TODAY'S NEUROSCIENCE, TOMORROW'S HISTORY

A Video Archive Project

Professor Sir Peter Mansfield

Interviewed by Richard Thomas

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Interview transcript

School days in south London

I was born in 1933 and the area that I was brought up in was Camberwell, and Camberwell is in south London, which is really - was - a slum area, and I think still is today, actually, but in those days I, like other children, played on the street. I was born into a family that was living in flats in Camberwell, not far from Camberwell Green, and my father was a gas fitter who worked for the South Metropolitan gas company, and my mother had various jobs.

I can't remember whether I was eleven already, but I remember the teacher in the school that I was at - my old local school - came along one day in a bit of a flurry. He said, 'You've got to take the eleven-plus exam.' And I'd never heard of the eleven-plus. I think it was something that had been recently implemented under the 1944 Education Act, and so I was sort of marshalled to take this exam the following week. I had no preparation and as a result, I didn't do very well at it.

So we started all over again at Peckham and, of course, once we got there, we found that they still did French and German which were the languages, so we kept that part of it going, and woodwork we did – and I was quite good at woodwork because of all the past work that I'd done as a boy in Devon. I was quite good at metalwork as well. So they were the areas that I really was quite good at, and the areas that I wasn't quite so good at were areas such as PT, sport – I was absolutely useless at that. I remember the gym master putting me in the boxing ring one day with other boys [laughs] and I had to - well we all had to do this - but when it came my turn, I was in the ring for about five or ten minutes, and [laughs] I remember the teacher, the PT master, saying, 'Mansfield, you really should have been a dancer, not a boxer.' Because I was in there for ten minutes, and I never got one hit at all [laughs]. I was ducking and weaving all the time, you see [laughs]. So anyway, that was my boxing career.

No qualifications but ambitious to become a scientist

The Careers Officer said to me, 'Now you're fifteen-years-old, what do you want to do?' And I said, 'Well, actually, I wouldn't mind being a scientist.' He said … he looked at me, you know, with incredulity. 'A scientist?' he said, 'What makes you want to be a scientist?' So I said, 'Well, I'm quite interested in rockets and rocketry,' because I had spent some time looking in the Science Museum in South Kensington, and I became, in my own way, a bit of an expert on rockets and technological things of that sort. I used to spend hours there, making drawings and all sorts of things, you know. I was really, really into it. I couldn't touch any of this, of course, it was just looking at it, but I was very, very interested, and there was - I think there still is - an aeronautical museum there out at South Kensington, so I spent hours there. And this chap was flabbergasted. He said, 'Well, you want to be a scientist but you haven't got any qualifications.' So I said, 'Well, no, I haven't, but even so you asked me what I'd like to do, and that's what I'd like to do.'

So he said, 'Well now, let's … let's get sensible. What other things are you interested in?' So, I said, 'Well' … one of the hobbies that I'd been working on for some time was printing. I was very, very much into printing all sorts of things - letterheads and visiting cards and so on - but one of the things that I was very much into was printing a small magazine.

Night school and a job in rocketry

When I got turned down from a scientific career at that school-leaving episode that I mentioned, I decided, because of what the guy said to me, 'Well, you don't have any qualifications' – I decided that I would look into getting some qualifications. So, at the age of fifteen I went out and joined evening classes immediately, on day one, and during all that I told you, I was actually going to evening classes every night, five nights a week at the Borough Polytechnic, which is now, you know, the City of London University or something. And so I went there, you know, religiously every night, and I could only do one topic per night, so I could only study five subjects. So, I did Maths, Physics, English, French and I think I may have done Chemistry or German. I can't remember what I did then. I know I subsequently did Chemistry but I couldn't get all the topics in five nights, but anyway, I went five nights and I did that, and I was doing that for the whole three years while I was working as a printer.

So, that's one point I wanted to mention but the other thing was that during my period working at Strakers, I happened one day – I mean, our family took the *Daily Mirror* as the paper that they looked at – and on Saturday, the *Daily Mirror* introduced an article called *Children's Mirror*, and I remember reading this one Saturday and there was an article in there about this grammar school boy that had decided he wanted to be a rocketeer. He wanted to work in rockets and he ended up working at this strange place in Aylesbury somewhere, and I was very interested in this because I thought, 'Rockets – that's what *I* want to do.'

