturing skills came to life and I designed a very sensitive calorimeter which would enable us to measure the heat of interaction between the molecules in the cage and out of the cage. The calorimeters were based on using Dewar vessels and it was a differential system which proved to be incredibly sensitive, really very, very sensitive. And I had a very talented student called Dennis Evans at about that time and he started the work on this calorimeter. And that brings me back. - my mind is just going over those early days - I was extremely fortunate when I went to Lincoln. I was quite dynamic in many ways and I did a lot of lecturing and used to do a lot of schools visiting, and I got marvellous pupils coming from all over the country, especially from Nottingham High School. I had a string of about five pupils over five or six years from Nottingham High School and every one of them won the Gibbs Scholarship which is the scholarship for the best chemist in Oxford, and Dennis Evans was one of those and he built these calorimeters with me and did a lot of that early thermodynamic work with others, and that kept my students going and kept my mind ticking over. And about 1948, or was it 1947, '47 or '48 I had discovered the letters in *The Physical Review* which appeared late in '45 or mid '46, from Harvard and Stanford University, describing the first observation of nuclear magnetic resonance and I was rather intrigued by this. I think I got into it because I had been messing about with magnetic susceptibility measurements and I had been making magnetic measurements.....

MB A natural link.

RR Yes. I think that's what made me read the papers really more than anything else and I went over and talked to some of my colleagues in the Clarendon about this and they said, 'Oh well, you know, you'll never make this work in a chemistry laboratory. It's hopeless.' I was a bit discouraged by that and in about in 1948, the academic year, '47-'48 or '48-'49, I can't remember which, Linus Pauling came as Eastman visiting Professor to Oxford and he was at Balliol really, but he was a very old friend of Sidgwick, so was always dining at Lincoln, so I got to know him quite well, a wonderful character he was. And I remember one night saying to him had he seen these papers and 'No, he hadn't,' and I said to him, 'It's rather intriguing, I'd quite like to have a go at that, but all my physicist friends say I'll never make it work.' And he said 'Well, the one thing I've learned in my life is never to pay any attention to what the physicists say.' So I thought, 'Well, why not?' And then I went round to the Clarendon again and saw Bernard Rollin, now dead, who had been interested in this and I talked to him about it and said to him 'Look, you don't think I'll ever make it work, but I'm going to have a go just the same.' And from that moment on he was tremendously helpful and co-operative. He said, 'OK, I'll help in any way I can,' and

he advised me to get what was in those days known as a Tickford magnet which the Clarendon was using for its magnetic work. It did a lot of work at low temperature. That was all very well, but it cost nearly a hundred pounds.

MB A lot of money in those days.

RR Oh, it was, a lot of money. But as I was new and I went to see the head of department who was in those days Hinshelwood, and explained what I wanted to do. He hadn't the foggiest idea what it was. I said I wanted to spend this money and he said 'Well, as you're just starting maybe we can find the money,' and he did and I was able to buy this little four inch magnet. It had low resistance coils, so the question of how we were going to produce a stable field was raised, so, I got some submarine batteries, which you could buy for next to nothing, to use to energise it and of course the problem was to keep the field stable because magnetic resonance requires a very stable field and I hit upon a simple little device which was really a tube with a column of mercury in it, through which we sent the current through the magnet, because it only drew 4 or 5 amps. And by boiling up a 4B pencil in caustic soda you can make the wood fall off and you get the rod of graphite, and I made a little chuck to hold it, with a screw on the top, and this graphite stuck into the mercury, and then I had a piece of manganin and measured with a potentiometer the current very accurately, and so as the current decays, - you know if you are using an accumulator the current just steadily decays the whole time, I just turned this knob at the top, winding the graphite rod slowly down into the mercury to compensate for the drift and that was pretty good. It took a long while to get to that point because I started off putting wires in, but what you find is that the mercury amalgamates with the wire and then as you lift it up or push it down the mercury goes in little gulps and it made the current jerk whereas graphite, you see, is not wetted by mercury and would go in perfectly smoothly. So that's how that magnet worked. I set the whole thing up, and of course this wasn't the sort of thing that was possible for a graduate student so I did all this in my spare time. The electronic equipment had to be homemade, mostly homemade. It was made out of components from surplus RAF and army radar sets and communications equipment which one could buy for next to nothing from what was known as 'the dump.' There was an aircraft hanger at Abingdon where all this stuff was put. You went in with a trolley and you just loaded it up and paid a few pence per pound weight and I spent hours and hours in the evening with a soldering iron unsoldering components and putting them into boxes and laying them out. Occasionally we were lucky enough to get a radio frequency amplifying strip which would do what we wanted, but mostly it all had to be rebuilt, so I learnt quite a lot of electronics the hard way really, by trial and error. I eventually built the whole thing and tried it out. I couldn't find a signal at all. It was absolutely hopeless. I spent hours and hours gazing at a little scope, you know, with a noisy line on it wondering did I see something go by as I was changing a field or didn't I and it became very discouraging. Then one day I thought well I wonder - what I was using was a radio frequency signal generator which was out of RAF test equipment, a Marconi signal generator and it had a great knob attached to a paxolin spindle which changed the range and then it had a beautiful little tuning dial so you could work over a certain range of frequencies, and then you had to click it round - and I thought, I wonder, and I carefully jerked it back a step and ran the field up and down and immediately an enormous signal appeared on the scope. What had happened was that this knob had got turned on the paxolin so that it was pointing at the wrong range and I got a signal

