Examples of writing by Ray Freeman

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As Luck Would Have It

Lady luck, blind chance, random variables, chaos theory; how does a single, apparently insignificant event change the entire direction of one's life? At my advanced age it seems natural to look back and examine the critical turning points, those abrupt changes of direction, that have defined the overall course of events. More often than not, random chance has played a key part. Perhaps there should have been a carefully designed career plan, but I doubt that it would have worked any better.

Human fallibility has been an important factor. At my elementary school (up to age eleven) I was wrongly assigned to a higher year than my actual age until the administration error was discovered and rectified. For this reason I took the examination for the local grammar school for two years in succession. On the first occasion the interview was a total disaster. The second time around was more successful and I was awarded a Thomas White Scholarship at the Nottingham High School, the top school in the region, which boasted some excellent teachers. Although I studied Latin and Greek, I always wanted to be a scientist, but I made the mistake, common among schoolboys of that age, of preferring chemistry to physics. Crunch time came when the Headmaster (ex Cantab) worked through the assembled fifth-form schoolboys and assigned them to classics (favoured) or science (discouraged) in preparation for the forthcoming examinations for the School Certificate (later called 'O' levels). Only five of us had managed to survive the Greek set, so without hesitation the Head consigned us all to a life on the Arts side. Only the timely intercession of the physics master, a Dr Somekh M.Sc. (a kind gentleman of a middle-eastern persuasion) allowed me to continue as a potential scientist. Incidentally he rather scared me by imparting the information that university scientists had to perform some independent and original research before they could obtain a Master's degree. I had fondly imagined it was a more straightforward study "by the book" and I was not sure I could be sufficiently original.

The next turning point was the Oxford Entrance Examination. Apparently some schools take their students aside at this stage and thoroughly coach them about the Oxford examination. Not the Nottingham High School. Actually the Head disliked me and I was probably not rated worthy of such special treatment. The written examinations took place in the Hall of Keble College. It was distracting to have to write for the first time on completely plain paper, whereas we were accustomed to lined stationery. (Interestingly, by complete chance I later discovered my original examination papers, torn into quarters and used in the Dyson Perrins Organic Chemistry laboratory for weighing chemicals.) It was the practical examination that caused the trouble. We were required to use a titration method to measure the distribution of some chemical (I forget which) between water and carbon tetrachloride. I knew that this required shaking the mixture to achieve true equilibrium, but all attempts failed because the two components always formed an emulsion, so no meaningful titration could be carried out. Increasingly frustrated, I restarted the experiment several times without any success. At this point a nice gentleman sidled up to me and asked if I ever did the dishwashing at home. I assumed that this was a trick Oxford question, and moreover this was a very bad time to be posing it, so I allowed my resentment to show, and he quietly went away. Much later I realized that he had intended to explain that the laboratory technicians had mistakenly used a powerful detergent to clean the glassware, so that the required "separation" experiment was quite impossible to perform. If only someone had warned me that the practical examinations were always considered of negligible importance, and were used merely as an informal method for interviewing the candidates. The nice gentleman (Richard Barrow, later a colleague and good friend) was in fact the chemistry tutor of my "first choice" college. That is how I slipped down to the second choice (Lincoln College) where the Chemistry tutor was Rex Richards, who was starting out into an entirely new and unproven research field - nuclear magnetic resonance. It was a surfeit of Teepol that determined the course of my entire scientific career.

It was a time of obligatory National Service. The smart money said "Go to university, delay military service as long as possible, it may eventually be abolished", but Lincoln College gave me no choice, offering a scholarship to start in October 1951 (it was December 1949). So on my eighteenth birthday I "volunteered" for the R.A.F. intending to complete the required service in good time for admission to Oxford. The army of North Korea had other ideas. After a few months, war had broken out in Korea and our military service stint was increased from eighteen months to two years. Eventually the Government realized that most potential university students would miss their slots, and at the very last minute arrangements were made for early release. In the R.A.F, I followed a course at Yatesbury, Wiltshire ("sausage country") on basic physics and airborne radar, and on completion I was made an instructor on the same course, along with colleagues who were radio amateurs almost to a man. So I learned some radiofrequency stuff that was to serve me well in research later at Oxford, and I also benefitted from an excellent R.A.F. course on teaching methods, the only formal instruction I ever had on preparing and presenting a lecture. There was a quite surreal interlude when the Air Officer Commanding was scheduled to inspect R.A.F. Yatesbury; panic stations, all leave cancelled so that we could paint anything that did not move, and clean up the parade ground with the proverbial toothbrushes. An enterprising few managed to discover an obscure proposal for an "Extramural Studies Week" at Oxford that coincided neatly with the dreaded inspection. In this way I spent a happy period living in Lady Margaret Hall and touring the Clarendon and Chemistry laboratories with the chosen few (including Tony Horsefield, later a colleague at the National Physical laboratory).

I found myself in my fourth year ("Part II") at Oxford pursuing a research project on NMR, and like many aspiring chemists, enjoying the respite from bookwork. It was fun in the basement of the Physical Chemistry Laboratory trying to learn how to do research. Some wag had attached a label to our two-ton electromagnet that read "Magnet, Not to be Removed from Room 16", and we then embarked on a never-ending campaign to find suitably sarcastic newspaper clippings or cinema posters such as "Le Monstre Magnétique - Fed an Outsize Dose of Electrical Power, It Threatens to Destroy the Earth". But I digress. During that term I was President of the Oxford University Jazz Club, responsible (among other things) for ensuring that nothing untoward took place at our meetings that could possibly offend the University proctors, who had once banned our club for an entire term for some minor infraction of proctorial rules (calling a "Social Evening" a "Jazz Band Ball"). That is how I came to attend a fancy-dress ball at the Oxford Architecture School where our jazz band was playing. The theme was "Revolution", symbolized by a fake guillotine; everyone had to pass under this gruesome device to enter the ballroom. Maybe I should have paid more attention to this obvious omen. It was there that I met a charming young French girl; blind chance had decreed another serious turning point in my chequered career. I invited Anne-Marie to a party at our flat in Park Town (a party that in fact had yet to be arranged, but my flat-mate Simon was enthusiastic and invited a bevy of Italian girls to make up the numbers). Life has not been quite the same since.

During my D. Phil. research stint at Oxford I spent most of the time building new radiofrequency equipment to study NMR of nuclei such as cobalt. One Saturday afternoon Rex Richards stopped by the laboratory to chat (I think he was surprised to find anyone there working). Knowing that Anne-Marie had returned home to Paris, Rex casually suggested that he might contact his good friend Anatole Abragam at the Centre d'Etudes Nucléaires de Saclay, who might then invite me over to Paris for an interview - just an idea, no obligation on either side. I leapt at the possibility of working in France, and it was soon set up that I could do post-doctoral research there. Except that by chance a Canadian scientist, Gordon R. Freeman, a gas-phase kineticist from the same Physical Chemistry Laboratory had also applied to work at Saclay at the same time, and the French administration could not believe that there were two quite separate applicants from the same laboratory at Oxford called Freeman. It took several months to resolve this accidental degeneracy. At Saclay I was fortunate to be able to work with a famous visiting American physicist, Robert Pound, who had narrowly missed a Nobel Prize for the discovery of NMR in 1952. We wrote two papers based on Bob's idea for a "super-regenerative" oscillator whose frequency tracked the magnetic field, based on a related device that he had built to detect aircraft by radar. My prototype high resolution NMR spectrometer was probably one of the most stable at that time, but the Saclay physicists had no time for chemical application of NMR and quickly dismantled my equipment when I left.