RPD (Rocket Propulsion Department, of the then Ministry of Supply) Westcott was actually … you know, they had lots of things going on. It was an old aerodrome, and so the Solid Propellant Division was in one corner, the Liquid Propellant Division was in another corner, and it was quite a place, and a revelation to me as a young boy. I was, by then, nearly eighteen, and so I went for this interview, and I saw Dr Crookes, and he was quite impressed with what I had to say. 'You know all about this, you know all about that, you know. Would you like to come and work with us?' So I said, 'Well, yes, but do I need any qualifications?' And he said, 'Well, if you came - you don't have any qualifications at the moment, but you say you're studying for your O-levels (or matriculation, actually). We'd give you a position on the understanding that you will get your matriculation. You must carry on studying but you can start as a scientific assistant.' And that's how I started.

Queen Mary College, London University, 1956-64 - the beginning of Nuclear Magnetic Resonance (NMR)

And in the third undergraduate year you could either do experimental physics, carry on the way you were going, or you could do theoretical physics, and I said, 'Well, I'm quite interested in the mathematics of theoretical physics' - but I was also interested in experimental physics. 'Could I do them both?' [laughs] And the tutor, when I picked him up off the chair, he said, 'Well, yes, you know, we've not really done this before but if you would like to do that, we'll do it.' So I did both theoretical and experimental physics and that's probably where I picked up my mathematical expertise.

One of the areas that was offered was under a chap called Jack Powles, who was a reader at the time, a reader in physics, and he had offered this third year project of building an NMR (Nuclear Magnetic Resonance) system for detecting underground objects. After that project, I had obviously formed a link with Jack Powles. I knew some of the people in his group already, plus, I had done rather well in the final examinations and ended up with a First, and so, you know, it was pretty well automatic. He said, 'Would you like to come and work with me on NMR?', which I accepted. I was highly honoured to be asked, to be honest.

The names that were common in the laboratory were Bloembergen, Purcell and Pound - a famous paper which helped to kick off the whole business of NMR. There was Hahn's paper and, of course, we were using Hahn spin echoes in those very early days, and people before me - before I started research in Jack Powles lab - people were using Hahn spin echoes to study the properties of liquids. So, there was before us quite a number of important papers and important contributions which really started the whole business and, of course, without them we would have had nothing.

First spin echoes in solids

There was this feeling that you couldn't get spin echoes to occur in solids. You could in liquids because the decay time was so long, I mean relatively speaking. A liquid-free induction decay would last maybe 50, 60 milliseconds, whereas in a solid, the free induction decay lasted 30, 40, 50 microseconds. So, the feeling was that there wouldn't be the possibility of generating an echo in a solid and so there wasn't really much to read. There was plenty on liquids but nothing much on solids. I mean, people could see free induction decays in solids, but no one, it seemed, had thought of trying to get a signal and an echo, but the apparatus that I built had a recovery time which was about three or four microseconds. So you had a one-microsecond pulse, there was a recovery time of about two or three microseconds, and then after that you could see the signal but it wasn't a coherent signal. So, in other words, you put in one pulse and there was no electronic coherence because I was pulsing the oscillator on and off that created the RF. You know, this was how I managed to produce the very, very short high-power pulses.

But one day, I decided to just try putting in two pulses, and I managed to get two pulses fairly close together within about five microseconds - one and then a second pulse - and I noticed that there was a huge jitter on the scope. I set to work to try to introduce some coherence between the two pulses and managed to actually do this by very careful timing. So, I had a timing circuit, which I could inch along until I got the phase just right, and when I did that, I found that I got a decay (looking at it from your way), a decay that came down here, then a pulse – a second pulse – and I could bring this down to zero, there was nothing. But if I inched the pulse along a little bit to get a different phase relationship, I would get a signal which started to grow. And this was… I went along to Jack Powles and showed him this, and his first reaction was, 'There's something wrong with the electronics.' He said, 'No, you know, you can't get an echo.' He said, 'This is a recovery problem that … it's the receiver's recovering, and it looks like an echo but it's not really an echo.' But I didn't believe that.

