
SIR PETER MANSFIELD

The Nobel Prize in Physiology or Medicine 2003

Autobiography



I was born on 9th October 1933 in Lambeth, London, the youngest of three brothers. I grew up in Camberwell, ten minutes walk from Camberwell Green, the epicentre of the Borough. My father, Sidney George Mansfield was the eldest son in a family of five sons and four daughters.

My father worked for the South Metropolitan Gas Company, as a gas fitter. My mother, Rose Lillian, was the youngest of three daughters. At the time of my birth she was not working, but looked after me and my two brothers, Conrad William, the eldest, and Sidney Albert.

Outbreak of World War II

In September 1939 when war broke out I was nearly 6 years old and my memory of this period is now hazy. However, I do recall very clearly the first time the air raid siren sounded on the day war was declared. I was playing in the street nearby and ran home asking what the strange wailing sound was. In the months that followed plans were instigated to evacuate all children from the London area.

I was evacuated from London on three occasions during the war years. The first was a relatively short period spent in Seven Oaks. Later I was sent to Torquay, Devon and stayed with the Rowland family. During a lull in the air raids I came back to London for approximately one year, then returned to Torquay shortly after the commencement of the V2 bombardment of London and was fortunate to be able to stay again with the Rowlands.

Secondary education

Back in London after the war I was hurriedly told by my school master that I should take the 11+ examination, something I had never heard of before. There was no preparation because time was too short. I took the exam and failed, but not completely. The mark I received was not quite high enough to get me into the local Grammar school, but it was sufficient for me to go to a Central School in Peckham. However, this situation lasted for approximately one year. Then all Secondary schools, in London at least, were completely reorganized so that Central schools were dispensed with altogether and pupils from Secondary schools and Central schools were merged into new schooling arrangements which were called Secondary Modern schools, (these were later renamed Comprehensive schools, but after I had left).

Rocket science

I left school at age 15 and started work as a printer's assistant. This took me almost to the age of 18. Because I had developed an interest in rocketry I applied for and obtained a job in the then Ministry of Supply at the Rocket Propulsion Department in Westcott, Buckinghamshire. I remained in this position for around 18 months and was called up for National Service to serve in the army for two years, returning to Westcott after National Service in 1954. I studied for Advanced Levels part time for approximately two years and gained University entrance.

University education

In 1956 I entered Queen Mary College, University of London. My subject was Physics and in my third undergraduate year we were given individual projects to pursue instead of the normal practical laboratories which we had taken during the first two years of the course. My individual topic was set by Dr Jack G Powles. It was to build a portable NMR spectrometer to measure the earth's magnetic field. This had been done previously using valve technology, but my task was to build a transistorized version. I knew very little about transistors at this time since it had not formed any part of the electronics course that we had undergone in physics. My experience was very much in valve technology. Nevertheless, I enjoyed the project very much and of course learned a great deal about transistors. Towards the end of my undergraduate studies I was approached by Jack Powles, who offered me a position in his research group working on NMR.

At that time Jack Powles was a Reader in Physics and I felt greatly honoured to be asked to join his research group. Jack Powles' main interest then was in studying molecular motion in a range of materials, mainly liquids. My task was to build a pulsed NMR spectrometer to study solid polymer systems. It was during this period that I discovered what we later called 'solid echoes'. This was towards the end of my three year doctoral studies and we were only able, therefore, to produce a short paper on solid echoes observed in a single crystal of Gypsum.

The American adventure

Towards the end of my studies I was asked whether I would be interested in going to the United States for a short post-doctoral period. Jack offered to arrange a position working with Professor Charlie Slichter in the University of Illinois at Urbana, Illinois.

I married my wife, Jean Margaret Kibble, on 1st September 1962 and we left to join the University of Illinois in October 1962. Charlie and his first wife Nini met us at the airport at Urbana-Champaign and installed us in what turned out to be temporary post-graduate accommodation at Orchard Downs. Orchard Downs was about two miles from the campus centre and was therefore within walking distance, so I thought. Of course I had not taken in to account the weather. In late October early November the typical air temperature in early morning could reach minus 20 degrees Centigrade even though the skies above were clear blue. I remember attempting to walk in one morning and I got roughly half way to the campus when the frost began to effect me. I continued to walk briskly and by the time I got to the Physics Department my limbs were beginning to seize up with the cold. My eyelids crunched when I blinked and I was extremely thankful that I managed to get to the Department before anything serious happened.