Anne-Marie and I were married in April 1958. Just before the wedding I had been assured that the required official permission for a foreigner (le perfide Albion) to marry a French person was merely a formality and that I had just to pick up this document at the Préfecture de Police a week in advance. It reminded me of a scene in that classic French film "La Ronde" because this turning point almost went the wrong way. The essential paper work was "not ready and probably wouldn"t be ready for weeks, we often have young fiancées in tears in this office". Brought up in the relatively cloistered environment of England, I never imagined that continental bureaucracy required that a few palms needed to be greased. We had to enlist all the help we could possibly muster: the British Consul, Professor Abragam, and a family friend with contacts in the Préfecture to get things back on course. No one knows which of the three approaches was successful, but eventually an official motorcycle courier carried the paperwork across Paris and it was formally handed to me by a very high Préfecture official in an office the size of a football field. Chaos theory had very nearly got things wrong this time.

After two happy and productive years at Saclay I joined the National Physical Laboratory (NPL) in Teddington, Middlesex, and we moved with baby Dominique to live in New Malden, Surrey. In those days many British scientists dreamed of going to the USA where science was very well supported in the aftermath of the Russian Sputnik launch. It was common practice for science students to book a provisional passage on a transatlantic ocean liner "just in case". In my third year at NPL, that opportunity finally came (in the person of Professor Britton Chance, the head of a well-known biochemistry institute in Philadelphia). In retrospect this must have been a result of a quiet word from Robert Pound, who had worked with Britton Chance in the famous M.I.T. Radiation Laboratory during the war. I was thus invited to Philadelphia. When I went to see my NPL boss, John Pople, with a tentative proposal to take a sabbatical in the USA, he surprised me by agreeing on the spot, but wisely suggested that I first canvass other possibilities in the USA. A virtual roll of the dice determined the choice. I contacted John Baldeshweiler at Harvard, Paul Lauterbur at the Mellon Institute in Pittsburgh, Britton Chance (naturally) and Wes Anderson at Varian Associates in California. Wes Anderson was the first to come up with financial support, only hours ahead of John Baldeschweiler, and I happily accepted. Varian had a tremendous world reputation at that time, Wes was doing some exciting double resonance experiments related to my own interests, and the California weather was a big positive factor. So in November 1961 our family of four flew on a (seriously delayed) Boeing 707 to San Francisco, arriving at 4.00 in the morning to find Wes waiting to greet us. This was typical of the warm welcome we experienced as newcomers to California; a few days later Martin Packard gave us a car on permanent loan; we later bought it for $100.

The California years were very happy ones. Louise, Jean-Marc and Lawrence were all born there. But little by little, Varian seemed to be losing its early pioneering spirit. Top scientists were leaving (Larry Piette, Jim Hyde, Richard Ernst, Warren Proctor, Harry Weaver), or being squeezed out by young managers who seemed to feel uncomfortable with more gifted underlings. In late 1972, completely out of the blue I received a letter from a former colleague, David Whiffen, noting an opening for a physical chemist at Oxford. I remember that the salary was so poor that I put the letter straight into the waste bin before later fishing it out for further examination. The University appointment came with a Fellowship at Magdalen College - a very attractive combination. Just for the heck of it I applied by Telex, and was surprised to be invited for interview. In Oxford it began to dawn on me that if I were to be offered the post, then I was pretty well committed. I later learned that a "hot shot" scientist also on the short list of four was not favoured by the Magdalen Fellows because they felt he would only use this as a stepping stone to higher things. So I was elected, in a sense, by default, although I had some friends in court (Keith MacLauchlan and Peter Atkins) who must have helped enormously. I learned of my appointment in the middle of the night after my return to California, during a quite surreal telephone call from Leslie Sutton (the retiring Magdalen tutor in physical chemistry). He was recounting a list of the dimensions of the rooms in a house in Headington that he felt would be just right for our family. The idea was that I should immediately put in a bid, sight unseen, but of course we were not ready to commit ourselves at that juncture. So, an almost casual letter from David Whiffen had set our entire family on a completely different course - academia. It turned out to be a brilliant move.

Chance again took a hand with our daughter Dominique's career. As a student at Queen Elizabeth College, London, she happened to attend a research presentation by Rex Richards. Rex has always been a charismatic lecturer, and his talks are a fine example of clarity and logical organization. Dominique was so impressed with his story of how NMR was revolutionizing biochemistry that she decided, on the spot, to follow that line of research, and did so with success, working with George Radda and Brian Ross in the Oxford Biochemistry Department to earn a D. Phil., and later being awarded a Boswell Fellowship at CalTech in Pasadena. In this manner she became a well-known NMR spectroscopist in her own right, with no help from her father. This was vividly brought home to me at a conference in Austria when someone came up to me with a question about a paper I had published on zero-quantum NMR. For the life of me I couldn't remember the details (it often happens) until it dawned on me that this was in fact one of Dominique's magnetic resonance spectroscopy experiments. I was able to introduce the bewildered scientist to Dominique who was at the same conference. More recently Dominique has risen to be the President of Pelikan Technologies, a spinoff company in California that designs and builds pain-free glucose monitors.

My stay at Magdalen was very happy and lasted 14 years. I really enjoyed being a College tutor. The great advantage of Oxford Chemistry is the Part II system, which allows students to devote an entire year to a research project, culminating in a short thesis. They can thus decide whether or not a research career is to their taste. This Part II year acts as a pool for the D. Phil. program for the most gifted students. I had some brilliant collaborators in those years, notably Geoffrey Bodenhausen, Gareth Morris, Malcolm Levitt, Steve Wimperis, Ad Bax, James Keeler, Hartmut Oschkinat, A. J. Shaka, and Peter Barker, to mention only those who later went on to form their own research groups. Magdalen has its fair share of brilliant dons and there was a widely accepted view that this was the best of all possible worlds, a job for life. The College grounds are spacious and very attractive. One day our daughter Louise was part of a school party that was visiting Magdalen, and we chanced to meet. As a proud possessor of a master key, I opened the gates for the children to view the deer park. One of her schoolmates later asked Louise "Does your dad own this College?" Magdalen Fellows certainly felt a sense of belonging, something that was sadly missing at NPL, and was only briefly part of the Varian experience. Perhaps this is the most valuable parameter of all.

But the demands on the time of an Oxford tutor and lecturer are severe, making it difficult to satisfy all the obligations -- lecturing, tutoring, examining, demonstrating, College posts, committee work, and research. Inevitably one or more aspects must suffer, often the last in this list. In 1987, completely out of the blue, came an offer of the John Humphrey Plummer chair at Cambridge. There was no interview - simply a question of whether I would accept. Anne-Marie was supportive of the move, and our children had already left home, with the notable exception of our fifth child, Lawrence. Given the choice, we have always opted for change, and this move was attractive for me because at that time university professors were few and far between, and there was no prospect of a chair at Oxford. It seemed reasonable to deem this a pure research appointment with minimal teaching responsibility, which meant far more time available for doing the stuff I loved best. The only drawback was the poor mechanism for recruiting research students, and in fact all my research collaborators came from outside Cambridge, mostly from overseas. In a sense it was a partial retirement -- in preparation for the real thing, which, according to the strict rules, occurred in September 1999. Fortunately another chance encounter helped smooth this "final" transition. In 1991, out of the blue, came a Latvian organo-metallic chemist, Eriks

Kupce, who quickly evolved into a brilliant innovator in NMR methodology. We are still collaborating ten years after my formal retirement, having written 61 papers together.

Ray Freeman FRS; 23 July 2009.

China Trip, September 1983

It was my mentor Sir Rex Richards who triggered this off. He had been invited to make a lecture tour of China, but was concerned about travelling alone as he has a recurring back problem that could let him down at any moment. I was delighted to agree to accompany him, and I can only assume that the Chinese authorities were prepared to accept two lecturers rather than one. After a few months Rex called to say that he felt he really could not manage such a demanding trip, and would I be prepared to find another scientist to fill his slot. The obvious choice was my old friend Keith MacLauchlan and he readily agreed to this joint adventure. Talks on nuclear magnetic resonance (NMR) and electron spin resonance (ESR) go well together. The travel arrangements were made by the British Council through a "Scientific Exchange" program, although in retrospect we might be tempted to conclude that "all the reciprocity was on one side" because the Chinese sent their scientists to study in the West and then return to the homeland with their new expertise. But we were certainly not complaining.