So, we sent away to buy a single crystal of material. I mounted it on the probe and tried these double pulse experiments, and sure enough, when I got the phase right I could get an echo, and when I got it wrong, I got nothing. So I got a zero response or an echo, and I showed these results to Jack and he said, 'Well, this should be easy – a lot easier to work out than what you were doing.' Because I was trying to do the many body problem whereas in calcium sulphate you've only got two molecules of water so, you know, the calculation's a lot easier, and there are orientations of the crystal where these two molecules give the same signal, in a certain orientation. So it became not trivial, but a much easier problem, so we published a paper on this and this was the birth of solid echoes.

Nottingham University, 1964 – first NMR images

Well, the whole business started actually over a discussion at coffee time, and it was the custom - and still is, in fact - the custom to have coffee in the coffee room down in the Physics Department at about 11 o'clock. And we started discussing, that is to say, myself, Peter Granell - a student at the time - and Alan Garroway. And we started discussing where we should go next, basically. We'd got all these very nice results using fluorine resonance and we were really looking for chemical shift properties of certain materials, certain solids and so on, but then, when we had exhausted all the materials that were readily available, we wondered what we should do next. And the conversation seemed to turn around to thinking, 'Well, we've got this very nice line-narrowing. We can narrow lines, in the case of calcium fluoride, down to something line one or two hertz line-width. Could we use that for anything?' I mean, if we could apply a gradient to it, we could study the atomic structure, and it was at that point that I suggested that maybe, you know, that's what we should do. And Alan Garroway was a bit sceptical, but Peter Granell, who was more amenable to suggestion because he was, after all, a student approaching his third year, he grabbed that and we worked together on it. I did some calculations and the whole subject started. This was in 1972.

It lead to the idea that perhaps what we should do initially was to create a sort of model lattice, which was made of layers of camphor in our case, and this was sandwiched together to form a discrete structure and then, by using line-narrowing and a field gradient we could actually see the structure. So, we simply had made, if you like, a simple crystal – not really a crystal but, you know, ostensibly something which had structure in it, and then we could look at that structure in a magnetic field gradient. So, we had several things working on those very, very early experiments – line-narrowing, the application of the magnetic field gradient, and the observation of structure.

When the special, or specialised Colloque Ampere was organised, Raymond Andrew was invited as the opening lecturer and I was the number two. So, he went on stage first and gave a talk on his spin solids work and then I followed with my so-called NMR diffraction work, and showed these results and that created quite a stir in the conference. So much so, in fact, that there were questions afterwards on my paper, and one of the people that raised a question was John Waugh, and he made the point that he thought he'd seen something very similar published in *Nature* very recently - and I hadn't seen it. I actually didn't really read *Nature* regularly. And so he said he thought he'd seen something very similar. Could I comment on it? And I said that I hadn't seen it. I asked him, actually, whether it was connected with imaging in solids, and he said he didn't think it was. It was a liquid system that had been looked at. So, when I got back to Nottingham, I went straight to the library, looked up the relevant article and there was Paul Lauterbur's work, and it had been published about two or three months earlier on his idea of 'NMR Zeugmatography', as he called it. And what he had actually observed was a crude image of two test tubes, and he had no structural algorithm for producing the image - he'd done it all by hand - but nevertheless, it was the first indication that you could do imaging. But it was done by a completely different technique to the way we were working.

The main difference is mathematical, in a sense, because Paul was trying to produce images in real space and the images that I was working on were brought about though K-space. Now K-space is reciprocal lattice space, and you know, without going into more mathematical detail, I think I'll leave it there, but it seemed to me much simpler to work in K-space rather than real space.

Echo Planar Imaging (EPI)

We started to think very seriously about how we could define the third dimension. We could get an image in two dimensions but it needed … to do that it needed really to have some sort of symmetry along the third axis, and obviously, for a proper imaging technique you really need to be able to look at a thin slice. So, we started thinking hard and long about how you could define a slice, and when I say 'we', I mean Peter Granell, myself, and Alan Garroway, because by then we'd managed to convince him that there actually was something in imaging. So, we worked together and he had - Alan had - actually come up with a way of defining a slice using a multiple pulse technique, and so this was written up, and before it was published, Peter Granell and I had independently come up with two other ways of defining a slice. And we told this to Alan Garroway - and we were sharing the machine, you see, and Allan wanted the machine - the NMR machine - to do his slice selection. And then, you know, I said, 'Well, we would like to have the machine for a few days to do our slice selection.' And when we got our results and he got his results we had a meeting of all three, and decided between us, maybe the sensible thing to do is to publish as a single paper, and which is what we did. And that paper, I think, was authored with - I believe Alan was the first author - Alan Garroway, Peter Granell and myself. So, that was the slice selection paper.