because it was working at only half the frequency, and so I was just looking at the overtone. You see I was looking at the first overtone which was much weaker than I thought it was, all the energy was going at only half the frequency. So by winding it up and coming in on the fundamental I got a beautiful signal. And then the question was what could one learn. I started measuring relaxation times with the nuclei in drops of fluid of various kinds, using what is known as progressive saturation and it wasn't very helpful. And then, at that time John Smith, who had just reached the stage of doing a D.Phil, having done Part ll on calorimetry, so I said, 'Look John, would you like to take a chance and have a go at doing some experiments on this?' and he said yes he would. and started measuring some crystals. Perhaps I ought to explain. Can I take a few moments to try to explain the principles of NMR. Well nuclear resonance is a technique that depends on the fact that the nuclei of many atoms have a magnetic moment. They behave like short bar magnets, and the strongest of these turn out to be the nuclei of hydrogen atoms. So one was always working on the hydrogen atoms to begin with. Now if you put a sample containing these materials into a strong magnetic field, those little nuclei behaving like magnets behave like compass needles and all want to turn like a compass needle would, so that their north seeking poles point towards the north pole of the magnet, and of course any other orientation in the magnetic field can only be achieved by doing work on them to turn them away from their preferred orientation. If you do work on them, of course you raise their energy. Now it turns out that the quanturn theory sets very severe restrictions on the energy that they can have, by virtue of the interaction of their magnetic moment with the applied field, and in the case of hydrogen nuclei they are only allowed to have two energy levels. You can think of them as corresponding to the most favoured orientation with the north seeking pole, pointing towards the north pole of the magnet and the least favoured one, in which they are pointing in the opposite direction. So when you take a drop of water and put it into a magnet all the little nuclei immediately have to occupy one or the other of those orientations, and it turns out that you can make them jump from one orientation to another by irradiating them with electromagnetic radiation which has packets of energy just equal to the energy difference between those two levels. In the sort of magnetic fields we were using in those days, the packets of energy corresponded to radio frequencies. In those days we were thinking in terms of 20 megahertz just actually close to the IF frequency of the radar sets are worked on. So that is what you do in an NMR experiment and when you've flicked the nuclei over you can observe the effect of this by measuring energy absorbed from the radiation. It's just a simple radio frequency technique. But because the magnetic moments of the nuclei are so tiny the effect is very small, so the signals are very weak. Now of course if you look at a crystal where you've got a set of hydrogen atoms rather close together, the magnetic field that any one hydrogen experiences is the field applied by the magnet, but then you've got to bear in mind that you've got other little hydrogen atoms nearby which are all little magnets and they alter the field of this one by increasing it a little bit perhaps or if they happen to be looking the other way, it would decrease it a little bit. So the magnetic fields that one nucleus experiences are modified by the nuclei around it and this gives rise to broadening of the crystals resonances and you can work out.... The theory of all that had been quite well worked out long before nuclear resonance had been measured, by a gentleman called Waller in the 1930s, and so John Smith and I decided we would look at some crystals where the distribution of hydrogen atoms was unknown, and see if we could work out what it was. Of course it didn't lend itself to anything but the very simplest cases and at that time there had been quite a controvesy in the literature