We flew from Gatwick into Hong Kong. It was hot and terribly humid and the first big mistake was made at the hotel in Kowloon when we both independently drank the tap water and discovered what should have been second nature to all British expatriates - never drink their water. Dire consequences, but I recovered faster than Keith for some reason. I ventured out into the hurly-burly of Kowloon to buy a camera and film, only to discover that the film was far too fast (1000 DIN) for any known camera of that epoch. And we thought the Americans had invented "bait and switch". On the whole we found the Hong Kong blatant commercialism very unpleasant, and sensed a barely concealed contempt for us gweilos. Fortunately the very next day we flew on to the mainland on a China Airlines flight to Beijing. The crew were all smiles and brought small gifts to the passengers, sweets and tiny toothbrushes for example (poison and antidote?). On arrival we were delighted to be met by two friends we knew - Mr Pei, Feng Kui, who had worked with me in Oxford in 81-82, and Mr Meng who had worked with Peter Mansfield at Nottingham University. We had accommodation at the huge "Friendship Hotel", built originally to house Russian visitors, with an appropriately inhuman architectural style to match. Keith was muttering about some comparison with East Germany, of which he had first-hand experience.

Left to our own devices, we had considerable trouble finding the hotel restaurant. We were then astonished to see, at the next table, our old friend Jack Roberts from CalTech. He had almost finished his own lecture tour and imparted the alarming information that in China we were expected to give talks that were three to four hours in length. For someone who had carefully prepared standard lectures of 45 minutes plus questions, this really threw the cat among the pigeons. Jack then carefully instructed us both on the "ultra slow mode" by which any talk can be stretched indefinitely - deliberate mistakes in the maths, later painstakingly corrected, time out for occasional breaks, and of course the two-for-one multiplier to allow for translation (sequential, not simultaneous). Still it did warrant some hasty reassessment of my lecture notes. In retrospect we should have included a few slides of Oxford College architecture to eke out the breaks; the Chinese were amazed by the few Oxford prints that Keith had brought.

The stay in Beijing was divided into lecturing, sightseeing and obligatory rubbernecking around the Physics Institute. The latter exercise I find quite exhausting, but we were the 'distinguished visitors', and were expected to behave accordingly. But we have long ago learned the trick of making superficially intelligent comments on the experiments of complete strangers in entirely different fields of research. Keith had a visit to the Summer Palace while I gave my first talk. As Keith kept careful written notes of our adventures, all the descriptive details are available in his comprehensive diary, later printed out. Early the next morning we were driven for about two hours to visit that part of the Great Wall that has been restored. In those days, before the more recent explosive industrialization of China, the only other road traffic was a vast sea of bicycles, all riding at the same pace, and paying scant attention to the occasional car, even though it would be sounding its horn virtually continuously. Maybe we were tired (and we had missed breakfast) but climbing along the wall was exhausting; the inclines are very steep and only revert to actual steps in extreme situations. Lawrence and Veronique will remember this experience. For the tourists there were two Bactrian camels tethered near the wall, and somewhere I have a photograph of Keith proudly enthroned between the two humps. Our trip continued on to see the Ming tombs, of which only two of the expected 15 have yet been unearthed, they were so carefully hidden.

The next stage was a flight to Changchun in the north, with an unexpected stop at Sheng Yang. The plane was an ancient Russian turboprop, and as our window looked out directly onto one of the propellers only a foot or so away, it was hard not to speculate on what happens if part of the blade breaks off. These internal flights proceeded with great caution, at low altitude, and with a careful eye on the weather, so I don't suppose they bother too much with timetables. In fact this was one of the delightful aspects of China (in those days) - timing was of no great consequence. Our arrival in Changchun was after dark, and since there are very few street lights (if any), driving was quite scary because at night there are Chinese couples walking hand-in-hand along the roads in the dark. This was the only time we actually saw any overt sign of affection between man and woman. Is this a cultural or a political thing?

Changchun is where Mr Pei works. It is a city of 1.5 million souls, built mainly during the Japanese occupation. We were surprised to see notices in English as well as Mandarin, probably as there was an effort to promote English for use with computers. Of course language (our lack of Chinese) created all kinds of problems throughout our stay; there are severe limits on sign language and wild gesticulation. Our accommodation in Changchun was the best we encountered - there were mosquito nets over the beds (greatly appreciated). There was even a bowl of sugar on the table, something we had sorely missed elsewhere, but it turned out to be an enormous disappointment when added to coffee - it was in fact coarse-grained salt. At the Institute where the lectures were held it was brought home to us how much trouble the Chinese had taken to make us welcome. We went up in a large freight lift, fitted with a beautiful Chinese carpet that was rather too large for the floor size. We realized that this was a special extempore "VIP" touch because later the carpet had disappeared. Mr Pei did the translations for my talk. I had already learned at meetings in Russia and Estonia how to break a talk into quanta of two or three sentences and then wait for the translation. Mr Pei had no difficulty as an interpreter. But I remember how at Oxford, when stuck for a translation into English, he would trace out the corresponding Chinese character in the palm of his hand.

We were given gifts of original Chinese paintings; fortunately the British Council had already warned us to have suitable Western gifts with us to return the compliment. The NMR laboratory had a Varian spectrometer and was treated with almost religious deference - one had to change shoes and step over a low wooden barrier to enter the sanctum sanctorum. It is true there was a great deal of dust around. That evening we had a very special Chinese banquet to celebrate the Moon festival. There were many, many dishes, three different alcoholic beverages, and several toasts back and forth; the alcohol definitely helped. We particularly enjoyed the moon-cakes, a four-inch round shell of pastry stuffed with various fillings and stamped with the appropriate Chinese character on the face. This was probably the only "pastry" we encountered in China, and we never saw milk products. Our hosts took it upon themselves to serve us the more exotic morsels - sea cucumber was not exactly my cup of tea - but it was unthinkable to decline, and this automatically ensured that they gave us repeat servings. All in a delightfully relaxed atmosphere, with lots of good humour. I found that here (as in Japan) there came a point where jokes in the local vernacular nevertheless seemed funny, and it was easy to laugh. All in all, Changchun was one of the real high points of our trip, but soon we had to fly back to Beijing. The unfortunate Mr Pei was bumped from the flight and had to take a long train journey to Beijing. I wonder if I really thanked him enough; we still exchange Christmas cards every year.

The next day in Beijing we were driven with Mr Meng to Tienamen square and the Forbidden city, constructed in 1410 - a must for Western visitors. I leave the descriptive details to Keith's excellent notes. We saw entrepreneurs developing film using several buckets of chemicals under a wooden bench, but it was all black-and-white stock of course. When I changed the (colour) film in my camera, I was surrounded by a crowd of curious onlookers, surprised to see that one could do this in daylight. There were several old men flying kites in the enormous square, mostly made in the form of birds. Mr Meng told us that there were very few laws in China, except exhortations to follow the Thoughts of Chairman Mao (not always easy to interpret). Later the brutal suppression of the "democracy" movement in this very same square rammed home the point that rocking this particular boat was not permitted.

Early the next day we set out for Wuhan, which is actually three adjacent cities - Hankow, Hanyang and Wuchang on the mighty Yangtse river. We were accommodated at a slightly run-down hotel frequented mainly by Australian tourists, and for once had to share a suite of rooms. It was back to the mid-Victorian age with the small TV set tastefully hidden inside a dark-red plush cover; we never used it. After fixing the flush toilet as we had learnt to do everywhere in China (there is no conventional ball-cock, just a suitably sized block of polystyrene as a float) and noting that the light switch was activated by a very ancient cord, we set out for dinner. On our own here in the hotel, the language barrier almost defeated us, and ordering a particular menu had absolutely no effect whatsoever. We were regularly offered "banana juice" to drink. This turned out to be a bilious green fizzy drink, and when one day they asked if we would prefer orange juice we leapt at the offer, only to find the same bilious green fizzy drink.

