

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 Baldeschweiler 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.