The research work that I was involved in during my stay at Urbana was the NMR study of doped metals. A scientific paper by Kohn and Vosko had predicted that conduction electron scattering around zinc centres would behave in a periodic manner so that the resonance shifts of copper atoms would vary in shells around the zinc scattering centres and these shifts would be measurable by looking for low amplitude side bands in the copper resonances of the two copper isotopes. My task was first to build a double resonance spectrometer capable of

looking at the copper resonances in a pulsed mode and secondly, to produce single crystals of doped copper as suitable samples to be studied. Building the apparatus was relatively straight forward since I had already built a pulsed spectrometer back in London, but the production of a single crystal of copper with sufficient surface area to give a measurable free induction decay signal was a novel experience for me. It involved growing a single crystal and slicing it into thin plates to give sufficient surface area. The slicing process was done with an electro-spark cutting process which left the surfaces severely disturbed so that it was necessary to etch the damaged surfaces away to reveal a clean undamaged surface.

I worked as a post-doc for approximately two years and made several samples and did many experiments on these samples but could not see the predicted effect. But the experience that I gained on this project and the knowledge and background that I learnt during my stay in Illinois were invaluable.

In Charlie Slichter's laboratory, I was not able to pursue the solid echo work started in London, but a colleague from Jack Powles' laboratory, Doug Cutler, who came over at roughly the same time as we did was working with Professor Ted Rowland in the Metallurgy Department at Urbana. The apparatus that Doug Cutler was using was very similar to the equipment that I had left behind in London. Towards the end of my stay in Urbana I persuaded Doug Cutler to perform a couple of experiments for me on pure aluminium powder. He was able to see solid echo effects and I produced a short mainly theoretical paper extending the theory of solid echoes to systems with spin greater than a half. In the same paper I also included solid echo effects on the two spin species system, NaF. My colleague, Dr John Strange (now Professor), while working as a post-doc at Cornell University, also allowed me to include some of his preliminary experimental data.

While I was busy in the Physics Department my wife took a post working in the University Health Centre as a Secretary. She quickly made friends with a number of other secretaries and introduced me to people with whom I would not otherwise have come in contact. We saw her friends on several occasions and had an enjoyable interaction with them. Of course we also interacted with the Slichters and the other members of the group.

The wanderers return

After the two year period we were sad to leave Urbana and the various friends that we had made there during our stay. But we were also pleased to return to London to see our respective parents after such a long period away. I had been approached by Professor Raymond Andrew while we were still in the States. He had recently moved from Bangor to Nottingham and in a letter to me in July or August of 1964 he wrote to offer me a Lectureship at the University of Nottingham. I had had other offers including one from Jack Powles who had recently taken the founding Professorship in Physics at the University of Kent at Canterbury, however I decided that I liked the offer from Nottingham and accepted. After several weeks in London my wife and I made our way to Nottingham to meet Professor Andrew and to take up his offer. I was still quite interested in pursuing my earlier ideas developed during my PhD studies in London and so I was given a room where I was able to set up equipment to pursue my studies in multiple-pulse NMR.

Life in Nottingham

I was extremely fortunate to be offered a Canadian research student shortly after my arrival at Nottingham. His name was Don Ware and he came from UBC in Canada. He had an MSc in NMR from the Chemistry Department at UBC. His MSc supervisor was Basil Dunell. Don had little experience in pulsed NMR but was keen to learn so I proposed that he build a pulsed NMR spectrometer capable of performing multiple-pulse experiments. This was in 1964. A year or so later Jack Powles came to Nottingham to give a colloquium talk in the Physics

Department. Afterwards he was shown around the various research groups. By that time Don and I had managed to perform our first multiple-pulse experiments and we were pleased to demonstrate these results to Jack. During the course of his visit he said he thought he had seen something similar from a pre-print that he had been sent by John Waugh from MIT. But he wasn't sure whether it was the same or not, and that he would contact me when he got back to Canterbury. A week or so later a copy of John Waugh's pre-print to Jack arrived on my desk. By then I had already drafted a letter of our own which was duly sent off for publication to Physics Letters. Our paper was published shortly after John Waugh's paper and its appearance triggered a period of inter-group squabbling that continued on up to the early 70's.