Although there was beer available in those familiar brown glass bottles with crimped metal caps, we politely declined because we had observed them refilling empty bottles with left-over beer and then recapping the bottles. Keith was quite dismayed when I pointed out a small mouse crossing the restaurant floor, but he apparently accepted my rather specious argument that this was a good sign that mouse was not on the menu. It was about this time that we both realized that the only thing that saved this trip was to be a party of two that could have a big laugh at the various contretemps. In particular, the shirt saga. Keith sent seven shirts to the laundry and only five returned. Protracted negotiations with the young girls that seemed to run the hotel only elicited giggles and the adamant message that all the shirts had already been returned. Careful consultation of a Mandarin phrasebook suggested that "chi" (qi) was the word for seven, so every time we saw the reception girl we laughingly chanted "chi, chi, chi", more in hope than expectation, until one fine day, much later, Keith's favourite two shirts came back. We had been allocated "minders" to make sure we got into no trouble. As we did not know their correct names we evolved our own private nomenclature: "laughing boy No. 1", not to be confused with "laughing boy No. 2" or "Shanghai wallah" for the third interpreter/political officer.

My first talk in Wuhan Institute of Physics had as translator Xi-Li Wu, who was the daughter of the Director of the Institute. Xi-Li later came to work in my group in Oxford and even moved with me in 1987 to Cambridge. A charming young lady with impeccable English, a hard worker who produced a total of 17 scientific papers while in my research group. Eventually we were able to bring her husband Ping Xu out to Cambridge (initially it had not been evident to us that she was married) and they worked together. Ping had a much poorer command of English, exacerbated by his tendency to say "No" when he meant "Yes". We mostly communicated in the language of product operators (a sort of NMR mathematics). Both wrote fine doctoral theses. They then went to work with Richard Ernst in Zurich and later emigrated to Canada (Toronto and then Montreal), where they now have two beautiful children. I imagine this would not have been allowed in China.

Wuhan therefore was our base. I managed to spin out my lecture material, apparently without offending the audience. I suppose this is where I fell into a rather slow measured style of speaking which stood me in good stead with foreign scientific audiences worldwide, but which irritated Cambridge undergraduates. Indeed my Chinese audience seemed very keen and would take away my slides each evening to work on them in detail. At the end of each talk a group would gather at the blackboard to explain to each other in Mandarin what I had really intended to say, often getting into quite heated discussions. We learned that several in the audience had travelled large distances to Wuhan to hear these lectures, actually arriving a few days early owing to poor communication across China.

Wuhan is a very large city but rather off the beaten tourist track, and we felt we were at last seeing the "real" China. Everyone travelled by bicycle, and these often carried enormous burdens, requiring help from one or two extra men hauling on ropes in order to negotiate any inclines. It seemed that everyone was dressed in white shirts worn outside dark trousers - it was not until we got to Guangzhou close to Hong Kong that we saw any colour. But Mao suits were clearly off the menu except at formal banquets. Work went on relentlessly from dawn to dusk, and there were hundreds of shops and small businesses opening onto the street making all kinds of clothing, wooden or metal artefacts, often spilling onto the pavement for want of adequate room inside. Further out into the country, roads might be blocked by huge piles of grain waiting to be threshed. Everyone seemed to be happy and always ready to smile. Young boys congregated outside our hotel for the hilarious spectacle of obese Australian tourists emerging. I could see their point; we saw no overweight Chinese at all, for obvious reasons. Each day we were driven a considerable distance back to our hotel for lunch, and then back to the Institute to perform. It seemed clear that feeding foreign visitors put a real strain on resources; except for one embarrassing occasion, we never had meals with the Chinese scientists. By this time Keith had become adept with chopsticks and was looking rather less emaciated.

In the evenings we were free to wander around the district unchaperoned. Prior to our trip we had been warned to expect to be surrounded by crowds of curious Chinese, but I never found this happened, although it was quite obvious how different we were, if only in height and colour of hair. Occasionally someone would appear and ask if he could walk alongside and practise his English. I even ventured into a huge port terminal on the Yangtse, bustling with activity; no-one paid the least attention. Innate politeness or indifference? If there was interaction, it was only to give a happy smile. Quite a contrast with Japan, where their in-bred politeness is always in conflict with their negative attitude to gaijin. What makes the Chinese such happy and relaxed people? Is it still true today, in their frenetic pursuit of modernization?

We were a little surprised to discover a department store not far from our hotel. It contained a chaotic jumble of products covering a range from clothing and bicycle parts, to lots and lots of electric fans, which seem to be the only way to combat the extreme heat in summer. In winter it appeared to us that there would be no heating at all in the Institute, so one presumably went to work in thick overcoats.

The Institute of Physics boasts an ornamental artificial rock formation that actually serves as a fountain some 25 feet in height. This is the (rather ugly) showpiece used as a backdrop for group photographs; somewhere in my files I have the result. There are many colourful flower-beds, but the effect is rather spoiled by the generally unkempt nature of the grounds - no funds for gardeners. This stands in stark contrast with Jesus College where gardeners are constantly at work keeping the lawns pristine and the flower-beds in tip-top condition. We are terribly privileged in Oxford and Cambridge.

It was clear from our explorations of Hankow that the Chinese employ tremendous ingenuity to make the most of the materials at their disposal, and that they recycle anything that can possibly be reused. Hotel maintenance adhered to the same principle. I have already mentioned the venerable cord used to switch lights in our bathroom, where the ceiling was some ten-foot high. When the cord inevitably broke, we called reception and explained the problem, mentioning that a ladder might be necessary to do the repair. Eventually a man arrived (no ladder) and surveyed the scene. At his second attempt he cobbled a repair by the simple expedient of tying a knot; of course this only led to a repetition of the problem; the cord was quite rotten. Apparently labour is so much cheaper than a new cord. I am reminded of the opposite extreme in Palo Alto in the 1960's when the Hewlett-Packard factory ran their fluorescent lighting continuously for months on end and then replaced all the tubes in a single operation, whether they were working or not.

Our departure from Wuhan was complicated by the fact that we had been bumped from the expected flight. It transpired that visitors to China fall into three distinct hierarchies: (a) tourists paying in hard currency, (b) invited scientists like ourselves, (c) foreign businessmen. Our seats on the plane had been pre-empted by category (a) visitors, so we were to take the train to Guangzhou. I think our hosts were rather embarrassed, but we had a great send-off by the entire contingent -- Director Wu, Mr Pei, Mr Meng, laughing boys one and two, and Shanghai-wallah. We were in fact sad to leave.

In England any steam train that has survived is a thing of beauty, all shiny brass and polished paintwork, lovingly restored by local enthusiasts. In China a steam train is an ugly black monster, the ironwork rough-hewn and strictly utilitarian. The hot weather ensures that all the windows must be left open, so a shower of black greasy soot invades the compartments. We shared our sleeping accommodation with two disillusioned French businessmen, who turned out to be very good company on the 18-hour journey to Guangzhou. Someone commented that the decorations in our compartment were reminiscent of *un bordel des années trente*, particularly the small red table lamps, as dim as they could possibly be. Actually electricity was always in short supply all over China, and through the night journey we saw very few lights at all in the countryside we were passing through. We slept little; all in all it was an uncomfortable experience. When we finally arrived at a Western-style hotel in Guangzhou, I took a bath and, horrified by the amount of grime that washed off, immediately ran a second bath. Western habits were beginning to reassert themselves already.

