A Life in Mathematical Science
Part I: Growing-Up, Student and Postdoc Years

Nicholas Stephen Manton FRS *

Department of Applied Mathematics and Theoretical Physics,
University of Cambridge,
Wilberforce Road, Cambridge CB3 0WA, England.

March 2017 – September 2018

Abstract

This memoir has been prepared in response to the Royal Society's request for Fellows to write an autobiography. There is more here of a personal nature than the Royal Society needs, but also a review of my scientific work and recollections about some of the scientists I have met and worked with.

*email: N.S.Manton@damtp.cam.ac.uk

1 Introduction

It is interesting to look back on one's life and career, recall some key memories, and try to assess the significance of one's contribution to science. My life has not been very eventful from the public's perspective. I'm little known outside a modest circle of theoretical physicists and some mathematicians, and have not appeared as a scientist on TV. I have hardly ever been interviewed by journalists, except once for Finnish radio when Stephen Hawking was unavailable. But I have had an interesting time in science, and most of my social life revolves around interactions with academic colleagues and graduate students. For over 30 years, family life has been with my wife Anneli Aitta and our son Ben.

I am currently 65 and expect to retire in about two years. I would hope to be involved with research beyond retirement, and to keep writing articles and maybe another book. But I have had some health problems recently, so felt that it was a good time to start on this memoir.

2 My Father's Family and Background

My father was born Franz Eduard Sigmund Manteuffel in Berlin on 22nd February 1921, only child of James Manteuffel and Elfriede Manteuffel (née Kaufmann). James was from Berlin and worked in clothes retailing. Elfriede was a talented pianist and moved from Neustadt, west of the Rhein in the Pfalz, to Berlin to study piano, later becoming a piano teacher. Both were Jewish, but probably non-practising. The family lived in Charlottenburg in the western part of Berlin, and my father had considerable freedom to roam the city on his scooter. He had some musical and scientific talent, learnt piano, and in his teens often went alone to the opera and concerts. Some of his heroes and heroines were the opera singing stars of the day, and especially the conductor Wilhelm Furtwängler, whose beat was very hard to follow. I don't think he had any involvement with the 1936 Berlin Olympics. He was also rather disdainful of the rituals of the orthodox Jews.

By 1937 the situation for Jews was critical, with no career prospects for a teenager. My father managed to get an apprenticeship with the engineering firm of Fleck's in Siemensstadt, helped by his mother who persuaded the manager that without this training her son's prospects and perhaps life were threatened. In late 1938 and into 1939 he was forced to transfer to an apprenticeship in an entirely Jewish enterprise, and a letter from that time names him as Franz Israel Manteuffel.

With his training in metal working (lathes etc.) he successfully applied for a permit to enter the UK. He also got official permission to leave Germany, which probably cost quite a lot, and in 1939 flew from Berlin to Croydon airport just south of London, the plane overshooting the runway and ending up in a hedge. But he was OK. He had some introductions in London arranged by those helping Jewish refugees, and by 1941 had got a job with a small motor car engineering firm, Boon and Porter. He also started evening studies at Kingston College, just outside London, and worked hard to speak and write good English, but always had a rather strong German accent.

By 1943 he had passed some exams at Kingston, and received strong recommendation letters from both Kingston and Boon and Porter for university study. So he could enrol at Imperial College in South Kensington for a BA in engineering, which included both maths and physics. At one of the examinations in Exhibition Road, a bomb fell nearby and the students had to blow the dust off their scripts, but the exam continued. There was a lot of thermodynamics in

the course, and by the end, my father had become particularly qualified in heat engines. Heat engines mainly meant steam engines, but his later job was more concerned with petrol (aero-) engines. One of the lecturers he recalled was Gerald Whitrow, who taught at Imperial just after the war and wrote books on space and time. My father got a First class BA degree, and accepted a one-year postgraduate studentship for further study of engines in 1946-47. He also enjoyed rambling in the countryside around London.

My father never saw his parents again. His mother wrote occasionally, and sent parcels including warm underwear – long pants – that he used. By 1943 contact was lost and both must have perished in a concentration camp, maybe Auschwitz. My father had an uncle, Onkel Richard, who escaped to Switzerland, and an aunt who I think got to Paraguay. I never met them but did meet Tante Helene, the wife of Onkel Richard, who lived in Basel. We met her while on holiday in Überlingen on the Bodensee in 1957, and later at her flat in Basel more than once.

During the war and afterwards, my father spent summer holidays in Cambridge, a kind of house party for German refugees on Parkside overlooking Parker's Piece. He mentioned having read Galsworthy's Forsyte Saga in Sidney Sussex College garden during one of these holidays, but the main activity in Cambridge was preparation of a domestic performance of some opera, with piano accompaniment. He sang tenor parts. At a similar gathering in London, he met my mother Lily, who sang soprano.

My father loved Cambridge and dreamt of studying there. He approached the sponsor of the houseparties – Mrs Birkill, wife of the Fellow and later Master of Peterhouse, John Burkill – to ask whether he could be considered for a place, but was told that while her husband supported her work on behalf of refugees, she couldn't interfere in matters of college admissions.

After the war he got a job with the helicopter manufacturer Fairey, but decided he preferred the security of the Civil Service, and moved to the Royal Armaments Research and Development Establishment (RARDE) at Fort Halstead, in Knockholt, between Bromley and Sevenoaks in Kent. His recommendation letter from Fairey said that if all talented young engineers left manufacturing companies to work for the government then British industry couldn't survive. Over the years his work for the Ministry of Defence (MoD) became more administrative, overseeing procurement projects for the Armed Services.

In 1948 he married Lily in a liberal synagogue in north-west London. At about this time he also applied for naturalisation, and changed his name to Manton. This was chosen from the telephone directory as a fairly rare surname starting with the same letters as Manteuffel. Occasionally people asked him if he was related to the famous von Manteuffels of German military distinction, but he wasn't. I am also asked from time to time if I am related to some famous British Mantons, like the 19th century gunmaker, or the biologist sisters Irene Manton FRS and Sidnie Manton FRS, but of course I'm not.

He continued to perform in amateur opera companies, and once sang Don José in Bizet's Carmen. These performances were written up in the local press. He also started his own opera company, as the conductor. This later became the Philopera Circle, and was a major part of his life's work. There was piano accompaniment for a few years, but in 1956 the performance of Nicolai's The Merry Wives of Windsor was with a small student orchestra, the first of many times my father conducted an orchestra. He continued as (sole) director and conductor of the Philopera Circle for many years. His job for the MoD allowed time for much of his effort to go towards managing Philopera and the music-making.

My father was keen on European holidays after the war, and his Civil Service job allowed 3-4

week breaks (6 weeks per year in total). Between 1948 and 1957 my father and mother travelled all over western Europe, including to Switzerland, where my mother got badly sunburnt once on the Rigi, to France, to the Netherlands and Austria. They also went to the Rhein valley in Germany, and to Bayreuth, discovering that Germany's economy recovered more quickly from the war than Britain's. So they enjoyed cream cakes, which were rationed in Britain at the time. They didn't report much remaining antisemitic feeling, except from just a few individuals, as the Germans had by then started to reject the Nazi aberration.

My parents lived in London in a basement flat at 16 Abercorn Place, St John's Wood NW8, and this was my first home after I was born in 1952.

3 My Mother's Family and Background

My mother came from a family of German-speaking Jewish merchants. She was born Lily Goldschmidt in Königsberg, the capital of East Prussia, on 17th February 1926 and grew up there till she was 12. Most importantly for her, she and her family were British subjects. Her grandfather Moritz Goldschmidt was born in Danzig in the mid-19th century (family records include the names and some information about Moritz's parents) but he moved later to London with his German wife, and their son Sidney was born in London in May 1881 and thereby acquired British nationality. Moritz died in London and was buried in Stoke Newington. Sidney presumably learned about trade between England and Danzig, and eventually moved back to Danzig, working in a shipping office; a speciality of his business, possibly from a later date, was the trade in watches. While there he met Lisa Grabowski from Königsberg, born on 21st January 1891, and settled with her in Königsberg, continuing in the shipping business. She was German, but through their marriage in 1911 she became British. Apparently, they could have decided to become German nationals at that time, but fortunately they didn't. Their son Erich was born on 29th June 1912, and his birth was registered at the British Consulate in Königsberg. At the outbreak of the First World War, Sidney was interned as an enemy alien in Ruhleben camp in Berlin, and was there for a few years. After release he returned to Königsberg, and subsequently he and Lisa had two daughters, first Ellen, born on 2nd December 1920, and then my mother Lily just over 5 years later. Erich was later called Eric, and Ellen acquired the nickname Puttel at an early age, and these are the names I will use in what follows.

The Goldschmidt family were quite well off and had an apartment at Am Schloss 2 in central Königsberg. They had a cook and a nanny with whom my mother spent considerable time, and for holidays they went to the sandy Baltic seaside resorts of Crantz and Rauschen. The family knew that Immanuel Kant was a famous son of Königsberg, but I don't think they knew of the famous mathematicians Euler, Jacobi and others who also worked there. Puttel, at least, was aware of the Seven Bridges of Königsberg problem, but by the early 20th century there were more bridges across the River Pregel, and less interest in the problem. The Pregel banks were more noted for smelling of fish.

My mother's mother Lisa was an accomplished pianist, like my father's mother Elfriede, and she also became a piano teacher. Eric, Puttel and Lily learned some instrumental skills, Eric as a violinist, Puttel as a cellist, and my mother as a pianist. It was not their forte. My mother could play some nice harmonised tunes, and could improvise quite well, but never mastered reading music. She sang well, and later, in England, contributed both as a soloist and to the chorus in the Philopera productions. However, my mother was really more talented as a visual artist. For Eric's wedding to Raja Stukator in 1935, she wrote or copied a homemade poem in

beautiful German gothic script which is now hard to decipher, and produced a pretty cover for it. Raja (later Raya, the name I use below) came from a family of dentists in Lida, Poland, now in Belarus. This was part of the Jewish Pale, so she had a different background, more religiously Jewish than any of my German Jewish ancestors, who were all more secular. Eric and Raya had a loving relationship throughout their long lives, but they had no children.

By 11 or 12 my mother could write good German, but she hardly knew any English. None of the family were good at maths or science – a family story was that Eric couldn't do anything in a school maths exam around 1928, and drew a boat instead. His mother Lisa was called in to the school for a ticking off but somehow Eric continued, and could study medicine after leaving school, as at that time maths was not a prerequisite for medical studies. Raya was studying dentistry in Königsberg when she met Eric.

Puttel recalled that in 1929 or so, when Hitler and the Nazis were rising to power, the feeling among Jews in Königsberg was that this could be good for the middle classes, and bring financial stability, which they craved after the German Inflation and the Wall Street crash. They didn't anticipate the murderous Third Reich.

By 1938 things were critical for the Goldschmidts, and the whole family sought to emigrate to Britain. As British subjects, this was not too difficult, but they had to leave behind considerable property and effects. Nevertheless, they could send over some porcelain figures and a plate made in Meissen in the 1920s, some books, linen, a substantial dinner service and monogrammed silverware. Sidney moved first, and at some time in 1938 Lily accompanied him to London and was placed in a boarding school in Epping. From there she wrote letters home, first in German, and in one of them she amusingly described the novel experience of corn flakes for breakfast. Fairly swiftly she also learned English. Sidney found a residence for the family and by 1939 they had all reached England. Possibly it wasn't their first home, but for many years they lived in the upstairs flat of 42 Dartmouth Road, Willesden Green NW2, a large Edwardian house. Shortly after settling in London they changed their names to Goldsmith. Several other relations also escaped from Nazi Germany, including the Grabowski family who mostly settled in London too. One of Raya's sisters escaped from Lida to New York, but her parents and other sister perished, and she never forgave the Germans.

Sidney was too old for military service in the Second World War, and Lily too young, but Eric joined the Army as a doctor, and was posted to India. He didn't see any action, but he acquired some Indian souvenirs, including a carved, foldable side table with brass inlay, some metal ornaments, and a picture book Wonderful India that I still have. He got a compassionate discharge in about 1943 because by then his father Sidney was ill with lung cancer (he was a pipe smoker). One bomb fell near to the house in Dartmouth Road, but they were not hurt. Sidney died in 1946, so Lisa had a long widowhood until she died in 1966. I got to know her well as my Omi. For her last ten years or so, she suffered from painful arthritis, and could hardly walk.

Puttel enlisted in the WAAF, and used her fluency in German to listen in to German planes over the Channel. She may have also translated what she transcribed. She was based at Capel-le-Ferne on the cliffs between Folkestone and Dover, near what is now the Battle of Britain Memorial, and described this time as the most interesting of her life. Everyone there smoked cigarettes to deal with the long hours and stress, and she reported that not smoking was regarded as antisocial. We recently saw photos from that time (not of her) at Bletchley Park, where the transcripts were received and analysed. She was the only family member still to be smoking many years later, but she eventually gave up under pressure from the family – being banished to smoke outside – and from more general social pressures. Despite her smoking, she was the

longest-lived of all my relatives. She died in a nursing home in Surbiton in 2012, aged 91.

My mother Lily, after settling in Willesden Green, went to school there and matriculated, although she needed some additional tuition to get through maths. She then went to Art School in Willesden, and art and design became her career. Before I was born, she worked in the costumes department of Covent Garden, and someone she looked up to there was the famous singer Elisabeth Schwarzkopf. After marrying in 1948, she was involved in stage design, and in the design and hand painting of ceramics for John Lewis. Later, after I was born in 1952, she worked at home on wallpaper patterns, papers for shelving, serviettes and doilies. She sold several of these every few months to manufacturers like Deeko and Swan, and a few were produced commercially. However, her greatest success came as a stage set and costume designer for Philopera Circle, with favourable comments in the national press. One year in the 1960s she also designed sets for The Marriage of Figaro at the Hintelsham Opera Festival in Suffolk. My father was reluctant to let her be away for an extended period during that performance week, because he could hardly cook. He and I ate out several times at the Orpington Chinese restaurant.

After the war, Eric continued as an NHS doctor specialising in anaesthesia, and particularly in eye operations. He and Raya spent a few years in Glasgow and also Dudley, but they really wished to move back to London, near other family members, and to enjoy the music and opera. After they managed this, they lived for many years at 39 Engel Park, Mill Hill NW7, a 1930s semi with curved metal-framed windows, which I remember well. Between the ages of about 2 and 5, while my parents went on their extended European holidays, I was farmed out to stay with them. Photos show me on the flight of steps in their steep garden, and also show the succession of Ford cars that Eric owned. He used his status as a doctor to get good deals, and acquired a new car every three years. In London he worked at several hospitals on a regular weekly rota, and was on call some nights, so driving was essential. Some of these hospitals, including the Dreadnought Seaman's and the Woolwich, were in south-east London, a long way from Mill Hill.

As well as going to the London opera, Eric and Raya went to Glyndebourne. Like my parents, they also enjoyed extended European holidays, to Switzerland and Italy in particular. Raya didn't work as a dentist, but she acquired an impressive skill in languages. She was fluent in Polish, German and English, but later ended up teaching classes in Italian and Russian to adults. Raya, more than Eric, was socially quite ambitious, and organised elegant parties for family and friends. Partly for this reason, they moved to a slightly larger house at 12 Holmdene Avenue, Mill Hill with mock-tudor features like lattice windows. There they had a gardener and a handyman, who was helpful to them until their final days around 1993.

Puttel married Bernard Charlesworth in 1943. A son was born on 4th January 1954; he is Anthony Charlesworth, my only cousin. Bernard's relationship with Puttel cooled, and they divorced some years later, but Bernard for many years sent Anthony fine Xmas presents, like large plastic modelling sets. I just about remember Bernard from the time when I was 3 or 4, while the Charlesworths were living in a basement flat at 192 Cromwell Road, SW5, near Earl's Court. Puttel worked in the travel business as a guide and later travel agent, spending many summers in Interlaken, Switzerland, and Anthony spent those summers at a children's home in nearby Beatenberg. Puttel's later work was at the Erna Low travel agency in South Kensington, mainly arranging and ticketing skiing holidays. I acquired interesting travel brochures from Puttel, including many BEA (British European Airways) guides to cities and countries. That piqued my interest in geography and maps. Puttel moved in the mid-'70s to a flat at 12 Finborough Road, SW10, still near Earl's Court.

Anthony went to his local primary school in Earl's Court but often stayed with us for about a week during his school holidays, while Puttel was working. During snowy January weeks, we made snowmen together, one of which collapsed and became a snowseal. After leaving primary school Anthony went to Westminster City School, and then studied English at University College London. He had a career in journalism and more broadly in the media, working for Reuters, the BBC and Associated Press, and later set up his own communications company Tony Charlesworth Associates, specialising in producing videos for various organisations.

Eric and Raya were very kind to me and my one-year younger cousin Anthony. We made up for the fact that they had no children of their own. An early present was one of my favourite teddy bears, which I remember receiving at Xmas when I was 3. I still treasure and often consult a much later present from them of Alfred Einstein's book about Mozart. For several years, Anthony and I would stay over at Mill Hill after a family Xmas Eve party, and then go with Eric and Raya to a ward at Woolwich Hospital for an Xmas Day turkey carving by Eric. Another early memory of mine, at just under 3, is a first long trip in Eric's car, back to Orpington from a holiday at Herne Bay on the north Kent coast. This was one of the short British holidays my parents arranged to compensate me for missing their European holidays. An earlier one at age 1 that I don't remember at all was to Swanage. But I do remember at age 4 going to Hythe, and travelling a few times on the Hythe, Dymchurch and New Romney miniature railway to the sandy beach at Dymchurch. I liked this, and a plea of mine was "I want to go to Dymchurch!"

4 My Early Years

I was born on 2nd October 1952 at Middlesex Hospital, Mortimer Street, W1, in the City of Westminster. As a baby I lived with my parents in the basement flat of 16 Abercorn Place, St John's Wood (an early connection with St John's), and was pushed about in a pram to Abbey Road of later Beatles fame, and as far as Regent's Park. My father was at that time commuting by train to Fort Halstead in Kent, a long journey, and the family in any case needed more space, so my father looked for a house nearer to work, and found one to buy at 13 The Avenue, Orpington. We moved there in the winter of 1954. It was a 1920s gabled house in the middle of a row of four, quite large, with four bedrooms; the kitchen was also large, with space for a table that could seat four or five, and was excellent for working. Initially, heating was mainly by coal stoves in the living room and kitchen, and it was a lot of work and messy to collect the ashes each day and throw them out. A mixture of additional heaters were upstairs – one gas radiator, a paraffin stove for the bathroom, and electric radiators. Later, all these were replaced by electric storage heaters, filled at night at a cheap rate. We never installed a central heating system using circulating water, although many others did this from the '60s onwards.

Some of my early memories of Orpington go back to just before I was 3. One was swallowing a button by mistake. I also remember singing some nursery rhymes in English, and also traditional German songs (O Tannenbaum etc.) accompanied on the Blüthner piano in our living room either by Mum, improvising, or by Dad from a score. A bit later I once got lost and frightened at night in my own bedroom, and couldn't find the door. At age 6, I went to the Fort for an open day; it was exciting to try out the sophisticated adding machines of the day.

The Avenue was at that time not adopted, and was a rather stony road that tended to flood at the Sevenoaks Road end. Opposite there were no houses but a nice wooded strip, and outside the front garden of the house was a big elm. Later, three houses were built on the other side of the road lower down, and our house was renumbered and became 25 The Avenue. The road was

then adopted and paved, and the elm tree had to be cut down. The house was about 7 minutes walk up to Orpington station, with good connections out to Knockholt for my Dad, and in to Charing Cross or Victoria. Trains were electric from Orpington to London, but there were steam trains on the longer distance routes to Tonbridge and on to Dover. They could easily be seen and heard on the embankment near the house. Occasionally, when I was about 4, we took the named steam train The Man of Kent back home from a trip to London, because this was one of the few fast trains that stopped at Orpington. Later I discovered that I was not a Man of Kent, but a Kentish Man (living west of the Medway). I also learned that Orpington station is at an interesting boundary between the chalk descending from the North Downs, which was about 1 foot down in our back garden, and the higher pebbly beds of the Eocene seacoast, which make up the Crofton area above the station with its fine view. The pebbles stretch to Keston and Hayes Common, covered with gorse bushes, and far beyond.

My father could commute to Fort Halstead fairly easily from Orpington, but eventually he got fed up not being in central London, with its museums, restaurants and libraries, and was transferred to an administrative branch of the Ministry of Defence, at Castlewood House in New Oxford Street. By then he was a Principal Scientific Officer. This was in about 1961. From there it was much easier to go to the evening Philopera Circle rehearsals.

My parents enjoyed walking, and at weekends we often went 'alleying' along the alleys of the Knole area of Orpington, ending up at the pedestrian bridge across the railway line north of the station, where I enjoyed waiting to see the fairly frequent trains on the four tracks. For several years I train-spotted, noting the numbers of the trains and engines in a book containing a complete list for Southern Region. We also sometimes went to Petts Wood (really in Chislehurst), a semi-wild National Trust area. I recall a monument there to William Willett, inventor of Summer time (BST).

From age 3, I attended Montclair House, a nursery school in Sevenoaks Road near the Orpington War Memorial. (The pair of buildings later became the Conservative Association, and I recall a visit by Edward Heath near election time when he was Leader of the Conservatives, probably in 1970. Orpington was famous in the '60s for having elected the Liberal MP Eric Lubbock in a 1962 by-election, but in 1970 the Conservatives re-took the seat.) I wasn't particularly quick at reading or writing, but must have been good with numbers, because before I left Montclair House, at age 5, I joined some of the arithmetic classes for the 6-year olds and learned to add and even some multiplication tables. I suppose part of the school was a pre-prep. After nursery school finished at lunchtime, I'd go home for lunch with Mum, and listen to Listen with Mother, presented by Daphne Oxenford. This was a radio programme with nursery rhymes and a story. The story was always preceded by the ritual catchphrase "Are you sitting comfortably? Then I'll begin." Next, my mother listened to Woman's Hour while I played with bears and toys. Afterwards we would perhaps do some drawing or play some game together, and quite often she worked on her designs for Deeko or for the stage.

From when I was about 2 until about 10 we had a lodger living in the bedroom upstairs that had a washbasin and gas ring. The rent was fairly low but the lodger was required to babysit once or twice a week while both my parents were in London rehearsing the operas. These rehearsals took place for most of the school year as an evening class in Lisson Grove, Marylebone, and my father was paid as an instructor. There was a long-term piano accompanist, Margaret Oppenheimer, who was also paid.

One story from the time of lodgers was that my mother at some point was speaking to a potential new lodger and said at the door that the rent was seventeen and six, or something, and

I said (nearby) isn't it fifteen shillings? I was too naive to realise that rents are often raised when tenants change. I wasn't good at business. My parents were also not very good at business, despite their family backgrounds. They aspired to the more intellectual life rather than the Jewish stereotype of being in trade.

At age 5, in January '58, I started at Warren Road County Primary School, about a mile away from home. This involved a bus journey or a fairly long walk. Initially I was accompanied by my Mum, and collected afterwards, but slowly I did more and more of the journey myself. The fare was a penny (1d) or a penny-ha'penny and I started to collect the tickets.

Collecting things became a key hobby. I began a stamp collection with the gift of a Maltese stamp, and later started a coin collection and a collection of matchbox labels. I spent hours poring over stamps and rearranging them in albums as the collection grew. Later I had a Stanley Gibbons catalogue and spent pocket money buying stamps from shops and better-provided friends. Like many other boys, I concentrated on British Commonwealth stamps. This continued until I was 16, when many boys discovered that the market was highly inflated by the dealers, and most amateur collections were pretty worthless. But by then I had acquired some treasures, like a Victorian twopenny blue and the 1953 Coronation set from Great Britain. I also had the complete Colonial Coronation set, a present over several months from my grandmother, and some of the higher value stamps from India and East Africa.

A further collection was of the picture cards from tea packets. These came with my Mum's purchases and also from others who knew I was collecting. After a few months, a book was available from the tea company – Lyons or Brooke Bond – for sticking the cards in. They would also supply any cards that were missing, usually a few. I learned how hard it was to get a complete set when cards are distributed randomly in the packets. One set I particularly treasured was of Australia, and it motivated me to eventually visit Australia with my family on a sabbatical in 1993. There we saw several items on the cards for real, including Sydney Harbour Bridge, kangaroos, emus, a koala, beautiful plants like bottle brush, Aboriginals, and Aboriginal art.

I learned quite a lot of general knowledge from these tea cards, and also from poring over maps. On visits to the Orpington library I was happy to spend an hour or two in the reference library looking through atlases. As I got a bit older, my mother would sometimes do some shopping while I was there, if she had some business at the far end of Orpington High Street (the St Mary's Cray end), and later still I went alone to the library. From age 9, my parents also started buying Look and Learn for me, a weekly magazine for children. I would read most of this, but not the stories. From age 10 onwards, I stopped Look and Learn, and started getting the more serious children's encyclopedia, Knowledge, that came out in weekly magazine form, and could be ordered from WH Smith's. Cleverly, the publishers started it afresh each year (with a different coloured banner on the front page) so it was quite well established. Eventually I collected the complete 18-volume series over four years, and made time to read most of the articles. Each volume combined 12 issues into a binder that we acquired by post. I still have my set of Knowledge, and even in this day of Wikipedia articles online, Knowledge remains a useful reference.

One more set of cards I collected was of flags, from bubble gum packs. These packs were a penny each, but I hated bubble gum – or maybe just the smell of it, as I have never chewed gum – and sold four bubble gum pieces for a penny. That was not good business, because obviously a bubble gum piece was worth more than a farthing (a coin that had only recently gone out of circulation) to those who liked it but didn't want the flags. The colourful flags were replaced by a

new series of black and white Beatles cards around 1963. I wasn't so interested in these. We did listen to a few Beatles songs at home, and saw their film Help!, but my parents seldom listened to the radio channel where the songs were played, and we only bought one Beatles record. Anneli told me recently that she acquired some of the same Beatles cards in Finland.

At school I learned to read and write at a standard age, being clumsy at first with a pencil. I was much better at arithmetic, and enjoyed counting to myself up to large numbers, well over a thousand. We had frequent multiplication tables tests. Sometimes these were conducted by the headmaster Mr (Edwin) Stevens with a stop watch – 3 seconds per multiplication. I was pretty quick and accurate and could see the pattern, so wrote down some answers before being asked, and checking too. For example, after going through the 2 times table with 7 times 2 first, we'd do the 3 times table with 7 times 3 first. Once, I was sufficiently ahead that I whispered an answer to my friend sitting next to me. This got me into trouble, and I was sent home to write out a hundred times $6 \times 4 = 24$, and another hundred times $4 \times 6 = 24$. My mother couldn't understand why I got such a punishment if I was good at arithmetic. I must have told some fib about this and she went to the school to find out. After that I was told off by her for lying. This was a good lesson, and I have been reluctant to lie since then. That's good for a scientist.

An interesting mathematical experience was taking the Kent test papers, which I took more than once. We weren't told these were exams, but they were, and marks were recorded. There was a mechanical arithmetic booklet and a more interesting problems booklet, with about 55 problems of increasing difficulty. You'd start near the beginning of the booklet, with the starting point adjusted by the teacher to allow for your ability. Then you'd work through the problems in order for perhaps an hour, and they would be marked. If you got five wrong in a row, I recall, that was the end, but if you still had some right, you'd continue the next day, and once I was allowed to have a go at the problems the third day, and got close to the last one. By then I was the only one in the class doing these, or maybe there were one or two others. I was then about 9 or 10, and really enjoyed the challenge of being stretched like this. This success probably set me up for admission to Dulwich College for secondary school.

I was also good at geography and music, but not so good at history, which required writing and drawing, and really poor at art. I will say more about music and sport elsewhere. Science at primary school was interesting, but I was less good at doing it, as it involved drawing leaves and such. We were taught something about solids, liquids and gases but I don't recall much. One thrilling experience was seeing an eclipse in the playground around midday. I have subsequently looked this up; it must have been the partial solar eclipse on 2nd October 1959 (my 7th birthday).

I think Warren Road was a very good primary school. It was run by the ferocious headmaster, Mr Stevens, and by his professional wife, Mrs (Celia) Stevens, who was mistress of the top class and quite academically minded, but slightly gentler in attitude. Each of the later years had three streams, and I was in the top stream. Sometime in the final year Mrs Stevens gave me some private tuition in algebra – simple linear equations. It wasn't easy immediately, but I got the hang of it and how to use it for problem solving.

I often got into trouble at school, more for exuberence and not listening than for serious evil. I spoke too much, and ran about after the bell went at playtime. Once the headmaster got his cane out after summoning me to his office, but he didn't beat me.

We got a third of a pint of milk every morning, but this I didn't like, partly because of the cream at the top and partly because of the ice floating in it in winter, and I got out of having it. There were quite good school lunches, and I liked the tapioca (frog spawn) but not the rice pudding. Some dishes I hated, particularly the cheese flan, but we were forced to eat them up

by a witch of a supervisor even when they were cold or had been chewed and spat out once. Like other mothers, my Mum volunteered regularly to scrape food off plates at lunchtime before they were washed.

There were some surprising opportunities at primary school. The Parents' Association raised funds and built a small outdoor swimming pool. We had swimming from May till July each year, preceded by a cold shower that I was scared of. I didn't learn to swim there, only to float, but did learn to swim with more personal lessons at Beckenham pool, and eventually got a 300 yards certificate at Dulwich, although I can only do breast stroke and rudimentary back stroke. Another opportunity was a little gardening. I learned to double-dig, and grew some thin but tasty carrots.

I made some steady friends at Warren Road, mainly boys, and they often visited our house or I visited their houses during holidays. We were enthusiastic about board games, and spent hours and hours playing Monopoly, Go, Railroader and other Waddington games. We also played football and cricket outside in the garden, or at the recreation ground (later site of St Olave's Grammar School). My parents were rather protective of me as regarded cycling and didn't want me to cycle on main roads, so I only got a bike later than others, at age 8. But I had a good scooter for a couple of years before that, and chased after my biking friends on the pavement. It was my biking friend Christopher Bartlett who taught me to cycle.

Other friends included Graeme Wildig, the son of an Englishman and his wife Renata whom he had met in southern Italy at the end of the war. Occasionally, I'd be asked by them to lunch, and hoped for spaghetti bolognese or something like that, but instead we got toad in the hole, the English food the husband required. Another friend was Jeremy Wootten, who was keen on home chemistry and making rockets, and had an older sister he didn't much like. Yet another was Ian Snape, the elder son of a couple who moved down from Burnley when he was 6. We made fun of his Lancashire accent, but by 11 he was the best of us at the board games, and also became a fine schoolboy cricketer. My best friend for several years, until I went to Dulwich, was Colin Crisp. He was calmer than the others I knew, and less academic. He had a rather overbearing older sister, and rather distant older brother, so often came to our house, sometimes too often for my parents. Colin invited me to his family's rather grand detached house in Sevenoaks Road for the 1962 FA Cup Final, which was then just about the only football on television, and I remember Tottenham Hotspur beating Burnley, much to the chagrin of Ian Snape. Colin invited me to at least one later final, as we didn't have a TV ourselves until 1964. Two girls who were my friends for a while were the rather gymnastic Gillian Harris, and later Rosemary Coulson who had moved to Orpington from Scotland and had a long ponytail that I sometimes tugged.