about the structure of the acid hydrates. Somebody called Louzzatti, working in Paris on x-ray structures was publishing on this and the question was for example, was nitric acid monohydrate $HNO_3.H_2O$ or was it $NO_3^-.H_3O^+$ and that was absolutely a gift for us and that was the first thing we tried and we were able to show straight away that it was $H_3O^+NO_3^-$. A beautiful experiment. It didn't require any great quantitative measurements, but you could see it straight away. So after that my students and I, for several years really, did a fairly exhaustive study of the structures of all sorts of simple inorganic materials where the hydrogen atoms were involved,. and of course it was useful because the x-ray people can't see hydrogen atoms. They don't scatter x-rays much. The x-ray people can tell you where all the heavy atoms are, but they couldn't see the hydrogens. We were particularly good at doing the hydrogens.

MB And so you were able to complement a lot of the work done by then.

RR Yes, that's right. We did all sorts of things, but it was a nice exercise showing that the method could be used, and then it turns out that if the molecules containing the hydrogen atoms are undergoing some kind of molecular motion in the crystal, for example, the ammonium ion in ammonium chloride doesn't just sit there, it turns all the time about any one of its four axes. Now when that happens it has a very dramatic effect on the nuclear resonance line widths because it averages out those local fields, if it goes fast enough, and so the lines become much narrower. And we looked at that by measuring the nuclear resonances as a function of temperature, from room temperature down to that of liquid hydrogen, the lowest temperature accessible to us in those days. That enabled us to work out the kind of molecular motion that was going on in these crystals and, also the potential barriers hindering it. That was of considerable theoretical interest and I was rather gratified when - that work was done in the 1950s - when in the middle and late 1960s, when there were a great many people working on neutron-scattering, an enormously expensive technique, they re measured all these things and I was gratified to find that generally speaking it was all there although they were a bit more accurate than we could do it, although I didn't feel at the time that the extra cost was warranted. So that was what we were doing.

MB What I should ask you at this stage is whether we ought to consider the magnets you were using. I think you yourself had to start up-grading magnets?

RR Well, all that work was done on the little Tickford magnet, and then I was lucky and went to Harvard for six months from January to summer 1955, and that was when I had already done a lot of this work on crystals, and I went and worked as a research fellow in the physics department. I thought it would be good for me to do that and Ed Purcell and Robert Pound who were the co-discoverers of NMR were there and they were very welcoming to me. I had an absolutely wonderful time there. Purcell was the most marvellous expositor. He had an extremely clear insight into all sorts of physical principles and I learnt more physics from him than I've ever learnt from anyone else, just in casual conversations really. He had a very good idea, his office was on the first floor of the Lyman Laboratory and the whole of the first floor corridor was lined with blackboards he'd had that put in, and so if anyone went to his office to ask him a question or discuss something, he'd get up, go out into the corridor, and the discussion would take place in the corridor so that any passer-by could stop and join in and listen. So I profited enormously from listening to those discussions. He could always strip away the difficult theoretical framework which is

used to understand many of these physical principles and see right through to what the physical processes were, and someone coming to the subject from the outside would find it tremendously enlightening. He also had a marvellous grasp of the orders of magnitude of physical properties, so if you said, 'Could it be due to this...?' He could say, 'Well let's see, the velocity of this is so much and such and such,' could do immediately a quick calculation and say, 'Oh no, it's a hundred times too small.'

MB A great rule of thumb man.