There was of course a cultural step-function between mainland Gangzhou (Canton) and commercial Hong Kong (still under British administration), but this interface had been smoothed somewhat by the relatively easy access by train. So we saw colourful clothing for the first time, and advertising billboards ("Plying Figeon Bicygles" [sic]). We were particularly impressed by several shops that sold only bicycle seats, spanning all the possible garish colours of the Chinese imagination. I suppose this is one way of expressing some individuality. On the theme of lost in translation, our hotel toilet was out of order, "Repressaction". Keith's notes remind me that we had to attend a formal reception to mark the 34th year of the People's Republic, but that memory has mercifully faded with time. The next day we took the train to Hong Kong, suddenly thrust back into the mad, mad world we had escaped for one brief, surreal episode.

Ray Freeman FRS; August 2009.

Six Sorcerers & One Apprentice

This is the story of six top scientists who were kind enough to guide me, as a complete newcomer, in learning the tricky business of unstructured research. I had been unable to find any accepted do-it-yourself manual that could be of any practical use to someone setting out on a research career. I read a book by Bright Wilson, but it did not offer any deep insight into the *philosophy* I was looking for. The arcane art of the pure researcher must be passed down from established experts - the giants in their field. Luck was on my side, for I found some remarkable men at the top of their game; what follows is an account of how, in their different ways, each one gave me a much-needed helping hand. How I came to work with them is largely covered in the story "*As Luck Would Have It*". There it will be clear that I had little choice in the matter, apart from recognising true leaders when I found them. So here is my personal list of six great sorcerers: Rex Richards, Anatole Abragam, Robert Pound, Ionel Solomon, Martin Packard, and Wes Anderson.

Sir Rex Richards FRS - Fellow and Tutor in chemistry at Lincoln College, Oxford in the 1950s.

Rex Richards was one of the very first to appreciate that the newly discovered phenomenon called nuclear magnetic resonance could have unexpected applications in chemistry. Against all the odds, he set out to build his own NMR spectrometer from scratch, despite the perceived wisdom that heavy magnets and masses of electronics had no place in a chemistry laboratory. Rex was an ideal tutor for undergraduates, a charismatic lecturer in physical chemistry, and an excellent research supervisor, so my fate as an NMR spectroscopist was sealed at an early age.

War surplus radar equipment featured heavily in this enterprising radiofrequency project, and I was lucky enough to have followed a radar course in the RAF. Indeed 90% of my doctoral research was devoted to building electronics for the excitation, detection and measurement of the resonant frequencies of the arcane nuclei lithium, cobalt, gallium, indium and thallium. Much of this work was purely exploratory, but it was Rex who suggested that the NMR spectra of aqueous solutions of cobalt-III complexes would have important implications for Ramsey's theory of the chemical shift, because the cobalt atom has a low-lying electronic state that dominates the calculation of the nuclear shielding, and which also gives rise to the colour of these complexes. This interpretation was confirmed by measuring the NMR frequencies and comparing them with the visible/ultraviolet *wavelengths* for symmetrical cobalt complexes. The result was a straight-line graph that confirmed that the reciprocal of the appropriate electronic energy gap was the dominant parameter determining the cobalt chemical shifts. It seemed that NMR did have some useful applications in chemistry after all.

I learned a great deal by simply observing Rex in action - teaching by osmosis. Rex clearly understood the importance of a "hands off" style of supervising research. A neophyte student was allowed the chance to shrug off the doctrine (inherited from the undergraduate courses) that there is always a correct, prescribed way to attack a scientific problem. For the very first time, one could use one's own initiative and perhaps discover new things for oneself. Trial and error, with a large dose of the latter. I suppose it was inevitable that, many years later, I adopted a similar *laissez-faire* approach to supervising my own students. As an analogy, I am reminded of an almost surreal episode recounted by our youngest daughter Louise. One day, on a visit to Magdalen College, she was passing the old squash courts when "it started to rain ducklings". Together with another girl, Louise tried to catch these fluffy little balls before they hit the hard ground. The ivy-covered walls of the squash courts must have been over 15 feet high, but Mama duck had decided that the time had come to ease the ducklings out of the nest so that they could find their own way to the river Cherwell and learn to fend for themselves. Darwin would have been very pleased. I found that my own research students seemed to fall neatly into two distinct groups (a) highly motivated ones who would have been seriously hindered by any micro-management, and (b) others who were not really interested and who would soon graduate to careers outside of chemistry. In this aspect the Oxford Part II (research) year has proved to be a valuable exercise to help young students make a choice about their future career.

In retrospect I realize that in the background Rex had been quietly guiding my career. In a sense he was a father figure to me, for I had lost my own father in 1940. Rex was instrumental in arranging my next position at a renowned magnetic resonance laboratory in France, in the belief that immersion in an environment of physicists would be a good idea. Better than I, he understood what my future *métier* should be. In a sense we are both *physiciens manqués*, rather than chemists. So in the summer of 1957 I went to work as a *stagiaire* at Saclay. As a wise precaution I took some evening classes in Oxford in colloquial French.

Professor Anatole Abragam, *Chef, Service de la Physique des Solides et de Resonance Magnétique, Centre d'Etudes Nucléaires de Saclay, France.*

The incredible intellect of Anatole Abragam was evident to everyone who met him. His comprehensive notes on magnetic resonance were already the stuff of legend. At that time, they took the form of two paperback volumes, written in French, and no longer obtainable in bookshops owing to the enormous demand. He was busy converting this material into the his new book "Principles of Magnetic Resonance" published in English in 1961 by the Oxford University Press. Naturally the focus was largely on the physical aspects of magnetic resonance, for the great surge in chemical applications had yet to take place. This immense work soon became the "Bible" for magnetic resonance aficionados. Many ideas for new experiments can now be traced back to almost passing remarks in that book, but one has to admit, it was never an easy read. Anatole had spent a year in the Clarendon Laboratory, and he held Oxford in high esteem. Perhaps that is one reason that he took the trouble to find me a slot in his group. To visitors he liked to joke that he kept me there simply to calibrate his English, but I am pretty sure that he was making a play on the word *étalon* (a calibration standard) because this also translates as stallion.

For someone like myself, trained as a chemist, the sudden immersion in a cold bath of pure physics at Saclay felt like another relentless application of Darwin's prescription for survival of the fittest. The physicists at Saclay were trained in the rigorous mathematical formalism characteristic of the French higher education system (particularly *l'Ecole Polytechnique* and *l'Ecole Normale Supérieur*). This stood in sharp contrast to the Oxford emphasis on a practical "nuts and bolts" approach to research. One is reminded of the unashamedly *practical* steam engine invented in Britain, whereas the key theories of thermodynamics that flowed from analysis of heat engines were developed in France. Here is one example of this cultural divide. Anatole Abragam was amazed to see a young Oxford chemist using a lathe to fashion an NMR probe - cutting a screw thread on the inside of a hollow Perspex cylinder with the intention of maximizing the filling factor of the radiofrequency coil. By contrast, the French physicists tended to delegate any such mechanical work to the support technicians, tacitly accepting any limitations of this second-hand solution. A chemist seeks pictorial descriptions of physical phenomena whenever possible; the physicist looks for mathematical rigour. Sometimes I found my colleagues dangerously overconfident in their blackboard calculations; once they were quite surprised when I challenged a computation that concluded that the concentration of a particular component in solution was 210 Molar!

At that time the Overhauser effect was a fairly new concept; indeed at first the acknowledged experts in magnetic resonance found it very hard to accept Overhauser's predictions. As I was trying to understand this rather surprising phenomenon, a Saclay colleague told me "Oh, you just have to write down the equations". This is of course true, but I believe that simple every-day ideas can offer valuable insight into the Overhauser effect (see for example the ski analogy set out in "*A Handbook of NMR*" page 143). A mathematical equation is just a shorthand representation of reality; we should always bear in mind that every variable carries a physical meaning. My own experience has been that physical intuition is often more productive than mathematical formalism for finding new experiments in spin choreography. On the other hand, our Oxford group did come across one marked exception to this belief. During a routine density matrix calculation, Tom Mareci observed that the conversion of double-quantum coherence into an observable NMR signal is best carried out by setting the pulse flip angle to 135° (instead of the traditional 90°) since this allows the determination of the signs of the double-quantum frequencies. My two years at Saclay certainly taught me the importance of the rigorous theoretical approach, but I still harbour the suspicion that scientific breakthroughs are more often triggered by insights gleaned from pictorial "hand waving" visualizations. Of course intuition can sometimes mislead, but then this quickly focuses one's attention on the unexpected counter-intuitive finding, and the resolution of this conflict can be rewarding.