I didn't feel very Jewish at this time, and in fact hardly knew I was. I went to Assembly at school and sang the usual hymns (All Things Bright and Beautiful, Onward Christian Soldiers etc.). There was a Catholic boy in my class, Peter Treloar, who seemed more special. Orpington was actually a very un-Jewish suburb, with no synagogue, quite unlike the north-west London suburbs where my grandmother and Eric and Raya lived. Eric was keen for me to have some experience of synagogue and the Jewish faith. When he took me there for the first time he asked afterwards what I thought of it. My reply was that it was the same number of steps going up and going down, so he realised my mind was elsewhere, at least then. Some years later he arranged for both Anthony and me to attend a class in London where they taught the Hebrew alphabet and introduced some prayers and singing. But I felt this was an alien culture, and didn't continue. Anthony, on the other hand, stayed longer and eventually had a bar mitzvah, but afterwards he also became detached from this world.

At the end of my final year at primary school the Stevenses retired, and the school made a collection to give them a TV, which I presented with the top girl Jane Moore. Five of us from Warren Road then went to Dulwich College – Ian, Jeremy and myself, and two other boys from our class, Nigel Brown and Simon Prideaux. My Dad called Simon the Napoleon of Warren Street (really Warren Road) after a GK Chesterton novel, because he led a group of detectives to work out where some red-pen anonymous letters were coming from. They in fact came from a group of girls having some fun at our expense. Dulwich was then clearly the best independent school for boys serving south-east and south London, though it didn't have the status of Westminster, let alone Eton or Harrow. An alternative school for me would have been Dartford Technical School, rather than the more usual St Olave's which was then in Bermondsey but planned to move out and become the Orpington boys' grammar school. Most of the academic girls went to Newstead Woods Grammar in Orpington.

There was an interview for me and my parents at Dulwich, which aimed to find out more about us, and see if we really liked the school; it also aimed to see if I would fit in, and cope with the commute. I did well because I could mentally work out 13 times twopence-ha'penny. We did a lot of 12 times tables and money at school, because 12 pennies made a shilling, so I knew that 12 times twopence-ha'penny was two shillings and sixpence (half a crown). Then it was easy to get to two shillings and eightpence-ha'penny as the answer. My Dad was asked what he thought about Dulwich being a school where there was boxing. His reply was that he wanted his son to go to Dulwich because it had a reputation for good teaching, and he didn't want his son to be hit about. My mother was a bit more diplomatic. I think the school was impressed by his challenging answer (and his funny accent). Dulwich stopped boxing about two years later, and I never did it.

My parents had quite a busy social life. My father had some friends from student days and the early opera – my mother too. My mother worked hard to maintain these friendships through meetings and phone calls. Most of these friends had got married around 1950 and had children of similar ages to me, who became friends of mine until late teenage years. Most lived in north-west London, so visits involved a full day trip both ways. I got to know the Bakerloo line and Northern line of the Underground pretty well and remembered the names of the stations at an early age. Someone on a bus was quite impressed when I recognised Piccadilly Circus while looking out of the window at age 4. My mother made a nice lunch if friends or relatives came to Orpington, and almost always made her speciality of apple cake with a lattice topping. This had quite hard pastry, and she never made a sponge. A favourite main dish was Königsberger Klops – large meat balls wrapped in cabbage leaves – but these look long to make and had to be tied with cotton. It was easier to make a roast chicken. There was a little wine, and children in their teens got cider, but apart from an evening sherry from time to time my parents were not hard-drinking. My Dad quite liked a small tipple daily while on holiday, even during a break in driving.

Our dining room was actually out of use for about seven months of each year, for at least 10 years. Our family could still have visitors but it was more cramped. The furniture was moved elsewhere, and the dining room became a workshop. Here, the scenery for the Philopera Circle operas was built and painted. My mother first designed the stage sets, then my father built scenery flats from 2 by 1 inch wood, which he put together as an engineer rather than carpenter, using nails and metal corner brackets. Simple pieces were rectangular, but some were triangular or even involved semicircles, and some had to be folding to fit in the room and be manoeuverable out of the house. Some pieces were heavier steps made of planks. When the frames were ready,

canvas would be stapled on, and then sized (horrible smell) and painted by my mother. For the performances, all this scenery was moved by furniture lorry, and returned about a week later. Pieces were initially stored in the house, and some reused the following year, but later my Dad had a big shed built at the end of the garden to store the increasingly large number of pieces. He also purchased stage weights, to save the rental cost. All this was done by my parents alone, although I helped a bit with sawing and nails as I got older.

In retrospect, all the preparation for the operas seems an enormous amount of work. It was amazing, really, how much my parents did each year to bring the operas to the stage. My father even designed the posters and programmes, secured some advertising sponsorship from Guinness, Seeboard and others, and also dealt with the mailing list and aspects of ticketing, although some friends helped with this. It was also initially at their own expense, assisted by small Arts Council grants, the evening class instructor's fees, and some income from ticket sales for the two performance nights. My parents had received some financial compensation from Germany for lost assets, lost schooling, and maybe for the death of my father's parents, and these helped to pay for the house deposit and mortgage.

From 1957 until 1967 the opera performances took place in May or June every year at St Pancras Town Hall, in the big hall whose stage was wide but not very deep. From 1961 to 1964 the performances were part of the Camden Festival, and quite well financed. My Dad could hire some of the best small orchestras, including the Pro Arte Orchestra and the English Chamber Orchestra, and the performances he conducted with them, particularly Verdi's Ernani, were the high point of his musical career. Ernani was a resounding success, with two full houses in 1963 (about 950 people each time), and it was repeated the following year. The heroine was played by Pauline Tinsley, who was on the verge of a professional career at Covent Garden. Another star was Edward Byles. For several years the soloists were young professionals who got a modest fee, but the chorus was amateur, and they needed to rehearse for the whole year. Some took understudy roles, and once or twice there were additional performances for them as soloists at Wimbledon Town Hall, requiring the scenery to be moved again.

On one or two occasions, I stayed for the whole performance week with the family of my mother's weekly cleaner at 5 Glentrammon Close in Green Street Green, and walked to school at nearby Warren Road. I was also taken to dress rehearsals from age 3 or so, on one occasion at least by this family. They had a dog, a new experience for me, and once it jumped out of their open car window to chase another dog while the car was moving.

Among other operas I remember were Glinka's Ruslan and Ludmilla (staged twice, 8 years apart), Auber's Fra Diavolo, Verdi's I Masnadieri and Weber's Euryanthe. These were operas that had not been performed by the professional companies in London for decades, and so music connoisseurs were keen to hear them, rather than yet another Traviata revival. I particularly liked the music of Fra Diavolo, with its memorable overture. The performances were written up in the national press and music magazines of the time, both as preview and review. I have preserved souvenir books with all this material, the stage set designs, photos, and tape recordings. (I don't know how good most of these tapes are, but have transferred the Fra Diavolo tapes to CDs.) There were few competing semi-professional opera companies in London then, and no good recordings of rarer works. However there was some competition from Chelsea Opera and Handel Opera, I recall.

My father hoped to be recognised as an opera conductor and be asked to conduct elsewhere, but his beat and style were not quite good enough for this. Colin Davis rose to prominence around the same time after successfully conducting Chelsea Opera, and my father was always

rather sniffy about him. But my father was honoured to be awarded a bronze medal by the Italian Cultural Institute for reviving lesser-known Verdi, and we dressed up for the evening presentation.

Remarkably, and unfortunately, my father had a heart attack, or at least severe chest pains, during one of these seasons. He was then 42, so this was in 1963 after the very prolonged, snowy winter, and during the Ernani preparations. He walked too fast up towards the station to catch a train. Heroically, he then managed to walk to Orpington Hospital, was assessed and sent home (he walked again, but shouldn't have). Following this, he was laid up on a bed downstairs for about six weeks, being visited by the GP, then slowly recovered, and went back to work and the rehearsals. He would carefully walk up the five flights of stairs to his office in London, twice a day, to get stronger. (My parents in any case disliked lifts after once being trapped in one, on a holiday in Europe.)

A contribution of mine to the operas was to be the photographer for some years. There had been a professional photographer who sold copies of pictures to the cast and chorus, but that maybe didn't pay, and as I had quite a sophisticated camera by age 12, I started taking photos at dress rehearsals. My Dad bought the fastest easily available 35mm black and white film (1000 ASA), and I took pictures at 1/30 second, without flash. The pictures weren't bad. I tried for a while to become a better photographer, and took photos regularly on holiday, mostly in colour. Decades later I got fed up with lugging an SLR camera and possibly an extra lens about, and now leave the photography to Anneli, who has a modern miniature camera with large memory.

Philopera Circle declined from about 1965 onwards, and stopped in 1968, when I was 16. The Camden Festival had withdrawn support, and costs went up. There were also several new, competing groups. My Dad produced one of the later operas, Bellini's Il Pirata, leaving the conducting to a talented Hungarian, George Badacsonyi. In 1968 there was a performance of a less exciting neglected masterpiece, Thomas's Hamlet, at the University College theatre, and not at St Pancras Town Hall. After this my Dad tried to pass the directorship on to George Badacsonyi, but he couldn't manage both the organisation and the musical direction, so that was that.

After Philopera finished, my father acquired new interests. He took up the cello, and read new things, especially history. (More about the cello in the Music section.) He also learned to read French quite well, though hardly to speak it. His health declined in the 1970s, and he had some kind of epileptic fits – shaking, but staying upright. He died after a stroke, rather suddenly during a visit to me in Cambridge. He probably would have been pleased that his end was in Cambridge, a city he loved. The following day, 2nd October 1977, was my 25th birthday. His body was transferred back to London for cremation at the Golders Green crematorium, in Hoop Lane, NW11, where all our family and friends could easily attend.

My mother had got tired of Philopera a bit earlier, perhaps by 1964, because of the disruption to the house. She had also stopped singing in the chorus. During one year at least, she didn't do the stage sets either. As I needed less attention after moving to secondary school, and just before, she started a new career teaching languages. She qualified as a teacher of English as a Foreign Language (EFL) and for most of the rest of her life taught this, being in on this growing industry quite early. She enjoyed most of all teaching youngsters sent to England for a few months to stay with a host family and learn English; most were from Europe but some from Iran etc. Her classes weren't very academic, and she only taught up to the lower levels of EFL exams. She was amused by her students saying, when they first arrived, that they were good because they had studied English 'since' (seit) 6 or 8 years at school. In her classes she focussed on colloquialisms,

and up-to-date newspaper and magazine articles, whose content and style were unfamiliar to the students. She also taught German part-time at adult evening classes and for some years at a small private school for girls in Beckenham. This career was vital for her after my father's death.

Her teaching was partly in Orpington, and partly elsewhere in south-east London and in central London, and through it she made new friends who lived in Orpington and nearby. Especially she was friends with Marlis Woodward, a non-Jewish emigrée from Germany. So she had no intention of leaving Orpington and moving back north of the Thames, as she and my father once thought of doing, around 1963. She lived a long time as a widow, and for a few years struck up a relationship with a neighbour Herbie Rowe who was a widower. But they didn't live together. She was thrilled with my academic progress, and when I later met and lived with Anneli, and finally when Ben was born. Then she became less well, developed bladder cancer, and lived about three years with that, in and out of hospital, until she succumbed at Bromley Hospital on 1st May 1994, aged 68, and was cremated at Golders Green. Eric and Raya were considerably older and aged more slowly, but they had had various illnesses, and they died a year or two earlier than my Mum (Raya in 1992, Eric in 1993), and were also cremated at Golders Green, as was Puttel nearly 20 years later.

5 The Antrich Family and other Friends

One of my family's best friendships was with the Antrich family. David Antrich's parents were Jewish immigrants from Krakow (although his father was born in Hanover), who married in England in 1911. David was born in 1921, the same year as my Dad. He studied electrical engineering at Battersea Polytechnic and maybe also at Imperial, and became a friend of my Dad at that time. David married Etain, an Englishwoman who formally became Jewish before their wedding. They had a daughter Janet and then a son Richard who was just slightly older than me. About eleven years later they had another daughter Elizabeth. Richard in particular became my friend as we grew up, and I'm still in regular contact with him, as well as exchanging Xmas cards with Janet's family. David became the electrician of Philopera and managed the lighting. We used to exchange visits with the Antriches at least once a year in each direction, and my Mum would meet Etain more often. Initially, they would regularly visit us in Orpington for a Boxing Day lunch, but later we would meet in central London for this. We usually visited their house at 36 Oakington Avenue, Wembley Park in summer, and I cycled around the Wembley Park streets with Richard and others after learning to cycle. Richard also had a fine electric train set to play with. I often sent the engines off the track, but Richard was a more skilled train controller. Later Janet took up the flute and Richard the bassoon, but they gave these up after a few years. Richard became a companion on several youth hostelling and other walking holidays from when I was 16 onwards (this will be described elsewhere). He also learned to drive without difficulty, and much later helped me to be a proper car driver on a visit to Boston when I was at MIT. Richard's main enthusiasm was for Formula 1 racing, and occasionally he went to spectate, but he didn't participate.

David Antrich worked in later years in the UK computer industry, in Stevenage, so the family moved out to Codicote, Hertfordshire. David visited America several times in connection with his job, and was enthusiastic about the US, which grated a bit with the left-wing attitudes of my Dad, but they remained friends. David lived for many years after my Dad, until 1998, and Etain much longer still, to the age of 94. Janet and Elizabeth each married and had children of their own, and now grandchildren. Richard worked in electronics in Harlow for many years,

testing avionic equipment, but the company eventually folded, and he then got jobs in catering, at Stansted Airport and later at John Lewis in Welwyn. He didn't marry, but at about 40 met Christine Fisher, and they have a happy partnership, living in Stanstead Abbotts, Hertfordshire, in a flat between the River Lea and the New River aqueduct.

Another close friendship was with the Foster family. Rita, a refugee from Hamburg, met my mother by chance while they were both out walking their dogs. Rita by then knew my father through singing in the opera group, and encouraged my mother to join too. This was how my mother and father met. Rita herself met and married Peter Foster, also a refugee I think, who had changed his name. Rita and Peter, and my parents, had a boating holiday on the Norfolk Broads in 1946. After their marriage they had two children, Helen about my age, and David a couple of years younger. David was rather small for his age, and was mercilessly teased by Helen. My first crush was on Helen. She had a caged bird for a while, which at some point managed to fly away, and she was devastated.

For many years Mum was frequently on the phone to Rita. At family gatherings, there were often animated discussions of significant issues, even of a philosophical kind. I got heavily involved when in my teens, and once Peter said he was impressed by my contribution to arguments like these. Peter had some ambition for success in the performing arts, comparable to my parents' Philopera Circle, and he created a version of Chaucer's Canterbury Tales, suitable for the stage. In fact there was an extremely successful Canterbury Tales musical that started about then, and Peter claimed the author of its lyrics had pinched the idea from him at some meeting, or through a publisher reviewing his manuscript. There was some legal action but Peter didn't get anywhere.

Rita and Peter eventually split up, but Rita then met a quite middle-aged man Jack Davies, who was reluctant for a year or two to commit to his first marriage and two step-children, but eventually he agreed, and was very happy in the arrangement for decades. Jack was a very anglicised Jew. Helen later married and had children, but sadly died of breast cancer before she was 50. David had a number of unfulfilling jobs, partly with Harrow Borough Council where Jack worked. Eventually, at age about 30, he studied Torah, met an orthodox Jewish woman and emigrated to Jerusalem, where he worked in an orthodox community as a scribe, and studied Torah further. They had six children I think. Jack said he enjoyed visits to Israel with Rita to see them, but it was a relief to get to the seaside resort of Netanya after a week at David's community. Rita had a beautiful singing voice, and late in life regularly sang in a liberal synagogue. She also entertained people with popular songs at parties.

Yet another family we met fairly regularly were the Goldsmiths (not related to my mother's family). Rudi, a refugee and friend of my Dad, had met and married Jean, an Englishwoman. They had four children, one unfortunately disabled. The oldest son David was my friend. He had the best stamp collection of boys I knew and I think he dreamt of a career as a stamp dealer. Rudi was rather exuberent, and Jean very laid back, not worrying where all her children were. They shared the early holiday with us to Swanage, and Rudi threw me up and down in a bus for fun. When I hit the ceiling, my parents were horrified and worried. Rudi ran a Market Research company and became the richest of our close friends. He bought a detached house overlooking Hampstead Heath extension – 11 Wildwood Road, NW11 – for 11000 pounds, twice what my parents could afford, and now estimated to be worth about 6 million. My mother worked briefly for Rudi on surveys, before starting her teaching career, and on one of these she went to Gillingham and elsewhere to see if people had noticed Guinness adverts on billboards. Guinness for several years ran a series of amusing advertisements, but I don't know how this

affected their sales – probably not badly, as they are still going very strong. Rudi and Jean divorced and he went off to America, but Jean and my Mum remained friends.

A distant relative I hardly knew, but a better friend of Eric and Raya, was Walter Gotell, who lived in a mansion in Borehamwood. He was in the film industry, where he played the villain Morzeny in From Russia with Love and other heavies in later films and TV dramas. I had a few second cousins whose parents were Grabowski cousins of my Mum (some becoming Oppenheimers by marriage), both in London and Birmingham, but we hardly saw them except at special events like bar mitzvahs, funerals, and at my parents' Silver Wedding party in 1973. The Grabowski family changed their name to Gray after settling in England. The trip to Birmingham for Peter Gray's bar mitzvah was interesting, because I'd never been there before, and there was the Bullring development in progress. We went by car shortly after my father had bought one in 1961, along the rather new M1. There was so little traffic that my father thought it safer to drive at about 50 or 55 mph in the middle lane of the three, where there was ample space and overtaking the occasional slower vehicle was easier. He was stopped by the police and told that at that speed he must go in the slow lane.

Finally, my parents and especially my mother had as friends Margo Freeman and her husband John, and they had two daughters. Margo's mother, who rather tormented the girls, lived in an annex. She and Margo were Dutch, and I'm not sure if they were Jewish refugees. John was English, and should have protected his daughters more from his mother-in-law's abuse. The girls became teenage dropouts, and one ran away. I lost contact after that.

My mother needed this circle of friends, but my father less so, because he was happy with his many acquaintances through work and music, and with his private reading. It was very important for my Mum to maintain this circle, which she extended to include new friends after my father died. Towards the end she would be friendly towards almost everyone, including ticket collectors at the station. For me, the effect was that I had a busy social life as a child, at school, and in the holidays. Later, I became more reserved in larger social groups and parties, but was busy instead working at my research and making many friends and acquaintances through that. So my social life is largely talking shop. A few of these old friends remain, and Anneli knows and likes Richard and Christine, and she also knew Rita and Jack. (I'm not certain what happened to Rita and Jack but they stopped sending Xmas cards about 10 years ago.) Our son Ben was not closely involved in this circle, and the friends he has made at school and university, and maintained over a considerable number of years, are his own, and several of them we hardly know at all.

6 Early Travels

My parents were very keen to go on European summer holidays, as I mentioned earlier, and started these before I was born. They continued after I was born, to as far as Italy and southern Spain. The first time I went along with them was in 1958, when I was 5. These trips were well planned by my Dad, and were almost always to somewhere new. They were to Western Europe, because it would have raised suspicions with the MoD if he wanted to visit the East. Until 1960 the trips were by train, and after that by car.

It would take far too long to recall all details of these yearly trips. They were generally four weeks long, as my Dad used most of his six-week holiday allowance for this, and the rest for the opera. We have diaries describing each year's trip, with the basic facts about journeys, hotels and places visited, but also many personal observations on the countries and people we

came across, the quality of the food etc. There are also photo albums, with B+W photos until about 1960 and then increasingly more in colour. Each photo received a caption, sometimes in an amusing (now embarrassing) family style. I myself contributed to the diaries and photo albums from quite early on. Because of these records, including precise route maps, I can recall where we went rather well. But I don't properly remember the places themselves very much from that time. Over the years, however, I have been back to many of these places again with Anneli and sometimes Ben, and can visualise them better. Through this travel, I became much more knowledgable about Europe and its languages than most of my contemporaries. I liked geography and maps, and travel stimulated this. Some of my class mates had travelled further (one girl came from Hawaii), but most had one- or two-week holidays in Cornwall and the like.

My Dad visited the national travel bureaux in London to collect pamphlets quite long in advance. Train tickets were pre-purchased, and hotels booked by post, often with half-pension. Trips usually started with a boat train from London to Dover, or perhaps Harwich, and one year at least we sped through Orpington, the place we had started out from, and could see our own house looking down from the embankment. We always stayed in hotels, usually of 2-star or 3-star grade, and in the nicest resorts for up to 5 days. My parents brought three suitcases, one on wheels. The itineraries carefully combined cities, with their museums, churches and other historical monuments, with places in the mountains and by the sea. My Mum was particularly keen on having some fun and sunshine by the sea, with me. My Dad had more stamina for historical sights, though my Mum particularly enjoyed art galleries. There was also a little shopping on these trips, and occasionally an opera visit or visit to a zoo.

I didn't mind the rather long walks and visiting churches, but at first was let off what for me were the boring art galleries, where I tended to run about. So my parents alternated there, and perhaps took me somewhere more fun, a playground or a park.

Things I really liked the first year were the endless procession of trains along the Rhein in Königswinter, with the level crossing gates going up and down, and seeing the barges on the river nearby. Later we met my Dad's aunt-in-law Tante Helena in Überlingen, and we ended up in Klosters in Switzerland, where I was quite strong walking on the mountain paths. It was not serious climbing, but my parents used mountain walking sticks. We visited Tante Helena again in Basel just before going home on a night sleeper. In those days, of course, part of the European experience was a Channel crossing by ferry. Another part of the European rail experience was long waits at frontiers while passports were checked. (I remember a particularly long one at Welkenraedt, on the way to Germany from Belgium.)

My Dad got guidance in the planning from Baedecker guides, some rather old, and in summer 1959 our first stop was in Bruges. The walk with suitcases from the station to the hotel seemed exceptionally long, because it turned out the station had been moved after the war. That year's holiday continued via Saverne in France and Wengen in the Bernese Oberland to the Italian Lakes (Pallanza) and the Italian Riviera (Santa Margherita), returning overnight from Genoa to Paris for a one night stay, and then home. I was too young to really appreciate Paris, but we have a souvenir colour photo of me on the Pont des Arts.

The following year was a German holiday again, with a stay in Bernkastel on the Mosel, and then via Ulm to King Ludwig's castles in southern Bavaria. We stayed at Hohenschwangau, and visited the two castles there along with an American tourist who exclaimed, on being shown some treasures, "Solid Gold – Ridiculous!" We made bets on the number of charabancs arriving with visiting tourists. There were quite a lot. The hotel there was quite good, and the brochure showed they had 11 cooks. We were reminded that too many cooks can spoil the broth, because

the soup was similar each day, just a bit more dilute. In Munich I was keen on counting things, and spent most of the time counting trams, which I had not seen in England. But my Dad also took me to the Deutsches Museum of science and technology, where it was fun to turn the knobs, as well as see the mine mock-up. That year's holiday ended in Vienna, where we stayed with some distant relatives in Alser Strasse, and visited the various monuments to musicians, among other things. In the Alser Strasse flat there was a sewing machine with oscillating foot drive that I enjoyed trying out one evening while my parents went to the opera. We returned by night sleeper from Wien Westbahnhof to Ostend, and then by ferry to Dover.

1961 was the year my Dad got a car. It was a 2-door Morris Minor, elephant-colour, which he bought from the firm Boon and Porter where he had worked from 1941 to 1943. For holiday planning, he now obtained from the RAC office two complete European road maps. One was for the plan, and the other to draw in the final route afterwards. I did much of the map-reading. The three of us and the three suitcases just fitted in the car. We went that year to Scandinavia, taking a ship from Tilbury to Gothenberg, and making a tour south to Copenhagen, north via Elsinore Castle, Karlskrona and Kalmar to Stockholm; we then headed west to Sunne and Rottneros House, Olso and the fjords of Norway before returning to Gothenberg and back to Tilbury. The car had to be lifted by crane on and off the ship. The outward 36-hour journey was very rough, and even the crew were surprised by the storm at that time of year. Fortunately the return was calm and sunny, and we could enjoy the meals.

This was the longest distance of any summer holiday, with very long days on the road. My Dad chose a mainly Swedish holiday because Swedes drove on the left. Driving on the right really was trickier because sight lines were poorer, and overtaking very difficult. There were very few motorways anywhere in Europe then, for eating up the miles. Between Kalmar and Stockholm there was a minor pile-up en route, after a driver in front suddenly turned off for a picnic stop. The car behind dented our rear bumper. In Stockholm, we stayed in a private home. Booking hotels was not so important when we had a car, but Stockholm was very full, and we needed the tourist office to find something for us. I wondered what would happen at the Swedish-Norwegian border – would there be a bridge to take traffic to the other side of the road? Of course there wasn't, and drivers just carefully moved across. The road towards Oslo was as good as the Swedish roads for a few miles but then it became a gravel track; most Norwegian roads were like this in '61 except in and close to the towns. Journeys at about 30 mph seemed to take ages, and endless thick mist crossing back over the Hardangervidda plateau near Geilo also made for a tricky journey. But we enjoyed the Vigeland Park and Viking ships in Oslo, and the Hardanger Fjord. In Balestrand on the Sognefjord I got badly stung by a mosquito, suffering for about three days. There were few hotels in Norway at that time, and the ones where we stayed are now famous 4-star or 5-star resorts. Scandinavia was rather expensive compared to France and Germany, so in Sweden we often had a buffet lunch, and a sausage from a street vendor in the evening. My parents never went to Scandinavia again, except briefly to Ribe in Denmark, and my father never went to Finland.

I shall be more sketchy about later European holidays. They tended to oscillate between the German-speaking world (where my parents were fluent) and the rest. In 1963 it was to Spain, and we put the car on a train both ways between Etaples, near Boulogne, and Narbonne, near the Spanish border. The brilliant sunshine led to some great photos. In the Prado in Madrid I was asked to sit for some time in front of the large painting The View of Zaragoza, the city we had visited a few days earlier, while my parents explored the galleries. From that time I started to enjoy visits to galleries more. I got very sick near the end in Pau, after perhaps too

much jumping about in the cool sea in Zarautz, near San Sebastian. That meant we missed a trip to Andorra. At that time I collected country and capital stickers to go on a duffel bag, and picked up many of these, including Liechtenstein and San Marino. Missing Andorra was therefore a disappointment, and I still have not been there. My parents brought back three or four bottles of wine from Spain, hoping three to be duty-free, but I was too young to be entitled to an allowance, and the customs officer in Dover charged 14 shillings or so. The wine had cost only six shillings a bottle in Spain, so my Dad asked if he could open two bottles and pour the wine into the sea. The officer said he was free to do as he liked with it, Sir, but since the wine had been imported, the duty still had to be paid.

In 1965 we went to Italy, as far as Rome, where by chance we met the Foster family in St Peter's Square. (We did know they were on an Italian holiday.) In smaller towns like Lucca, I'd guide my parents to the sights, following the numbers in a brochure. I particularly enjoyed exploring the town's layout, following walls and going in and out of gates. In 1966 we watched England win the World Cup in the Bierstube of our hotel in Reutlingen, near Tübingen in Germany. I think there were two other English people there. The Bild newspaper shouted the next day "Das war kein Tor!" (That was no goal!) about England's third, and I suppose modern science suggests they were right. It was good that England won 4-2, and we were proud of the achievement for many years, particularly as the Germans won most of the important football games against England subsequently. Later, in Salzburg, I was offered and ate a whole Mozart Kugel when I was only supposed to eat half of it. My Dad missed out, and was sorely disappointed.

In 1968 we went to the Netherlands and north Germany (and also Ribe in Denmark), including a return Pan Am flight from Hanover to West Berlin. That was the first time I flew, and the only time my father returned to Berlin. He showed us some familiar places from his youth, in Charlottenburg and the Kurfürstendamm. Once, we walked out to the Grunewald and the Havel, and got lost, but were rescued by an English army officer based there who had a map. My father was not keen even to stare across the Wall or through the Brandenburg Gate at East Berlin – he claimed one couldn't get a good view of what he thought of as the socialist paradise. I don't think he was aware of the economic failings of the Eastern bloc. He did though admire the prosperity of West Germany, which seemed then a long way ahead of the UK for the general population. He also admired what General de Gaulle had achieved as President of France, and didn't support the UK joining the Common Market, because he thought the UK would spoil it. (That was rather prescient, now that Brexit is underway.) My mother was more critical of the German complacency, and the conservative fashion of hats among middle-class and elderly German woman. By 1968 London was swinging, but most of Germany was not.

We were generally well treated in Germany, having British passports, and a car with a GB plate. My father spoke German fluently, and possibly was recognised as a Jewish refugee, but there was only one occasion, in Bremen, where we encountered an antisemitic hotel keeper, and stayed somewhere else. My mother could claim, like me, to be truly English, and when asked how she could speak German so well, would say that she had learned it "in der Schule", which was a white lie. I spoke rudimentary German, but picked up useful colloquial phrases, and quite a good accent.

The UK in the late '60s was economically something of the sick man of Europe, and from then into the '70s, private people were subject to foreign currency restrictions. It was very difficult to have a 4-week European holiday on 50 pounds per person. The package holiday flourished in response, because the cost was paid at home. However, we never took one. Helpfully, France

and Italy set up petrol coupon schemes, where you paid for petrol in advance in sterling. My father sometimes pleaded for a cheap rate at a hotel, because we had "keine Divisen" (no foreign currency). Once, he managed to get some extra Swiss francs through Tante Helena, offering to pay through sterling bank transfer, or something similar. This way, and through careful economising, the holidays continued. For example, with meals we would have tap water and not bottled, but once a barman in Ostend quickly opened three small bottles which we had not really ordered, and then insisted we pay. There was argument and pushing, and later my father wrote to the mayor of Ostend complaining. There was some apology but no compensation.

One thing I did a great deal on holiday was spot things for my I-Spy books. I eventually had about 50 of these, some repeated or in new editions. If you spotted an item in one of these books, you noted the place and date, or perhaps answered a question, and got points, from 5 to 50 depending on rarity. European trips, always in different directions, were a great opportunity to build up a good score, although I also did I-Spy in Orpington, London and elsewhere. Sometimes we bent the rules a bit, because something in Europe was not exactly the same as the British item in the book. In the evening I often totted up the score. As the years went by, from age 6 to 12 or so, I sometimes got to the key threshold of 1250 points in one book, or the complete total of 1500, to claim a feather from the publisher. After sending in the book to Big-Chief I-Spy at the News Chronical newspaper, signed by an adult, and for a small fee, I received a coloured feather, as well as the book back. The feathers were sewn by my mother into a Redskin headband, which I was proud of but hardly ever wore.