Well, you had to switch in any of the imaging techniques. You had to switch the read gradient so that you're defining three axes - trying to anyway. So, the first thing is to define a slice, so that's one axis, and there is also a gradient associated with that slice selection. And then having defined a slice of material, you then start to look at that decay that you've created – that free induction decay. You start to look at that in another gradient, and then a third gradient so you can define the image - thin slice - and then the two axes. And all the techniques work like that. They're not as fast as some of the techniques but, whichever way you look at it, whether its Fourier zeugmatography or whatever, they all have to define three slices.

It wasn't too long before we were able to get rather nice line-scan images, so we were taking a line at a time rather than a point at a time. And one of my students, actually my very first student in imaging, was a chap called Andrew Maudsley, and he had started with me, well, I'm not exactly sure whether he started on imaging. I think he had, actually, and during his second year or so, we had evolved to the point in our line-scanning technique that I one day suggested that one of us should actually try to image our finger because we had a very small coil and we could just about get a finger in.

I was very concerned in those days that line-scanning, which, you know, was faster than point-scanning; but line-scanning was still too slow. I mean, to get these finger images it took something like about ten or so minutes per image, and I felt that was a little bit …well, not a little bit … a lot lot too slow. I mean, Ienvisaged doing imaging much faster but I couldn't see how to do it in those very early days, but then I had this sort of brainstorming session which went on for weeks, I suppose, where I was thinking how, how we could improve the imaging, and the imaging time. And, of course, I knew that you could get a set of spin echoes – this is back to Erwin Hahn again – you could get a set of spin echoes if you put in pulses, a sequence of pulses, very quickly. You would get a whole set of spin echoes, and I mean that was common knowledge, of course, at the time, but the question is, 'Could I use that in any way to produce a very rapid image?' And the idea slowly occurred to me that maybe one could do something along those lines by applying … getting a set of spin echoes and at the same time applying a gradient - encoding gradient - along so that as the spin system evolved, the first echo would evolve in a certain time in this gradient; the second echo would evolve a little bit further in the same gradient; the third echo – so it was a spin echo sequence but with an additional gradient applied. 'And how could I bring this about?' And I wrestled with this for weeks, actually, and the breakthrough came, actually, on my way home from lunch one day. There used to be - well, there still is, actually - a route that goes around Beeston, and on that route round I got held up at the traffic lights, and that's where it suddenly occurred to me, while I was waiting for the lights to go red. Suddenly, it came in a flash. It really did happen that way, and suddenly it all came together, and that was it.

It involved a little bit of mathematics. I worked it out to my satisfaction and shortly afterwards, when I'd done this, there was a conference convened by Raymond Andrew, of all the interested parties in imaging. And we had people from Aberdeen – Richard Ernst came over - and all the Nottingham people were present at this meeting, and I gave a paper. I mean, we were all asked to give papers, you see, and I decided that I wouldn't talk about the stuff that was well known. I'd try this new idea on them. And I spent about half an hour going through it, but it was a bit mathematical, I have to admit, and it really left the audience dumbfounded. I mean, they either didn't understand it or did, but had no comment. I mean, normally at question times, people are saying, 'How about this, how about that?' But there was nothing, not even from Richard Ernst, and I was very surprised. I might as well have spoken to myself, really, but that was the technique which used spin echoes and looked at a whole plane of material in one go, so I called it Echo Planar Imaging (EPI) and that name has stuck. And of course in those days we hadn't actually produced an image – this was pure theory - but shortly after that, I put quite a lot of experimental effort in to try to produce an image by EPI, and that was it. It started a new and very, very rapid technique. It was very hard, I have to say, for people to grasp, you know.