At that time the key project in Abragam's group was to use all kinds of clever tricks to align the nuclei in a solid sample at very low temperature in order to prepare a polarized target for the particle physicists to bombard. After many years of really hard work they gradually achieved a degree of polarization approaching 100%, a *tour de force* that perhaps never received the full recognition that it deserved. Here lies the dichotomy - should one seek for the quick *eureka* moment, or work towards an important long-term goal? Watson and Crick managed the enviable trick of successfully combining both. Abragam himself commented that although there were many brilliant Frenchmen in scientific research, it appeared to be the Anglo-Saxons (Americans and British) who took the lion's share of Nobel prizes. An unfortunate obsession with the purely theoretical approach perhaps?

Some years after I left Saclay, Abragam asked me to translate his book "*Réflexions d'un*

Physicien" into English. His own command of English was excellent, so I could only assume that he was too busy with more important matters to undertake the translation himself. The book in question consisted of a compilation of short articles and some letters that he had written at various times. He told me that he would leave the details entirely to me, but he omitted to say that one or two sections had originally been written in English and later translated into French, leaving open the possibility that Anatole could have a laugh by comparing the English originals with my translations. This was my first foray into preparing an actual book, and it served as a useful dry-run when I set out to write a book of my own.

Robert Pound, Professor of Physics, Harvard University, USA

By a fortunate turn of fate, Robert Pound was taking a sabbatical at Saclay during the academic year 1957-58, and he took me under his wing. Bob had been part of the prestigious Radiation Laboratory during the war and he retained an active interest in all things electronic, particularly those related to radar. Among magnetic resonance spectroscopists he was perhaps best known at that time for his "Pound spectrometer", a regenerative oscillator/detector that allowed one to scan large radiofrequency ranges in search of weak "unknown" NMR frequencies, without the need for high-gain amplification. Yet physically the device was very simple - a mere double-triode vacuum tube. I had built a copy of this "Pound box" for my doctoral research at Oxford. Naturally I was delighted at the idea of working with one of the real pioneers of NMR, a key figure in the Harvard research group (Purcell, Torrey and Pound) that shared a Nobel Prize in physics in 1952 with the Stanford group (Bloch, Hansen and Packard).

Realizing that I was interested in high-resolution NMR applied to chemistry, Bob suggested I should work on a project involving two of his untried ideas. The first was a novel concept to employ weak modulation sidebands (rather than the main radiofrequency transmitter) to detect the NMR signals. He suggested that in this manner we should be able to use the very simple Pound spectrometer as the detector. Although this marginal oscillator device operates at such a high radiofrequency level that normally it would completely saturate high-resolution NMR signals, the use of modulation sidebands of low modulation index would circumvent this drawback. The Pound-Watkins spectrometer was versatile because it had a wide operating frequency range and required no high-gain intermediate-frequency amplifiers. Incidentally we worked from some primitive photocopies of Watkins' doctoral thesis, kept in a vice because otherwise the pages curled up and became unreadable.

I built the radiofrequency parts of this new high-resolution spectrometer from scratch, including that notoriously tricky item - the probe. A radiofrequency probe is very sensitive to field distortion from trace inclusions of paramagnetic material, such as tiny particles of iron (one prescription was to boil in hydrochloric acid all the plastics that had been machined). Furthermore the magnet that I borrowed was not intended for high-resolution work, so the available field uniformity was unknown. Consequently I was disappointed (but not terribly surprised) to discover that all the resonance lines that I recorded were broad (about 200 Hz) whereas we were aiming at a resolution of a fraction of 1 Hz. Once I had eliminated all the practical problems of materials that made up the probe, I began to wonder whether the regenerative feature of the Pound spectrometer might be the culprit, enhancing the broadening by radiation damping. Now the key paper on radiation damping was that of Bloembergen and Pound, so it was with some trepidation that I suggested to Bob that this might prove to be the Achilles heel of his device. By this time, Bob had returned to Harvard and was incredibly busy on a brilliant new experiment to measure Einstein's gravitational red shift (Pound and Rebka). However, after a flurry of transatlantic letters I eventually convinced him that radiation damping was indeed the problem, so I replaced the Pound spectrometer with a conventional transmitter and all was well again. With the misplaced excitement of youth I was thinking of writing a brief communication to warn about radiation damping in regenerative oscillators, when a casual visitor to Saclay pointed out that Lösche's group in Leipzig had already analyzed this problem in an East German scientific journal that we had all overlooked. Take home message for beginners: if all else fails, read the literature.

Bob's second idea involved a super-regenerative oscillator, essentially a pulse-modulated Pound box with the feedback turned up very high. He had already used this machine to detect aircraft by radar. Consider first of all the condition where the oscillator is quenched by the gating pulse. Then, once the gate is slowly opened, the high degree of regeneration induces a rapidly growing oscillation where the phase is triggered by random circuit noise. After the exponential build-up of these noisy oscillations, the gating pulse quickly quenches them, and the cycle is repeated. The radiofrequency spectrum of this sequence of incoherent pulses spans a broad frequency band. However if there is a returning radar echo stronger than the circuit noise, this triggers the subsequent oscillations in phase, and the oscillator becomes coherent. Essentially the device has "locked on" to the radar echo.

An analogous regime is established if the super-regenerative oscillator is used to excite an NMR signal, for this would trigger the conversion from incoherent to coherent mode. So I set out to build this device. Here the cultural divide showed up once again. Conventional wisdom suggests that the Q-factor of an NMR coil should be as high as possible, for this improves the signal-to-noise ratio. On the other hand, a high Q-factor in a super-regenerative oscillator tends to "pull" the desired NMR frequency. When I asked Bob what kind of compromise should be adopted, he embarked on pages and pages of differential equations - a very long calculation that never did converge to a useful conclusion. So I simply went ahead with the Q-factor I already had. In the NMR application, the wideband incoherent oscillator excites a free precession signal from any point within that broad frequency range. If this signal exceeds the circuit noise, the subsequent oscillations become coherent, synchronized with the NMR Larmor frequency. When the main magnetic field drifts, the oscillator frequency follows, maintaining the appropriate NMR resonance condition, thus compensating the magnet instability to a very high degree (with errors less than 0.5%). Fed to a second (high-resolution) NMR probe, this radiofrequency signal keeps the second field/frequency ratio essentially constant so that excellent stability is achieved. We probably had the most stable high-resolution spectrometer at that time, permitting very slow scanning rates of the order of 0.2 Hz per second, such that the usual transient sweep "wiggles" were not observed. However the Saclay physicists were not at all interested in high-resolution experiments ("When the chemists arrive, it is time to move into another field") and my equipment was soon banished to a basement *oubliette* when I left.

Ionel Solomon, Polytechnician and Saclay physicist.

Ionel does not fit into my rather glib generalisation about the French physicists. Although he graduated from the same rigorous Gallic educational system, he also had a natural gift for visualizing the behaviour of nuclear spins in a simple pictorial manner. For this reason he was an ideal teacher and supervisor, and helped me enormously, particularly after Bob Pound had returned to Harvard. In one instance he very gently led me to discover for myself why my radiofrequency amplifiers were misbehaving. The problem was solved by teaching me about the concept of a "grid stopper", a trick that prevented positive feedback.