Through I-Spy, and travel, and poring over factual books, I acquired a good general knowledge, which was appreciated in schoolchildren at the time, but now called trivia. I wasn't so good at understanding people, though never anywhere near autistic. However, I was baffled for years by the entry 'a Character' in the I-Spy People book. It was explained a little what a character was – someone who dresses or behaves unusually – but I didn't really get it. Actually, my father was a character, with his hard work for Philopera, funny accent, and his combining working for the MoD with being an armchair socialist.

I ceased joining my parents every year on their European holidays around the age of 20. In 1971 I got into the UK team for the IMO (International Mathematical Olympiad, see elsewhere) and the holiday had to be rearranged to be a bit later, but I still went along. Not long after this, the grey Morris Minor was retired, and replaced by a slightly larger blue Renault. In 1972 my summer holiday was with student friends to Austria, after a short spell working at Hamley's, the toy shop in Regent Street. In 1973 I joined my parents again, and we had a memorable few days in Florence, where I particularly remember the church of Santa Maria Novella, and the Michaelangelo chapel in San Lorenzo. Our visit was during a cholera outbreak in the south of Italy, and the authorities reacted by closing camp sites throughout the country, so cities like Florence got very crowded, and we avoided eating fresh fruit or drinking tap water. In 1975 I joined them one more time for a week, then continued to CERN where I was a summer student. They eventually caught up with me in Geneva for a couple of days. In '76 and '77 they did more modest trips to very small German and French destinations, including Dillingen, and Mayen, as they had seen enough of the big cities, and they combined these trips with a music course at Berwang in Austria. And then that finished, with my father's death.

My mother was still a keen traveller for years afterwards, and she came to Paris and America when I was there, and also went to Berlin again with her friend Marlis. She once visited Helsinki and Aittola (Anneli's family home in Utajärvi, Finland) when Anneli, Ben and I were also in Finland. She spent some time in Helsinki learning elementary Finnish, to avoid having to be

all the time with us. We also went together to Savonlinna, and saw The Flying Dutchman at the opera festival in the impressive castle there. She planned, in her last year or so, to go on a package tour to Kaliningrad (the former Königsberg) but wasn't well enough. This was just after the end of communism in Russia, and Kaliningrad had a reputation for being quite lawless. In any case most of old Königsberg had been flattened by British bombing, and not rebuilt by the Russians.

Going back a while, my mother was concerned that we never saw Britain, so she insisted we take some short breaks there, usually in spring. A first break, when I was 5, was to Cambridge during a cold Easter-time. I enjoyed chasing round the college courts, at some point getting lost at Trinity (I think) behind a doorway or up a staircase. My parents thought that I would clearly be happy in Cambridge as a student some day. A later trip was to the Kent and Sussex coast – Rye, Bodiam and Chichester – and another was to the Cotswolds, as far as Broadway, Worcestershire. I got a taste for the charms of the English countryside, villages and towns, but I think my father thought these charms rather small scale compared with the grandeur of both the landscape and monuments in parts of Europe. Also he found it rather hard to communicate effectively with English people he had hardly met, who struggled to understand his accent. He quite enjoyed an occasional pub drink, but my mother less, and children were not allowed in pubs at all at that time.

My father actually travelled much more, and further, because of his job. He went to Patricroft in Greater Manchester, Aberporth on Cardigan Bay, and Droitwich in the Midlands, and had been on a course at the Royal Military College of Science at Shrivenham near Swindon. But he rather hated the North, with its frequent rain, though liked Edinburgh and the heather and 'shivering sheep' he saw on a car trip from there to Perth and the nearby highlands of Scotland. Later, he regularly, almost weekly, went to Farnborough (Hampshire) or Frimley, or Hatfield or Stevenage, to check on progress with procurement contracts, and this he liked better. Once for his work involving radomes, he needed to go with some colleagues on a 3-week trip to the US, including Atlanta, a weekend car tour of the Smoky Mountains, Washington and New York. This was the first time he flew since arriving in England, and we saw him off at Heathrow. He wrote letters home everyday, commenting especially on the size of the hotel table lamps, and the exaggerated descriptions of food on restaurant menus. Once, in a hotel, a security guard came round at night and insisted he lock his door from the inside. After this trip he was keener to fly, and that is why we flew to Berlin a couple of years later. My mother flew several times in later years.

I was aware as a teenager how little of Britain I still knew, and grabbed opportunities to see and experience more. My teenage scout camps and later youth hostelling provided such opportunities, and will be described below. I am still comfortable travelling in Europe, including to some extent in Eastern Europe, but I also enjoy travelling in Britain. It has become easier, with B+B's and hotels usually being centrally heated, and the number of eating places much increased. And it doesn't always rain in Manchester!

7 Dulwich College

Undoubtedly, the most important formative experience of my life was to be a pupil at Dulwich College. If I had not gone there, my life would almost certainly have been very different. Dulwich was a large independent school for boys housed in three connected main buildings of the Victorian era in a splendid italianate, partly gothic style, with all kinds of terracotta decorations and a

clock tower with spire. The school had been founded by the Shakespearean actor Edward Alleyn in 1619, as a small school in Dulwich village, but the college estates extended up to Sydenham Hill, and the school became rich, first through awarding rights to the suburban railways crossing the land, then through the development of the Crystal Palace exhibition on the hill in the years after it moved from Hyde Park, and lastly because the area subsequently became fashionable and villas were built.

In about 1870 the school could move to a new site on Dulwich Common, overlooked these days by the Crystal Palace TV mast about a mile away. The original buildings were designed by Charles Barry, son of the architect of the Houses of Parliament. Various newer buildings went up later, including a Lower School block, a swimming pool, and importantly for me a Science block. The school had 1400 pupils when I was there, of whom about 10% were boarders. But I was one of the majority of day boys, commuting by train from various parts of south and south-east London. My train journey was 25 minutes from Orpington to West Dulwich. There were about 100 Masters, and almost all had degrees, many from Oxbridge.

Dulwich had been a moderately successful fee-paying school comparable to other London independent schools, but was not one of the great public schools. Then, in the 1950s, the school commenced the Dulwich experiment, in which two thirds of the intake were supported by full scholarships from the Inner London Education Authority and neighbouring local authorities like Bromley (which included Orpington) and Croydon. Entry was then highly selective. My parents and the rest of the family were thrilled that I got into Dulwich on one of the scholarships, and my parents were happy to pay the ancillary costs for the uniform and sports kit. As mentioned earlier, five boys from Warren Road went to Dulwich that year – quite a few from one primary school. Before I left Dulwich, it was just about the most successful school academically in the country, competing with Manchester Grammar School to boast having the largest number of pupils gaining Oxbridge Scholarships and Exhibitions. I myself got a Scholarship to St John's College, Cambridge.

On arrival, I acquired the uniform which was mainly black, topped by a black cap with blue stripes and a crest. My Dad thought we looked a bit like undertakers. Blue and black were the college colours, and the college flower worn at special events in summer was the cornflower. There was a motto and school song in Latin, and much of the other paraphernalia of a public school, like Houses for sport, detachable shirt collars that had to be fresh each day even though a shirt could last a week, a Combined Cadet Force, and Masters sporting gowns. Much of this was simplified in later years, and caps were abolished. I was in Marlowe (the six Houses were all named after Elizabethan/Jacobian figures).

In the first year we were put in one of six forms. These mixed a few boys who had been in the prep and junior schools with the intake of scholarship day boys and some new fee-payers. These distinctions were carefully hidden. I never really knew who was fee-paying, although it was more obvious who was a boarder. Three of the forms were the more academic streams, and three less so. We were assigned to a form, based (I believe) solely on a single test on the first day, in mathematics. I got 84 out of 100 on this, and only one boy got more (85, I think, or possibly he also got 84). This was Colin Vout. So he was in form 1A and I ended up in form 1B. I was surprised to have done so well. Apparently, I wasn't just good at maths by the standard of Warren Road, but compared to some of the brightest boys in south London. I didn't think the test was very difficult. One question I got right (and I don't think I had previously seen it) was whether it is possible to put dominoes on a chess board whose diagonally opposite corners have been removed. The answer is no, because each domino covers one black and one white square,

and the whole board has equal numbers of these, but the diagonal corners have the same colour. I suppose this one test set me on the path towards an academic career in mathematical science – although not in pure maths.

In the first year we started French and Latin. There was a lot of vocabulary learning (vocab), which I could do but didn't really enjoy. Several of us from Orpington used to test each other on the train journey, in preparation for a class test. In the first couple of French lessons the teacher just spoke, using phrases like 'Qu'est que c'est que ça? C'est un stylo.' which is very hard to write down. My mother thought this was great, as it was her style of teaching, but things soon got more academic, with reading and writing. I gave up Latin in favour of German after two years, and got O-levels in French and German. It was only through school, and not home, that I systematically learned to speak, read and write some German, but the sound of the language and even the grammar came easily to me. History and Geography were quite interesting, and I still remember some of the history we did, about the Tudors, the Stuarts and the Civil War. I didn't take either as far as O-level, doing Physics, Chemistry and Biology instead. One Geography teacher was Australian, and I did a very good project in the second year on Australia, after visiting Australia House and the offices of the various Australian states in London to collect materials, and including labels from tinned and dried fruits. My tea cards of Australia were also reused.

I did quite well at the sciences, but didn't like them all. We had Physics and Chemistry in the first year, then Biology and Chemistry in the second. First year Physics wasn't easy to understand, because it involved things that are genuinely tricky, like batteries and electrolysis, magnets, and electrical conduction. Biology I didn't like when it got to the dissections of worms, rabbits and frogs. In Chemistry, I was less tolerent than others of the sharp smells of sulphur dioxide, hydrogen sulphide etc. But I liked the basic concepts of chemistry, and was good at writing the material up in homework notebooks. I liked learning about key elements like sodium, and reaction equations, and the idea of valency. The theory appealed more than the experiments. Physics got better when it was related to mathematics, through the ideas of velocity and acceleration, force and energy, and fields. For five years I had the same physics teacher, Mr Bain, and he is someone I have to thank for helping me on my way.

There was also English. English was not my forte, and I almost failed O-level English. (It didn't help that I arrived ten minutes late for one exam after oversleeping and missing a train.) But I did better a year later in O-level English Literature, for which we had studied Shakespeare's Twelfth Night, and Alan Paton's Cry, the Beloved Country about the plight of South Africa. We had earlier been studying Shakespeare's Romeo and Juliet in class, whose plot is easier, but our teacher Mr Thornton decided that boys probably wouldn't be able to write well about the more obvious love story, and would cope better with the intricacy of Twelfth Night.

To improve in English, we were encouraged to read widely. As a child, my mother had read Winnie-the-Pooh books to me, and the railway engine series by the Rev. W. Awdry. I must say I preferred to hear more about cheeky Percy, proud Gordon and childish James than about anxious Thomas the Tank Engine. However, after learning to read, I always liked non-fiction more than stories, and read hardly any fiction for many years. Exceptionally, I did for some reason enjoy reading The 101 Dalmatians several times through. A bit later I devoured Sherlock Holmes.

After taking O-level English Literature I became keener to read fiction. Let me digress a bit and jump forward several years to say something about this. I ploughed through most of Dostoyevsky's Brothers Karamazov, and then got captivated by Kafka – both The Trial and

The Castle. Books describing personal struggles I liked, especially Pirsig's Zen and the Art of Motorcycle Maintenance, and also The Glass Bead Game (although it gets frustrating that the game is never properly described). Most of all, these days I like books about personal relationships, including all of Jane Austen's masterpieces, and those of E.M. Forster too, which I have read during the years since leaving school. I'm indebted to my mathematician friend from student days, Martin Gant, for recommending Pride and Prejudice. I had thought that this would be of less interest to me as a male reader, but of course that was my prejudice. Later, when I had more time, I read Tolstoy's War and Peace. It probably is the greatest of all novels, with its enormous range of characters and so many different things going on. Each chapter is thrilling by itself, and it appears to translate into English very well. It took me about two years to get to the end, as I tend to read slowly and carefully. After a break of some years I read Anna Karenina, and also Madame Bovary, whose tragic plights are rather similar.

Not everything I have read is so serious, or old. I've enjoyed Julian Barnes' clever writing, and David Lodge poking fun at academic life. Parkinson's Law is very funny, and I have re-read its chapters on topics impinging on my recent professional life, like elections to high office, building projects, and retirement. I found even the Iliad witty in places.

One good thing I learned from Mr Thornton at Dulwich was rhetorical devices, like using Therefore, Moreover, Furthermore and Nevertheless, rather than a string of Buts and Thens; also, how to use semicolons. I still teach this stuff to my graduate students and collaborators. We read in class The Importance of Being Earnest, and were taught (in a rather witty way) the differences between Irony, Wit and Sarcasm. My English writing has slowly improved since school days, but it has required hard work, and I have been rather self-conscious about it. I'm also a conscientious editor and re-editor of texts, and aim to be pedantically accurate. My papers and books have almost no textual or mathematical typos. But my later writing doesn't have the authority and style of American theoretical physicist Ed Witten, or even the convincing manner of my occasional collaborator and friend Martin Speight. Curiously, at school, we weren't taught much more of substance in English, except such trivialities as the difference in spelling between embarrass and harass, but through practice we slowly learned to speak and especially write more coherently and with better organisation. I never learned to make a good speech, or be good at debating, lacking the ability to lighten the tone, or make jokes. Nor could I do imaginative writing well, with the exception of once getting my poem The Excavator published in the school magazine; this described the machine digging a deep trench for a large sewer outside our house in The Avenue. But I think I can organise a maths lecture well, although my students don't always agree.

In Maths, which I enjoyed, we were taught in the first year by our form teacher the Rev. Nick Earle, who wore a dog collar. Actually his Christian belief had ceased, and he turned into Mr Earle a year or two later, with ordinary clothes. We started with basic Euclidean geometry, and by the second year learned trigonometry (tangents first), quadratic equations, and also did a lot with trig and log tables. I got a slide rule at this time, which was particularly useful in science classes. In the third year we started calculus. By the time of O-level Additional Maths three years later we were really good at school calculus (a British strength, compared to High School teaching in the US). In the fourth or fifth year we had a very ambitious maths teacher Mr Nunn who taught some really sophisticated Cartesian geometry, like lines of conics, going beyond coaxial circles. He got rather stressed by the demands of a very clever class – by then we were setted for Maths and I was in the top set with Colin Vout and another talented mathematician Christopher Hills – and unfortunately he had to take some months sick leave to recover.

From the third year (when I turned 14) we were in the Middle School, based in the Barry buildings. In the Lower School there had been fierce discipline, exercised by the prefects on behalf of the staff. I often got into trouble and sometimes into detention. As at primary school this was for doing silly things, like talking while lining up to go in. I wasn't really bad, like some boys who threw other boys' caps out of the train window going home. But in the Middle School the oppressive discipline relaxed and I didn't get into trouble anymore. Curiously, I didn't feel that I myself had changed, just that the system was more tolerant and accepted me as I was. Later I became a prefect, and hardly ever sent boys for punishment, but rather anticipated trouble and warned boys to be quiet or play somewhere else.

There was some Religious Instruction, and daily Assembly. I was one of the Heathen (unlike at primary school), and waited outside Assembly with the Catholics, Hindus etc., before going in for announcements. There was one Jewish teacher of French, with yarmulke, who took a weekly service in Hebrew for a dozen boys from the whole school. I went for about a year, but didn't comprehend the prayers and got nothing spiritual out of it, so my parents asked for me to be excused, which I was. One Jewish acquaintance, a boarder, was more observant and sometimes went to home games of Tottenham Hotspur on Saturday afternoons. I was a fan of Spurs, having seen them win the Cup in '62, but I didn't know till later that this was the team supported particularly by London's Jews. In the second year, we were set an essay on an Old Testament topic by the chaplain Mr Boxley. I did an essay on Jeremiah, though knew very little about such biblical figures before, and didn't get significant help from my parents. Very surprisingly, I won the prize for this.

The school was very well tuned to academic progress, and all homework and class tests were marked. We got termly reports at first, showing our form order in every subject and overall. This faded out only after O-levels, when we were supposed to be more self-motivated. I didn't do well in everything all the time, but gained several form prizes over the years. These I redeemed as books, including a complete Shakespeare and the AA publication Treasures of Britain, both of which were useful for decades, and also some Sherlock Holmes. Once I was put near the bottom of the class in Geography, having missed perhaps one class too many for a violin lesson and missed a test. My mother went to see the teacher Mr Knight (father of the well-known cricketer Roger Knight), and complained at his attitude. He lived in a boarding house and owned a real tigerskin rug from his time in India. I think Mr Knight was annoyed that boys like me who did Maths and Sciences were going to give up Geography at the end of the third year.

I played chess after school, moderately well, and eventually captained the school team successfully from a lower board, for which I got House Colours. There were some chess stars at Dulwich, notably Raymond Keene a few years ahead of me. My greatest triumph was to beat Keene in a simultaneous display he gave at the school a year or two after he had left. It was very nerve-racking for me when I saw the mating chance. There were few tables left by then, and he speeded up to unnerve me further. My friend Ian Snape at some point broke an arm when he fell playing playground soccer with a tennis ball, and was stuck at home. There he studied chess seriously, and a few years later became British under-18 champion. But Ian gave chess up when he went to Sussex University, preferring bridge.

As I wasn't too good at chess, I followed my father's suggestion to start playing Go (the Japanese board game, rather than the Waddington game). He had learned it from a Scientific American article by Martin Gardner, and built a Go set with black and white buttons as stones. I started a Go club at school, and made a poster "Go, Man, Go" for a school open day, which impressed Marlis Woodward. About ten of us played Go weekly at lunchtimes, and I was about

the best. From 16 to 18 I went to the London Go Club in Marylebone several times, and once almost missed the last train home. My parents got very worried, since I usually got the penultimate train. In those more talented circles I wasn't too good, and I also got frustrated losing games after playing for more than a couple of hours. A good feature of Go, however, is that it is a game allowing handicaps, so a game can be challenging for unequal players. I gave Go up after trying it once at Cambridge and seeing how high the standard was there.

Post O-level, friends in my class were keen to play bridge in breaks and even in private study hours, rather than playground football. These friends included Ian Snape and Andrew Watkinson. Andrew had joined the 6th form from the Menuhin School, to take A-levels. He was a very talented violinist and immediately became leader of the school orchestra. He also played the Mendelssohn concerto first movement at one concert. Ian once slammed his hand down over Andrew's hand and the playing cards after losing a round of bridge. Andrew had to warn him not to behave like this, as his career was going to depend on his hands. I wasn't too bad at bridge, and enjoyed playing with adults at a bridge club near Orpington, where Marlis's husband Herbert played very regularly. I continued to play bridge there occasionally during Cambridge vacations after leaving school, but then gave it up.

After the first round of six O-levels – Maths, English, French and three Sciences – I entered the Remove (5th) form for the remaining three – Additional Maths, English Literature and German – and then the 6th form specialising in Maths and Physics, preparing for A-levels in Pure Maths, Applied Maths and Physics. We had 24 classes of 45 minutes per week on these subjects. A-level Maths seemed rather easy, but some topics were more tricky, like mechanics. Applied Maths and Physics were well integrated in those days. Once, we were very keen to get the Maths teacher Mr Payne to tell us what sinh and cosh meant. These were on the syllabus, and seemed exotic, but he said we'd do them later and find them easy. He was right. In Physics we studied some quite tricky topics that are usually taught at university, like the Biot-Savart law from which one can find the magnetic field around a current loop, and Poiseuille flow describing viscous fluid flow down a pipe. I recall at the start of A-level Physics being indoctrinated with the idea that Physics is the science of measurement, so we spent quite a lot of time thinking about systematic and statistical errors, significant figures, and about the dimensions of physical quantities. I'm not sure now that this is the heart of physics.

There were some additional classes not on main A-level subjects, in particular a sequence of Liberal Studies options that all in the 6th form did. I did some Economics, Music and Philosophy, read through the Communist Manifesto with Mr Earle, and learnt about Darwinian Evolution. We also had time for some Poetry with an enthusiastic English teacher, and one boy did some homework that was described as brilliant so he was given 11 out of 10. (I should add here that our class had some amusing mimics, Paul Vereycken in particular, who mocked the teachers for things like this.) All this was interesting, and prepared us for the A-level General Studies paper, which you had to pass to get into university. For a whole year we had time for a History class, with a rather strange teacher, Mr Langley-Webb. As this was not for an exam, we were given some choice what we wished to do. Mr Langley-Webb offered to tell us about the Second World War, and in particular the American Pacific campaign, so we learned about Midway, Guam and Okinawa. Then he offered to tell us about Modern Art and Van Gogh. He had a vast personal collection of modern art postcards that he brought in, and I got really enthused about looking at and thinking about paintings after that. I am very grateful to him.

An unusual opportunity that Dulwich offered was to learn computing. Mr Earle had introduced this to Dulwich when I was about 16. Hardly any other school did anything similar. A

suite of rooms went up inside the Science block courtyard for maths and computing. Mr Earle taught Algol programming, in two of the sixteen weekly Maths classes, and the school acquired a paper tape punch for preparing programmes, which we used at lunchtimes or after school. There must have been a tape reader to see what we had typed, but there was no actual computer at Dulwich. Two or three of us, including Mr Earle, Christopher Hills and myself, or other boys, went weekly to an Elliott computer at Chelsea College (part of London University) to run the tapes of the dozen or so interested pupils. Output was printed there and brought back to school. Unfortunately there were often errors and the programmes failed. We would then have to cut and splice a new piece of paper tape, or often it was safer to retype the whole programme. I tried to write programmes to play a game I had devised called Pentominoes, based on the 12 pentomino pieces described (as with Go) in a Martin Gardner article, and played on a chess board. Each piece had five adjoining squares arranged differently, in the same way that a domino has two adjoining squares. The rules were: Divide the pieces out between the players, then take it in turns to place a piece, non-overlapping, somewhere on the board, and the last to be able to make a play wins. My game-playing algorithm involved a standard complete tree search that was too hard to implement, and I eventually gave up. Chris mastered some machine code, and tried to make the computer sing a tune, based on its cycle frequency of 2000 operations per second. His code produced one or two notes but didn't really work either. A short TV documentary was made about computing at Dulwich, but I didn't feature. We took a further Maths A-level that included questions involving Algol. This was quite easy, as most of the questions were not on computing. I therefore have three Maths A-levels, as well as Physics.

Chris Hills, and another friend George Bichard who was in one of the science forms, went into computing as a career later, but for me, computing was a small item in my armoury, and I have mainly relied on others (collaborators and students) to do numerical calculations. But it has been interesting to see the enormous changes since the 1960s in the way computers affect our scientific and everyday lives. Like others, I now spend many hours of the working day on emails, on the Internet, and writing at the screen.

In the final year, we went beyond the A-level syllabus in Maths and Physics, and started to prepare for Oxbridge Scholarship exams (Cambridge in my case). The syllabus was not so clear for this, but we did some advanced topics in physics, and I did a project on conductivity, learning about Drude theory and Umklapp processes (which then and still now are a bit mysterious). Boys taking the Oxbridge exams stayed an extra term after A-level, which for me was until Xmas 1970. We had a very interesting new Physics teacher at that time, Mr Hart, who not only taught us things, but said that the next question was always 'Why ?'. He taught some Philosophy too, including Social Darwinism, and explained that Beethoven's music as well as pop music was successful because it increased fertility, and so propagated itself. Maybe. In Maths, Mr Payne was keen to teach us some projective geometry using homogeneous coordinates. He had probably done this at university. I rather struggled with it, and by the time I got to Cambridge, it was off the syllabus. We also did some probability, but hardly any statistics.

There was a Maths Society at Dulwich, organised by the 6th form boys with some help from the teachers. At one of the meetings I gave a well-received 5-minute talk about Conway's Game of Life, which several of us had enjoyed investigating. I was better at talking about maths than speaking in debates – a useful lesson. Another well-attended meeting was a maths quiz in the style of University Challenge, with a team of three boys (Colin, Chris and me) against three teachers, and questions posed by one or two of the other boys. The boys' team knew their stuff, and we were quicker on the buzzers, and were proud to be the winners.

One of my classmates in the 6th form, Rick Lane, had shoulder-length hair, which was his way to be a rebel. Such long hair wasn't really tolerated, although we were well past the short-back-and-sides era, and he was sent away once to get a haircut. Fairly regularly in the evening, he also went to pubs in Bromley (at age 17) and youth clubs. I occasionally joined him and others, but it wasn't really my thing, and I certainly didn't enjoy loud pop music. But for some years I had enjoyed Top of the Pops on TV and songs like Sandie Shaw's Puppet on a String, which won Eurovision in 1967.

Several of us learned to drive while we were in the 6th form, but not at school. I had a few lessons in Orpington, and then tried driving our Morris Minor around the block, with my mother as passenger. This was surprisingly difficult, because the clutch was unforgiving. I needed several more lessons from a driving instructor in a more modern Morris 1100, and then took my driving test. I passed first time, despite one error where I scraped against a kerb at a tight roundabout. However, my father was very unwilling to let me use the family car regularly, especially as the first time I asked to do so was a damp, dark November evening. So, despite having a full licence, I ended up not driving for about the next ten years.

Even before the A-level exams, and without an interview, I received an entrance offer from St John's College to study Mathematics, with the nominal requirement to get two E grades. In the end I got three A's (and actually a fourth, for the computing exam). The next letter from the tutor Dr McMullen wished me good luck in the Scholarship exam, and I succeeded to get one of these. The most significant comment in one of my final Dulwich reports was from the physics teacher Mr Bain, who wrote " ... his critical and searching attitude to the subject shows that he has the potential to be a first-class scientist." This comment really encouraged me.

Important subjects at school were Music and Sport, but these deserve separate sections. I was also in the Scouts in a school troop, and this and my later Youth Hostelling also deserve a separate section.

8 Music

Music, and mainly classical music, has been my principal hobby and love among the arts. A life without some of the great musical masterpieces is almost unthinkable.

I started piano lessons with Miss Sheila Mossman in Orpington at age 6. She was well known as the founding director of the Orpington Junior Singers, a choir for girls, and was demanding of her piano pupils. I had a good ear and could sing in tune, though never with perfect pitch. I got as far as the E-major scale, but didn't have the dexterity to progress rapidly, and perhaps struggled with left/right hand coordination. After a year I gave up the piano, but there was an opportunity to start learning violin at Warren Road. It was unusual that a primary school offered instrumental tuition at that time, in groups. I did quite well, and practised at home with Mum and Dad's encouragement. At age 8 I started lessons with Mrs Shaeffer, a private teacher in Beckenham, and also joined the small Warren Road orchestra that was mainly violins, accompanied by the music teacher Mr Arnold on piano. I progressed from grade 2 to grade 4 of the Associated Board exams by the age of 11. A musical highlight was preparing the theme and later the first variation of the slow movement of Beethoven's Kreutzer sonata with my Dad, which we played at my Mum's birthday when I was 8. I also played a duet with Eric on viola at my grandmother's 70th birthday party, in 1961. A later party piece was the Polacca from the 1st Brandenburg concerto, arranged for violin as a grade 4 piece. I was good at reading music and at the theory tests, but didn't hold the violin up well, and only mastered vibrato some years

later.

At Warren Road I performed short pieces as a member of the orchestra, and once played a duet version of Once in Royal David's City at the school's Christmas event. In the middle of this a string broke, and we had to grab a spare violin and start again. At the end of these events for parents, with music and drama, Mr Stevens the headmaster would stand up to thank everyone, and conclude by saying in a loud voice "The King", meaning that we would all stand and play or sing the National Anthem. Queen Elizabeth II had been on the throne for nearly 10 years by then, and there were giggles.

My Dad bought a gramophone in 1960 or '61, and slowly accumulated a record collection, particularly of music seldom performed on radio. His first purchase was Berlioz's Damnation of Faust, conducted by Igor Markevitch, and I was given one or two records a year as presents – notably, the Corelli Christmas concerto with other baroque masterpieces by Pachelbel and Albinoni that have become well known, Handel's Water and Fireworks music, and Mahler's Knabenwunderhorn songs. My Dad bought the Solti recordings of Wagner's Ring cycle as the sets came out, and later collected most of the Bruckner symphonies, conducted by Bernard Haitinck and others. In addition to records, we usually listened to some music in the evening on the Third Programme, although I also had homework to do. On Sunday mornings we listened to Music Magazine, and Building a Library, the programme comparing different recordings of one work. Through all this I got to know and like much of the standard repertoire. We also acquired quite a lot of chamber music recordings. Mum listened too, but purchased less. She particularly liked the Mozart clarinet concerto with Jack Brymer as soloist, and the Schubert piano trios. By my teens I knew most but not all the Beethoven symphonies, just about all the string quartets, and had heard perhaps once each all the piano sonatas, because Dad borrowed from the Westminster music library sets of the Schnabel and Kempf recordings. Beethoven has remained my favourite composer. We did take an interest in later music, like Debussy and Stravinsky, but were never enthusiastic for the avant-garde. From the Baroque, Dad liked Monteverdi.

Dad was particularly keen on Wilhelm Furtwängler, the conductor he had hero-worshipped as a boy in Berlin, despite Furtwängler's need to collaborate with the Nazi regime to keep his job at the Berlin Philharmonic. Furtwängler had died in 1954 but had left a few commercial recordings, and also a legacy of less professionally captured performances. These were slowly made available on LP and CD after remastering, and their quality improved over the decades through technical wizardry. We had a Beethoven boxed set, and Bruckner's 9th, and later my Dad acquired Furtwängler's complete Ring cycle, recorded in Rome, which he preferred to Solti's more polished recordings. He also acquired Bruckner's 8th, a great performance. We borrowed a miniature score of this, over which, near the end of the long slow movement, someone had written "By now, either you think this is the greatest masterpiece ever, or you are asleep." Despite having lots of miniature scores, we never got involved in serious musical analysis. Over time, I learned to admire and appreciate Furtwängler's dramatic and intellectually rigorous approach to the classics, and was moved by hearing his performance of Beethoven's 7th on the radio (a recording made in Cairo) while I was a postdoc in Paris. The slow movement sounds so sad, even though it is not taken too slowly. Famously, it is marked Allegretto. His recordings of the Freischütz and Coriolan overtures, which I got to know later, are also very exciting.

I won't repeat here the history of the Philopera Circle, but just say that of the operas I heard and saw performed, I particularly liked the music of Auber's Fra Diavolo and D'Albert's post-Wagnerian, rather sentimental opera Tiefland, which had been terribly popular in Germany

pre-war. It is about innocent characters descending from a mountain village to the less innocent lowlands, and has some catchy tunes that come back several times.

At Dulwich, the threshold for getting into the orchestra was grade 4, which I had recently passed (with Merit or Distinction, grades between which I oscillated over the years), so I joined and stayed in it the entire time I was there. I started in the second violins and later progressed to the firsts, eventually sitting a couple of times on the first desk next to Andrew Watkinson. He told me to play less loudly, as I had a rather rasping tone. We played all kinds of ambitious works for orchestra and the school choir, including two parts of Bach's Christmas Oratorio in my first term, a substantial part of Messiah, Beethoven's 1st piano concerto, Britten's St Nicolas and much more. Just once I sang an alto choir part in Mozart's Requiem rather than playing violin, as there was often a shortage of altos in the school choir. The soloists were talented teachers or semi-pros, as were a few players strengthening the orchestra, and I found the sound of the baroque trumpets exciting. Concerts were private events at the Festival Hall on the South Bank, or at the Fairfield Hall in Croydon, as the school hall was too small. These venues were pretty much filled by families of the schoolboy performers (a choir of over 200, an orchestra approaching 100). It was a thrill and a matter of pride to be on stage at such venues. My Dad would pass judgement afterwards on the conductor Mr Morgan and the performers, saying it wasn't quite like a professional performance, and he would have liked Mr Morgan to have been more inspiring and energetic, in the manner perhaps of Leonard Bernstein or Furtwängler. High expectations!