How EPI works

So, at the time, the typical imaging time was round about ten minutes or more, and for point-scanning it was something like an hour to get an image, and here I was in this … in my presentation, talking about a technique which could produce an image in something like twenty milliseconds. And, of course, this left everyone dumbfounded. They really couldn't grasp that you could actually get an image that quickly and I think some people even dismissed it as being improbable nonsense or something.

Echo Planar Imaging or EPI technique involved, first of all, having a static magnetic field so that you could polarise the spin system. Secondly, there is a selective pulse which defines a slice of material. The slice selection process which defines the slice has to be applied in the presence of a gradient, so let's call that the Z gradient. So, the Z gradient is applied momentarily, a slice selection pulse, which defines the slice thickness, is fired off, and immediately after that pulse is applied, the Z gradient's turned off. You have then … basically, you've brought down a thin slice of material so that it is … rather than being aligned along the magnet axis, or Z axis, it's now in the XY plane. And if you can just freeze time momentarily, you then switch, once it's (inaud), you then switch on the X and the Y gradients. So, it's a very quick switch from Z axis to the XY plane, and then in the XY plane you apply a series of modulated pulses to produce a set of spin echoes and then, at the same time, a third axis which broadens the spin echoes as a function of time. So, it's all done sequentially except the last bit, where the X and the Y gradients are applied at the same time.

As far as the image is concerned, we sample the data points so that all these echoes that are formed are sampled one after the other, and then that data, as a complete string of data points, is like a bead necklace, and each bead is a point in the image space. And then a single Fourier transform of this whole bead - of this whole row of beads - is done, and it turns out that that's the quickest way to do things. But these days I think people don't, because computing has come on enormously now, they don't bother to do that. They actually do what they call two-dimensional Fourier transform, rather than one-dimensional, which is what I did.

The race to image the body, 1978

We got this grant and there was this huge – it wasn't a race exactly, but a huge pressure to push forward - and I'd got my way and ended up with a whole body magnet, and the question was, 'Could we now demonstrate the use of this magnet?' And there was an important conference coming up in the States in Blacksburg, Virginia, and so we … there was a deadline for this in 1978. So, we were really working almost around the clock trying to get the equipment working so that I could - or someone could - jump in the magnet and we could get an image. And it was decided at the last minute that it would be me that would get in the magnet but by then, of course, the magnet that we were using was an electromagnet – it wasn't a superconductive magnet – and it got rather hot when it was in operation. After maybe ten minutes or so, you couldn't touch the surface of the magnet, it was really about 55 to 60 degrees Centigrade. I stood between the coils and, of course, it got jolly hot, and I stood there for about fifty minutes while, in the end, I came out like a wet rag.

There were so many things that had to be done and checked and double-checked and so on, and so it ended up at about seven, seven-thirty in the evening, getting this image. And, of course, there was no time to get slides made so Peter Morris and Ian Pykett took photographs off the screen and I took the roll of film with me, and we went on the flight to the

States, found a local chemist that could do the developing – I think it was a chemist shop or something equivalent, pharmacy shop – and it was all done. And I got the slides back, mounted, a day before the conference started so it was all touch and go, you know. It wasn't … it wasn't relaxed at all.

The world's first Magnetic Resonance Imagine (MRI) movie, 1982

In 1982, at the Bowman Gray meeting, Roger (Ordidge) and I went and he presented his work, or some of his work, on the application of very fast EPI, and he showed what was the world's first movie image of a live rabbit and you could see its heart beating in real time. So that was Roger Ordidge. And I gave a general paper about the advantages of high-speed imaging and spoke in general about efficiencies and things like that. It wasn't terribly important. The important stuff was what Roger gave.

The advance was really, you know, one of these … part of this incremental process where he put together an EPI sequence and actually made it work. Running - not taking one frame but taking a series of frames. So you know, EPI takes a snapshot image in, say, 20 milliseconds or whatever. You can then repeat that and take another image, and this is what Roger did. He just simply put a whole series of EPI sequences together to form a movie, and the point of that whole process was to demonstrate to the people at the conference that EPI is not only fast, it's fast enough to do movie imaging, and he showed what was basically the world's first MRI movie.