Ionel had written the first analysis of the effect of chemical exchange on NMR spectra - formalized in the famous "Solomon equations" (*I. Solomon, Phys. Rev. 99, 559, 1955*). As one practical example, they describe the complex chemical exchange behaviour of hydrogen fluoride, with and without small traces of water. This was the first use of "I" and "S" to designate two coupled spins (in adopting this nomenclature we unwittingly render homage to Ionel Solomon). Incidentally Ionel was a visiting scientist at Harvard at the time of this work, and benefitted from advice about the chemistry of hydrogen fluoride from Rex Richards, who was also visiting Harvard at that time. It was a small NMR world in the 1950s.

I was fortunate to work in the same room as Ionel and was able to observe him in action every day. On one occasion, in an Oxfordesque manner, Ionel was playing with the NMR spectrometer, and quite by chance discovered a completely new physical phenomenon (his "penicillin moment"). While applying pulses to the radiofrequency crystal in the Varian spectrometer, he observed a strange new response that had the appearance of a new kind of echo. Other mortals might have dismissed this unexpected effect as an instrumental glitch (of which there were many), but Ionel persisted, deduced exactly what was happening, and was able to maximize the new response by adjusting the pulse length by trial-and-error. This was the first rotary echo, analogous to the Hahn spin echo, but attributable to refocusing in the spatial inhomogeneity of the *radiofrequency* field (*I. Solomon, Phys. Rev. Letters 2, 301, 1959*). The optimum condition corresponds to a 180° radiofrequency phase shift that exactly reverses the sense of rotation of the spin isochromats, bringing them back into focus. Ionel was then able to generate a sequence of multiple echoes (more than a thousand) demonstrating that, in contrast to conventional spin echo experiments, errors in the 180° pulses were not cumulative. This proved to be a new way to study spin relaxation in a liquid sample.

From my own narrow parochial perspective, it seems a pity that Ionel moved out of magnetic resonance soon afterwards. I realize now that this is often the destiny of research scientists. They may change to an entirely new field - Wes Anderson left magnetic resonance and went on to develop an important ultrasound imaging device. Others move into administration and are forgotten by the NMR community. The revolutionary invention of magnetic resonance imaging might have tempted me to change fields in 1973, but at the time I was starting a new appointment at Oxford, so I stayed in NMR spectroscopy. I consider myself very lucky to have worked in the same field for almost sixty years. However, today many more people recognize the term MRI than have ever heard of NMR.

Dr Martin Packard, head of Varian Instrument Division.

When I arrived at Varian in November 1961 I was enormously impressed by the extremely warm welcome from everyone in the group. The whole NMR field was so new that any enthusiast from abroad was accepted, apparently without question. I had been there only a few days when Martin Packard handed me the keys to a car on indefinite loan, a kindness that would have been unheard of in Europe. The car was in fact a rather elderly Ford; the accepted American jargon would have dismissed it as mere "transportation" but it served our family very well indeed. A physics graduate of Stanford, Martin was one of the original pioneers of NMR. He may well have been the very first person to *actually see* an NMR signal (but see below). Following Felix Bloch's suggestion, Martin spent all day vainly searching for the first proton signal from water by slowly varying the magnetic field. It must have seemed like looking for a needle in a haystack because their calculations of the actual magnetic field strength were rather unreliable (in fact the field was too high). It was only after he finally decided to abandon the search for the day and shut down the magnet, that he saw a fleeting water signal flash across the oscilloscope (*F. Bloch, W. W. Hansen and M. E. Packard, Phys. Rev. 69, 127 1946*).

An independent group at Harvard had also observed an NMR signal (*E. M. Purcell, H. C. Torrey and R. V. Pound, Phys. Rev. 69, 37, 1946*). Their report was submitted earlier (December 24, 1945) than that of the Stanford group (January 29, 1946), and it was more comprehensive, but the Nobel committee clearly believed that the East coast and West coast developments were independent and virtually contemporary, and the prize was divided between Bloch and Purcell. Had the Harvard group been judged to have absolute priority, thus scooping the Stanford group, Bob Pound might well have secured a share in the prize. His later brilliant determination of the gravitation red shift might also have attracted the attention of the Nobel committee, but instead they gave the prize to Mössbauer who had discovered the recoilless emission of gamma rays on which Bob's experiment relied. Two near misses in a single lifetime?

Not content with his first discovery, Martin can also lay claim to be the first person in the world to observe a high-resolution NMR spectrum, the famous three lines of ethanol. It was an Indian chemist, Srinivas Dharmatti at Stanford, who had pointed out to the physicists that almost any organic compound should show a proton spectrum of several distinct lines, provided that the magnetic field was sufficiently uniform. For this purpose Jim Arnold built a permanent magnet with resolution approaching one part in 10^8 (see "The Dawn of NMR" in another section of this website). Martin then ran the spectrum of ethanol, and chemistry was changed forever (*J. T. Arnold, S. S. Dharmatti and M. E. Packard, J. Chem. Phys. 19, 507, 1951*). In fact that group later resolved additional splittings on the three chemically shifted responses, but whenever they had a chemist visiting the laboratory, they intentionally degraded the effective resolution to disguise this effect because it represented a rather messy complication to what was otherwise a beautifully simple concept - one resonance for every distinct proton site. One step at a time.

Although Martin Packard was not directly involved in my research at Varian, his influence was enormous. I suppose one could say that I learned mainly by observing someone who had already "been there, done that". As head of Instrument Division, Martin had the foresight to allow his young scientists to pursue their own interests without interference. Fortunately these enthusiasts were naturally disposed towards investigations that advanced the methodology of magnetic resonance, either to improve Varian instruments directly, or to generate valuable publicity for the company by writing innovative scientific papers in the field. There was little need to assign practical goals; largely unstructured research was producing useful results anyway. As one specific example, our attempts to understand the theory of double resonance led to the development of high-stability NMR spectrometers based on the internal field/frequency lock. This extraordinary freedom of action was possible because Varian was a small emerging company that had only just accumulated a critical mass of young talent. Scientists from abroad sometimes assumed that Varian was one of the many California universities. It was only later, when the company grew much larger, that complacency and stagnation set in. Then top scientists began to leave, and a flurry of young masters of business administration (with no knowledge of magnetic resonance) began to impose the conventional American management philosophy.

Weston Anderson, Physicist at Varian

When Felix Bloch was appointed director of CERN in Geneva, he took his two research students, Jim Arnold and Wes Anderson, with him to Switzerland and told them to write up their thesis work for publication. These became the classic papers on high-resolution NMR. Wes Anderson's thesis was written up in the *Physical Review, 102, 151, 1956*. Compared with our own modest experiments at Oxford in 1956 on cobalt chemical shifts, this classic served as a real wake-up call, demonstrating for the first time the full beauty and versatility of high-resolution NMR of protons. Martin Packard's famous three-lines spectrum of ethanol turned out to be merely the tip of an enormous iceberg, because Wes was able to record well-resolved proton spectra of quite simple organic compounds that showed many chemically-shifted lines, together with their associated fine structure. Physicists had ventured into chemistry, with the result that this new spectroscopy eventually revolutionized the study of molecular structure.

At that time a spectrometer could be thought of as an essentially neutral device. There was a direct one-to-one relationship between the *sample* and its *spectrum*; the operator could not interfere (except for trivial changes in pressure or temperature). NMR spectra recorded in Paris or Palo Alto were essentially identical. Wes took the first steps along a new path where spectra could be *manipulated* by the hand of man. This innovation can be regarded as the forerunner of all the present adventures in spin gymnastics that have made the field so productive. This deliberate intervention involved decoupling protons from protons, demonstrating which pairs of groups were related through the scalar coupling. From this modest beginning flowed more sophisticated manipulations like spin tickling, determination of signs of coupling constants, the measurement of the rates of chemical exchange, and the nuclear Overhauser effect. Few doctoral theses have been so influential in opening up new fields.