At school I also continued weekly violin lessons, with Mr Petts, taking half-an-hour out of some other lesson or the lunchbreak. We were only allowed to miss a particular subject twice per term, so times kept changing. Mr Petts often accompanied me on violin or viola in duets, which were partly sight-reading exercises. I took all the further exams up to Grade 8 by the 6th form. There, I remember Mr Morgan accompanying me in the 1st movement of the Brahms Opus 100 sonata, and by then I had just managed a vibrato. I was surprised that he had to practise to get on top of the piano part, but he played it well in the end.

There was also Music as a school subject, with lessons once a week, though we didn't take O-level Music. I particularly remember being guided through the first section of Brahms' 4th symphony by Mr Morgan. This was the first time I was made aware of musical structure and development in a conscious way, bar by bar. Previously I had only noticed repeats, and tunes coming back in a rondo as in Beethoven's Spring sonata. The music lessons often turned into choir practice. This was fine for me as I could sing parts that I was going to accompany on the violin in the orchestra, but those not in the choir sat at the back, and were mainly left out. This highly annoyed Colin Vout in particular, who had some interest in music, and later played the guitar a little, but couldn't sing in tune. He felt cheated.

There was an elite part of the choir, the Madrigal Group, who performed separately. I almost got into that as an alto, but fortunately did not, as it would have been a further distraction from academic work and violin. Small instrumental and singing groups also occasionally rehearsed and gave performances at the chapel in the Old College in Dulwich village. In the later years I sometimes played in one of these, but mainly remember having to sit long periods before my turn came round in some Bach work (maybe the St John Passion), because the solo singers and winds were often accompanied by just one desk of strings.

I had some opportunities for chamber music outside school. A couple of years after my Dad took up the cello in the mid '60s, he started going once or twice a week to chamber music evening classes, and sometimes the players met again in the holidays in their own homes. I joined in on a few occasions, including later, during vacations from Cambridge. One early unforgettable

experience was playing second violin in the Schubert string quintet at a class in Lewisham, at age about 15. This was challenging for everyone, but we got through the first movement, and at least the main part of the second movement (until it gets rhythmically difficult for the cellos). I was as good at the sight-reading as everyone else, and coped well with the very slow 12-in-a-bar theme of the second movement, occasionally whispering to my neighbours -10-11-12-1 to keep us in time. I was thanked afterwards for my contribution. I perhaps didn't realise then, but did later, what a heavenly masterpiece this is, and the first chord, and the whole second movement bring tears to my eyes. Fortunately I had a chance to play it more completely later, with chamber music friends, but it was always difficult to find two willing and able cellists.

Sometimes, during the Xmas vacations from Cambridge, my Dad would arrange with his friends a grand chamber music event at our house. By then we fitted into the dining room, which was no longer a scenery workshop. We played the Schubert octet, the Beethoven septet, and the Brahms string sextets and clarinet quintet on some of these occasions, and I could lead effectively. The clarinettist by then was Jackie Brown, whom I got to know well (see elsewhere).

I went on vacation music courses too. Despite being at an independent school, I was allowed, reluctantly by them, to join the London Schools Symphony Orchestra. This was training for aspiring professional musicians, many of whom went on to music college. The rehearsals were far more demanding than for the school orchestra, with a fierce violin tutor who expected more of me than I could manage. My Dad thought the conductor was not a great inspiration to the youngsters, in his rather dry performance of Vaughan Williams' 5th symphony. But my Dad didn't much like the 20th century British masters, even Elgar, whom he found simultaneously dreary and vulgar. I have grown to like much of Elgar's music and also Walton's Belshazzar's Feast, both by playing the music, and from recordings by master conductors like Adrian Boult who can introduce feeling and subtlety and tell the brass to pipe down. My Dad played this kind of music with good amateur groups, like the Morley College orchestra, but probably too much of it, as the large wind and brass sections can get bored by the earlier classics.

As a fresher in Cambridge I joined CUMS (Cambridge University Music Society) and got into the second orchestra, near the front desk. After a couple of years I progressed to the first orchestra (second violin, and maybe first violin later). The second orchestra did purely orchestral music – Tchaikovsky's 2nd, Dvořák's 7th and Borodin's 2nd symphonies I recall well. With a rather good student conductor, we prepared the Schubert Great C-major symphony, but I couldn't play the last movement triplet figures fast enough (seemingly they repeat hundreds of times), and almost resigned. I asked the conductor to take it a bit slower, but at the performance in the Senate House he took it even faster, to get it over with.

The first orchestra was actually more fun, and not so demanding, as we often played major choral works with the CUMS choir (Dvořák Requiem, and no doubt some Bach) or some subtle Ravel, which is mainly for the winds. The conductor was Philip Ledger, and I also remember the CUMS secretary, Irene Seccombe, who had done that job for countless years. All the performances were in King's College Chapel, so I enjoyed looking up at the ceiling, windows, and heraldic sculpture during gaps in rehearsals. One highlight was Elgar's violin concerto, with Yehudi Menuhin as soloist. By then he was quite old, and he was not a tall man, so hard to see. At the rehearsal he really struggled because the chapel was so cold, but by the evening it was warm, and he played magically.

I also played in several college orchestras and in a chamber group, so altogether would play perhaps three afternoons or evenings a week, as well as practising. We did a few quartet performances, including Beethoven's Opus 18 No.4, and one of our performances of this was in

Worcester College chapel in Oxford. The St John's College orchestra did Sibelius' 5th, although I didn't like this too much. College conductors weren't always good, and I thought I could do better; however, I didn't succeed in becoming a conductor, despite perhaps trying one audition. I did briefly conduct the Newnham College orchestra in part of Schubert's Rosamunde.

A contemporary of mine at St John's was Jonathan Seers, a rather successful student conductor. The college supported his ambitious projects. He put on Fidelio at the Arts Theatre, and I was in the second violins, the only time I played in an opera. I knew the music reasonably well, because my parents had sung bits of it at home, and had once put on a performance with piano half-way through a Philopera season. The Cambridge show lacked a thrilling sound from the orchestra, as we were often asked to play quietly so as not to drown out the student singers. A more senior Cambridge musician almost intervened and took over. The best part for me was hearing the talented oboist play the accompaniment to the last part of Florestan's aria in the dungeon (Ich seh', wie ein Engel ...). I later learnt that this was an inspired addition to the music that Beethoven tacked on to an earlier version. Since that time I have always loved Fidelio as one of my favourite operas. With Jonathan again conducting, we also played the second act finale of Don Giovanni at a St John's College summer concert in Hall, and I learned how fiendishly tricky it can be to play Mozart well.

In the summer vacation in 1972, I went on a chamber music course at Canford in Dorset, and remember working on a single movement of a quartet for one or two days. We never did this in our amateur chamber music. I also recall a tutor saying how crucial the timpani part is in Beethoven's 4th symphony, which was also rehearsed on the course.

In Cambridge, we also listened to records, and a favourite of my roommate Edgar Knobloch and myself, when we shared a set in New Court in our second year at St John's, was Janáček's Glagolitic Mass. We sometimes put this on too loud, or too late, and the porters came round to tell us to turn it down or off. I was also keen on Bartók's music at this time, and read a collection of Bartók's letters, which I found interesting not just for the insight it gave into his music, but also into his personal struggles. But Colin Vout thought my interest in such writing, rather than science fiction for example, was pseudish.

Sometimes I would go to hear college concerts where I didn't play. Particularly memorable was a concert at Clare, which ended with Stravinsky's Symphony of Psalms. The harmonies of the final section were brilliantly conducted by Andrew Greenwood who smiled and brought tears to the eyes of some in the choir. He was undoubtedly the star student musician of my time, and went on to a professional career at Covent Garden as repetiteur, and also conductor. I have seen his name as conductor of Albert Hall extravaganzas including the 1812 overture, and he conducts many operas. In more recent times Robin Ticciati was a similar star, and a more serious musician. He impressed Anneli and me conducting the Parsifal prelude at West Road, and he became principal conductor of the Scottish Chamber Orchestra shortly after leaving Cambridge.

I did have some interest in pop music. At Cambridge a fellow student introduced me to Tubular Bells, and someone tried to convert me to Christianity using some gospel-type music. Friends suggested I listen to Joan Baez, and Bob Dylan, and I have a few records of this type. They took me into a musical world independent of what I had picked up from home, and made me feel I was a bit of a rebel. I found some of this kind of music a bit pretentious, like Simon and Garfunkel, and some cross-over bands who sounded a bit like Puccini as they expanded from singles to albums. But Abba are great, as is the Bohemian Rhapsody of Queen, and some John Denver I like. My favourite singer is Deborah Harry of Blondie. I thought for a while that the Norwegian singer/violinist Alexander Rybak, who won Eurovision, stole his theme from Brahms'

4th symphony, but maybe I am wrong about this. Obviously Abba pay homage to Schubert, with their Thank You for the Music inspired by the song An der Musik.

After I had been in the first CUMS orchestra for two years, and was starting out as a graduate student, it was suggested by the secretary Irene that I leave, and give someone else a chance. So then I joined the Cambridge Philharmonic, the city's amateur orchestra (and also choir). This was conducted by Timothy Brown, and rehearsed at Chesterton School in the evenings. A highlight in summer 1977 was the great Missa Solemnis, which I played 'mit Andacht,' as Beethoven marked it.

Overlapping with this, I had a curious, rewarding experience during my middle year as a graduate student. I was contacted in 1977 by one of the leading string players in the Cambridge Phil, and asked to join a new orchestra, the Cambridge Symphony, as a second violinist. That was the time when my violin playing was just about at its best. This orchestra appeared initially to be just a competitor to the Phil, with some players in common, but leaving out some of its weaker long-term members. Actually, it was the start of what was planned to be a professional (not full-time) touring orchestra for East Anglia. The other players were music teachers, and some freelance people from Cambridge, London and elsewhere. The founder was Jonathan Wearn, a charismatic entrepreneur. He promised to pay the musicians, but as I was a university student, I was one of a handful who agreed to play for free, at least at first. This was hushed up to prevent the Musicians Union members refusing to play. For the first concert there were only a couple of rehearsals, typical for a professional group, but not what I was used to. I was amazed by the beautiful and coherent sound, especially from the winds, the leading strings, and the small number of brass players. I contributed, but learned to play within myself so as to blend in and not spoil things. When the passages got difficult I took it easy, knowing that others would cope. This was not the way I usually played. It was definitely the best orchestra I ever played in.

One concert was in Cambridge, and a couple of months later there was a second concert in Cambridge, repeated in Great Yarmouth. The conductor was Tamás Vásáry, whom I knew from a record I had. He was principally a pianist, and he played the Liszt 1st concerto. I can't remember if this required a separate conductor. We also did a big Tchaikovsky symphony, the 4th or 5th. I was prepared to practise, but it was hard to get a second-violin part in advance. The audience was pretty reasonable in Cambridge, as the Cambridge Evening News had made a big splash about the new orchestra. In Yarmouth, on a cold evening by the seaside, the audience was disappointing.

Unfortunately, some financial problems were becoming apparent, and the whole enterprise then collapsed. Although there was some effort to rescue the Cambridge Symphony Orchestra, I was never involved again. A lesson for me was not to be too trustful of recently started enterprises. Had I not continued in university-based research and teaching, I would have probably ended up working for a large business or government. My Dad was also pleased I stopped playing in this kind of orchestra, because he thought I could be successful as a theoretical physicist, but would struggle terribly if tempted to become a professional violinist.

In Paris as a postdoc, I joined the small UNESCO orchestra, which had several Chilean refugees as players. Rather often we played Winter from Vivaldi's Four Seasons. I made some short-term friends, one British, through this. In Paris I also went to several concerts at Radio France.

My following postdoc jobs were in the US. In Boston, I got into an amateur orchestra, then joined a better one, and eventually played in a performance of Beethoven's 9th. This was recorded and I have a copy of the LP, which sounds quite impressive. Through music I tried to make

friends, female as well as male. That never worked too well. In Boston, I also once sat on the stage at Symphony Hall close to Maurizio Pollini playing the Chopin Etudes. His recordings of Chopin and the late Beethoven sonatas became among my favourites. I also admire and enjoy piano recordings by Claudio Arrau and Emil Gilels. In Santa Barbara I auditioned for the Santa Barbara Symphony Orchestra, but didn't get in. Instead I joined a string quartet with some rather mature and charming people. This I will say more about later, in the section on my years in Santa Barbara.

I rejoined the Cambridge Phil on returning to Cambridge after six years as a postdoc abroad. We did a very grand Mahler 2nd in King's College Chapel in 1985, which was when I really learned to like Mahler; also Elgar's Dream of Gerontius. The conductor was Graeme Jenkins who has gone on to great things. I believe that Mahler's 2nd is the last major symphony in classical sonata form (at least the first and last movements), as Mahler's later symphonies have freer organisation. I can't really stand his 8th. Later concerts with the Phil were conducted by Russell Keable for many years. Highlights were the Berlioz Grande Messe in Ely Cathedral, with widely spaced brass bands in the Dies Irae, and Berlioz's Damnation of Faust in the Corn Exchange, which I had known since the time my father had bought it as our first set of records. On the fourth or fifth desk of first violins I played as though inspired (perhaps by the Devil), as noted by my desk partner. I think in the latter part my blood sugar level dropped, and I struggled near the end.

By 1995, when Ben was about 8, he started violin and we sometimes played duets at home. He didn't enjoy playing much, and although he played in small school groups until age about 14, he then gave the violin up. Meanwhile I had practised less so as not to disturb his homework, and also suffered an injury to my left-hand little finger (a violinist's fourth finger) that swelled a bit. Possibly it fractured but I never checked. That limited what I could play. I also got fed up with sitting in the cold school hall on hard chairs at rehearsals of the Phil. So slowly I gave up playing the violin too. It had been a big part of my life, but I don't really miss it now.

Subsequently I listened to a lot of music on LPs, CDs, and more recently on the radio. I tried to relearn the piano after inheriting my mother's Steinway and moving it to Cambridge in 1994, but this didn't succeed. (My Dad had replaced the old Blüthner with the Steinway just a year before he died. This was partly an investment during the time of high inflation.) With Anneli, I go every two or three months to concerts, mainly in and near Cambridge, and about once a year to the opera. We have been to hear the Endellion string quartet at West Road several times. The leader is Andrew Watkinson, my contemporary at Dulwich, and we chatted with him a couple of times after concerts. He mentioned that the quartet often play bridge in their spare time – in fact the glue holding them together is their mutual keenness for bridge – and this reminded me of the times when Andrew and I played both violin and bridge at Dulwich.

My musical tastes have changed a bit. I really like to hear piano music at home (all the Beethoven sonatas plus Diabelli variations, Chopin, Schubert) and am a fan of most of the music of Brahms, especially the German Requiem, the symphonies, and the string chamber music, although some of his music can seem a bit dull. I also like to hear professional opera on CD or radio (broadcasts from the Met, and Covent Garden). This makes up for hearing rather too much amateur and semi-professional opera in my childhood. I like the great Verdi works, especially Falstaff and Aida, much of Wagner, and the Mozart masterpieces. Particular favourites must be Fidelio, Falstaff, Figaro, and most of Rosenkavalier, which I have listened to at least a dozen times each, but seen only once or twice. Going with Anneli to Parsifal in Vienna was a great occasion, and we also went to Turandot and The Merry Widow at Savonlinna, and Don

Giovanni and La Traviata at Covent Garden. Seeing these works on the stage is quite different from listening to them at home, and often the scene-setting is a surprise. The real fire at the end of Don Giovanni was a shock.

For me, an amusing experience occurred at a summer Shakespeare production of The Merry Wives of Windsor in Girton College garden. As the plot unfolded, I was singing to myself relevant fragments of Verdi's Falstaff, including the beautiful music of the ballet around Herne's Oak.

Anneli has warmed to hearing classical music just about every day, and we often listen together to the operas on radio. She listens more than I do to Composer of the Week with Donald Macleod. Ben listens to classical music very occasionally, for example he came to Turandot at Savonlinna, but he likes an eclectic range of more modern music including Robbie Williams that I hardly follow.

I am very aware of how composers struggle to refine their style, and make breakthroughs into new types of music. Beethoven is the archetype, with his sketchbooks and multiple crossings out, his struggle over Fidelio, his various versions of the Eroica final movement, and the painstaking development of his later style. Similarly, one sees van Gogh consciously developing his range and competence through his 900 or so paintings, which we have more-or-less complete at home, reproduced in a large book. This conscious hard work has encouraged me to develop ideas slowly over time in my scientific research, and not just throw off one paper after another on distantly related topics.

Another thing I find interesting about music is the enormous range of time scales involved. High frequency notes are at about 5000 Hz, and low frequency audible notes are at about 20 Hz. Very rapid music, like some of the Chopin Etudes, has about 12 notes per second, and more normal tunes are from 1 to 2 notes per second. Musical phrases take a few seconds, and songs about a minute per verse. Symphonic movements take 10-15 minutes and whole symphonies approaching an hour. Opera takes up to 4 hours and the whole Ring cycle about 17 hours, although this doesn't sound like once piece of music any more. Somehow, composers can organise filling this time, and we can respond to music on all these time scales.

Also interesting is how we can memorise music. Many people who listen to classical music regularly can recognise an enormous range of pieces from just a short fragment of melody or maybe one or two chords. I can do this too. After a few notes I can recognise just about any string quartet or symphony by Beethoven, although it may take some thought to work out which piece exactly it is (by thinking forward to how it connects to the next theme, or next movement). There is no doubt that a composer's melody and harmonies, and the instruments used, make the fragment unique to just one work, but I am amazed that our brains can store so much of this information and that we can recall it after years or decades. I can also sing out loud, or to myself, some well known pieces like Beethoven's 5th Symphony, but not very accurately and there will be gaps, so I am impressed by those who can learn by heart a long concerto part or operatic role.

Visual impressions seem to last less well. I have visited places after ten or twenty years, and sometimes found that I couldn't remember them at all, or very little. The same applies to many paintings and films. Some people have a photographic memory, but not me.

9 Sport

My mother wasn't too athletic, and my father was advised as a teenager not to do fast running. He had some heart weakness that was medically recognised even then. So it was a great surprise

to my parents that I was a very fast runner compared to most children at my primary school, Warren Road. At sports days I would be in the relay team for my house (Yellow), and usually came first or second in the sprint for my year. More often second, in fact, as the strongest runner at age 11 was Graham Armstrong. Other sports I quite liked were football and a bit of cricket in the garden, but these I was less good at. I fancied being a goalkeeper but let in some soft goals in a school practice, which didn't impress the sports teacher, and in a few primary school matches (in the second team) was moved to left back or the wing, where not much happened. The first team had a star striker, Watson, the best in Orpington of his age, so the school overall did well. This was the first of several times where I was at a school or college that was very successful in sport, but not through me.

I didn't have stamina for longer distance runs, and would feel some exhaustion in my chest and throat. This was sometimes a problem. At Dulwich, football meant rugby, and I was assigned to some low house team. Our games were on the muddiest fields, and very unpleasant in cold weather. I didn't like falling on the ball, nor tackling, nor getting my kit covered in mud. Basically I wasn't very tough. Sometimes in winter the pitches were in too bad shape for any rugby, and we went instead on a long-distance run around the school fence two or three times. That was two or three miles, but I usually had to walk for part of the distance. Peter Currie was the champion here, but he wasn't very good at other sports. After sport, in winter, we had warm baths in the old bath house, although showers were available later after a rebuilding. The baths were communal, and were great at the start of the afternoon, and hot, but then got progressively colder and more muddy. Fortunately my games usually finished sooner than others'.

I had one unpleasant experience in the showers. I was bullied by a slightly bigger and more athletic boy, and made to wait unnecessarily long for a shower. After this I tried to kick him, but he found this a cowardly way to fight and punched me in the stomach. I was badly winded.

Dulwich, as an independent school, had lessons on Saturday morning and sport on Wednesday and Saturday afternoons (Tuesday or Thursday for the Lower School). We played games midweek, but were expected on Saturday afternoons to stand on the touchline and cheer the 1st Fifteen during the rugby season. This was fortnightly, at home games. Dulwich played against independent schools of similar status, like Tonbridge and St Paul's, but not Harrow or Westminster. At some point a little later, Dulwich started to win all its rugby games, being unbeaten for two or three years. I didn't enjoy shivering on the touchline, and missed a few games after the first year, like other boys. My parents wrote in, saying we had other important things to do at the weekend. The requirement to stay on Saturday afternoons was dropped at about this time.

In summer there was cricket and I fancied myself at this, but my talent was more for quick-wittedness, encouraging others to run when the ball was fumbled in the outfield, and stumping people, rather than at either batting or bowling. Again I was in a lower house team. Most fun, perhaps, was the playground football I played almost every morning break and lunchtime for some years with my classmates, including Colin Vout and Colin Kalra. Colin V. dribbled with skill in the George Best style, whereas Colin K. was a forward who hung around for chances and could score on a swivel. I wasn't the most skilled, but had a strong, even fearsome right leg, and scored goals at the far end, even though I was often the goalkeeper at my end.

From the third or fourth year there was an option on Wednesday afternoons to do no sport at all, but instead some community work. We couldn't do much in a couple of hours, but each term there was a full day for this, and I remember painting the walls and ceiling of a flat in one of the typical brick blocks of south London council flats. I was shocked that the inner walls were only of the thinnest material and not the solid brick we had at home. My mother was annoyed I got

paint in my hair, from touching the ceiling, but it was emulsion and maybe washed out. Some had to be cut away with scissors. Having learnt from this activity, I did quite a lot of painting and even a little wallpapering at home. In later years still at Dulwich, Wednesday afternoons were used for visits to the computer at Chelsea College, to run the programmes we had put on paper tape.

I didn't go to spectate at any professional sports, but enjoyed with many others listening to the radio coverage of cricket test matches. Off and on we surreptitiously listened to trannies (transistor radios) during some classes, and one at least of the teachers would stop to ask "What's the score?" I had first been captivated by test cricket one afternoon at the hairdresser in Orpington, where the radio was on. It was a famous match against the West Indies with Wes Hall and Charlie Griffiths as their fearsome fast bowlers. In the last Wes Hall over, England could have won, needing about 8 runs, West Indies could have won, needing two wickets, one of which they took on the 4th ball with a run out, but it ended as a draw. It was very exciting, but my parents were amazed that one of the most exciting test finishes could end as a draw. A bit later I saw Hall bowling on television, and was stunned by his style and speed. It was nothing like the bowling I had seen at school. I watched quite a lot of Test Match Special on television in my teens, especially in the summer holiday.

At Cambridge I did a little sport. I was recruited to be the cox of a rowing eight of first-year St John's College mathematicians. St John's rowing club is called the Lady Margaret Boat Club (LMBC) because of a tragic accident in the 19th century that led to the St John's club being permanently banned. We were the LMBC seventh boat. Actually I was the second choice cox, as the preferred one was a smaller chap, John Proctor, who went on to be a national leader at the United Reformed Church. But he ran the boat into the bank a couple of times, so I took over. In the May races (in June) we bumped twice, and then rowed over twice. I could have steered better on the third day to get a bump, but missed the momentary overlap, and the boat in front got away. I gave up coxing immediately after that because the practising and racing gave me a stiff back for a few weeks, despite the fact that I'd used an inflatable cushion.

After this I played a little badminton for fun, but ultimately disliked competitive sport because I'm a rather bad loser, especially after a humiliating defeat by much better opponents. Such a defeat means you have no hope of really excelling. As a graduate student we frequently played snooker at the University Centre (the Grad Pad), close to DAMTP in Silver Street.

I also tried golf. The first opportunity was in Southampton, on a free afternoon during BUSSTEPP, the summer school for graduate particle theorists. We played at the public course, and I was quite good at putting, and hitting slightly longer shots with a 5-iron. But I never mastered lofting the ball high, which was a problem if there were any low obstacles. Despite further attempts in Edinburgh, and many years later near Cambridge, and on holidays in Germany and Finland, where I tried a driving range, I could never hit long shots more than about 50 metres. This was disappointing, because there were good opportunities for the Fellows at St John's (of whom I became one in 1997) to play at least once a year. Dr Matthewman, a Fellow and golf enthusiast, encouraged me to take lessons and improve, but I never did. Occasional putting was still fun, and particularly challenging was the 'Himalayas' putting green at St Andrews, close to the famous golf courses, where I played during a later BUSSTEPP when I was a lecturer there.

During three of my postdoc years, when I was in Santa Barbara with its mild climate, it was fun to play some kick-about football, but I didn't like the tough grass they plant there to cope with the sun and lack of rain. In February 1983, I joined a small ski-trip from Santa Barbara to Lake Tahoe, on the Nevada side. Our group from the Institute for Theoretical Physics included

Emil Mottola, whose jeep-like car we took, the secretary Sharon Krieg, and Eshel Ben-Jacob. It was a long drive, starting in the afternoon, and we didn't arrive and find a motel till well after midnight. I drove Emil's unfamiliar car a short part of the trip through California's central valley. The trip was a bit of a disaster. It was my first time skiing, and Sharon helped me a little at first, but going downhill I could only stop by sitting on my bum. The slope wasn't steep, but it would have been better if I had been on a nursery slope in a beginners' class, and practised some turning movements. It's difficult to turn with your ankles held rigidly in the boots. After falling once more I gave up, took the skis off and walked down through the snow in my boots. That was the end of downhill skiing for me. Unfortunately, an hour or so later, Emil seriously injured his leg and had to go to the local hospital to see if it was broken – it wasn't. I helped drive him there. While waiting around, I parked the car on some residential street, and was approached by a suspicious Nevada cop, who did, or nearly did, pull a gun! In England, and even in Santa Barbara, I was used to parking at random spots in quiet streets, so this was a shock. The next day, it was a great relief to cross the state line back into California. Eshel, who had experience in the Israeli army and lots of stamina, drove most of the long way back. The trip wasn't a total disaster, because we all survived without broken bones, and Emil and Sharon later married and now live near Los Alamos NM.

The year that Anneli was in Santa Barbara, 1984, was the year of the Los Angeles Olympic Games. I'd had some interest in the Olympics earlier, and had been with my Mum to see the feature film of the Tokyo Olympics in 1964, with its long sequence of the world-record setting Ethiopian marathon runner, Abebe Bikila. But now there was the opportunity to see the Olympics live. Anneli was keen, because she came from a more sporting family, and her Dad had been to the Helsinki Olympics in 1952, and the Moscow Olympics in 1980.

Tickets were hard to get, but many had been allocated to the participating countries, and a batch of tickets for Finns became available through a Finnish organisation in Southern California. So Anneli and I went three times to the main stadium for athletics, twice in the evening, and once to a qualifying session in the morning, and we stayed at a hotel in LA for a night or two. Finns were good at javelin, and we were there to see it. Anneli was a fan of the Finnish javelin thrower Tiina Lillak, who won a silver medal in the final. British women won the gold and bronze, so there was a lot to cheer. In the men's javelin, a Finn won gold and a Brit the silver. It was a great sporting triumph for both countries. We also saw the race where Sebastian Coe came second to the Brazilian, Joaquim Cruz, in the 800 metres final.

Many Eastern bloc countries boycotted these games, so Americans got many dozens of medals in a host of events. Macdonalds had launched a promotion in which, if you bought something, you got vouchers offering a burger if the USA won gold in a particular event, a fries for a silver, and a soda for a bronze. We cashed in when the USA got so many medals. It helped that when you went to cash in your voucher for a meal or part of a meal, they handed out more vouchers, at least in the first week. Of course vast numbers of people did the same as us, and Macdonalds nearly went bust. We have never eaten so many burgers in a couple of weeks either before or since.

We have followed the Olympics pretty much each time since then, and especially got caught up in the London games of 2012. I applied for tickets for five different events, for myself, Anneli and Ben. We only got allocated tickets for the event for which I had splashed out the most money. This was one evening of athletics in the main stadium, for which each ticket cost 295 pounds. We again saw some Finnish men in javelin qualifiers, but also stars of the track including Usain Bolt in a 200 metres semifinal, and the diminutive American Allyson Felix winning the

200 metres final. We were impressed by the Olympic Park, with its beautiful wildflower areas. The rest of the Olympics we watched for a few hours each day on TV. The Olympics provide a chance to get to know sports that are otherwise quite unfamiliar, because they are so seldom on TV.

I have managed to see quite a few Olympic stadia on my travels, including both the 1896 and 2004 stadia in Athens, Wembley stadium from 1948, the stadia in Munich, Amsterdam, Helsinki, Stockholm and Montréal, and the 1964 Tokyo stadium. Finding the Tokyo stadium took some searching and detective work, because it became a football arena, and there was nothing easily visible on the outside to indicate its Olympic past. Recently it's been demolished to prepare for the 2020 Tokyo Olympics.

I'm not a fan of any particular football team any more, but have watched with interest the skills and successes, and blunders and failures, of the well-known British clubs and the England team. Particularly extraordinary was when Manchester United won a European Cup with two goals in a couple of minutes of extra time. As mentioned earlier, my parents and I also celebrated England's World Cup win while we were in Germany in 1966.

The professional sport where I have been a spectator most often is baseball, but still only about five times. It was a relief to go to Fenway Park in Boston on some hot evenings, and I also went to a pre-season game more recently in Phoenix AZ, and an exciting regular-season game in Denver CO, where the local Rockies beat the visiting Cardinals with a big score in the 8th inning. Baseball is a game with a slow fizz. You don't have to concentrate all the time, but the excitement builds. One thing that surprised me was that there are hardly any away fans at the games, so only home team successes are cheered.

I was involved quite a lot with sport on trips to Finland, from 1985 onwards. That first winter I was encouraged to try cross-country skiing in the beautiful sandy and pine-forested hills of Rokua, part of which is a National Park. Rokua is just a few miles from Anneli's family's farm house, Aittola, in Utajärvi. Cross-country requires smaller boots than downhill, and one's heels are free to go up and down. I enjoyed this more, but could only go very slightly downhill without getting frightened by the speed, and only very slightly uphill. I did a little bit more skiing practice in some subsequent years, going round Aittola, where it is just about level, but then gave up. Ben did more skiing later, in Chamonix and on the Jura mountains, and he once spent a week with Anneli in Adelboden in Switzerland while I was at CERN, but there he cracked his leg after jumping too high over some snow mound. He recovered, but that was the end of his downhill skiing, up to now. And Anneli, who had been expert in her childhood, and skied to school across Ahmasjärvi, the nearby lake, also got out of practice and once had to swerve severely to avoid hitting a tree, injuring herself. After that, she gave up. Just recently, while he has been living in Oslo, Ben has restarted cross-country skiing, and seems to have enjoyed it.