RR Terrific. So that was a very valuable experience for me and I worked actually with Robert Pound who was a brilliant experimental physicist and I learned all my electronics from him really. I realised that I had really been just spuddling about at home. Then I went back to Oxford and in that year the first chemical shifts were published and as soon as I realised what was happening, from my infra-red experience, I realised, 'Gosh, this is going to be really important, in terms of analytical procedures.' Now the chemical shift arises because the nuclei in ordinary molecules are not by themselves, of course, they are surrounded by electron clouds which bind the atoms together, and it turns out that these electron clouds behave like superconducting shells of perfectly conducting materials. Now you know that when you put a little coil of wire in a magnetic field you induce an electric current in it, that's the principle of the dynamo, Faraday discovered that electromagnetic induction. In the same way, when you put a drop of water into a magnetic field, you induce, in those electron clouds holding the atoms of hydrogen and oxygen together, you induce circulating currents. Because these electron clouds are perfectly conducting those currents go on flowing indefinitely until you pull the material out and cancel them all out again. That is the effect which is responsible for the bulk diamagnetisation of all matter. You know all material is repelled from a magnetic field for that reason. You put the stuff into the field, the field induces a current which opposes the field, and so it is trying to push it out. Now it turns out that these little electrical currents, that are induced by the clouds of electrons, vary according to the distribution of electrons, rather sensitively. So a hydrogen atom in a CH_3 group has a slightly different electron distribution from the hydrogen in an OH group. The currents induced around those electrons are different, so the magnetic fields that the nucleus of a hydrogen atom experiences in a CH_3 group are just a tiny bit different, and so when you look at a hydrogen resonance spectrum you see the CH_3 hydrogens occurring with a different field to those from the OH hydrogen; that is the chemical shift and as soon as that was observed, and of course it could only be observed when the magnetic fields became stable and homogenous enough. So as soon as that was observed I realised this was going to be quite a big thing and I decided I must build a magnet capable of doing this. The only way of getting the homogeneity of course was to make a big magnet. You had to have big pole pieces relatively close together. So I looked into this and got some advice again from my friends in the Clarendon [Laboratory] and I got a hundred and eighty pounds to build that magnet. Very little. The copper wire, I remember, cost about a hundred pounds, and so there was a little left over and we decided to make the yoke from cast iron, which we got from the Cowley Iron Works. They just cast four great hunks of cast iron. It was an enormously heavy thing. They shipped those hunks of cast iron over to the Pressed Steel Company where I went up and begged them to help. They had great grinding machines and were able to grind flat surfaces so that the thing could be assembled into a rectangular yoke and then the Pressed Steel Company cut some pole pieces out of what they called steel plate, which

was actually four inches thick steel. They cut discs out and drilled them and I assembled those into pole pieces and then the Cowley Iron Works also cast some aluminium formers onto which coils were to be wound and shipped them down to the laboratory in Oxford. The laboratory workshop made a sort of frame a sort of mangle frame to hold the formers, and I wound the glass covered wire on by hand, simply by turning a handle which rotated the former, and guiding using a glove, guiding the wire on. Oh, it was a terrible job. As you know, the terrible problem of winding a coil the winds go on side by side but then when you get to the edge you've got to turn it round and come back, and it then sometimes slips in and gets a bit tangled, so you've got to go back. You've got to make the winding run absolutely smoothly, and then when it gets a bit difficult you can put a piece of cardboard in or something similar, just to make a smooth base to start with again. Anyway, I wound four coils, two of them with thick wire and two of them with 28 gauge wire. There was miles and miles of wire on them and it took me several weeks just grinding away winding this wire on, and we assembled the magnet and it worked quite well, but wasn't good enough really. We could see chemical shifts with it but the homogeneity wasn't good enough and I began to realise that it wasn't sufficient to have a very flat steel surface in the pole piece, but you had to worry about the grain size of the iron in the pole piece. If the grain size wasn't uniform, of course, that introduced inhomogeneities of its own. So I went and read a lot of metallurgy about pole piece design and realised that we were never going to get any good proton high resolution resonances from that magnet, and so we decided to use it for looking at all sorts of other nuclei and ranged over the whole of the periodic table making measurements on most of the elements in the periodic table, looking at structures of chemical substances. And we published a lot of work on nuclei that has only been taken up more recently, with modern commercial machines, because in those nuclei, although the signals were much weaker, the chemical shifts were much bigger so we didn't need such good homogeneity and stability. Then later on I still used that magnet to do electron nuclear double resonance, which I'll mention later perhaps, but as that went on I realised that you couldn't do the high resolution work and so I thought the right way to do it was to use a massive permanent magnet. That avoided the problem of producing heavy electrical currents and stabilising them to a very high degree. Let me say that one was talking about stabilities and homogenetics of the order of a few parts per hundred million. I mean it was a formidable engineering problem and so I touted this idea of building a permanent magnet round various places and was very lucky and met a wonderful man called Tyrrell. I can't remember his other name I'm afraid, but he worked for Mullard and had been the genius in the design of the magnetron magnets during the war. He was very enthusiastic about this. He was a great gadgeteer, always interested in new challenges, and with some difficulty I persuaded Mullards to agree to build a magnet which we would design together. I said, 'Look I've got to raise some money and what will it cost?' and they said off the cuff 'Just £2,000.' So I went off and made applications for money, and you know in those days grants were very hard to come by. And I got first a grant from the Shell Oil Company, to their enormous credit. They gave me £500, and then I got some money from the Royal Society. I then went to the DSIR as it was then and said, 'Look, I've got these two sums of money, will you make up the difference?' and they did. And we started work on the design and by that time I already understood a lot about the metallurgy of pole piece design, and Mullards built that magnet over a period of two years. I went every month, the wearisome trip to Redhill in Surrey, really to keep them at it. They were losing money terribly because it cost them a great deal more, but in the end they built it and