Gradient Coil Screening stabilises the magnetic field, necessary for imaging, 1986

You see, in the early versions of the superconductive magnet, they originally started out with a metallic core in the centre - a tube. They changed that because of the problems of gradients interacting. They changed that to a fibreglass core but then, because of the way the superconductive magnets are made, in the superconductive region they have a copper screen, or several copper screens, which help to make the superconductive magnet work better. And it's those copper screens that would pick up the eddy currents - and without magnetic screening, those eddy currents would actually last for, in some cases, as long as a second. You could upset the magnetic field and it would take maybe one second or so to recover, and of course if you tried to do imaging in that one second, you couldn't, because everything was moving. But then I had this idea that came. I remember one of the postdocs that was working with me at the time, a chap called Barry Chapman. He was down in a room next to the lab and I remember racing down to him one day and telling him that I'd solved the problem, and he listened patiently while I said, 'What we should do is not create one magnetic field in the gradient coil but two magnetic fields - one to shield the gradient coil itself.' And if we could do it properly (and I'd done some calculations, very elementary ones, but I'd done some to substantiate what I was saying), that you could perhaps shield the inner gradient coil with this second coil, and the magnetic field outside the second coil from the gradient coil, would be zero, which meant you could place the whole thing inside a supercon, switch as fast as you wished, and there would be no interaction with the main magnet. That was the idea, and it needed, obviously, fleshing out in detailed calculations, which I had already started, but I continued with Barry Chapman. And we published the paper on magnetic screening.

We had a new fellow that had joined the department, and his name was Bob Turner, and he was very keen, when he heard what I was trying to do … he was very keen to join in and actually do some calculations on his own. But he tried one method which didn't work. It was a very complicated theoretical method which he tried and it didn't work, and so we went ahead with the filing of our first patent. But then Bob Turner spoke to another member of staff, who is a theoretician in the department, and the theoretician had done some similar work – not on gradient coil design – but it was a related problem and the mathematics was very similar to what he wanted. So the two of them collaborated and produced a very elegant piece of work, and it really did a much better job than I had done with my calculations but nevertheless, we said to them, 'Well, wouldn't it make sense if we included you on the patent, because then we've got everything, you know, all our ideas, my ideas, Barry Chapman's ideas, and then Roger Boly, the theoretician and Bob Turner.' So, we combined efforts, added some extra material to the patent.

The magnetic screening was a major step forward and it was actually picked up almost immediately by all the manufacturers who started to introduce magnetic screening. The only firm that we had problems with was GE (General Electric). They wouldn't pay royalties for the first year or so, but then when it was settled in the European and American courts, I had to go to the States, stand up and swear that we'd done this when we did, and so on, and it was accepted and that was that. So, they paid royalty and I think the university and the inventors did reasonably well out of it, but it *was* essential because EPI really wouldn't work in a superconductor if we didn't have that magnetic screening.

Resolution - the limits of functional imaging

The best sort of resolution that you can get is sort of sub-millimetre - maybe half a millimetre - and that is nowhere near enough to look at neural networks. The problem with functional imaging, as I see it anyway, is that one is looking, basically, at the arterial network in the brain and the blood supply that goes with it. So, that if there is a demand from a particular part of the brain - if you're deep in thought about a particular problem - then that would bring in a certain fraction of the brain, and that part of the brain then calls upon the blood supply to increase. And its that sort of thing that you can see in imaging of the brain - functional imaging does just that - and you can see that because there is a change in the oxygenation level of the blood, and that changes the magnetic properties, which changes the character of the image that you're watching. So, you *can* see these changes, but you have to remember all the time that what you're looking at are changes to the blood supply, not to the neural network, and you can only infer in a sort of secondary way, what's happening in the brain itself. So, I think there is a limit to what one can expect to get from functional imaging.