During summers in Finland, Anneli's father Heikki watched many hours of athletics on television, and I enjoyed watching this too. The focus was of course on Finnish athletes. The commentary was spoken very fast, because Finnish words and phrases can be very long, and I had particular trouble trying to follow it. For example, third (place) is kolmanneksi. Numbers often occur in the commentary, for the athletes' times, and these are hard to pick up too, because of the speed. Fifty seven, for example, is viisikymmentä seitsemän. Very few English words translate to shorter Finnish words, but some do, like voi for butter, and poro for reindeer.

Once we went to spectate at a World Athletics Championship at the Olympic stadium in Helsinki. It was fairly easy to buy moderately-priced tickets. We went two or three times and got quite caught up in it. A highlight was seeing Yelena Isinbayeva in the pole vault. She

was so good, she only joined the competition after almost everyone else had dropped out. She then competed against herself, and broke her own world record by one centimetre, clearing 5m 1cm. Several times over the years she increased the world record by one or two centimetres, entertaining the crowds in different places. Part of the thrill of sport is seeing such a champion, excelling all others in the world; similarly, seeing Usain Bolt striding away with ease from the rest of the field in a 100 metre final. But I also get caught up in the times, and the records, and whether some British sportsperson will win, or medal (only recently has this become a verb).

10 Scouts and Youth Hostelling

I had joined the Cubs at the Orpington Methodist Church when I was about 8. That was fun, but they didn't do anything adventurous. At age 11 I progressed to the Scouts, at the same place. It wasn't too well organised, but there was a large age range and the older Scouts enjoyed adventures. Once they planned an overnight walk from Orpington to Hastings, about 50 miles, with some assistance by car. My father and also mother were horrified that I was considering doing something so foolish and dangerous for an 11-year old, and I didn't go. Altogether, the Scouts reminded my father of the Hitler youth, with their uniforms and short trousers, and he never liked my being in it, though my mother was more encouraging, because she thought my father was being too protective.

At Dulwich we were expected to join either the Cadets or the Scouts, and it was natural for me to transfer to one of the school's own Scout troops, of which there were three. I suppose both Scouts and Cadets are some sort of quasi-military training, and in retrospect, Scouting seems a more modern version, based on small, flexible groups, whereas the square bashing of the Cadets was more like infantry training for battles of the Napoleonic era. At least in the Scouts there were no guns. My Scout troop leader was a genial Welshman and schoolteacher, Barry Evans. The head of Scouting at Dulwich was a rather overbearing teacher, John Cottle, who also had a big role in the local Camberwell district.

Scouts met once a week after school in the Gym (at least, after the new Gym was completed in my second year), and I was the junior in one of the patrols of six. We started with the standard rituals and uniform inspection, and then had some activities like checking camping gear and practising putting it up, first aid practice, cooking sausages, or some Wide Game involving competitive orienteering around the neighbourhood. We tried to gain badges for skills like tying knots, athletics, and camping skills. My favourite knot was the bowline, which allows you to attach a long rope in a loop around your waist. I also became for a while the keeper of the tin of badges and patrol leaders' stripes, and collected the cash which people paid for these. I was required to keep careful accounts, tell Mr Cottle when new badges needed to be ordered, and pay for them.

Each term, there was a Field Day to a wilder area, usually to the Surrey Hills around Leith Hill, Ranmore Common and Box Hill, and we would do a hike, following clues. Sometimes we were dropped off after being blindfolded for a while, and had to work out where we were. On these hikes I started to become a good map-reader of Ordnance Survey 1-inch maps, although we still sometimes got lost taking shortcuts through bracken.

By far the most memorable experiences of my time in the Scouts were the 11-day summer camps. I went to three of these, in my first, third and fourth summers, as well as to a briefer and much colder Easter camp in Ashdown Forest, where it was frosty overnight. I acquired a rucksack, ground sheet, sleeping bag, boots, and a showerproof jacket that was called a Cottle

jacket. The first summer camp was close to the eastern part of the Lake District, near Tebay, Westmorland and the River Lune (nice name). The campsite was a farmer's rough field just west of the main-line railway viaduct at Low Borrowbridge. The field was between two small streams that flowed into the Borrow Beck, and these supplied all our water, including for drinking. The farmer had moved his sheep and cows away a couple of weeks before. I had never been so far north in the UK before, and recall the long coach journey through the Midlands along the A5 (the M1 existed, but not much of the M6). The landscape, the bubbling streams, and some of the local words like fell and beck were all unfamiliar compared to the words describing the countryside of Kent and Surrey. My Cobras patrol, led by Daniels (we always used surnames, in the typical public school way, until things relaxed in later years), set up tent quite close to the bigger stream but on a small ridge – a good location. We collected dry wood and started chopping this up, and even put some of it under cover in a small tent. This was wise as rain showers and heavier rain came frequently. I became quite good at using a hand-axe and bow-saw, but was always scared of the bigger axe that you swing over your head.

The tent was a typical 6-man army tent, held by guy-ropes and wooden pegs. In rain it was very important, but difficult, to avoid touching the inside of the roof, as rain water would drip from the spot that had been touched (something to do with surface tension). The ropes also needed adjusting, made slacker when rain started, and tightened as they dried out, and of course the tent walked and the poles needed to be shifted after two or three days. But we didn't get flooded, as one of the patrols was.

We had to prepare a camp fire twice a day, for breakfast and supper. This was started with a firelighter, and only in heavy rain was some additional paraffin used. We daily had to stock up on wood. After ablutions and dressing at 7, we'd start the fire and cook porridge, heat water for tea, and fry something. Of course the fire had to be big enough for successful cooking, and if it wasn't, Barry, the scoutmaster, would angrily say "You'll never cook anything with a stingy fire like that!" I was good at stirring the porridge, but didn't otherwise cook. Afterwards, we'd heat water for washing up, wash up and put the plates on a rack that got built with small poles and rope on the second day. I think we also had some kind of table and our own folding stools for eating, covered by a light tent. The hardest and most boring bit was scouring the blackened pots with Brillo pads. All this took about three hours, and after that there was an inspection of our tent and kit, and the cooking area. A similar three hours from about 5 till 8 was needed for the supper preparation. One supper dish I liked was a hash based on potatoes and tuna. At some point I tried to fry an omelette, but what I really knew was how my Mum made scrambled eggs, stirring repeatedly. I left the omelette too long and it burnt underneath, and was uneatable. That was the first and last time I cooked that year, but in the subsequent camps I did better, and cooking was one of the things I learnt through the Scouts.

The daytime activities were short hikes, one Wide Game, work towards badges, and some rope bridge and raft building. During an hour's free time I would write to my parents on prepaid letter-cards, or read a bit. There was a day outing by coach to Kendal, where I bought Kendal mint cake, and on to Ingleton in Yorkshire, but the caves there were flooded. Some people got miserable when the rain went on too long, but I survived with a nylon mac over the Cottle jacket, a fisherman's hat, and a towel around my neck inside the jacket. Barry told us that we were "not allowed" to get wet, but one or two smaller boys got hypothermia and had to be moved to a dryer tent and revived with glucose. With my patrol, I did a longer hike over the fells to Grayrigg, where I bought postcards and a Mars bar. Otherwise we saw little civilisation, aside from on the outing. Food supplies came from Tebay and local farms, but my only involvement

was to carry some of these across the campsite. Fortunately the weather was more pleasant the last couple of days, and we could splash around in the streams, but then there were midges in the evening. On the last day we cut down bracken to help the farmer.

Each evening there'd be a campfire with songs and cocoa. One song was: In 1861 the American railway was begun ... . I enjoyed learning these songs, and being entertained by guitar. That was something I only experienced by being in the Scouts. We sang some of them on the coach trip home afterwards. That coach journey seemed quicker than the outward trip – maybe we partly slept – but it is common for an outward trip to seem longer.

My parents were relieved when I got back, but ever afterwards my Dad referred to Scout camps as Rolling in the Mud, and he continued to think it was too adventurous. He was partly vindicated when, at a school open day, I tried to cross a rope bridge put up by the Scouts, but couldn't make it and had to jump down to the ground, badly twisting my ankle.

In one way, it was fortunate that I missed the next summer camp, in 1966. That was at Parracombe in North Devon, but my parents wanted me to join them on summer holiday instead. It was the summer of the World Cup in England, and Colin Vout, who belonged to the same Scout troop, recalls that at the camp they couldn't follow the football matches on television, and there weren't many radios either. Colin himself went on an overnight hike the day of the final, and only heard the result later. My parents and I, on the other hand, watched the final on television in Germany, as I have mentioned before.

The next camp, one year later, was to a farmer's field just north of the Brecon Beacons, west of the hill Cefn Cyff. This was my first trip to Wales, and I admired the scenery. The location was high enough to be in the drizzle even when it was sunny in Brecon. The weather wasn't too bad though, and the only really heavy rain we experienced was on a hike over to the southern side of the Beacons, which catches the rain, and below which are a couple of reservoirs. I don't think we ever got to the top of Pen-Y-Fan, the highest point. (Much later, I did this with Anneli and Ben, when Ben was a graduate student in Cardiff.) At the camp I did more cooking, as I was a second by then, deputy to the patrol leader. The day out was to a show cave and then down to Neath, busy with heavy industry. Our scoutmaster Barry wanted to show us what South Wales was really like.

I didn't mention the latrines (the lats) before, and how we used them. but that was another thing I learned about through the Scouts. They were deep narrow trenches, cut with spades into the soil, and screened off. You did your business squatting, paper was kept dry in tins, and you threw just a little soil over the top afterwards, and washed hands with some disinfectant. Peeing was usually into the bracken or behind some tree, but could be unpleasant to do at night after the cocoa, if it was raining. One year the Senior Scouts cut the trench through an old latrine that another Scout troop had dug when they were in the same field the previous year, so they had to start again.

The last summer camp I went to was in North Wales, in Glyn Ceiriog, near Llangollen and Chirk. Again it was in a field. The landscape was not so wild, though there were green hills around. I was patrol leader of the Alligators by now. Earlier in the school year I'd had a rather exuberent, over-adventurous second called John Rye; I thought I couldn't cope with him at the camp, and asked for a replacement. This was a slightly younger boy, Mike Chetwood, who was very mature for his age, and also quite tough. We got on well, although he sometimes thought I was a bit weedy. The rest of the patrol of five (Mike Ashley, Kevin McQuarrie, Mike Daniels) was competent too. I would do some task, and with minimal instruction Chetwood would do another, and we'd be helped by the others. The weather was good, which made things easier,

and we soon mastered the domestic chores – the wood collection, fire lighting, cooking, washing up, tidying up. That was another thing I learnt in the Scouts – domesticity – and it was useful during the five years I lived alone in flats as a postdoc. During the day activities, we were good at the map-reading and at building things with wood and ropes, and could hike quite fast. We easily won the patrol competition, which was based on points for the day games, but also on inspections of meals and their timing, and of the tent and our kit. This was the first time I had some success in a leadership role, and it seemed as though it didn't require much effort, or shouting at people.

At that camp I did my overnight hike, accompanied by one of the youngsters in my patrol, Mike Daniels (unrelated to the Daniels who had been my patrol leader the first year). This gained me a final badge. We carried a tent, cooking pot and stove, and set the tent up for the night near a farmhouse where the farmer only spoke Welsh. We had to get signatures that we had covered the route and camped and tidied up responsibly. Daniels was very helpful with observing nature. He recognised various birds that I didn't know, like buzzard and curlew. Since then, I have always liked curlews, with their long downward curving beaks. I wrote a hike report later, describing and illustrating these birds and the mosses we had seen, as well as the route.

At the end of the camp, when Mr Cottle was visiting, he asked me if I would stay on the following year as troop leader. I said no, as I had decided to leave the Scouts and concentrate more on academic work. I still had three O-levels to do, and we were also starting on A-levels. He was disappointed, and said that I was obliged to give back as much to the organisation as I had gained from it. It was a message that stuck with me, and I have implemented it to some extent in later life. But I left the Scouts.

Tragically, my father's concern about the riskiness of being in the Scouts was fully vindicated a few years later, in 1972, when I was an undergraduate in Cambridge. A Dulwich Scout group had gone to Snowdon in February, during half-term, and three boys slid off some ice near the top to their deaths. This was on the front pages of the national newspapers, and the teacher mainly responsible was devastated and never really recovered. Mr Cottle, as the boss, was also blamed. Looking back, for me, the benefits of Scouting outweighed the risks.

After leaving the Scouts I started Youth Hostelling. The first trip was with Janet and Richard Antrich, together with friends of Janet. We spent a few days in Sussex, including Alfriston. On hostelling trips we needed to carry clothes, maps, snacks and water in a rucksack, but no tent. Also, linen could be rented at the hostel. Sometimes we could order a meal in advance; alternatively we bought some simple things to cook for supper and breakfast. It was necessary to do some bed preparation on arrival and a bit of tidying up before leaving, but the day was much longer for hiking than during Scout camps. Hostellers were not supposed to travel around by car, but using public transport was fine. In practice, we usually walked from one hostel to the next, typically about 10 miles.

Later I did several hostelling trips just with Richard. In 1970, during a cold Easter week, we hiked the first day from Finchingfield to Saffron Walden, where the hostel was in a medieval building and it was freezing at night in the dorm under the rafters. We then continued to Cambridge, and had a look around the city and colleges. That day I saw the recently completed Cripps Building at St John's College, with crocuses and daffodils in front, and decided I'd like to apply to St John's. The hostel in Cambridge, close to the station, was pleasantly warmer than the one in Saffron Walden. Next day we continued by train to St Ives, shortly before that train service ceased, and walked along the Ouse to the attractive hostel in Houghton Mill.

Another trip was to the Surrey Hills – Ewhurst Green to Milford – and yet another was to the

New Forest. Since then I always thought Brockenhurst, Lyndhurst and Ringwood were romantic names. More adventurous was a summer hostelling trip to Derbyshire – Edale and Miller's Dale. We saw a Blue John cave, but most exciting was climbing up to Kinder Scout. That's a pretty flat peat bog on top, dissected by deep channels. After going down and up to cross one of these channels, it's easy to lose one's bearings, and hard to follow a straight path. There are cairns to mark the main path, but in mist these are hard to see. We went partly across the top, before returning to Edale via a different route that got quite steep.

A longer trip was to the Lake District, where we started out from the more civilised Windermere and Coniston, and then from Elterwater Youth Hostel followed the very steep road over Wrynose and Hardknott passes to get to the wilder Wastwater. From there we crossed the Black Sail pass to the hostel of the same name, and stayed two nights, giving us a chance to climb one of the lower peaks, maybe Red Pike, on a day hike with less to carry. We continued via Honister, Borrowdale, Thirlmere and Sticks Pass to Patterdale on Ullswater, steering clear of the challenges of Helvellyn, and from there returned home.

A final hostelling trip in the UK was to Snowdonia, where we started at Conway, Bodnant garden and Betws-y-coed, before staying at the rather high-class Pen-y-pass hostel, from where we climbed Snowdon. Descending on the west side, I got nipped in the leg by a small farmer's dog, and took a car or bus to Caernarfon to get it checked and have a tetanus booster. But I was OK, and we visited Caernarfon castle the next day. The last day we got as far as Beaumaris castle on Anglesey before returning home by train.

So, through hostelling trips, I saw much more of the UK than before, especially the Lake District and Snowdonia, which we had only skirted on Scout camps.

At Cambridge, I continued to be a member of the YHA, and did two hostelling trips abroad. In the summer of 1972, I went with Richard Antrich and my supervision partner and later roommate Edgar Knobloch to Austria for about three weeks, starting in Innsbruck and ending up in Bregenz. We stayed in B+Bs more than hostels. The trip was mainly hiking on valley roads and woodland paths. Edgar wanted to climb some peaks, and got a bit frustrated by our unambitious route. For a few days I was laid up in Ehrwald and that gave the others a chance to climb the local mountain, Grubigstein. While recovering, I did a gentle walk across the beautiful Lermoos, and next day we climbed Säuling together, on the Bavarian border, from where there is a great view down to King Ludwig's castle of Neuschwanstein.

The last hostelling trip, and a good one, was to Norway in 1974. This was with Richard, Colin Vout, and Martin Gant, the friend I had got to know at St John's. In Norway, hostels are much too far apart to walk from one to the next, so we had a rail pass to make a tour of southern Norway. Starting in Kristiansand, after a ferry crossing from Harwich, we went by train and bus to Dalen in Telemark, near the impressive rock cliff called Ravnejuv, and on to Oslo. Then, after changing at one of the highest railway stations on the Oslo to Bergen line, we descended via the Flåm railway to the Sognfjord and crossed by ferry to Balestrand. Our route continued to Voss and Bergen, where the hostel was at the top of the Mount Fløyen funicular, and from there we took a hydrofoil to Stavanger, and the train back to Kristiansand. I have some great photos from that trip – they were particularly well processed, and posed.

The last day that we were in Bergen, we had lunch near the harbour in a simple cafeteria. The four of us sat there for about an hour at the usual lunchtime, and during the entire time, no other customers came in. We wondered afterwards what the average number of lunchtime customers was. I thought it was close to zero, because for certain we were one-off customers and would be on our way to Stavanger the next day. Colin thought it was four, because that was

what it was the day we were there, the only day for which we had evidence. To this day I don't know the right answer. Obviously, if it really were near zero, then the cafeteria would quickly close, unless they were busy early in the morning with fishermen, or busy in the evening – but it wasn't that kind of place. Another memory of Bergen, a city I really liked even though it rains a lot, is of women chatting under umbrellas near the fish market, quite oblivious to the heavy rain. For them it was quite nice summer weather.

We got back to Kristiansand with more than a day to spare – I was unnecessarily worried about missing the ferry, which was the last of the season. We found Kristiansand rather boring. We were also short of money by then. It didn't help that there was a storm on the North Sea, and the incoming ferry was 12 hours delayed. It was still pretty rough for a few hours after our departure, but fortunately it then calmed down, and we enjoyed the buffet lunch with lots of prawns. In fact this was a bonus that we hadn't paid for, in compensation for the delay, and I suppose the incoming passengers had eaten very little. We ended up at Millwall, close to where the Canary Wharf towers now are, rather than Harwich, because it was the end of the season.

That was the end of camping and hostelling for me. Afterwards, on travels, I usually stayed at hotels, the only exception being on a geological tour I made while in Santa Barbara.

11 Between School and University

Like others taking the Oxbridge Scholarship exams at Dulwich and elsewhere in the UK, I stayed at school for one term into my seventh year, until Xmas 1970, before leaving. By then I had a place at St John's College, but was aiming for a Scholarship, and was fortunate to get that too.

With help from my Dad, I arranged to work for the first six or seven months of 1971 at RARDE, Fort Halstead, where he had worked before transferring to central London. I joined a trials section. For part of the time, they went on field trials of new equipment for the Army and Navy, and the rest of the time they were preparing for these trials. I had to sign an Official Secrets Act paper, but being under 21 was highly limited in what material I could be shown or be told about. I am bound by secrecy for life so cannot write much detail about what we did, even the timing of the tea breaks, or whether there were tea breaks at all!

What I mainly did at the Fort was build some electronic equipment to monitor aspects of the trials. I built a kind of clock (an oscillator) which could be started and stopped by electrical signals. This was a transistor circuit, with individual transistors and other components to be soldered on to a board, and a digital display using glowing wire numerals inside glass bulbs. I was impressed by the great skill of the two technicians who built more sophisticated equipment along the bench from me. The boss and the technicians were less interested in the underlying physics, about which I was curious. I had some previous experience of making things, starting with plastic Airfix models and then the more prestigious Revell models that I glued together and painted; I had also built a valve amplifier, but transistors were new to me. I sometimes botched the soldering, and burnt out at least one transistor. There was some spare time during the working day, during which I could study one or two course books for the first term of Cambridge maths. It was particularly straightforward to learn some probability from a book, getting as far as expectation and variance, and some of the standard probability distributions. Algebra and analysis were going to be harder.

The section's key activities were 3-5 day field trips to various locations around the UK, monitoring tests of equipment under development. We went to some truly beautiful parts of the country, and stayed at B+Bs. The Army and Navy ranges are normally closed to the

public, so nature flourishes despite the occasional loud bangs. One trip was to Kirkudbright, in Galloway, the first time I'd been to Scotland. I was stunned at seeing the light blue station signs at Dumfries, after being used to the maroon signs of the LMS railway at all their stations in England. Paradoxically, many buildings and the tombstones and large monuments in the graveyards of Dumfries are made of maroon sandstone. I also liked the yellow-flowering gorse on the hills of Galloway, then at its best. The trial here was of some explosive snake, fired forward from a truck, then detonated. At the B+B we always got tea and shortcake before bedtime, but unfortunately this once made me sick, and the mess I made upset the landlady. Perhaps my problem was too much beer drinking, each evening at the pub.

Another trial was at Lulworth in Dorset. At that time, the eastern part of beautiful Lulworth Cove and the path round to Kimmeridge Bay was off limits to the public, because of the Army Range just inland at Tyneham that fired stuff out to sea. That range is still used, but after a careful clear-up of unexploded ordnance is now open fairly often to walkers. While we were there, a number of soldiers fired wire-guided, short-range missiles into dud tanks. The soldiers handled a control stick, requiring the kind of skill later needed in video games. They were being tested to see if they were good at this. We monitored whether there was a hit, and my clock was used. The whole exercise was under the command of an Army Officer, who told us all, in quite posh English, to be bloody careful. Strictly speaking, my group was not subject to Army command and discipline.

One further trip was to Eskmeals on the west coast of Cumbria. That's the only time I have been along the Cumbria coast, on the local railway round Morecombe Bay, through Barrow to Bootle village. The beach there has a variety of colourful pebbles derived from the Lake District. I treasure a couple of them. The trial was of a naval gun, the largest type of weapon I came across.

During my time at the Fort I got a half-day off to go back to school to take the British Mathematical Olympiad (BMO) paper. In a few previous years I had done well at the Mathematical Association of America problem-solving tests, with their multiple-choice answers. Being able to quickly estimate an answer is very helpful here, as a good estimate often rules out all but one answer. The final time I took this test, a few months earlier, I had got 135 out of 150, I think, and qualified for the BMO. Colin Vout and Christopher Hills took the same BMO paper, and we were all successful enough to get invited into the UK team for the 13th International Mathematical Olympiad (IMO) that July, hosted by Slovakia. We were all just below the age limit of 19. Dulwich, and especially our maths teachers, were thrilled to have three members out of eight in the team that year. Previously they had once or twice had one. A few years later, Mr Payne, the head of maths, was the teacher accompanying the UK IMO team to Moscow, and thanked us by letter for our achievement, which made his trip possible.

My Dad was reluctant to let me go to Slovakia at first. He was worried I'd be kidnapped or compromised by the communists, and that he or even I could be blackmailed, because we worked for the MoD on secret projects. Moreover, he was worried about sending my passport off to an unknown businessman in London who was sponsoring the team and offering to apply for visas for the whole team and accompanying adults. Finally, he was annoyed that the family summer holiday plans were being upset, because some ferry and hotel bookings had already been made. Mr Payne visited our house, I recall, and persuaded us that it would all be OK, as these Olympiads had been going on for several years, and that it was a fantastic opportunity and honour for me to be in the team, so I went.

At that time, there was no formal training, but we did practise on the previous two years'

IMO papers, and happily, I could do some of those questions. We flew to Bratislava, via Prague. There we spent a day or two, before taking a bus to Žilina, further east in Slovakia. Žilina was our base for the next week. It was several days before the two days of exams, because the questions were only chosen and finalised by the teacher delegates when they met, and then had to be translated and printed. In total there were fifteen teams, of which five were from western countries (UK, NL, SWE, AUT, FRA), and the rest from the Eastern bloc. Most teams were of eight, but Cuba had a smaller team. On the days of waiting, we were shown tourist sights. One day we went to visit the 'plants', which I thought meant a botanic garden, but actually for us meant a tyre factory! That was interesting, because I hadn't been shown around any large factory in England. Another trip, possibly after the exams, while they were being marked, was to the resort of Štrbské Pleso in the Tatras mountains. Our team had a local Slovak guide, Nora, a charming lady slightly older than us, who was learning English. She later had a career teaching advanced English at the School of Economics in Bratislava. One of our team was smitten with Nora.

The exams were two 4-hour morning papers, with three problems on each paper. Disappointingly, the exams were harder than average, and although I could do some fragments, I couldn't answer a complete question, and my total score was about the same as for one whole question. Fortunately several of my teammates did better, with David Jackson gaining a silver medal and Christopher Hills being awarded a special prize for finding an answer to one question that the examiners hadn't thought of. The team as a whole came fifth equal, the best of the western countries participating. The winning team was Hungary, with the USSR in second place. It wasn't a bad performance by the UK team, given our lack of special training in the kind of mathematics that was required – mainly algebra, number theory and geometry. UK school mathematics involved quite a lot of calculus, and applied mathematics verging on physics, but that was not needed for the IMO.

Partly because of my poor performance, in Cambridge I steered away from pure maths, and especially number theory and combinatorics, and focussed on my interest in the overlapping areas of maths and physics.

We had been entertained by jolly Slovak folk music at various times. It was broadcast through loudspeakers in parks and on some streets, and we also had a live show with singing and dancing one evening. On another evening the UK team tried to communicate with the Mongolian team, despite having no language in common. An eastern European helped us using his Russian. Another pleasant memory is of the tea we were often offered with our meals and at breaks. It was rather sweet, and seemed to be made from rhubarb rather than black indian or ceylonese tea leaves. We called it Olympiade! I never experienced it again, despite returning once to Slovakia many years later.

At the end of our trip I bought some souvenirs, including an ocarina, a large book of Karel Plicka photographs of Slovakia, and a Supraphon record of the Glagolitic Mass by Janáček, the one later enjoyed by Edgar Knobloch and me in Cambridge. We returned to Bratislava by train. That was the last mainline train journey pulled by a steam engine that I ever took.

Shortly after I got back to England, I left with my parents on the delayed family holiday to Brittany with its prehistoric sights, the Loire Valley, and then through Burgundy to Switzerland. From Villeneuve, at the east end of Lac Léman, we made a car trip to the high mountains above Martigny, spoilt by heavy rain. I had tonsilitis twice in France, and it wasn't cured by the medicine prescribed there, but in Villeneuve I got penicillin, and that worked. We returned home via Solothurn, and Colmar and Lunéville in eastern France. We met Tante Helena for

lunch in Basel on the way; she was 75 and I don't think we saw her again.

Having been to the IMO, I got invited to annual gatherings of the UK IMO teams in Cambridge, and met students who had participated in other years than 1971. At that time, almost all IMO team members went to Cambridge to do Maths, but they were scattered among many colleges. Later, Trinity College arranged training for IMO teams and their reserves, and as a result, almost all these people applied to and got into Trinity. That made the mathematical talent in Cambridge far less evenly distributed – not a good thing in my view.

It was a surprise, in early 2016, to receive an email from Hans Alberg of the Swedish IMO team of 1971, saying that for about ten years his team had had an annual reunion in some European city, and that recently they had been joined by some of the Netherlands team. Now they extended their invitation to the UK team, and they hoped eventually to contact the more than 100 participants from all the countries for a bigger reunion. The plan for April 2016 was to revisit Bratislava and Žilina. For me this was a great opportunity, as I had not been to Slovakia at all since 1971.

At the invitation of Heinz Engl of the Austrian team, the reunion actually started in Vienna, at a Heuriger restaurant in the suburbs. Heinz is current rector of Vienna University, so next day we were treated to a tour of the 19th century university building on the Ringstrasse, and a meeting over lunch in his splendid offices, where we each gave an outline of our careers since the IMO. There were about twenty of us in total, including a couple of the Slovak team guides, Nora included. Colin Vout and I were the only UK representatives, but we were accompanied by our spouses. Nora was delighted to meet two of her 'boys' again. Only three of the original participants were girls, and two of those were from Sweden. They both came to the reunion. We learnt that few of the participants present had become pure mathematicians. Two had become academics working in theoretical physics (I was one of these), some had gone into maths and science education (teachers and popularisers) and some had gone into the computer industry as software writers and consultants. Heinz Engl did research on inverse problems, and had been director of the Applied and Industrial Maths research institute in Linz before transferring to Vienna.

From Vienna, we travelled by boat on the Danube to Bratislava. The next day there was a walking tour of Bratislava, including a climb to the castle that I just about remembered, and this was followed by a day-trip by train (not steam) to Žilina. There, we were met by someone local who had been involved in the 1971 IMO, and were taken back to the modern school building where the exams took place (I hardly remembered this).

Anneli and I in fact made a longer holiday out of the reunion, and went to Parsifal at the Vienna State Opera the evening beforehand. We also made a day-trip to Nitra from Bratislava afterwards, and had time to visit the main hall (aula) of the Comenius University in Bratislava, where the IMO final ceremony and prize-giving had taken place in 1971.

There was a further reunion in Dresden in 2017, before Easter, hosted by Arnuld Moebius of the 1971 DDR team. Anneli and I planned to go, but unfortunately I was unwell, and we had to cancel our whole trip at the last minute. I was well recovered for the subsequent reunion in 2018, in Groningen in the Netherlands, and we could go to that. Four UK team members took part, not only Colin and me, but also David Allwright and Chris Hills. It was good to meet Chris again, after very many years. A highlight was a visit to the Elsa Eisinga planetarium (really a room-sized, clockwork orrery) in nearby Franeker, one of the world's oldest, and to the Afsluitdijk which closes off the Ijsselmeer and is composed of vast quantitites of interlocking basalt columns probably imported from Germany.

12 Cambridge Undergraduate – First Year

I came up to Cambridge and matriculated at St John's College in October 1971. My room was I7 First Court in the oldest part of the College, overlooking St John's Street and the double-decker buses that went past at that time. The floor of the room was not at all flat, bowing up towards the dormer windows, and there was a rope fire escape with waist harness. There was a small separate kitchen, with a faint smell of gas at all times, and the toilet and shower were at the bottom of the staircase. The location was quite romantic. Dinner was in Hall in three sittings, with just the final sitting being formal. Our dinner tickets had to be clipped each time by a machine that removed a small square of card with a satisfying clunk.

This was the first time I had been away from home on a semi-permanent basis, which made a big change in both my life and that of my parents. Dulwich sent so many boys to Cambridge that I immediately had friends at St John's and in other colleges, but over time I drifted away from many of them, and made new friends among the twenty or so students doing Maths at St John's in the first year.

I went to Maths lectures in the morning at the Arts School in Benet Street, but occasionally overslept and had to be roused by the bedmaker. To try to avoid oversleeping I had built a relay-driven electric bell, with my father's help. This was activated by a clockwork alarm clock, which was already a loud Russian one, but somehow insufficient. As the alarm key started to unwind it would close a switch on a low-voltage circuit, and through a relay set off a mains-voltage electric bell of burglar-alarm type. This contraption usually worked but was cumbersome, with its 9- or 12-volt battery, clock, bell and wiring. Unfortunately the wiring was rather crudely insulated in places, and one morning in the following year, when I was in New Court, there was a smoke stain on the skirting board. It could have turned into a more serious fire. After that I survived on clockwork alarms alone.