In 1972 Dr Alan Garroway, an American post-doc, joined my group to study multi-pulse techniques. He had carried out his PhD studies at Cornell University. His work had been concerned with using NMR to study fluid flow. He and his first wife, Mary (now deceased), arrived in February 1972, and on the day they arrived we invited them to dinner, but the country was in the grip of a series of electricity power cuts so that Alan and Mary's first experience of Britain was a candle lit dinner. Earlier in 1971 I had received an SRC research grant to pursue multi-pulse NMR and included in the equipment support was money for a mini-computer. I had, therefore, purchased a Honeywell computer with 4 k bytes of magnetic memory and had spent quite a bit of the summer of 1971 adapting the machine so that we could connect it to the experimental apparatus in a computer controlled mode. One of the first things that Alan was engaged in was the implementation of the Cooley-Tukey fast Fourier transform algorithm so that transient signals could be rapidly captured and transformed into a spectrum. This work was very successful and later in 1972 we were regularly using the computer controlled spectrometer to study the response of a number of suitable compounds including Calcium Fluoride. Computerization of the NMR spectrometer meant that we were able to pop specimens in for extremely rapid analysis and as a result we quickly ran out of suitable compounds to study with interesting chemical shift anisotropies. It was at this point in early summer of 1972 that I started to think seriously about applications of the line narrowing process.

The birth of MRI

In those days coffee and tea breaks were taken in the tea room of the Physics Department. It was an opportunity for members of staff and students, sometimes in different research groups, to interact and exchange ideas. On this occasion all other people had left the tea room except for me, Alan Garroway and my student Peter Grannell. We had exhausted all the readily available materials for studying chemical shift anisotropy. Among the results that we had obtained was an extreme elongation of the free induction decay of Fluorine in a single crystal of Calcium Fluoride. It suddenly occurred to me that by removing the dipole-dipole interaction in Calcium Fluoride it ought to be possible, at least theoretically, to look at the atomic structure of Fluorine by imposing an external magnetic field gradient. Alan, who had worked with gradients during his PhD studies, was skeptical, but Peter Grannell was more amenable to the idea. I rushed away from this meeting and wrote up several pages of calculations showing the theoretical possibilities and handed them to Peter Grannell. Peter was in the last year of his PhD studies which finished formally on the 1st October 1972. We had already discussed the possibility of him staying on as a post-doc and I suggested that one of the topics he should work on was NMR diffraction studies along the lines of the calculations I had already produced.

Alt Heidelberg du Feine

I had also made arrangements for sabbatical leave to work in Professor Karl Hausser's group in Heidelberg starting in October 1972. My family and I left for Heidelberg in late September 1972 with the plan that Peter

and I would continue to interact via post while he prepared to do some experiments on NMR diffraction. I had already decided that there were extremely difficult problems to overcome using real single crystals of Calcium Fluoride and that a simulated approach was called for initially. In this approach a model lattice was to be constructed with several plates of camphor separated by thin sheets of plastic. Although a solid, camphor contains a large number of highly mobile protons which rotate to give a relatively narrow absorption line. The sheets separating the camphor plates contained far fewer mobile protons which gave virtually no signal component to the free induction decay. Our first experimental results were obtained as early as November 1972, and they showed the diffraction effects when the magnetic field gradient was switched on. When the gradient was turned off a single absorption line shape was observed with no splitting. After repeating these results many times during the early part of 1973, the results were written up for presentation at the First Specialized Colloque Ampère in Krakow, Poland in September 1973. At the same time a more formal publication was submitted to the Journal of Physics, C which appeared in November 1973.