it worked beautifully. It had fantastically good resolution for the time. In fact I came across some recordings done with it, only the other day, and they are pretty creditable even by today's standards. So that was very good. That magnet was used for years and years and is now being used in the Clarendon Laboratory.

MB I think your first magnet is now on display?

RR Now that old magnet, I used it for some dynamic nuclear polarisation work. This is a beautiful technique which was discovered by a physicist and made it possible to build up the nuclear polarisation by saturating electron resonances. This is a rather technical matter, but I thought I'd have a good go at that and started. I did some preliminary experiments with that old magnet. Then when I'd made it all work beautifully it was obvious, by that time, when we were moving into the early sixties, when scientific research was being supported more strongly, I was able to go and get money, and I bought a Varian commercial magnet and built an even bigger permanent magnet. So that old magnet was superfluous and John White, a former student of mine, used it a little bit and then when I didn't want it any more the Science Museum came and fetched it away and it's up on the shelf on the mezzanine floor there, together with some of the recordings that were made with it. It did some sterling work. Then with the high resolution permanent magnet we did a lot of work on the early use of NMR to find chemical structures. I stomped round the country lecturing chemistry departments telling what a good thing this was to very sceptical audiences. They were very, very sceptical about it. It was thought to be using a steam hammer to crack a nut, but of course as the technique developed it became enormously powerful and now no organic chemistry laboratory can manage without it of course. So I'd done quite a lot of solid state physics, you see, in the early 1960s, and then we'd explored the applications in chemistry, and then the dynamic polarisation was really physics again, but I was really quite interested in biochemistry by that time and thought could we use this method to study biological systems? And it was pretty hopeless really, because of the signal to noise ratio. You see biochemical molecules are large molecules, not very soluble, and so the solutions were very dilute. We simply couldn't get enough signal and it wasn't really a viable proposition. Now the signal noise that you get from an NMR machine goes up roughly as the square of the field strength so obviously we pushed the field up, but with an iron magnet because of the hysteresis of the iron, as you increase the power you feed into the field goes up and flattens off because of the hysteresis, and you can't with an iron magnet get to fields above about 2 tesla, perhaps 2½ tesla, and this was simply not enough. It was about that time that all the hard super conductors were discovered and wires produced, and Martin Wood, who by that time had established his little company making magnets for physics experiments, went over to the States and came back, it must have been about 1960, with a length of niobium-zirconium wire and wound a little magnet, about so big, which he could drive from a motorcar battery in liquid helium, and produced a field of about 4 tesla, twice the field that you could hope to get with an iron magnet.

MB Sir Rex, this is a point where I am going to have to close this first interview. I think the link with Martin Wood and what was to happen next is there, and we will talk over what happens at our next meeting.