There were just two lectures each morning, six days a week. We covered four courses in the Michaelmas term and four in the Lent term. The Part IA Maths exams in May/June were just on these, although there were some short Easter term courses in preparation for the following Part IB year. I knew some of the material from Scholarship work at school and my reading of probability, so I also stayed on at the Arts School to hear a few IB lectures, including some on fluid dynamics, but didn't study these seriously. More seriously, I went to at least one physics course, and also enrolled for physics practicals one afternoon a week. The practicals were mostly interesting and quite fun, but I felt that what we were 'discovering' could usually be understood better theoretically. I was particularly frustrated (early the following year) in trying to set up an optics experiment, because I couldn't get the optical elements to line up, and this convinced me to concentrate on theoretical physics.

I had in fact decided earlier, with guidance from both school and home, to change to Natural Sciences in the second year and specialise in theoretical physics. By doing Maths Part IA, I avoided the need to do three science subjects in Natural Sciences Part IA, and learnt more maths instead. There was the option to do Maths with Physics, which included the Natural Sciences physics course, and had one fewer maths course per term (Dynamics, and Vibrations and Waves), but I decided to do the proper IA Maths, which was a good decision, as I learnt these extra maths courses thoroughly and didn't miss much physics.

In the afternoons we had two supervisions per week, one on pure maths, and one on applied. In addition I arranged to have a physics supervision, weekly or fortnightly. Some supervisors I remember were Drs Smithies (Analysis), Tonge (Pure), Budden and Inglesfield (Physics), Gull (Maths for Physics), not all in the same term. My maths supervision partner was Edgar

Knobloch, who had been to a small school in East Sheen, near Kew, after arriving with his parents in the 1960s as an escapee from Czechoslovakia and its repressive regime. (His father was a historian who had travelled extensively in Soviet Asia, but hardly ever to the West. Somehow he managed to go to a conference in Luxembourg, almost at the same time as his wife and two children travelled independently to the West for holiday, without the regime noticing the overlap.) For supervisions there were example sheet questions to do, and I was pretty conscientious about trying all these.

I was struck by how different university maths was from school maths. We seemed to cover material much faster, and of course there was far less interaction between student and lecturer than between school pupil and teacher. I could take detailed notes and think at the same time, which is how lectures are supposed to operate in maths. But I was also used to asking questions, and like a few other students, didn't hesitate to try occasionally to ask a lecturer a question afterwards. In fact later, when the classes got smaller in the second and third year physics courses, I didn't hesitate to interrupt a lecture to point out a typographical error (a typo) on the board, or to ask for some small clarification of a point. One or two of the smarter students did likewise, and appreciated my interruptions as they helped everyone. Lecturers usually didn't mind either, but I probably overdid it.

In pure maths, I liked Groups, including its basic theorems and examples, but there was no representation theory at that stage. This was curious, because in Part IA Chemistry, and also a bit later in Physics, just about all the group theory you got was some matrix representation theory, and the applications of character tables to (quantum) molecular vibrations. I was pleased that by doing maths, I learnt to clearly understand these logically separate ideas. Confusion in physics often arises because some mathematical idea and its physical application are not separated clearly. An example occurred later, when I met a physics student at CERN for whom the (flavour) SU(3) Lie algebra, with its Gell-Mann matrices, was closely tied to the theory of physical quarks. I knew these ideas were distinct, even though SU(3) had only been introduced to me in a particle physics course. I should mention that I only started to understand the geometry of SU(3) as a Lie group considerably later.

Part IA Analysis was the most tricky subject to learn. With its epsilons and deltas, it went much beyond what we had done at school. I wasn't too enthused by the examples of functions that were continuous nowhere, and by the distinction between continuity and uniform continuity, although I did master this for the exams. Particularly noteworthy was Dr Smithies' effortless way of writing elegant answers to the example sheet questions, improving on my muddled but not totally incorrect logic. He knew exactly when one should use the 'one third of epsilon' trick. I was rather mortified, but perhaps neglected to take account of the fact that he had been teaching this material for about 25 years. This, together with my IMO experience, persuaded me to stick to my decision to change to physics and not continue with pure maths.

In applied maths we did some fairly novel Methods of Mathematical Physics (strengthened by a pure maths course on Vector Spaces), and I liked the Electromagnetic Theory that followed, which was also important in the physics course. I was very pleased that Dr Gull taught us about the Helmholtz decomposition of a general vector field. This goes beyond the results that a divergence-free field can be written as a curl, and a curl-free field can be written as a gradient. We were also introduced to tensors, and there was a little book called Cartesian Tensors by Jeffreys that covered just this. It was tricky at first, but soon I liked to contract indices. Like others, I was curious what a tensor really was, and after a year or two got my head round the idea that it is a physical quantity attached to a physical object (e.g. a conductivity tensor attached

to an anisotropic crystal) that changes in the way one learns under a change of axes – either an active or passive rotation – in order to preserve some physical relation (e.g. between the electric field and current vectors).

Some afternoons I did example sheet questions, and some others I had supervisions and physics practicals, but I also went to rehearsals of the CUMS second orchestra once a week. (I described being in CUMS earlier.) In the evenings, after dinner, I still sometimes worked, but also tried a few university and college societies. Then I got into a routine of meeting up with a group of friends at 10 pm. We would sometimes have a drink at a college bar, but more often we'd meet for coffee and listen to music on record players. This group consisted initially of Dulwich friends from maths and science classes, and included Christopher Hills and Colin Vout with whom I had been to the IMO, and also George Bichard and Iain Mackay. We discussed some maths and physics issues, but also much else besides. Our tastes differed, as Christopher, Iain and I were keen on classical music, and the others weren't. Iain got very enthusiastic for Mahler, and would listen to Mahler symphonies in his college room at Emmanuel when there was more time, like at the weekends. These 10 pm meetings circulated around our colleges (Trinity, Emmanuel and St John's) and we'd plan the meeting place in advance. They were not every day, but several days a week, and often lasted till nearly 2 am. We had to get back into our own colleges before 2 otherwise the porters had to be woken, to their great annoyance, and a book signed. There was no longer any need to climb over college railings to get in, as had been the case until a few years earlier.

These group meetings in the late evening extended to Edgar and his chess-playing friends after a while. Edgar knew Michael Stean from schooldays, as they both lived in south-west London. Michael was by then one of the UK's leading young chess players, and he went on to become a grandmaster. Martin Gant, a maths student at St John's who got to know Edgar and then me, also joined this group. All these friends were male, but that was partly because the ratio of men to women among Cambridge undergraduates was then about six to one. Over the following years, several of us formed close friendships with women, and got married, but it wasn't easy. Particularly surprising to me was how soon after graduating George Bichard got married, as I hadn't suspected he had a girlfriend. In fact, many years later, when I was back at St John's interviewing candidates for admission, I once interviewed George's son, though didn't let on that I had known his father rather well.

In the Easter term, Stuart Bell, who like me had been at the IMO in Slovakia, started a rowing eight of first-year St John's mathematicians. I have described elsewhere how I got roped in as the cox.

I did well in the exams at the end of Part IA. Being a Scholar, I was expected to get a First, and did so, but was surprised to come as high as third in the classlist (unofficially). At that point, my tutor suggested I might continue with maths, but I'd made the decision to switch to physics.

I was looking that year for a new activity for the vacations, to replace Youth Hostelling, and saw an advert for volunteers for an archaeological dig. I signed up, as this was a way of getting to know the area around Cambridge better. On the first of these digs, during the Easter vacation, I spent a week where the Devil's Dyke was to be cut through by the Newmarket bypass (now on the A14). Most of the digging was done by a very careful bulldozer driver, but some hand-trowelling was needed too, and I did some of this. I think I found one interesting item. The age of the Dyke was not certain, but one Roman coin was found at the base, confirming the Dyke as being from post-Roman times. In the summer I got involved in a dig excavating a small

surviving chapel at the site of Bradwell Abbey, in the area that was then being developed into Milton Keynes. The chapel has been preserved as part of a park there.

A later dig I joined was at Wendens Ambo, near Audley End and Saffron Walden. Here the M11 was going to be built. The dig was to rescue parts of a Roman house – I don't know if it deserved to be called a villa. It was easy to find all sorts of treasures, including small and large animal bones and many pieces of brick pipework, one of which I kept.

13 Second Undergraduate Year

As planned, I changed to Part IB Natural Sciences in my second year, taking Advanced Physics and Mathematics. Chris Hills made a similar switch. There were several substantial and interesting physics courses that year, including the first proper Quantum Mechanics and Thermal Physics courses, and an introduction to Atomic and Nuclear Physics. The lectures were given in the newly opened Cavendish Laboratory in west Cambridge, off the Coton footpath.

Quantum Mechanics I liked. My father had found a second-hand book on quantum mechanics when I was about 18, and this made the subject very difficult, as it presented eight or nine chapters on classical dynamics, including Hamiltonians and Poisson brackets, before starting on quantum mechanics. It went on to give explicit formulae for Heisenberg's infinite matrices representing position and momentum. In Cambridge I got hold of a book by Matthews which stated the basic concepts of quantum mechanics in a couple of pages. It also focussed on the Schrödinger approach. This was much better, and was also the approach of the Part IB course, although there were some preliminaries to do with particles as waves, and Bohr orbits.

There was also a short course on Lagrangian and Hamiltonian dynamics. This was given by a doddery lecturer who could only be heard in the front row. I didn't appreciate this subject much, though Lagrangian dynamics was a cornerstone of my later research interests. I learnt this properly from Arnold's inspiring book on Classical Mechanics.

Classical thermodynamics was interesting. The Cavendish Professor Brian Pippard had written a book on this topic, although he wasn't lecturing the course any more. Ultimately, I found the subject rather confusing, because it is very formal, with lots of interlaced definitions but few examples. One needs to assume more about a physical system to draw clear conclusions. For example, progress is possible if one knows the equation of state, or the functional dependence of a specific heat on the temperature, but the theory doesn't give insight into where these further properties come from. My impression was reinforced when I read Pippard's book again many years later. Statistical Physics, which Pippard taught himself in Part II, would fill some of these holes. A party piece of Dr Ken Budden, one of my physics supervisors at St John's, was to derive the Gibbs distribution for particles by maximising the entropy, using Lagrange multipliers to constrain the total particle number and energy. This is the basic result in statistical mechanics. We went through this on two occasions I recall. My supervision partner was now Howard Covington, with whom I got on well, and with whom I sometimes discussed problems that the supervisors struggled with.

I can't remember much about Part IB Mathematics for Natural Science, but there was some important material on Fourier series and Sturm–Liouville theory, and on Green's functions. This was similar to what was in the Methods of Mathematical Physics course in Part IB Maths, which I had tasted during the previous year. I recall a good book in this area by Dettman. An important thing to learn here, though not easy to make rigorous, is that the Fourier transform of a constant is a delta function, and vice versa, and that 2π comes into this.

In the Part IB exams I did well at the physics, except messing up a question about the thermodynamics of an elastic band. I made the basic error of thinking the system had only one variable, temperature, and not properly writing a First Law using length and entropy as the independent variables (and tension and temperature as their conjugates). I also made the mistake of thinking that the maths exam would be fairly easy for me, because I had the strong background from Part IA Maths. I didn't do as well as I should have done on this. I still got a First overall, and could progress to the Theoretical Physics option in Part II the following year.

There was more to student life than just physics. That year I shared C6 New Court with Edgar Knobloch, so we were quite busy socialising with both our circles of friends. At St John's the buttery dining room opened, so there was no need for three sittings in Hall, just one formal dinner that continues till today. The new chef at the buttery was very good, and meals there were popular with both St John's students and their friends from other colleges. A dish I particularly remember was a pork chop smothered in ratatouille. The chef left after about a year – the rumour was that his subsidised meals were costing St John's too much, but probably he moved on to some more ambitious project.

In 1972, and also before, there were the Apollo Moon landings. I'd missed the first one in 1969, but the later ones in the early '70s had better TV coverage. The final landing was of Apollo 17 in December '72. The show went on for days, between launch and splashdown, and it got a bit boring waiting for the key moments. The Moon landings themselves were often not well timed for UK viewers, but it was certainly remarkable to see the astronauts hopping about and driving the rover on the Moon surface. Students watched on college TVs in the common rooms, as few students had personal TVs.

In the summer I joined my parents on a holiday that took us to Florence, mentioned before. There was also a Long Vacation course in Cambridge, six weeks long I think. For the theoretical physicists this consisted mainly of maths courses. One was on contour integration, and especially the method of steepest descents, which requires contour deformation. We got as far as the Fresnel integrals, one of which is the integral of $\cos x^2$ from zero to infinity. There was also a course on group theory, although part of this was in the second-year maths course proper. The lecturer here was Dr Volker Heine, who appeared in colourful and varied Hawaiian shirts. The course got as far as orthogonality theorems for representations of finite groups, and included derivations and applications of character tables. This was the most substantial maths course offered during the two years I did Natural Sciences, and I appreciated the precision of Dr Heine's teaching. This material has never formed part of the applied maths offering in the Maths Tripos, which is a shame. I have taught it several times myself, to Natural Scientists as the last part of the Part IB, Maths for Natural Science course. Group theory is one of the few bits of maths that they see that is nonlinear.

14 Final Undergraduate Year

In my third year, doing the Physics and Theoretical Physics option of Part II Natural Sciences, I could specialise in the theoretical physics courses, and there was a large choice of these reflecting the various research interests of the physicists in the Cavendish Laboratory. Others who attended the lectures were Howard Covington, Chris Hills, and also Athene Donald and Richard Friend, both of whom had successful academic careers in Cambridge and became Dame and Sir, respectively.

Solid State Physics was an important course, as many in the Cavendish did research on this.

We were introduced to solid state theory using quantum mechanics, and the ideas of Brillouin zones and Bloch states I found interesting. There was a very beautifully produced CUP book by Ziman on solid state physics, which I acquired as a college prize. The following term there was a further course that introduced more difficult topics like superconductivity. I also did a project on pseudopotentials with Volker Heine. This was an approximate method for dealing with the valence electrons that were fairly free to move in a crystal consisting of larger atoms; the electrons in closed shells screened most of the nuclear charge, creating a fairly weak pseudopotential. After all this, by the end of the year I felt I had had enough of solid state theory. The more advanced topics were mathematically a bit messy. For example, in a crystal with impurities we had to consider electrons simultaneously as Bloch waves and as having a degree of localisation, which was difficult to reconcile with the uncertainty principle. Interacting electron systems, which were clearly important, were hardly discussed, except in the simplest phenomenological terms.

Other courses were on Electrodynamics and Relativity, given by Professor Hewish, and on Stochastic Processes, which discussed wave propagation in random media. The Stochastic Processes course started with some basic revision of probability theory, which I had known well for several years. I felt this was a backward step in my education, although there was some novelty in the wave aspects. This course, like some others, was not very fundamental, but it had practical applications (in this particular case, to submarine detection in the oceans), and would be useful if one moved into a career requiring this knowledge.

Potentially more interesting to me was a Geophysics course. This was a fashionable area of applied physics, with a whole range of physical techniques being exploited to study the shallow earth. These techniques involved gravity, magnetism, seismic waves, electrical conductivity, and extended into areas of materials science and the chemistry of rocks. At the end of the course, in December '73, there was a field trip to Bovey Tracey, near Newton Abbott in Devon. The aim was to study a clay basin. I recall doing a gravity survey along streets in Newton Abbott. The machines were very delicate, and I wasn't too good at calibrating them and taking measurements. Being out all day wasn't bad while it was dry, but spells of rain made the week rather depressing. I decided that I would not like a career involving regular outdoor work all the year round. More interesting were the theoretical discussions we had in the evening about how to interpret the measurements. I knew about the geoid of the whole earth, and how it could be parametrised with spherical harmonic coefficients. But what could one do with local gravity readings? We were taught how to subtract out the effects of local topography, but after doing this, what did the measurements tell us about local density variations underground? This obviously had something to do with Poisson's equation, but what were the boundary conditions? At what depth did density variations become irrelevant? I discussed these matters with other students and thought about simple models for the basin and its edges. The problem was hard, but thinking about it clarified in my mind that I wanted to be a theorist, and use mathematics to understand physical phenomena. I'd leave measurements to others.

My parents had got to know Michael Stean by now, and in the vacation just after New Year we went to Hastings together to see Michael play in the annual chess tournament there. That was quite interesting, although chess is not a great spectator sport unless you are an expert. On the return trip, on the A21 at the Flimwell crossroads, a car driver ran into us head-on, after failing to notice the bend. My Dad anticipated the danger and had braked to some extent, and so had the other driver. Our Renault had its engine at the rear, which meant the front end crunched up spectacularly. Fortunately, the windscreen didn't break, and we were not seriously injured. The car was written off, and after the police had taken statements, we could go home

by train, nursing our bruises. The replacement car that my Dad purchased was an orange-brown Datsun, with its engine at the front.

Back in Cambridge there were two short courses later in the year that crystallised what I should do next. One was by Michael Green on the Dirac equation, which describes relativistic electrons and their antiparticles, positrons. Michael was then a postdoc at the Cavendish, and not yet a famous string theorist. The course had some interesting maths, and was more inspiring than the general survey course on particles and nuclei. The second was by Philip Anderson. He was a regular visitor to the Cavendish from Bell Labs over a period of years, and gave an introductory course on quantum field theory. He introduced us to second quantization, which can be a bit confusing, but extends ordinary quantum mechanics to a multi-particle quantum theory, and at the same time involves position-dependent field operators. There was probably something on its relativistic version, based on the Klein–Gordon equation, which also leads to the idea of particles and antiparticles. This was impressive. The course was interesting, but frustrating in some ways, as it was rather short. Anderson was supposed to give 12 lectures, but cancelled the first. He would finish very promptly, delivering his last remarks while holding the exit door open with one hand or toe, and then immediately disappearing. So there was no chance to quiz him or ask for clarification, as there was with most other lecturers. But the material was challenging, and obviously much more could be done to understand particles with these techniques. So I wanted to learn more about the Dirac equation and about quantum fields.

In the winter of 1973-74 there were power cuts that went on for weeks. Cambridge had a rota system, so different colleges lost power at different times. I often visited my friends Chris and Iain at Emmanuel to catch the times when the lights were on there, and they came to visit either me at my room D4 in the Cripps Building or our friends Colin and George at Trinity, when we had light. Still it was all unpleasant, with reduced power, and the need to purchase candles (which initially all sold out, even those at the church suppliers).

At the end of the year, Professor Pippard gave us a very downbeat speech about the prospects for doing academic physics research. He said there were no jobs in academia. (Indeed there were very few in the '70s, following the growth of new universities in the '60s.) The Cavendish had a few graduate studentships, but they were unlikely to lead further. This reflected the economic mood of the whole country. Prospects would be better outside universities, especially if we could contribute to reviving UK manufacturing and exports.

I did explore the option of working with Richard Eden as a graduate student. He had been a particle physics lecturer at DAMTP and was author of one book on the subject and coauthor of another, but had moved to the Cavendish, given up particle physics, and started an energy research group in response to the oil crisis following the Arab-Israeli war of 1973. This had some reasonable government funding, but the physics didn't seem too interesting. The crisis was soon resolved in the UK, anyway, with the development of North Sea oil. Eden advised against going into particle physics, which he perceived as a morass of dozens of particles with complicated properties. I didn't follow the advice, because I really wanted to study more fundamental quantum physics and relativity.

That meant moving back to DAMTP to do Part III Maths. Having got a good First class in Part II Natural Sciences, and therefore a First class BA degree, I could make this switch with my college's encouragement, and I also qualified for fourth year funding from Bromley, my local authority.

In the summer, I had arranged to work for a geophysical company, Seismograph Services, whose headquarters were quite near Orpington in the historic country house of Holwood in

Keston. It was quite difficult but possible to get there by bus, and would have been a nice place to spend a few summer weeks. I was offered a job after an interview, in which I told them about my nearly completed physics degree from Cambridge, with some experience of geophysics. However, rather than getting me to work on some theoretical calculations in an office, they decided to send me to join a survey team in Nottinghamshire, searching for deep coal. I arranged to stay at a B+B in the West Bridgeford area of Nottingham, where we would be picked up at 7 am and taken to the site. I didn't fancy being available the first Monday morning, so took an early train from London to Newark and a bus to the village where the survey was, arriving at about 12 as I had said I would. That was OK with the boss in Keston, but the local team weren't too impressed. The work involved hauling cables attached to a chain of well-sealed seismometers across fields and through gaps in hedges, placing them in the desired place and pressing the seismometers into the soil, and finally waiting for the firing of explosives. The data was captured in a specially equipped van, but I was not involved with that, let alone with any analysis, which would mostly be done later. I thought there would be a lunch break to a local pub, and then a full afternoon's work, but the team preferred to keep going till about 2.30, at which point the day's work was done. So there was quite a long evening to spend in Nottingham.

By the second or third day, with an early start before it was possible to have a proper breakfast, I was getting a bit exhausted hauling cables around, and the strong Irish guys who made up the team thought I wasn't coping too well. Still, after a trip home for the weekend, I continued into the next week, this time being available promptly on Monday morning. By Thursday however, the site boss had had enough of my contribution and more-or-less sacked me. At the last moment, he offered me work in the data collection van, but by then I'd had enough. I hadn't yet got any pay, or travel expenses, despite having spent a considerable amount. It was promised that I would get these later, but in the end they said I hadn't worked effectively, nor for either week completely, so I didn't get very much.

There was one amusing, if embarrassing, incident in Nottingham. I went up to the university one evening and asked a gate porter if there was any entertainment there (meaning music at a bar or something similar) but he said that this was a women's residential block, and asked what I had in mind!

A final thing to note is that the survey, with my small contribution, was a success. There is plenty of deep coal all over central England and down into Oxfordshire. Much later I saw a large open-cast coal mine operating somewhere close to the area we had surveyed.

Back in London I searched for another job to fill the six weeks before my planned hostelling holiday with Cambridge friends to Norway (described elsewhere). I found one at an office of Post Office Telephones in Shaftesbury Avenue, run by a female boss. The work involved record-keeping for about 2000 telephone engineers working all over London. I had to record training course information, sick leave, promotions etc., but the main task at that time (July 1974) was to note changes in pay. Because of the rampant inflation, and the agreements struck between the unions and the government, pay went up by about one percent every month or two. (I think the peak annual inflation was 15% in the year 1975.) So the personnel forms designed to last for a lifetime were getting filled up rapidly, and we had to staple subsidiary sheets to them. I quite liked this kind of work, on a temporary basis, and could do it well, completing what was needed each day in about a couple of hours. No-one complained or asked me to do more.

The environment stimulated my creative thinking. First, I realised there were several ways the forms and processes could be made more efficient. We were encouraged to write in to the central office with suggestions for improvements, and a couple of months later I got a reward of

fifteen pounds for one of them. Also, I had time to tackle a problem that had remained from my study of Brillouin zones. The definition of the second, third, and higher zones, based on how many bisecting planes of the reciprocal lattice vectors you have crossed while moving out from the origin, did not make it obvious that the volumes of all zones are the same, and yet they are. I tried to prove this by shuffling the pieces of the zones around (in two dimensions) in order to come up with a general argument. In the end I managed to show something but it wasn't too elegant or convincing. I still wrote to Professor Pippard to tell him of this, and he replied to invite me to discuss it when I returned to Cambridge. There, he agreed it was an interesting problem, and he took out a couple of pages of his notes from years back in which he had written out an elegant argument based on a variant of the definition of a higher zone as the region closest to a reciprocal lattice vector other than the origin. This makes it obvious that all zones are isomorphic Wigner–Seitz cells (whereas the first definition cuts these up and moves the pieces around). I was a bit disappointed not to have seen this argument, nor to have obtained any original result, but it was encouraging to know that I could engage with a top professor on a research subject. What surprised me most was that Pippard had done this work at all, because in lectures he stressed the Cavendish Laboratory dogma, going back to Rutherford, that deeper maths and subtle mathematical reasoning were not of much use in physics, and back-of-the-envelope calculations were usually sufficient. Having seen his geometrical insight at work, I don't think he privately believed this dogma.

I have a different view of the relation between maths and physics. I don't think there should be a dogmatic position on how much maths, and what type, should be applied to different areas of physics. Individuals have greatly varying knowledge and interest in mathematics, and should be free to develop their interests as they see fit when faced with research problems. Sometimes simple maths will be enough, but sometimes it helps to explore 'deeper pathways'. When a subject is developing rapidly and there are lots of new experimental results, as in Rutherford's day, then simple maths is usually what's needed. But when a subject matures and stabilises, as elementary particle physics has done recently, then a deeper understanding is called for.

15 Part III Maths

In October '74 I returned to Cambridge and St John's to take Part III of the Maths Tripos. At that time, about 50 students took Part III, and UK students usually got a continuation of their local authority grant for the fourth year of study. For the 30 or so students doing applied maths subjects the lectures were all in DAMTP, the Department of Applied Mathematics and Theoretical Physics, then located in Silver Street. I had probably visited the building briefly before, but now spent part of each weekday there. In fact, I would go on to spend a considerable part of my life there. I knew a few of the 20 or so students taking the lectures in theoretical physics – Howard Covington being one of them – but about a third of the class were newcomers to Cambridge for the Part III year, who had done Bachelor's degrees at other UK universities. There were hardly any students from abroad at that time.

The lectures were in the mornings in Room A or Room B of DAMTP, rooms that were used for research seminars in the afternoons. In the Michaelmas term there were courses on Quantum Field Theory and on Elementary Particles, with a good introduction to the important symmetry groups, like SU(2) isospin. The lecturers were Ian Drummond and Alan Macfarlane. This was the era of the large zoo of mesons and baryons, classified by Gell-Mann in the 1960s using what is now called SU(3) flavour symmetry. This theory coped with all the particles whose constituents

were the up, down and strange quarks that Gell-Mann had also introduced. Gauge theories existed and were studied as a research topic, but hadn't yet made it into Part III. In addition to these High Energy Physics courses, I also took a course on General Relativity, but found this more difficult.

In the Lent term, there was a course on Electromagnetic and Weak Interactions of particles, whose understanding at that time relied on currents and matrix elements, and their approximate conservation laws. The lecturer was Peter Landshoff. There were hints that weak interactions were mediated by heavy W-bosons, but this was rather conjectural, and the W-boson had certainly not been detected yet. There was also a course on Strong Interaction Dynamics by Hugh Osborn, discussing hadronic cross sections, and proton and neutron structure. This was partly an introduction to partons, the main research interest of Professor Polkinghorne, the head of the High Energy Physics group.

All this was quite challenging, and there seemed to be lacking a single unified theory. I enjoyed Part III nevertheless, because it was at the right level of mathematical sophistication for me. The courses made good use of the quantum mechanics that I had learnt well in the Cavendish, and combined new ideas from special relativity, whose basics we'd also covered earlier. The novel mathematical material was the Lie group theory, especially, and its application to particle classification and to field theory using Noether's theorem.

During my Part III year I had to move out of college, but found a bedsit through the university accommodation office. This was at 45 New Square, close to the Kite area that was being redeveloped into the Grafton Centre. It was about 10 minutes walk to St John's, via King Street with its many pubs. My room was rather poky and not very warm in winter, so I didn't spend very much time there.

Around Xmas '74 I met Jackie Brown at one of our musical events in Orpington, not for the first time, and on this occasion asked her if she would like to meet up more often and go out with me. She did, and this was fairly easy when I was visiting Orpington during the vacations, especially as she had her own car. She also came to Cambridge a few times during 1975, and visited me at New Square once or twice. But I usually found a college guest room for her if she stayed overnight, as our relationship remained platonic. I don't think she was too impressed with the bedsit lifestyle of her Cambridge graduate boyfriend, nor by DAMTP, but she liked seeing the old colleges. In summer '75 I got tickets for the St John's College May Ball for us. It's the only time I've been to a May Ball. We enjoyed it to some extent, as she liked the bands and dancing, and there was some food and wine (although we didn't have the fancy dinner). But by 2 or 3 in the morning we were exhausted, and went to our separate lodgings. So we were not among the survivors who stayed till dawn and then went punting to Grantchester to have breakfast.

I took the Part III exams in June '75 and got a respectable Distinction. This was what was needed to be offered a place in DAMTP to start research in theoretical physics. In all, four students got places to work in elementary particle physics. I accepted gladly, and planned to return in Autumn. The arrangement then was that supervisors for new graduate students were usually only fixed in the first week or two of the Michaelmas term, but I was advised by Dr George Reid, Fellow and Director of Studies in Maths at St John's, that there was a new lecturer in Cambridge whom I had not met, Dr Peter Goddard, who was also going to be a Fellow of St John's. I should talk to him, and see if he could be my supervisor. We did meet, or perhaps just exchanged letters. He was an expert on string theory. I knew nothing about this, but it seemed an attractively mathematical topic of research, with potential applications especially to meson

physics (mesons being the family of particles that includes pions and kaons, made of one quark and one antiquark). He agreed to take me on, and I became his student in October. He already had one other student in DAMTP, Roger Horsley, who had started the previous year.

Encouraged by someone in DAMTP (probably Professor Polkinghorne) I had successfully applied to spend two months as a summer student at CERN, the particle physics laboratory just outside Geneva. So I joined my parents for about ten days of their summer holiday through the Netherlands and Germany, staying a few nights in Düsseldorf and Frankfurt, and then took a train via Basel to Geneva. At CERN, the students were accommodated in a couple of barrack blocks, quite nice and clean inside, with a memorable cleaning smell.

I was assigned to join a group led by Emilio Pagiola, an Italian who worked full-time at CERN. This was my first experience of working in an international group, although one member was a sociable British guy from Liverpool. The group was involved with analysing results from a hadronic physics experiment in which a kaon beam was fired into a hydrogen bubble chamber. Here the target is principally protons – the nuclei of the hydrogen atoms. It was exciting to look through a glass window into the bubble chamber and see the collisions, and the particle tracks they produced. They were lit up by flashes of light once every one-and-a-half seconds and photographed. Each collision was different, partly because the beam has a small but finite width and each collision has a distinct geometry, but partly because of the quantum unpredictability of the events. This was the first time I felt I had directly observed quantum uncertainty. The group's scientific goal was to observe known resonances with non-zero strangeness in the kaon-proton system, and possibly find new ones.

Data was available from collisions recorded weeks or months earlier, after the photos had been scanned and the momenta of the outgoing tracks measured. To detect unusual events, the Lorentz invariant masses of outgoing particle pairs or triplets had to be found and peaks in their distributions noted. Dalitz plots were used to analyse 3-particle systems, although I didn't get involved in this. Instead, I became interested in looking for clustering in the various invariant mass combinations that arose when there were more outgoing particles. I learnt from one of the CERN staff specialising in numerical methods about cluster-searching algorithms. These can recognise clusters of points, or a pair of separated clusters, in a multi-dimensional space. I thought about modifying the algorithm a bit, using multi-dimensional geometrical ideas, and wrote a computer programme to make it work. The programme got longer and more complicated as the weeks went by. It was in Fortran, which I had learnt in Cambridge. Something new for me was to edit the lines of the programme on a (green) Tektronics screen, using a cursor and keyboard. After an editing session, the programme or a section of it was printed out on punched cards, with one card per line. The programme ran successfully some of the time, but it never got to the point of being really effective in the cluster analysis, except when applied to some model data set. The cards were stored in a box that got quite heavy, which I needed to carry back and forth between my room in the barracks and the computer centre. I did this many times, including some Sundays. Once, unfortunately, I dropped the box on the pavement, and the cards got mixed up and damaged. I rescued them but it was a demoralising experience, and together with the programme not really working, rather put me off complicated numerical work in physics. At the end of my CERN stay I left the cards with the Liverpool guy, with some description of how it was supposed to work. He said he would see if he could develop it further, but I don't think he did.