Journey to Krakow

My wife and I made the long journey to Krakow by coach stopping overnight in Leibzig. My talk at the Conference was scheduled as the first invited lecture entitled "Multi-pulse Line Narrowing Experiments: NMR Diffraction in Solids?". My recollection is that the paper was well received and created some discussion afterwards. One person in the resulting discussion asked if I was aware of similar work that had been carried out by Professor Paul Lauterbur. In fact I was completely unaware and asked where I might find this work. I also asked whether it was concerned with imaging in solids and was told that it was not, that it was concerned with imaging of water in test tubes. The person raising the matter was none other than my old sparring partner, Professor John Waugh. When I returned to England I looked up the relevant publication in the library and decided that the approaches though completely different were not entirely unrelated. The paper by Paul Lauterbur had been published several months earlier in the year. But there were several concerns that I had concerning Paul's approach to imaging liquids and indeed our own approach to imaging in solids. The first which applied to both techniques, was the question of defining an active slice of material. In the case of Paul's work he had used the method of projection reconstruction to obtain an image and in that technique a large number of experiments needed to be conducted in order to extricate the image with reasonable resolution. This took valuable time. In my own approach my first concern was the complication of producing images from multi-pulse experiments. There was also the question of defining a slice. We very soon decided that the pursuit of imaging in solids was perhaps ahead of its time and could be deferred for future work. It would be so much easier to look at biological specimens where the relaxation times were shorter and where the line widths were generally speaking narrower.

Passage to India

Early in 1974 a number of colleagues from the Physics Department travelled to India for an International Conference on NMR. Among these were Dr Bill Moore and Raymond Andrew's post-doc, Dr Waldo Hinshaw. At this conference Paul Lauterbur gave a talk on his work on imaging which created some excitement and provoked quite a bit of discussion among the Nottingham group during their return flight to England. Shortly afterwards they conceived a different approach to imaging which was called the "sensitive point" method of imaging. By applying time dependent magnetic field gradients along two orthogonal axes they were able to define a point volume of the specimen which could be swept thereby interrogating the whole plane, point by point, to produce an image. Inevitably this was an extremely slow process.

The slice of life

I was still very much concerned with imaging speed and also the question of sliced definition. After a lot of thought and discussion with Peter Grannell we came up with a method of slice selection which looked as though it might work reasonably well. Alan Garroway also came up with a different method of slice selection using a string of short pulses to define the slice and between us we thought that the sensible approach would be to combine our efforts and publish a short note on the general technique of slice selection. This was sent to the Journal of Physics and was published in the form of a letter. The question of imaging times was still concerning me and during the course of 1974 I spent a great deal of time turning over my thoughts on how this may be achieved. One way forward was what I called line scan imaging. In this method a line of magnetization in a specimen was selectively excited and read out. This process was repeated until the object had been scanned. The technique was much faster than the sensitive point scan method of Hinshaw and also turned out to be faster than the projection reconstruction method of Paul Lauterbur, but I was still not satisfied. Nevertheless, line scanning was used to produce a number of images and in particular it was used to scan the finger of one of my early research students, Dr Andrew Maudsley. The scan times for these finger images were 15-23 minutes. These were the first images of a live human subject and were presented at a special meeting of the Medical Research Council which was convened in 1976 to review the work of the several imaging groups that had sprung up both at Nottingham and also in Aberdeen. All groups were vying for MRC support and this meeting was called specially to review the topic and to decide how best to support the work. The images demonstrating live human anatomy were annotated by Professor Rex Coupland, then head of the Department of Human Morphology. They produced a startling response at this meeting and convinced the MRC that our work should be supported. We produced a grant application requesting a substantial sum of money to produce a whole body MRI machine.

The application was handed to Professor Andrew for his comments before sending it off to MRC. In fact the application was delayed by a month or so with no comment or explanation. However I learnt subsequently that Raymond Andrew himself had already sent in a research grant application to MRC and he had decided that he should wait until he had received a decision from MRC before allowing my application to go forward. His application, which was granted, was concerned with building an intermediate size imaging machine with a sample access diameter of about 10 cm. The intention was to establish an intermediate step between the small scale approaches that we had already demonstrated and the whole body machine which I was keen to press on with. My grant application was sent in subsequently and considered at the following round of grants. The application was successful and the result was announced in 1977.