Resolution - higher magnetic fields, faster imaging

The extra that you get by going to higher magnetic fields in technical terms is the signal to noise ratio. So, if you're looking at an image at 3 Tesla – or let's start at our original field strength which was 0.1 Tesla - you can get an image but it takes a while to get, and when you do get it, if you want to get a reasonable signal to noise ratio in the image, you have to spend time getting the data. So, that's the first thing. So, as you raise the magnetic field two things happen. First of all, you get a better signal to noise ratio, which makes the image clearer, but also, you can spend some of the gains in decreasing the capture time of the image. So, instead of taking an hour to accumulate the data, you can get it in, say, ten minutes, and as you go to a higher field, you can get it in, say, one minute. Or, you know, as you go to higher fields, you can get a sensible signal to noise ratio maybe in ten seconds. So, it's a matter of playing one thing against the other. So, that's the first thing. And that, you know, is basically what is behind going to 7 Tesla. You can get, as they say, exquisite quality images and, I mean, here they're looking at the brain, they have a 7 system, which is dedicated to looking at brain imaging and that some of the problems in functional magnetic resonance imaging, and the images are obtained reasonable rapidly. I think they are using fast imaging techniques to get the data.

Safety - protecting patients

These magnetic field gradients, which we produce very rapidly, can actually induce currents in the body, and of course these currents can produce a number of irritating effects. The first one is that they produce tingling sensations in the shoulders, the arms, and so on, but they can also produce what we call muscle twitch, where you find that when the gradient switches, the body twitches like this, which is irritating for the patient if they don't understand what's going on. But worst of all, of course, if they are strong enough, you can produce interactions with the heart and induce either cardiac fibrillation or a full heart attack, and that would be fatal, obviously, for a patient in the magnet. So, it's a serious problem and when you want to do faster and faster imaging, as people want to do, then the problem gets worse, because it depends on the speed at which you're switching the gradients.

Well, there have been hundreds, thousands of scans where there's been no problem with patients with the current arrangements that exist, but I have to say, that the reason that there are no reported cases of people having cardiac fibrillation or any of the other problems that I mentioned, is because through safety reasons, all the current commercial scanners have been slowed down. They do not switch gradients very rapidly and, as a result, the images that they get, whether they're doing EPI or other standard imaging techniques, the fastest scans you can get there are taking something like eight or so seconds. Now that sounds fast, but of course, in terms of a heartbeat it's not fast, so what we're saying is if you want to get really fast images with no motional artefact, you really have to pull the stops out and go very rapidly. And once you do that, you immediately fall foul of the safety regulations but we can solve that problem - in fact, have solved that problem, by effectively, what we call, active e-field screening. So we can screen against the electric field which is introduced and it does require a special design of gradient, which we've designed and are publishing.

Acoustic Screening – noise control

Our coil, which is static, was wanting to move when you switched these currents on and off and so that was a major problem, but the second problem was that once the coils moved, they generated noise. You're talking about maybe a micron of movement, but it's enough on a whole gradient coil system to create enormous noise which can exceed 130 decibels. Now there is legislation at the moment which limits the noise in the workplace from normal machinery and so on to about 95 decibels, so anything above that actually is illegal, and so there is a major move at the moment to try to reduce the acoustic noise. Now, I started on the acoustic noise problem in the year, maybe a year before, I retired, and I retired in 1994. So, I've been working on it now, if you tote it up, probably fourteen years. And we came up with, well, in the early days in the late last century, we came up with an idea which would solve - or we thought would solve - the acoustic problem and it does reduce the sound level very considerably, but it doesn't solve the problem completely. But nevertheless, we're able to get in a small coil system that we made very recently - this is made by my son in law, Dr Hayward, and he made a small imaging coil and we produced a fast image in the 3 Tesla magnet with acoustic noise control, and so we were able to get the noise level down to about 40 decibels.

New science and the continuing struggle to get published

I'm still publishing but I have as much difficulty today getting stuff published as I did right at the beginning. Yes, I mean, I find, and I have found throughout my scientific career, that if you come up with an idea which is not completely accepted or is new on the scientific scene, there is a struggle to get it published, and that's been a theme throughout my scientific career. It wasn't easy then and it's no easier now.

Well, I don't think there are any fewer doubters around today than there were then, so you can have a Nobel Prize, you can have a knighthood, it makes no difference. When it comes to getting work published, it goes to 'independent' referees, and of course, these referees are people who are regarded by the editors of journals as experts, but when they're given something which is new, really new, they're no better at judging it than anyone else. And, quite often, worse.

I'm always thinking about today and tomorrow's problem. You know, what's done is done; what's gone is gone. That's …to be honest, I'm concerned about getting my next paper published.