In addition to having a project with a research group, summer students at CERN spent a few weeks listening to lectures, and could also briefly visit the experimental facilities and com-

puter centre. The most novel accelerator at that time was the Intersecting Storage Rings, a proton collider, but also interesting to see was the big bubble chamber, Gargamelle. Some of the lecture material was familiar from Part III, but there was novelty in our being introduced to gauge theories. These were just becoming fashionable, and became the standard way to think about the interactions between elementary particles. I acquired some notes on gauge theories by Chris Llewellyn Smith, who was then a brilliant young Oxford theorist. Another topic that must have been mentioned was the discovery of the J/Ψ particle in 1974, now interpreted as a bound state of the charm quark and its antiparticle. This was the fourth quark type to be discovered and revolutionised thinking about both strong and weak interactions. Its discovery implied that Gell-Mann's SU(3) flavour symmetry describing three quark types was not really fundamental, but replacing it by SU(4) was not very helpful either, because that symmetry was badly broken. However, having four quark types (and later six) was consistent with the Weinberg–Salam electroweak theory, and its expected quark mixing structure. It was also consistent with the possibility of weak neutral currents in neutrino interactions, that had earlier been discovered at Gargamelle.

CERN published Yellow Reports on various particle physics topics that hadn't yet got into the textbooks. I picked up a few of these at the CERN library, including the hard-to-understand report called Diagrammar, by 't Hooft and Veltman, which presented the theoretical tools needed to understand the renormalisability of gauge theories, the property that led to their general acceptance. I rather liked studying in the CERN library, as they had a comprehensive collection of particle physics and maths books, journals and the latest preprints.

We lived quite well at CERN. There were two good restaurants. One was in the old Swiss part of the site, near the barracks, which offered homely and digestible food. I got to like the yogurts there. (Martin Gant had recommended yogurt to me as delicious.) They had fruit or chocolate flavours, and tasted quite different from the sour yogurt I had tried before and disliked. The other restaurant, Tortella's, was higher up the site, near the computer centre. This was an Italian-style restaurant, with a more exciting menu, sometimes more challenging on the stomach. One of the amusing things about being at CERN was frequently crossing the border. The computer centre was in France but Tortella's was in Switzerland. Passports were not needed unless you went outside the site. At both restaurants, espresso coffee was available for 90 cents, a price that eventually rose to 95 cents, and people would invariably agree to meet at one of these places for coffee, at any time of day. Jeffrey Goldstone, whom I got to know later at DAMTP, told me he once felt rather ill while visiting CERN, and couldn't understand the cause. He realised after a while that it was due to having five strong coffees a day!

CERN had a local orchestra, and I joined this. There were a few rehearsals during the summer, but I can't remember if there was any concert. One of my fellow students was Henrik Bohr, a grandson of Niels Bohr and nephew of Aage. He was also a violinist, and managed strange things like playing violin while standing on his head. It was with Henrik that I discussed whether SU(3) was mainly a construction to understand quarks, or whether it had an independent mathematical existence.

I left the CERN site fairly frequently. There was the X-route bus to Geneva city centre, and a reliable machine to purchase tickets in advance. This kind of machine usually didn't work in Britain, because it would get vandalised, and high inflation meant that fares were going up all the time. Switzerland had neither vandalism nor much inflation. On Sunday morning, when the CERN cafeterias were closed, I'd walk to old Meyrin village for breakfast. One brasserie there offered very good fried eggs, and omelettes. Some evenings I'd go with others to Geneva centre

for a fondue (bread and melted Swiss cheese) plus white wine. In mid-August, Geneva had a splendid fireworks display over the lake, and some types of firework – exploding spheres of lights – I saw for the first time.

My parents caught up with me about two weeks after we had parted in Frankfurt. They stayed a weekend in Geneva, and I showed them around CERN. I also encouraged Jackie to visit me at CERN, but this didn't materialise.

The visit to CERN was worthwhile in several ways, and I visited CERN five more times in the coming three or four years. One of the few annoying things there was that people often talked about money. CERN salaries, at least for permanent staff, were very generous and also tax-free so people bragged about how they were saving substantial sums, and thinking of moving back to their home countries later after making currency transfers at favourable exchange rates. At the same time they claimed they couldn't afford to live on normal salaries in their home countries. Italian physicists often managed to have part-time jobs both in Italy and at CERN. So the laboratory was a great success as an international scientific collaboration, but there were rather a lot of personal jealousies.

16 Graduate Student in DAMTP

During my first year as a graduate student I could move back into college accommodation. I lived in a St John's graduate hostel at 69 Grange Road (absorbed later into the St John's College School) which had 12 student rooms. There was a resident hostel keeper who lived with her husband downstairs from me. The atmosphere was less free than in the undergraduate rooms of the main college, as the husband was not mobile, and the couple hardly ever went out. There were complaints about my violin practice, so I had to negotiate to do this at very particular hours and not too often. One of my neighbours wanted to have his girlfriend stay, but they weren't completely intimate, so I lent my key to him so that one of them could use my room while I was away. Unfortunately they were found out, and we both got into trouble with the dean of discipline, but we made a fuss, pointing out that nothing immoral had happened and that we were 23-year olds and should be able to have some personal freedom. I was fined 50 pence, a rather nominal sum, but I think the college took on board our arguments, and as others had also complained about the hostel keeper, she ended up retiring at the end of that year. My friend Jackie also visited me from time to time, and I saw her again during my Xmas break in Orpington, but our lives were rather separate and our relationship wasn't going anywhere. She didn't get on with my circle of physicist friends in Cambridge. Without discussing it much we drifted apart, and didn't meet anymore. I was heartbroken, briefly.

In DAMTP, at the start of October 1975, I acquired a desk for the first time. I shared an office (F41) on the first floor near the stairs and lift with Mike Lowe and Ray Gorley, two other first-year PhD students. The fourth in our year was Graham Shore. My supervisor, as I mentioned, was Peter Goddard. The HEP (High Energy Physics) group was headed by Professor John Polkinghorne, assisted by Peter Landshoff, and they worked on the phenomenological parton models of strongly interacting particles. Partons were a dynamical version of the quarks that Gell-Mann had discovered in the '60s. Feynman originated the parton idea, and didn't like to call them quarks, because of personal rivalry with Gell-Mann at Caltech. Polkinghorne and Landshoff extended the parton idea into a more rigorous relativistic model, with diagrammatic methods that looked like Feynman diagrams. One diagram was called the handbag.

The remaining members of the group had more theoretical interests in quantum field theory

and in symmetries of particle physics; aside from Peter Goddard, they were Ian Drummond, Hugh Osborn, Alan Macfarlane and Jeffrey Goldstone. In addition to these academic staff, there were 13 PhD students and just one postdoc, Uday Sukhatme. More ambitious and able students wanted to work on the more theoretical ideas; those who were less so were encouraged to work on partons. Jeffrey Goldstone was a Reader, so he was senior to everyone except John Polkinghorne but disconnected from the administration of the group, and he had no students. He was famous for his work in field theory, especially his understanding of what is called a Goldstone boson, and he had also taken an interest in string theory, which was by then about eight years old. Peter Goddard was a string specialist, having coauthored with Jeffrey and others the paper showing that bosonic string theory is only consistent in 26 dimensions.

My first project, suggested by Peter, was to understand and try to generalise an elegant formula that Jeffrey had found, a generating function for the energy spectrum of a basic string. String states arise by quantizing a sequence of oscillators with integer labels, and then imposing some constraints that project out physical states. It is fairly easy to count the number of states with each allowed energy, and Jeffrey's formula gave all of these at once. I was looking for an alternative derivation of this formula, perhaps number-theoretic, and some way to go beyond it. In practice I worked from a copy of a tidy page of notes by Peter, who had understood Jeffrey's rougher notes. I was able to reproduce the individual energy degeneracies, but couldn't see a useful pattern in them.

Jeffrey was very unwilling to write things up. He produced copious notes of his own on many topics, and occasionally understood something really well, which others would have been interested to read about, but by then he thought the matter trivial and not worth writing up as a polished paper. He thought anyone else could just as easily work out what he had. Fortunately, others took a serious interest in Jeffrey's ideas and either helped him write a joint paper, or occasionally persuaded him to write a sole-author paper.

There were quite a lot of exciting new ideas being discussed at the time. The Standard Model field theory was becoming popular, and students were being encouraged to learn its details, although it was still not yet taught in Part III. The Standard Model combined the electroweak gauge theory of Weinberg and Salam with the more recently developed QCD (Quantum Chromodynamics), the gauge theory of strongly interacting quarks and gluons. Gluons therefore joined quarks as possible partons within protons and neutrons. But this got messy unless one worked precisely within the formalism of QCD, and the simpler, phenomenological parton models started to become unfashionable. Partons still survive as a key ingredient of QCD, because one still needs to consider the probabilities of finding quarks and gluons with their various momentum fractions inside colliding protons and neutrons, but proton and neutron scattering calculations require the full QCD formalism. It is similar for electron and photon scattering off protons and neutrons, and for electron-positron pair production in the context of quark collisions.

The idea I got interested in next was the MIT Bag Model. In the parton model there is no clear inside and outside to a proton (or neutron); in fact, the most important property of a parton is its momentum rather than location. The Bag Model introduced a new field which distinguished the inside from the outside. Quarks could only propagate inside the proton, capturing the important idea of quark confinement that was experimentally verified, but still hard to understand in QCD. The basic picture was of a spherical bag; the quarks satisfied a Dirac equation inside and some boundary condition on the surface. I tried to extend the model to more general boundary shapes, and also tried to understand what might happen if two protons, and hence two bags, collided. Two spherical bags would merge into a dumbbell-shape and then evolve further, perhaps splitting

up again. The bag itself had some dynamics – at least an energy density – so it wasn't always necessary to think about the Dirac equation in this complicated geometry.

Bag dynamics was hard, so I constructed a one-dimensional approximation, and some simple equation for the bag field, involving linear profiles. I could then just about manage to calculate what happens in a collision, but when I presented this, it was pointed out that my calculations ignored the Lorentz contraction of a relativistically moving bag. At this point, in about April '76, I gave up on MIT Bags and took an interest in a better formulated theory of one-dimensional particles of finite size and energy. This was the kink soliton of ϕ^4 field theory, and its relative, the sine-Gordon soliton. My roommate Mike Lowe was working with Hugh Osborn on the detailed properties of kinks in quantum field theory, including the quantum corrections to their masses at first and second order in perturbation theory. They followed work by Sidney Coleman, and especially a review by Rajaraman which he later turned into his book Solitons and Instantons. I got interested in the collisions of two kinks, and of kink-antikink pairs. This required solving a classical nonlinear PDE (partial differential equation), a well-defined problem, whose solution has no singularities or discontinuities, unlike bag collisions. The problem was interesting but too difficult to study without heavy numerical calculations, which didn't appeal. But I did understand that the long-range tail of a kink is a linearised field that is responsible for the small force between well-separated kinks.

The next idea that was talked about was magnetic monopoles. Dirac had discussed the interesting notion of a monopole, a pointlike particle with a magnetic charge, in the early days of quantum theory. In fact the idea of an isolated source of magnetic field was much older, but Dirac had shown how quantum theory constrains magnetic charges to have values that are integer multiples of some basic unit. Simultaneously he had given an explanation for why electric charges are integer multiples of their own basic unit, the proton charge, provided monopoles exist. This property of electric charges is what we observe, and it doesn't have any other simple explanation. Now that nonabelian gauge theories were fashionable, monopoles made a fresh appearance. 't Hooft and Polyakov independently discovered that many nonabelian theories have classical solutions representing magnetic monopoles. This new type of monopole avoids the pointlike singularity of a Dirac monopole, and because the fields are smooth, the monopole is more like a one-dimensional kink, and has a finite energy that can be identified with its rest mass. Higgs fields are ingredients of the monopole, and because of the Higgs symmetry breaking mechanism, the asymptotic monopole fields are purely electromagnetic, although the core is more complicated. This allows the monopole's magnetic charge to be calculated. The charge is essentially the same as what Dirac had predicted, but for a rather different reason.

Jeffrey Goldstone introduced these new monopoles to DAMTP in a seminar series, and had a novel idea for understanding their magnetic charge using a topological relation between the charge and the asymptotic behaviour of the Higgs field. This involved the homotopy theory of Lie groups and their cosets. Again, Peter Goddard made good notes on this after he had given similar lectures of his own. Peter and collaborators later found elegant generalisations of the topological theory of magnetic charges, that would work for a large class of gauge groups and their possible symmetry breakings to smaller groups by the Higgs mechanism. Many other papers on monopoles followed those of 't Hooft and Polyakov too. Two in particular were by Prasad and Sommerfield, and by Bogomolny. These authors found a special case of the nonabelian monopole theory where the equations simplified and exact solutions could be written down. Bogomolny's paper also explained that in this case the monopole fields obey a first order PDE. This is special, because field equations are usually of second order in derivatives. Remarkably,

monopoles and other solitons obeying variants of Bogomolny's first order equation have an energy (mass) determined purely by their topological charge. I got interested in monopoles after this, towards the end of my first year as a graduate student, and this became a productive area for me to work in, particularly during my second year.

We didn't spend all the time at DAMTP thinking about our own research. There were seminars to go to, including a weekly colloquium, where attendance was more-or-less compulsory. Most PhD students were also asked by their colleges to supervise Maths undergraduates, and we did this in our DAMTP offices. This required some negotiation about timings. Supervisees would arrive in pairs to discuss their week's work on the example sheets handed out by the lecturers. I supervised three or four pairs per week, each for an hour or more. I didn't find this bad. The example sheets have questions that can be tricky for the undergraduates, and I often felt that the way things really worked in Cambridge was that the graduate students learnt courses from the lecturers, by supervising, and the undergraduates were mainly there to catalyse this. Successful graduate students would then eventually go on to become the next generation of lecturers, in Cambridge and elsewhere.

In the summer of 1976 I went to the BUSSTEPP summer school (British Universities Summer School in Theoretical Elementary Particle Physics). It was at Southampton University that year, and organised by Professor Ken Barnes, a charming personality, and Tony Hey, a field theorist who later concentrated on making a purpose-built computer for lattice field theory, and after that became a professor of electrical engineering and expert in high-performance computing. There were lectures in the morning, followed by exercises in the afternoon based on them. Later in the afternoon were tutorial groups, with postdocs and other young academics mainly from the UK recruited to take these. One tutor was Neil Craigie, who was then based at CERN. He thought I was much too critical of some of the material that was being lectured. I had, for years, frequently interrupted lecturers to correct minor or sometimes logical errors. This was usually appreciated, sometimes grudgingly, but Neil concluded that I was the type of person that was likely to go into the government after graduating with my PhD, and cut off the funds for High Energy Physics. In fact, the UK became more committed to particle physics experiments, and although UK facilities like the Rutherford Lab and Daresbury accelerator were wound down, funding towards CERN increased.

BUSSTEPP lasted two weeks and on one free day we had a trip by ferry to Cowes on the Isle of Wight, where I'd never been before. On another free afternoon, we went to play golf at the public golf course on Southampton Common. I'd never played proper golf before. I was quite good at putting, because of hours of putting in the garden during childhood, and from playing minigolf. I also managed to strike the ball about 50 metres with a 5-iron, but never managed to loft the ball high and far, as I mentioned before.

Mike Lowe and his friends, including Graham Shore, became good friends of mine. They had both been students in Edinburgh before coming to Cambridge for Part III. There, they had been particularly influenced by Nick Kemmer, the Professor of Theoretical Physics, and had also had good lectures from Peter Higgs. Mike was a keen pub-goer and beer drinker, and had something of a beer-belly. In his first year he lived in college accommodation, but in his second he joined a group living in a rented house in Devonshire Road, by the station, close to the Salisbury Arms pub. I was encouraged to join them, but preferred a better organised life in a college flat by myself. The Devonshire Road housemates included John Bishop, who was Canadian, and Mike Stone, a smart third year PhD student in our group at DAMTP who later became a Research Fellow at St John's. Mike had an American girlfriend with whom he had an on-off relationship.

She was rather demanding of his attention in the evening (Mike – Come to bed!). Mike Stone had been in the Officer Training Core, and alarmingly had a revolver, which he once showed us. He later flourished in America, as a condensed matter theorist, becoming a professor at the University of Illinois in Urbana. There was also a history PhD student at Devonshire Road who had gone on much longer than the three years covered by his grant. So he lived on the dole. He had to be available for employment if a job arose, but claimed that his only skill was teaching 18th century Austrian and Polish peasant history (or something like that) so no jobs came up. This was a time of high unemployment and economic problems in the later 1970s. Another friend was Mike Gunn, who had also studied in Edinburgh and became a graduate student at the Cavendish, but lived somewhere else.

Through the two Scottish Mikes, I made a few visits to Scotland. It wasn't my first time there – that had been to Kirkcudbright during my pre-university time with the MoD. But this was the first time I'd visited Edinburgh and Glasgow. Mike Lowe's parents had a large, old flat near Edinburgh Castle, in Johnstone Terrace. The flat had a good view across the Grassmarket up to the turrets of George Heriot's school. The staircase was open, and the residents were required to take it in turns to give it a good scrub with bleach. I was impressed by Edinburgh, its solid stone buildings, the spectacular layout of the Old and New Town, and Arthur's Seat – the extinct volcano. I have a nostalgia for the sweetish smell of malt from the breweries near Waverley and elsewhere in the city, which has now disappeared from Edinburgh. Mike Lowe's parents later moved to a fine flat in Buckingham Terrace on the edge of the New Town, where I also stayed for a few days.

Mike Lowe's Dad was a successful businessman, being manager of the European arm of Uniroyal, an American manufacturer of car tyres. That meant working and living in Brussels, but he and his wife visited Scotland when they could. They all liked spending evenings out in the pub, but I was also offered haggis at the flat. One Saturday evening, or maybe a Friday, we got caught up in the crowds of drunken people on the High Street (Royal Mile) ejected from the pubs right at 10 pm. We then went to a chippy for a fish supper, and got jostled by drunken, swearing lasses, something that Mike thought I should experience, although he suspected I was a bit too refined, and English, to enjoy it. Once we went to one of the many public golf courses in Edinburgh to play, where I was outshone by the skill of Mike's younger brother Pete. Pete was a much more business-minded character than Mike, but not a talented mathematician/physicist in the same way.

From Edinburgh, Mike and I took the train to visit the other Mike in Glasgow. He was the son of Professor John Gunn, a particle physicist who occupied one of the large professors' houses on the main square at Glasgow University. From there we visited the Botanic Garden in Kelvinside, where I took an excellent photo inside the glasshouse called the Kelvin Palace, and we also saw the large crane that still survives beside the Clyde. I was surprised how narrow the river was, given its shipbuilding history. We also witnessed some clearance of the slum tenements, and patronised one of the grim pubs in Glasgow, with their bare floors, and men drinking whisky and beer from glasses side-by-side. Mike had a girlfriend Eileen from nearby Cumbernauld. They later married, but it didn't last very long. Mike was very sharp and witty, and gave me some hints about what pop music I should listen to (Blondie was a particularly good recommendation), but I also think he enjoyed learning from me something of the more highbrow European culture that I knew better. We also had good discussions about physics and the physics personalities we came across. Generally we were pretty critical of much that we heard about. This made sense at a time when ideas about fundamental physics were changing fairly rapidly. Mike Gunn

later became a professor himself, and head of the Physics department at Birmingham University, where I met him once.

Back in Edinburgh, I wanted to climb Arthur's Seat. We got up to the impressive Salisbury Crags, but in April it was too cold and windy to get to the top. I only made it there a few years later, one summer while staying at Pollock Halls on the edge of Hollyrood Park below the Seat. There's a great view of the City and across the Forth from the top. The eastern side of Arthur's Seat is surprisingly less steep, and softer and greener than the western craggy side.

I had a summer holiday in 1976 with Martin Gant to the Netherlands. This was a smaller-scale version of the hostelling trip to Norway a couple of years earlier. We started in Belgium, visiting Edgar and his family who had a flat in Rixensart, not far from Brussels. With Edgar and his girlfriend Margaret we went to Amsterdam, and then Martin and I continued by ourselves, seeing a bit more of the Netherlands before taking the train and ferry north to the island of Terschelling. We didn't have a booking there, and it was rather full, but the tourist office found us a bed and breakfast place rather far from the main town, to the east. From there one could explore the beautiful dunes and sandy beaches. I recall doing this by myself, so probably Martin was unwell for a day or two. On the way back to England we stopped in Delft and the Hague.

On a different occasion during one summer vacation I joined a group going to Lindisfarne (Holy Island) off the Northumberland coast for a week of music. This was arranged by a viola player I had met in Cambridge. It was really a Christian group, but they tolerated me joining in, as they needed a violinist. It was a long car trip to get there, and the next afternoon some of this new group of friends went walking around part of the island, whose beaches and sand dunes are rather similar to those on Terschelling. I immediately rather liked one of the girls in the group who came from Sheffield, but she had some relationship already, and perhaps I wasn't Christian enough. So nothing came of this. Maybe the Cambridge violist, who also conducted, liked me, but I didn't fancy her. The week proceeded with some chamber music that we eventually performed at the castle. But the highlight was the whole group practising Verdi's Stabat Mater in the church next to the ruined abbey, an austere work mainly for chorus, with rapidly shifting harmonies. It was a rare experience for me to sing rather than play violin. The work should have an orchestral accompaniment, but we only had an organist. One afternoon we had an outing to the Farne Islands, by boat from the little fishing village of Seahouses. (Possibly this was an outing from a summer workshop at Durham another year.) The group had been joined by some further people that day, with children and extra cars. It was quite an adventure to be on the open sea in a small boat, and someone was sick on the way back.

Let me now return to research. Following on from my interest in MIT Bags and ϕ^4 kinks, and their interactions, I started to investigate the fields and forces produced by several monopoles of the 't Hooft–Polyakov type at different locations. I assumed that the gauge group was SU(2), the easiest nonabelian case, and looked for a simplification of the equations, valid far away from the cores of the monopoles. There was no need to assume any symmetry for the configuration. The useful condition I found was that the gauge covariant derivative of the unit (i.e. normalised to unit magnitude) Higgs field should be zero asymptotically. This was more flexible and more accurately valid than requiring the covariant derivative of the complete Higgs field to vanish, because the field magnitude could still vary. My condition implied that the gauge field reduced to being of U(1) type, that is, purely electromagnetic. There was an interesting relation between the electromagnetic field and how the normalised Higgs field changed its direction around a monopole. The gauge and Higgs field could both be smooth, and still support one or more magnetic charges; there was no need for the Dirac strings of pure U(1) monopoles. The same

result had been discussed by Goldstone and others for the basic monopole with a single core region, but this was more general. It wasn't necessary to assume that the fields obeyed the first order Bogomolny equation, but they could do so consistently, in which case the Higgs field magnitude had an asymptotic Coulomb tail matching the tail of the magnetic field.

I gave my first DAMTP seminar on these results in October or November of my second year as a PhD student. One calculation relied on an algebraic manipulation that I made rather difficult. Ian Drummond pointed out that what I needed was the equality of a 3 by 3 determinant expanded by rows, and expanded by columns. That was nice.

Having understood quite well the monopole asymptotic fields, I thought of trying to calculate the way that monopoles move as they interact. This meant solving in some approximate way, at least, the full nonlinear field equations for the gauge and Higgs fields. One possible approach involved a matched asymptotic expansion, where the core fields of each monopole are treated as close to spherically symmetric, and additionally, these fields are required to match at large radius on to the combined asymptotic fields of all the other monopoles. I learnt about this method through work of Peter D'Eath, a relativity PhD student in DAMTP, who had calculated the force between two black holes this way. His result was the appealing one that the force is the same as the Newtonian force between massive pointlike bodies. The finite size of the black holes doesn't matter, just their masses. A similar result for two monopoles would be that the force is just the Coulomb force, proportional to the product of the magnetic charges. This is what I wanted to verify. I mentioned this project one coffee time to John Polkinghorne, who strongly encouraged me to pursue it, saying that it sounded like a good idea. This was one of the few occasions when John influenced my research directly. Peter Goddard also encouraged me to pursue research ideas of my own, like this.

The method of matched asymptotic expansions was hard to apply to monopoles, at least for me, but instead I discovered a method based on the idea that each monopole obeys a modified first order Bogomolny equation (one version for a monopole of positive magnetic charge, and another for an antimonopole of negative charge). The modified equation describes a monopole with a small acceleration, and is some kind of Lorentz boost of the standard static equation. It has an asymptotic form where it can be simplified to a relation between the U(1) electromagnetic field and the Higgs field. These fields need to match the combined tail fields of all the other monopoles and antimonopoles that are present, and this is the way to determine the acceleration of the original monopole, and hence the force between two monopoles. I found the Coulomb force that I wanted, which was exciting.

I discussed the result with Hugh Osborn, who also knew quite a bit about these monopoles. He thought I had made a mistake, because for two monopoles there should be an exact solution of the static Bogomolny equation, and therefore no force or acceleration. I went back to my calculations and found an error in a factor of two. For two monopoles the force was indeed zero, but for a monopole and antimonopole the attraction was double the Coulomb force. This was the expected answer. In the situation where the Bogomolny equation holds (of either sign) the Higgs field is massless, so it can produce a long-range force between monopoles. This Higgs force, being a scalar, is always attractive, but the magnetic force can be repulsive or attractive. For two monopoles the magnetic repulsion exactly cancels the Higgs attraction. For a monopole-antimonopole pair, the magnetic and Higgs forces are both attractive and of equal magnitude, doubling the basic Coulomb attraction.

This was interesting. Bogomolny monopoles have both a scalar charge and a magnetic charge, producing long-range forces. Werner Nahm later explained the physical effect of a monopole's

scalar charge. It reduces the Higgs field magnitude in the monopole's tail, which affects a neighbouring monopole by reducing its mass. This is typical of a scalar field's effect on a particle – it leads to a local mass modification that in turn produces a force if the scalar field has a spatial variation.

I wrote this up as my first paper, including a first section on the field asymptotics, using my idea for the vanishing covariant derivative of the normalised Higgs field. The paper was accepted by Nuclear Physics B, with very minor modification, if any. I was careful then to get my text and equations as error-free as possible, and have continued to aim for this. I was also fairly pedantic in working through the calculations in the paper, and trying to make them comprehensible as well as interesting to read. That makes the published papers a reliable source of methods and results for later work.

The rough work leading to the paper was mainly done on the backs of computer printout. At that time there were few printers, and results of numerical work were printed out at a central computer in another building, though DAMTP got its own printer later. The printout sheets were large and printed on only one side, and much output was discarded, so there were plenty of sheets piled up to reuse for theoretical calculations.

Before papers were accepted for publication in a journal, the HEP group produced preprints of these papers for informal distribution around the world. In exchange, we received two or three thousand preprints per year from elsewhere, greatly speeding up the dissemination of research. These were sorted weekly into different theoretical and experimental areas, and the titles typed up by the secretary. Our own preprints were produced four to six times a year, and there were annually about twenty in total. We had one of the worst duplicating machines, compared with other institutions, and graduate students had to staple the preprints together. Then they were mailed out in batches to about 300 addresses. This was one of the biggest jobs for the secretary. We joked that the main aim of the group, in these economically challenging times, was to boost preprint production! A key recipient of preprints was SLAC, the Stanford Linear Accelerator Centre. They had a preprint library, and sent us lists of all they had received. We often posted cards requesting preprints of interest to those physicists who had not sent them to us automatically. Equally exciting was to receive cards personally, requesting our own preprints. This was a measure of esteem, and of the interest our research had generated.

This system of preprint distribution was the forerunner of the arXiv repository of electronic preprints that started around 1991. We often thought the arXiv would make journals redundant, but it hasn't so far. Journals offer refereeing, quality control of content and layout (although less these days), but the most important reason for journals' survival is that governments have required that scientists continue to get their papers published in refereed journals. Such publications, and their citation counts, are important for job prospects and for getting research funding.

A few months after finishing my paper on the forces between monopoles I received a letter from a physicist at Syracuse NY, representing a group of researchers there. In analogy with Paul's Letters in the New Testament, I called this the Letter from the Syracusans. It said that my force calculation was ambiguous, and they had a different way to work out the result, getting the magnetic Coulomb force. Their method was a variant of the asymptotic matching method, in which they evaluated the rate of change of momentum of one monopole by integrating the stress tensor over a surface surrounding it. This method is a sound way to find the force on a monopole, and I have used it myself to study forces between other types of soliton. Because the Higgs field is massless when the Bogomolny equation holds, the Higgs field also contributes to

the stress tensor, and to the rate of change of momentum. In their published paper that came out later, the Syracusans referred to my work, and criticised it, but they derived exactly the same results as I had, so I don't think their criticism was valid. They ignored the way that in my approach, the acceleration of one monopole is indeed arbitrary when that monopole is considered by itself, but the acceleration is then determined unambiguously after matching the fields asymptotically to the fields of the other monopoles. This experience gave me confidence that I could do research as well as a group of more senior academics in a respected US university. They probably were not aware that I was only a second-year graduate student.

Now that the force between monopoles of equal charge was confirmed as being zero, it made sense to try to find exact, static multi-monopole solutions of the Bogomolny equation. A unique spherically symmetric solution was known, but this had unit charge. I tried to find a solution representing a chain of monopoles in a line, with axial symmetry and higher charge. I made considerable formal progress with constructing a consistent form for the fields, with axial symmetry, and finding the reduced equations, of which there were six. There was also a way to reduce the six equations to one, using some insight from instantons. Instantons were like monopoles, but in one dimension higher. They were solutions of a pure gauge theory without Higgs fields in four Euclidean dimensions, and had an interpretation as controlling certain amplitudes for quantum tunnelling in gauge field theory. I didn't work on the quantum aspects, but was interested in the mathematical form of the known instanton solutions. Some of these, due to Ed Witten, had axial symmetry. However, the related, simplified monopole equation had only complex solutions, and no real ones with the desired boundary behaviour and smoothness. So my approach didn't lead to new monopole solutions, only to some insight into how one might still find them, and the relation to instantons. This was written up as my second paper.

Yet other approaches to axially symmetric instantons appeared, in more than one guise, and could possibly have given new monopole solutions. However, I found that the various formulae that appeared in the papers, by Corrigan and Fairlie at Durham, Witten, and Wilczek in the US, could all be transformed into each other by gauge transformations. For instantons, explicit solutions emerged from these formulae, but unfortunately they didn't convert to explicit monopoles, even though monopoles are dimensionally reduced instantons. One reason is that a finite monopole charge corresponds to an infinite instanton charge. After a few weeks' work I wrote this up as a third paper, which Ed Corrigan described to me as "another breakthrough", a further confidence boost. This was early in my third year as a graduate student.