The delay in the submission of my MRC grant created some acrimony which continued in one form or another for several years until Raymond took early retirement and left for the University of Florida in 1984. During this period strife within the Andrew group occurred when Bill Moore decided that he wanted to split his imaging activities away from Raymond's group. This occurred in the late 70's when a whole body magnet was obtained with a grant from the Wolfson Foundation and Bill Moore together with Neil Holland decided that the results of this new work should be independently managed. Thus a period of internecine squabbling broke out between Raymond Andrew's remaining group, which included Dr Paul Bottomley and a member of staff, Dr Peter Allen, who was loosely associated, and the third group headed by Bill Moore. After a while, under this new regime, matters began to settle down again in a quasi steady state.

Blacksburg

Dr Peter Morris, who in 1977 had just finished his PhD studies on multi-pulse NMR in solids and liquid crystals, stayed on at Nottingham as a post-doc for two years or so. This occurred at approximately the time when the MRC funding was granted. He together with Dr Ian Pykett were heavily involved in the installation of our first 0.1 T electro-magnet and subsequent RF and gradient coil designs. We had all been working flat out to get a large scale image in time for the ENC Meeting in Blacksburg, Virginia in April 1978. In the event, the night before we were due to fly out, I volunteered to climb into the machine for an attempt at imaging my abdomen. Just a day or so before, I had received a typewritten draft of a piece from Professor Tom Budinger of the University of California at San Francisco suggesting that the gradient levels and switching rate that we were proposing to use to produce my abdominal image, were potentially dangerous. I personally did not believe the calculations and had performed alternative calculations which suggested that the expected induced transient currents flowing in response to the gradient switching were much lower than those predicted by Tom. I climbed into the machine and signaled to Peter and Ian to push the button for a single pulse. There was an audible crack but I felt nothing. I then signaled to start the scan. The magnet was enclosed in aluminium sheeting forming an RF screen. Due to lack of time there was no light inside. I was therefore clamped in the magnet vertically and in pitch darkness for 50 minutes until the procedure was completed. Our wives and fiancées were present ready to haul me out of the magnet in an emergency, but the whole experiment went well and images were recorded. Photographs of the raw images were taken, but the film was processed in a local store in the USA a day or so before the presentation.

Trouble ahead

During the early 80's there were growing difficulties within the University of Nottingham concerned mainly with the promotional prospects in the various departments on campus. This meant that aspiring members of staff were being artificially held back because of arbitrary constraints introduced by the government of the day and forced upon the Vice-Chancellor, Professor Basil Weedon. These constraints induced a feeling of great uncertainty throughout the University which was heightened when calls were made to introduce an early retirement scheme in order to reduce the over staffing levels that were considered extant in many Departments. These considerations produced a rash of requests for early retirement which in some cases carried a substantial early retirement premium especially for younger members of staff. In the exodus that followed the Department of Physics lost four members of staff to early retirement. These were Professor Andrew, Dr Moore, Dr Peter Allen and Dr Bill Derbyshire. Dr Moore went to the United States to take up a Professorship at the Brigham and Women's Hospital in Boston, Massachusetts. Dr Allen went to the University of British Columbia in Canada to set up a medical imaging group there and Dr Derbyshire took a Senior Lectureship at Sunderland Polytechnic. Sadly Dr Moore collapsed and died while playing tennis approximately six months after taking up his new position. Raymond Andrew died of cancer in 2001 at the age of 78. Dr Peter Allen successfully created his own MRI group and continues to research his particular interests in medical imaging. Dr Derbyshire later took a position working with Rank-Hovis and is now retired.

With all these staff changes occurring over a 1-2 year period, my group was the only one in MRI to survive in Physics at Nottingham. In fact in 1984, I myself was courted to return to the States with no fewer than three offers, from the University of Alabama, University of Illinois at Urbana and the University of Maddison, Wisconsin. I decided to stay at Nottingham despite the attractive offers.

A new start

Shortly after my return to Nottingham, I received a call from the Department of Health telling me that there was some funding available to purchase a 0.5 T superconductive magnet - was I interested? I accepted immediately and later in 1984/5 the magnet arrived and was installed in a small extension of the Physics Department. These events helped to settle me firmly in Nottingham and removed all doubts I may have had about moving elsewhere.