For someone who had struggled with solid-state NMR, and with NMR of the "other" nuclei, it seemed clear to me that high-resolution spectroscopy of protons was the way ahead. Whereas it had taken me many months to determine a single inter-proton distance by recording a Pake doublet from a polycrystalline sample of potassium amide, a high-resolution proton spectrum could be obtained in a matter of minutes and promised a wealth of important molecular data. The idea that even more structural information could be gleaned from double irradiation experiments was an intriguing possibility. One could argue that double-irradiation experiments were just the first step in the "manipulation" revolution. A decade later the invention of two-dimensional spectroscopy by Jean Jeener suggested a brilliant new scheme for recording the kind of information that had formerly been painstakingly recorded by double irradiation.

For these reasons I decided to join Varian in late 1961 and work under Wes' guidance. Immediately the pace of research accelerated. Only three days after my arrival I was asked to give an impromptu evening seminar at Wes' home attended by a handful of Varian enthusiasts. There I showed some slides of spin decoupling experiments carried out previously in England. Harry Weaver, a physicist in the audience, commented that certain features in these spectra seemed to show an effect sometimes given the jocular name "spin tickling". In fact there was no general agreement about what the term actually meant, and nothing had been published about the concept. Incidentally this was an unusual case of an experiment acquiring a name even before it had been properly implemented in practice. Later I realized that any new spin manipulation should always be given catchy name or acronym in order to replace an unwieldy description along the lines "*You remember that pulse sequence where proton polarization is transferred to carbon spins in order to enhance sensitivity?*" Like the early explorers of our planet, we claimed the privilege of naming each new discovery, and thereby establishing priority.

Out of pure curiosity Wes and I decided to look into spin tickling in detail. The basic observations turned out to be relatively straightforward - any NMR transition that shares an energy level with the tickled transition splits into a doublet, the splitting being proportional to the intensity of the irradiation field. In the published article (*J. Chem. Phys. 37, 2053, 1962*) we bowed to political correctness and never mentioned the actual word *tickling*; this necessitated some awkward circumlocution. What did take us quite by surprise was the observation that the new doublets were sometimes well resolved and sometimes poorly resolved. This did not seem to be a mere instrumental glitch. It was Wes who found the explanation. Tickling involves quantum-mechanical mixing of an allowed transition with a connected forbidden transition (akin to Fermi resonance in infrared spectroscopy). There are two kinds of connected transitions, which David Whiffen named *regressive* and *progressive*. When the tickled resonance line is regressively-connected to the observed resonance line, mixing involves a (forbidden) zero-quantum transition. Because the latter is insensitive to the spatial inhomogeneity of the magnet, this situation gives a particularly well-resolved doublet. In contrast, in the progressively connected case, mixing involves a (forbidden) double-quantum transition, which is doubly sensitive to magnet inhomogeneity, so that tickling doublet is poorly resolved. How fortunate to be working with a physicist!

The tickling condition is very sensitive to the position of the irradiation frequency with respect to the centre of the chosen resonance line. If there is a slight offset, the connected doublet becomes asymmetric, one component getting stronger and the other weaker (at very large offsets the weaker line becomes a purely forbidden transition).

Careful adjustment of the tickling frequency to give a symmetrical doublet offers a precise method for measuring line frequencies in a high-resolution proton spectrum, with accuracies approaching ±0.01 Hz. However highly accurate frequencies were not a major concern to most organic chemists at that time, for they were mainly interested in chemical shifts, where an accuracy of ±1 Hz was more than adequate for the purpose. Chemists were busy exploring the exciting (and seemingly endless) applications in organic chemistry, and were not really interested in spin tickling.

At that time (1962) spectrometers necessarily operated at quite low magnetic fields (typically equivalent to a proton frequency of only 60 MHz) and there was therefore considerable interest in the computer analysis of strongly-coupled proton spectra. The first step in such programs was the assignment of the observed transitions to an energy-level diagram, something that tickling achieved very effectively. Once this assignment was complete, the iterative fitting program for determining shifts and coupling constants converged reliably and rapidly. Later, as magnets began to be constructed with higher fields, strongly-coupled proton spectra became less common, and computer analysis consequently less important. With hindsight I would now have to admit that tickling experiments aroused only minor interest at that time. Tickling appeared to be an interesting exercise in spin physics, but not terribly productive.

Tickling offers a good example of the dichotomy between curiosity-inspired and applied research. The former is often given the adjective "blue-sky" after the work of the nineteenth century scientist John Tyndall, who set out to show (erroneously as it turned out) that the colour of the sky arises from scattering by dust particles or water droplets in the atmosphere. The alternative category of "focused" research works towards solving a specific problem, such as a search for a synthesis of a natural product like quinine. Both kinds of endeavour are important, and both are useful in their different ways. But we should never assume that "pure" research does not have direct practical uses. In order to study spin tickling we had first to stabilize the field/frequency ratio to permit experiments where the irradiation "tickling" frequency could be held fixed, while the field was held constant and the rest of the spectrum was scanned by sweeping the observation frequency. For this purpose we borrowed an idea suggested by Hans Primas at the *Eidgenössiche Technische Hochshule* in Zürich for using an error signal derived from the internal reference compound tetramethylsilane (TMS). There was already a Varian stabilization system that used a mirror galvanometer and two sensitive photocells to provide a correction current to counteract magnetic field drift. However this could not handle very slow drifts of the resonance condition because it lacked an absolute reference point. The insertion of a TMS error signal provided this crucial locking information. Our new stabilization scheme proved to be so successful that it became the basis of the next generation of Varian spectrometers (HA60 and HA100). A project born out of pure curiosity turned out to have widespread practical usefulness after all.

In parallel with this work Wes and I spent some time investigating the details of spin decoupling, calculating the so-called "magic curves" that map the way the structure of a proton spin multiplet changes when its coupling partner is irradiated at various decoupler offsets. Eventually these diagrams evolved into three-dimensional "stacked plots" of intensity as a function of the observed frequencies in one dimension, versus the decoupler frequency in the second dimension. This form of representation anticipated the later explosion of three-dimensional spectra engendered by Jeener's concept of two-dimensional spectroscopy. The first decoupling magic curve was actually published much later in the *Journal of Magnetic Resonance 26, 133 1977.* Not long after this work we decided that my apprenticeship was essentially complete, and Wes and I moved into separate projects, the most important of which was carried out by Wes, and is described below.

Wes loved practical gadgets and built various devices at home in his spare time. Telescopes and seismographs were two of his favourites. One evening while we were having dinner with several other guests at Wes' house, he asked if we would like to see his latest home-made seismograph in the garage. I don't know how he contrived to do this, but at the very moment that we entered the garage, the pen recorder started to go absolutely crazy. It turned out later that it was recording a major earthquake in Alaska. Since boats in San Francisco Bay were also being visibly shaken by the quake, the extreme sensitivity of Wes' device was not strictly necessary on that occasion. But it was an impressive party trick nonetheless.

Wes' gadget that had the greatest impact of all was one that was never actually tested in practice. The details of this story are set out in section 9.1 Multichannel Excitation in "When Chemists first 'Discovered' NMR" on this website. Wes was the first to appreciate that the accepted frequency-sweep slow-passage regime, used by all high-resolution NMR practitioners at the time, sadly lacked sensitivity. He predicted that simultaneous multichannel irradiation of the entire spectrum would improve sensitivity by a large factor, roughly equal to the square root of the number of independent channels. So he set out to build his "Prayer Wheel", a gadget designed to create a comb of equally-spaced modulation sidebands. It was never used in anger because Wes soon realized that pulse excitation followed by Fourier transformation of the transient NMR signal would achieve the same predicted order of magnitude improvement in sensitivity, and would do so far more effectively. The resulting paper by Ernst and Anderson (*Rev. Sci. Instr. 37, 93, 1966*) changed NMR forever. The actual Prayer Wheel went to the Smithsonian Museum in Washington, DC.

I remain greatly indebted to these six gifted sorcerers who eased my transition from formal book-learning to the exciting arena of scientific research.