I had been greatly concerned about our proposals to use the 0.5 T magnet for EPI. My major worry was whether rapid magnetic gradient switching within the close confines of the magnet would induce the static magnetic field to quench. I had been thinking seriously about the problem for several months prior to the arrival of the magnet, but with no clear solution in mind when it arrived. Shortly after installation of the magnet, I recall rushing down to the laboratory in great excitement to announce to my post-doc, Dr Barry Chapman, that I believed I had solved the magnetic problem by introducing a magnetic screen between the gradient coil and the inner bore of the static magnet. The idea was that the screen, itself, would be another coil designed to make the total magnetic flux from the screen and the primary gradient coil zero beyond the screen, thereby removing any interaction between the gradient coil and the magnet.

We hurriedly put together some calculations and filed the idea as a Patent. Meanwhile, Dr Robert Turner, who had joined my research group to learn about MRI in general and EPI in particular, had been thinking about the problem from a different view point, using a so-called annealing algorithm, but without success. I mentioned my approach to him and within three months or so, he together with Dr Roger Bowley, a theoretician in Physics, had come up with a much more rigorous mathematical analysis of my idea. This material was added to the Patent within the one year allowed before final filing. So active magnetic screening was born and quickly applied to the development of EPI at Nottingham. Once published active magnetic screening was rapidly taken up by the MRI industry and forms the basis of all commercial MRI machines today.

The golden era

The 10 year period from 1980-1990 was exceptionally fruitful in terms of research output, development and medical applications of high speed imaging. Considerable effort was put into the implementation of EPI, initially by two research students, Roger Ordidge and Richard Rzedzian (now deceased), then later by several other students including David Guilfoyle and Mark Doyle. While this was going on, other work continued in non-medical applications of MRI principally by Stephen Blackband and Richard Bowtell. Other types of whole body imaging were tried out by Volker Bangert, a German student from Berlin who had a grant from the Deutsche Akademische Austauschdienst. The major effort made in EPI produced increasingly better images as a result of further work carried out by research students Martin Cawley and Alistaire Howseman. Whole body EPI work with a series of patients became possible with the help of Dr Michael Stehling, a medical graduate from Germany who came to Nottingham, initially as a visitor with a grant from the Deutsche Forschungsgemeinschaft, but later decided to stay on to study for a PhD in MRI.

By the end of the 80's the quality of EPI data had improved dramatically so that most regions of the human body could be studied rapidly, bringing relief to many patients. It was at about this time that Dr Penny Gowland joined the group and made substantial contributions to our work on foetal imaging.

This was the end of a Golden Era in the development of MRI at Nottingham. There were, of course, further developments well into the 1990's and beyond including the trial experiments using EVI at 0.5 T, carried out by Paul Harvey, and at 3.0 T, carried out by Paul Glover, Ron Coxon and Jonathan Hyking.

Following my retirement in 1994 from teaching in the University, I continued my research activities both at the University and with the help of my small company, General Magnetic. Our main interest has been concerned with MRI safety matters. In particular we have tried to reduce the acoustic noise levels generated by the gradient coils when pulsed in EPI. This work, started in the University by Joanna Beaumont and Ron Coxon, has subsequently been further developed by Dr Barry Chapman, and my son in law, Brett Haywood.

Another safety issue also related to gradient coils is the unwelcome level of electric field that naturally accompanies high levels of changing magnetic field. Electric fields are responsible for neural stimulation induced in patients when being imaged. Currents flowing within the patient may induce peripheral muscular twitch for relatively mild stimulation levels. However, for larger and faster gradients applied to the thorax, induced cardiac fibrillation is a real and serious danger. This aspect of our work is a joint venture between the University of Nottingham and General Magnetic and involves Professor Roger Bowley and Brett Haywood.

None of the work in MRI could have been achieved without the enthusiasm and dedicated support of a highly motivated team of technical and academic staff, research students and post-docs sustained over the period from 1972 - to the present day. I wish also to thank my three secretaries, Mary Newsum-Smith, Lesley Key and Pamela Davies for their unstinting help and the patience shown to me over the course of a long and exciting career. Last, but no means least, I would like to thank my wife, Jean, and my family for their unflinching and unswerving support during the good as well as the not so good times.

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