At about this time I received an invitation to give my first seminar outside Cambridge. It was from the theory group at the University of Sussex, whose members included David Bailin and Norman Dombey, and where Fred (Alfred) Goldhaber was spending a sabbatical. Fred came from the State University of New York (SUNY) at Stony Brook on Long Island, and was very interested in magnetic monopoles. He subsequently wrote very broadly about these, with novel insights into how fermions interact with monopoles of various types, and comparing the modern ideas about monopoles with historical ones going back to the middle ages. Monopoles old and new were never detected experimentally, which made Fred think that monopoles were analogous to unicorns – beautiful, single and mysterious, never seen, and retaining a fascination over many centuries. I met Fred at Sussex and several times later during visits to Stony Brook, and we collaborated on one paper during a sabbatical he spent in Cambridge in the early 1990s.

Mathematicians had become interested in instantons around this time. Physicists like Witten had found a limited class of exact instanton solutions, but mathematicians proved that the family of all solutions had higher dimensionality than this. The proof used a variant of the Atiyah–

Singer index theorem. Michael Atiyah himself went further, and with Nigel Hitchin in Oxford found a construction of all instantons. This wasn't quite explicit, but reduced the PDEs defining instantons to pure algebra. In cases where the algebraic equations were explicitly solvable, one could find explicit instantons. Atiyah and Hitchin didn't hurry to publish this, but when they heard that Drinfeld and Manin in Russia had a similar result, they quickly agreed to put out a joint paper, referred to as the ADHM paper. Atiyah came to speak about this in DAMTP – it was really the first time he had close contact with applied mathematicians, since previously he was much better known in pure maths circles. He was more famous than I realised at the time, being a Fields Medallist, and Royal Society Research Professor. After his talk, I was encouraged by Peter Goddard to speak to him, and told him about the Bogomolny equation for monopoles, and what we knew about monopole solutions. He found this a fascinating variant of the instantons he knew well, since formally, monopoles are a type of instanton, with the monopole fields being constant along the fourth (time) coordinate direction. But monopoles are not simpler than instantons because of the boundary conditions.

Eventually, in the 1980s, Atiyah and Hitchin, and their younger colleagues at Oxford including Simon Donaldson, Stuart Jarvis, Michael Murray and Andrew Dancer, made great progress understanding monopoles, using sophisticated methods that physicists were not familiar with. For many years, Michael was grateful to me for enlightening him about monopoles and their physical interest, on that day he spoke at DAMTP. One thing he liked was that monopoles are solutions in three space dimensions, so unlike instantons, they are truly a type of particle.

During my second year as a graduate student I had lived in a pleasant college flat at 19 Park Parade, overlooking Jesus Green. The only problem was that Chris Rogers, another student, wanted to practise his horn at the same time that I wanted to practise violin. This was the year I got involved with the Cambridge Symphony Orchestra, and needed to practise. Chris liked drowning out my noise but I couldn't concentrate with a competing horn. Eventually we agreed to play at different times. Chris was a mathematician who later became Professor of Statistics in Cambridge.

In my third year I had to live out, and found a room in a shared, rented house at 16 Highworth Avenue, off Milton Road. There were two girls there who didn't study or work at the university, but the principal renter was a curious character who studied parapsychology for his PhD. He told me about experiments involving telepathy and card reading, which he took seriously, although admitting that the evidence was never strong. He was trying to strengthen it, using subjects who had shown strong abilities at least once. The topic had enough academic support that he went on to a postdoc studying it, but I believe he left the field eventually. The spoon-bending tricks of Uri Geller had created great popular interest in these kinds of phenomena, and had initially convinced a prominent theoretical physicist from London, John G. Taylor. The reported 'paranormal' spoon-bending by kids who had seen Geller's tricks turned out to have been possible because they were left alone and had time to unfasten the containers holding the spoons. So public belief was shattered, as it was similarly for an extraterrestrial explanation of crop circles, which was a fad one summer, several years later.

A hobby of mine during graduate student days, that I continued later when back in the UK, was collecting Ordnance Survey maps. Previously I had very few of these, but in the 1970s many were sold off at discounted prices, and I bought them, a few at a time. The country had gone metric around 1970, so 1-inch maps were superceded and replaced by maps at 1:50,000 scale. In turn, these were rapidly revised and reissued, as the contours were redrawn at metric spacings, and the typefaces were modernised. So many types of superceded map were sold off.

I particularly liked one style of 1-inch map from the 1960s, for its elegant look. As I acquired more of these maps, I enjoyed spreading them out on the floor of my rooms at Park Parade and Highworth Avenue, and seeing whole fractions of the country like East Anglia or the south-west peninsular, on a large scale. Today I have 200-300 of these maps, but because they are at different scales, and from different series, there are still a few gaps in my coverage of the UK. Having all these maps has suggested many ideas for places to visit, and they have been very useful for finding the way around. I still much prefer having a map on paper, rather than a satnav or map on a mobile phone. However, I was impressed recently how my son Ben can find nearby petrol stations and places to stay rather easily by consulting his smartphone.

Shortly after moving into Highworth Avenue in late summer 1977, my parents visited at the time of my 25th birthday, staying in a St John's College guest room. Unfortunately my father became unwell that day with a stroke. He was visited by a GP at St John's and then transferred by ambulance to Addenbrooke's. We were advised to leave him around midnight, but about that time he died, and in the morning, when back at the hospital, they told us. It was devastating for my Mum, because she was still quite young (she was 51 and my Dad 56), even though she had been more aware than me of his health problems. I took more than a week off, and went back with her to Orpington. There she had lots of support from numerous friends, work colleagues and our relations. With Eric and Raya's help, we arranged the funeral at Golders Green crematorium.

After a week or so of stress I was unwell and feeling weak, but the Orpington GP diagnosed this as hypoglycemia, due to not eating properly. It was a problem that recurred over the years, so since then I have carried fruit-and-nut snacks with me wherever I go. Fortunately, I have seldom needed them, except when unexpectedly stuck in a train or something similar, or having to wait very long at a restaurant.

During my time at Highworth Avenue I needed a bike to get about easily, especially in the evening when the buses stopped. I enjoyed cycling, to visit my friends in Devonshire Road, for example. Once, I got into trouble with the police for cycling on the pavement on the Elizabeth Bridge and at nearby Mitcham's Corner when returning home. I thought that using the pavement was safer late at night, but at that time it wasn't allowed. (Now there are many shared paths for pedestrians and cyclists.) I had to appear before the magistrates and was fined five pounds plus two pounds costs. After that I stayed more on the road, but once got knocked off by a car driver opening his door without looking. I wasn't seriously hurt, but the bike was a bit damaged, and I didn't fancy cycling after that.

The same year, my third as a graduate student, Peter Goddard was away at CERN on sabbatical, and Hugh Osborn partly took over Peter's supervisory role. Hugh recommended that I try to understand some aspect of monopoles as particles in quantum field theory. I couldn't do this, but looked at a simplified problem involving kink solitons in one space dimension. I managed to construct an effective Lagrangian in which kinks had their own field which coupled to the more basic scalar field defining the kink dynamics. In the quantum theory, the kink field became an operator that could create or destroy kinks. This construction was a more informal version of ideas that worked rigorously for sine-Gordon solitons, but ultimately they didn't work too well in a more general theory, and certainly had no rigorous foundation. The coupled fields did give the right kink soliton profile in a certain classical limit, but one couldn't study truly quantum processes like kink-antikink pair production in high energy collisions. To this day, there is really no complete understanding of this strong coupling problem of dynamical solitons being produced or annihilated in pairs. The work was still good enough to convert into my fourth published paper.

I visited CERN about four times during Peter's sabbatical there, staying in the CERN hostel and having discussions with various theorists. This is where I met Werner Nahm, who made important contributions to monopole theory. Peter was also working on monopoles with Jean Nuyts and David Olive, leading to their understanding of monopole charges for general gauge groups. Peter had rented a house across the border in France for his family, on the slope of the Jura. I visited and played in the spring snow with his young children, and we also enjoyed a little music together. That summer I also joined a group including the former DAMTP student Brian Combridge (Combridge from Cambridge) for a day trip to Chamonix and a walk on the Brévent mountain opposite Mont Blanc.

While at CERN, I enjoyed some of the opportunities to eat out, although the two CERN cafeterias were also good places for food and coffee breaks, as I had learnt earlier. With Brian Combridge and others we had cheese fondue in Geneva, and once a beef fondue, where you cook the thin beef slices at the table. It was important with a cheese fondue not to consume too much cold white wine, as the cheese could solidify in the stomach, and there was a story of someone ending up in hospital as a result. With Peter (and perhaps his family) we once went to a village restaurant for dinner with fine steaks. They offered a choice of potato dishes to accompany these and I chose a gratin potato. It was then a grave disappointment that the chef was not willing to make this for one person late in the evening, because it was too complicated and time consuming, so I had to have something else. On another occasion I was invited to join David Olive, Raymond Stora and another physicist at a classy restaurant. Raymond was a permanent CERN staff member, known for his work on quantized gauge fields, who also held a position at Marseilles. Raymond and his friend, not unusually, smoked between courses, and I remember the waiter bringing bread so that they could clean their palates to better appreciate the course to follow. I was willing to pay my share of the bill, because I had reasonable support for my expenses, but in the end David and I had our shares paid by the others. That was some compensation for CERN staff being very well paid; their pay was also tax free, so I felt no guilt. But this did remind me that an academic career in the UK was not something one did for the money.

David Olive had been a lecturer in DAMTP until the time I was an undergraduate, but I didn't know him then at all. He resigned to take a 5-year staff position at CERN, a prestigious post. There was a vague promise that at the end of the five years he would be reappointed at Cambridge, but the job situation changed, because of a jobs freeze in the university and economic problems in the UK more generally. So he couldn't return to DAMTP. But at about the time of my visits to CERN, he managed to be appointed to a Readership at Imperial College in London. I didn't get to know him very well even then, but we met several times subsequently, and it was always stimulating to hear about his latest ideas. I heard him give several seminars, particularly on symmetries within particle theories, and on the interplay of symmetries with monopoles. He became an expert on the role of monopoles in supersymmetric field theories. With Witten he understood the supersymmetry algebra, including monopole charges, and this led to a precise prediction for the mass of a monopole or dyon in a supersymmetric theory. (A dyon is a particle with both an electric and magnetic charge.) He clearly found my work on monopoles interesting, and gave me encouragement. He also acted as one of my referees when I applied for postdoc jobs, and must have given me strong support.

While at Imperial, David invited me once to his flat in Putney, where he had a splendid collection of classical music recordings. Then, after he had been more than a decade at Imperial, he moved as a professor to a revitalised mathematics and theoretical physics group in Swansea,

led by Ian Halliday, who had also previously been at Imperial. For family reasons, David retained a house at Barton, near Cambridge, and visited this frequently. Much later, after his retirement from Swansea, I – as head of the HEP group by then – offered him a desk in DAMTP, which he used occasionally. He died and was buried at Barton in 2012, and I wrote the article about his life for the DNB (Dictionary of National Biography).

It was in November of my third year that I applied for postdoc positions. Having two or three papers, Peter Goddard thought my chances were good, so I applied to just six leading departments in the US, to the Ecole Normale Supérieure (ENS) in Paris, and also for a Science Research Council fellowship. Quite early in the New Year I received a 1-year offer from ENS. This was nice, but I still liked the idea of going to the US, so tried to chase some of the places I'd applied to for a quick decision, especially Harvard, where Sidney Coleman was a distinguished theorist interested in solitons. Harvard politely wrote to say they didn't have a job for me, so I accepted the ENS offer and withdrew the other applications. The ENS position was a Joliot–Curie fellowship, named after the Nobel Prize winning Irène Curie, daughter of Marie Curie, and her husband Frédéric Joliot, who together had created and recognised a number of artificial isotopes of nuclei. Just after accepting this offer, my tutor at St John's, David McMullen, asked if I would be interested in applying for a college research fellowship, possibly to be taken up after a year in Paris. Having been seven years in Cambridge, I was keen to have different experiences and interact with different researchers, so declined, but it stayed in my mind that I might be welcome to return to Cambridge sometime later, after a few postdoc years.

After the visits to CERN I wrote up my PhD thesis, which wasn't hard because it was just my four published papers put together in an organised way. But the thesis needed to be typed, and this was done by a professional who lived in the Cambridge suburbs. After completion, the equations still had to be written in by hand in the spaces left for them, and there were one or two figures for me to draw, too. Then the thesis was bound. It was quite short, about 80 pages, but this was normal. Mike Lowe and Graham Shore also completed their theses at this time, on soliton quantization and on quantum field theory in curved spacetime, respectively. Graham's was much longer than mine. Ray Gorley, the fourth student in our year, didn't progress so well, and didn't submit a thesis.

I had my PhD viva just before leaving for ENS. The external examiner was David Fairlie from Durham, and the internal was Hugh Osborn. David asked some detailed questions about the first section of the thesis, and was satisfied with the answers. We did not go through the whole thing, but a few other points were raised, and Hugh still had some scepticism about the last chapter on the quantum soliton ideas. Fortunately, a viva is mainly supposed to test if the candidate really understands what he or she has written, and to verify the authorship. At that time, it was also expected that PhD candidates should know some more general physics; Hugh asked me to say something about the parton model, which I could do. So I passed. I don't think David, on the train down from Durham, had got much further in detail than the first section of the thesis, but he was familiar with some of the later sections because of their relationship to work he had done with Ed Corrigan. David was probably quite impressed by the thesis, because he was keen for me to come to Durham later.

Let me conclude this section by recalling a few other graduate students in the HEP group. In addition to Mike Stone and Brian Combridge, starting ahead of our year were Roger Horsley, Peter Collecott and David Scott. Roger was Peter Goddard's first student in DAMTP, who worked on a string theory project until his PhD. Later he changed to working on lattice gauge theory, and had a long spell at Kaiserslautern in Germany before moving to Edinburgh. By the

time he finished his PhD hardly anyone was interested in string theory anymore (although it flourished again in the '80s). Peter was aware of this change of fashion, and had deliberately changed his own research interest to monopoles and encouraged me to work in that area, because he thought it would be the only way to give me and his subsequent students a chance of continuing in research. So I am very grateful for that. Peter Collecott was a rather suave character who went into the Diplomatic Service, eventually becoming ambassador to Brazil. David Scott was working on partons, but John Polkinghorne thought he may not flourish as a postdoc in a competitive environment, where the Cambridge version of partons wasn't much appreciated. So John recommended him for appointment immediately after his PhD to a Cambridge lectureship in Chemical Engineering. Those were the days! This showed us something we already knew, that theoretical physics was a high status subject, with smart people.

I remember a story concerning Brian Combridge's PhD work. He had done a parton calculation, and found a good match to experimental data. Then he discovered an error of $(2\pi)^4$ in the calculation. Remarkably, after further calculation and adjustment to the model, his new result also agreed quite well with the data. This indicated to the rest of us that the parton model was too flexible to have strong predictive power, and made us rather sceptical of the whole programme. Mike Stone also taught us a related lesson about calculations in high energy physics, and quantum field theory in particular. It is difficult to get all the factors right, he said, and first you should sort out the factors of $(2\pi)^4$, then the factors of 2, then the factors of i, and finally the signs. They are treacherous in that order of difficulty. I took this on board, and was always careful to go through a final round of calculations before completing a paper. This approach meant starting the calculations afresh in the right place, with some consistent conventions and notation, and tying all the bits of the paper together in a logical way. Frequently there were corrections to preliminary work necessary at this stage.

One student starting a year after us was James Stirling, who came originally from Northern Ireland. He worked with John Polkinghorne, who wanted by that stage to understand quantum chromodynamics (QCD) calculations properly and how they made the parton ideas firmer. But QCD calculations are technically hard, involving gauge fixing choices and new, unphysical fields called ghosts, as well as the quarks and gluons. All this is rather unintuitive, unlike the simpler partons whose spin and mass aren't too important. James learned the details of gauge theories and got his PhD in 1979, but John was not enthusiastic to move in this direction. John then made the brave decision not to continue with theoretical physics, and he resigned his professorship. This happened in my first postdoc year. He studied for the Anglican priesthood, eventually returning to Cambridge as chaplain of Trinity Hall and later becoming President (Master) of Queens' College. During this time he wrote some books, appeared on television, and became well known nationally as a spokesperson for how 20th century ideas of relativity and quantum mechanics can be quite consistent with Christian belief, and he did much to popularise and demystify, to the extent it's possible, ideas like quantum uncertainty and the idea of the universe starting with a Big Bang – ideas not literally consistent with what the Bible says. With his cheery smile, he also took on the hard religious sceptics.

James Stirling flourished in particle physics as a researcher and academic, working on parton ideas in the context of QCD. This was essential for understanding and predicting the physics at particle colliders like LEP and LHC at CERN. He became a professor in Durham and initiated the Durham Theory Institute (which replaced the theory group at the Rutherford Lab), then became a senior physics professor in the Cavendish Laboratory in Cambridge, from where he moved to be Provost at Imperial College. James combined important research with effective

administrative activities. I recall his excitement when we happened to be on a train together near Birmingham after a research council meeting, and he received a phone call saying that his bid for the Durham Institute had been successful!

17 Postdoc in Paris

In early October 1978 I moved to Paris. I travelled by train from Orpington, changed to a hovercraft for the Channel crossing from Dover to Boulogne, and then continued by train via Amiens to the Gare du Nord. The hovercraft trip was quite exciting, rather like being in a plane, as there were seatbelts and it was noisy.

I moved into a flat at 22 rue Mouffetard, in the 5th arrondissement in the heart of the left bank, near the Panthéon. My mother thought this was rather romantic. The flat was owned by a physicist, Cirano de Dominicis, based at Saclay. It was in an old, timber-framed building that had been modernised, with new plasterwork and windows, and modern electrics, kitchen and bathroom. The bedframe was fairly new, but basic and rather hard. I had some of my own things like a radio and record player, also books, clothes and violin. I can't remember if I transported all these myself from the UK, by lifting boxes and suitcases. Probably they were delivered by van.

The theoretical physics groups in Paris had split a few years earlier on political lines, in the aftermath of the Paris spring of 1968. A group of left-wingers stayed at Orsay, just south of Paris, but the more right-wing physicists formed a new group, closer to the city attractions. This was called the Laboratoire de Physique Théorique de l'Ecole Normale Supérieure, and located at 24 rue Lhomond in the 5th arrondissment. The founding head of the group was Philippe Meyer, descended from a rich banking family, and I believe he had helped endow the Joliot–Curie fellowship that I was awarded. I must say that the ENS group were not all conservatives, but they were more interested in physics than politics. The ENS lab was in a rather run-down early 20th century building, which housed some experimental physics groups, but the theory group had a nicely modernised and furnished broad corridor on one floor, with rooms each side, and a quite cosy atmosphere where it was easy to meet people. The afternoon tea and biscuits were more refined than in DAMTP.

I clearly recall deciding, on the train from Orpington, what I wanted to work on in Paris. I had become expert on gauge fields with symmetry from my work on monopoles and instantons, but the symmetry was always spherical symmetry in three dimensions or perhaps two-dimensional axial symmetry. I was familiar with the general formulae for SU(2) gauge fields with spherical symmetry first obtained by 't Hooft, and exploited by Witten; they were obtained using tensor arguments that were specific to three dimensions. What I now wanted to understand better was where these formulae came from, and how they would appear for general symmetries in more general geometries, in different dimensions, and for general gauge groups.

At ENS I started work on general symmetries in gauge theories, as planned. That meant learning some differential geometry, and the differential geometry of Lie groups in particular. After a while, I found classic books by Parisian mathematicians on this in the ENS library, and one especially useful one by Chevalley. I knew something about group actions and orbits from Cambridge, but Lie derivatives and left- and right-invariant vector fields on Lie groups were novel to me. The project made slow progress, but at some point I was getting new understanding each day, in small steps, often while thinking things over in bed at night. I made and kept notes of the progress in spiral-bound notebooks provided by ENS, so from around this time I have retained

a good record of my research activities. The basic condition associated with one symmetry is that the Lie derivative of the gauge potential should be equivalent to an infinitesimal gauge transformation. Combining the gauge potential with this infinitesimal gauge transformation leads to an analogue of a Higgs field. This was nice but not surprising, because in simple cases I knew that a symmetric gauge field looked like a gauge field in lower dimension, together with Higgs fields related to the components of the gauge potential pointing in the directions of the symmetry generators. The problem that remained was to determine how all the symmetry conditions could be satisfied simultaneously. What constraints were there on the Higgs fields?

My roommate at ENS was a Hungarian student from Budapest, Peter Forgács. He had a scholarship for a year or two to finish his PhD studies at ENS, and lived in the student accommodation. He knew a lot of formal mathematical physics, like functional analysis and the theory of operators on Hilbert space, but said that he'd got too much of this in Budapest. He became interested in my more geometrical project, and we decided to collaborate. Together, we eventually found the general form of symmetric gauge fields, and the algebraic constraints one has to solve to construct them fairly explicitly. The algebra is nontrivial if the symmetry group is nonabelian and its action involves a nontrivial isotropy group. We wrote this up. An important section of the paper was near the end, where we calculated the dimensionally reduced Lagrangian for the symmetric fields, which determines their dynamics. Starting with the standard Yang–Mills Lagrangian for a pure gauge theory, we ended up with a Yang–Mills–Higgs Lagrangian as the dimensionally reduced theory. The equations of the reduced theory are the same as the equations one derives from the original theory when the fields are assumed to be symmetric. The behaviour of the Higgs fields was a pleasant surprise, because their dynamics in the reduced theory is very close to what is usually assumed for physical Higgs fields, with standard covariant derivative terms and a quartic potential energy term that often implies spontaneous breaking of the gauge symmetry in the vacuum. I gave a talk on this at Jussieu, home of the universities Paris VI and VII near to ENS, where we often went for joint seminars. Peter Forgács was in the audience and contributed some comments. My seminar went on too long, because there were lots of technical details that I tried to explain. Several times subsequently, my talks were longer than the allotted time because of questions, and what I thought was the interest of the audience, but running over isn't generally appreciated.

Not long afterwards, Peter Forgács and I discovered that much of what we had done was covered under the topics of Invariant Connections and Wang's Theorem in the differential geometry textbooks of Kobayashi and Nomizu. Their language was hard for us to penetrate, because we had avoided talking about fibre bundles, but the final algebraic constraints were the same. Fortunately, the way we had applied our results to derive a Yang–Mills–Higgs Lagrangian was quite new, and our approach was also more accessible to physicists. So our work was noticed and appreciated. We checked, of course, that our general results were consistent with what was known in special cases. In particular we assured ourselves that the 't Hooft/Witten ansatz really gave the most general spherically symmetric fields for describing monopoles and instantons.

As a follow-up to this work I investigated how a pure Yang–Mills theory in six dimensions could reduce to a Yang–Mills–Higgs theory in four dimensions, and wondered if the standard electroweak theory of Weinberg and Salam could emerge this way. That would give a six-dimensional interpretation to the Higgs fields in nature. The idea works to a large extent, provided the two extra dimensions are in the shape of a sphere and the symmetry group is SO(3). There is a limited choice of possible gauge groups in the six dimensions, because of the algebraic constraints, so one gets predictions for the Higgs boson mass and for the Weinberg

mixing angle. In all cases, the Higgs boson has the same mass as the Z-boson, and three possible Weinberg angles are allowed. This was exciting, and I wrote up the results in a paper, but subsequent discoveries didn't match the predictions. Precision measurements of the Weinberg angle fail to match the closest prediction, by about 10%. The Higgs is also more massive than the Z, by about 50%, but this wasn't known till 2012. Other evidence for the extra dimensions is also lacking, but many variants of these ideas have nevertheless been pursued, not just in the context of gauge theory, but also with gravity included, and in the context of ten-dimensional superstring theory. I was involved with some of these later studies, as the dream of unifying our existing theories in the framework of a higher-dimensional spacetime remains attractive.

One discussion I had with Peter Forgács near the end of our year at ENS was about my ansatz for axially symmetric monopoles, developed earlier in DAMTP. We made slight progress simplifying the equations by a careful gauge choice, but didn't solve them. He took the problem back to Budapest, where he and his colleagues made significant progress in the next year or two, discovering that the equations could be reduced to the Ernst equation, an integrable equation well known to relativists. Using this insight, they could find true monopole solutions with axial symmetry by the iterative technique of Bäcklund transformations. So my earlier work had been useful after all.

There were other exciting things going on scientifically at ENS. The reason I had applied there was that the group had a strong reputation, and had made important contributions especially to supersymmetry, supergravity and string theory in the recent past. Those most active were Bernard Julia, Eugène Cremmer and Joel Scherk. I got quite friendly with Bernard. He was very knowledgable about a wide range of topics in mathematical physics, including monopoles, but his most important recent work had been on the maximal supergravity theory, and its relation to simple supergravity in 11 dimensions. I tried to understand this work, and decided it was necessary to master six key topics that I hardly knew. These were supersymmetry algebras, dimensional reduction of fermion fields, Fierz identities, Lie group representation theory, scalar field sigma-models, and some obscure duality ideas. Each was a large subject, and I made little progress on any of them. But I continued on-and-off to wish to work on supersymmetry, and eventually did so to a limited extent. I spoke a little to Joel, and to Eugène. Joel was often depressed and sadly, the next year, he died. Other physicists in the group included Jean-Loup Gervais, the head at the time, who was interested in solitons, and André Neveu, who had done important research on fermions in both field theory and string theory.

Some further discussions were with John Schwarz, who was a visitor at ENS for the whole year I was there. He and his collaborators had their own view of dimensional reduction, called consistent truncation, which had its own rules that never quite meshed with what I knew about symmetric gauge fields, although it seemed especially effective in supergravity theories. Our discussions to try to link the ideas up didn't really get anywhere. Things were a little clearer later, following publication of a book by Coquereaux and Jadczyk with the amusing title Riemannian Geometry, Fibre Bundles, Kaluza–Klein Theories and All That ... . They showed that in gravity theory, symmetry works slightly differently than in gauge theory, but their work was not specifically about supergravity.

When not working, John Schwarz was keen on tennis, and he also swam almost daily at an indoor pool near ENS. He suggested we go one day to the Roland Garros tennis stadium in the Bois de Boulogne during the French championships, but I can't remember who was playing.

I was impressed with how popular it was for physicists to visit ENS. A galaxy of well-known people came from Europe and the US sometime during the year, particularly in mid-summer when

ENS hosted an institute. They included Ludvig Faddeev, Peter van Nieuwenhuizen, Gerard 't Hooft, Louise Dolan and Dan Freedman. It was delightful to spend a few weeks of the summer in Paris, one visitor told me. Where better? Faddeev criticised me for not citing his group's work in my DAMTP paper about soliton quantization. I hadn't used his ideas, and didn't really feel bad about it. It was good at least to know my paper was noticed.

I did many other things in Paris, apart from the work leading up to my two papers and attending seminars. Particularly at the weekends, I explored many parts of the city with my green Michelin guide. Eventually, I had been through, or got in or out at, all the stations of the Paris metro except perhaps one. I have done nothing like this in London. I found it interesting to see the different types of trains and stations on the different lines, some historic and some modern. It's also exciting when the train climbs or descends rapidly from a tunnel to high above street level and back. I visited the famous sights like the Arc de Triomphe and Eiffel Tower at the beginning, but slowly got to know many less famous places, like the parks Monceau and Buttes Chaumont in the north, and several of the smaller museums, and many bridges over the Seine. The double-decker Bir-Hakeim bridge near Passy looks striking from various angles, and there's Paris's own Statue of Liberty near the next bridge. I didn't travel much by bus, because buses were slower and cost more than the metro except for short trips, but on later visits to Paris I preferred using buses, because one avoids the crowds and heat of the metro. There were several museums in Paris with collections of minerals and crystals. Particularly impressive, and beautifully displayed, was the collection attached to the universities at Jussieu. Across the road from there was a specialist bookshop where I acquired a geological map and guide to France. The map has been useful during subsequent years, and Anneli, Ben and I found a good ammonite in the cliffs above Boulogne with its help, and also the possible site of a meteor strike in the Dordogne region.

In the Louvre I saw many of the famous paintings, for example, Géricault's Raft of the Medusa, and I particularly liked the picture by Caravaggio of a Fortune Teller, slipping a coin away from an innocent young man. I acquired a booklet in French about this painting, which is calmer than some of Caravaggio's more bloodthirsty works that I only got to know later. Another place I visited several times was the Centre Pompidou. This was still fairly new, and had diverse events and exhibitions. The building itself was the main attraction, and there is a great view of Paris from the top of the long escalator.

For the first six months that I was in Paris, my cousin Anthony was also there, working for Reuters. He arrived six months before I did and lived in the 9th arrondissement, and we met several times for meals or sightseeing. He knew more of Paris than I did at the beginning, including a good, cheap canteen near his flat, with a fixed menu each evening and benches to eat at; I went there once or twice. Anthony had met Gisele earlier that year. She became his partner and eventually wife, and I recall meeting her for the first time when she came to Paris. After that, Anthony and I didn't meet so often.

Some of my colleagues at ENS were gourmets and tried new restaurants reviewed in the press, or revisited well established places that they knew. I was not invited to join these dinner outings, because it was thought my salary was too low for such extravagance. But the ENS physicists also went a couple of times a week to have good, modest lunches at restaurants nearby, including a Vietnamese restaurant, Duc Phuc, whose name raised a smile. The ENS canteen had lunch too, with a regularly repeating menu that included meat dishes, lentils, very nice tossed salad, and goats' cheese. So I had many novel dishes, without going to the gourmet restaurants. Sometimes, especially in summer, an alternative to a restaurant or canteen lunch was to buy a sandwich and

a drink, and take them to the Luxembourg Gardens. The metal chairs there were remarkably comfortable, and seemed not to get stolen. The flowerbeds were well stocked in the traditional French style. On trips out, I also enjoyed various types of food including Alsatian Choucroute, the French version of sauerkraut with sausage, which I had at the Gare de Lyon. Just once I tried escargots (snails) and found they tasted like shrimps.

Near my flat was a traiteur who sold ready-made delicacies. In the evenings I often had bread, cold cuts, fruit tart, and something more special from the traiteur, like egg and gherkin in gelatine. My breakfast was a more traditional coffee and cereal, as in the UK. Another local service, near place Monge, was a laundry, where clothes would be washed, dried and folded (but not ironed). That was useful.

An annoying part of walking near where I lived was the large quantity of dog poo left by the numerous dog walkers. The streets were washed most days by the city street cleaners, using a clever system of water valves and sacks of sand to send the water along the required gutters. So for a short time they were clean, but not for long. This was really a problem in most parts of the city, but I did notice that a few districts like the 16th arrondissement were noticably cleaner, perhaps because the houses were further apart, and there were private places for dogs to exercise. Maybe the local byelaws were tougher.

My Mum came to Paris for several days in November and stayed at a hotel on rue Gay Lussac. She knew Paris well so I worked part of the time, but we also did some sightseeing together. I showed her the lab at ENS, and my flat, and we had dinner at least once in my street, the rue Mouffetard. The immediate neighbourhood of rue Mouffetard was famous for having innumerable restaurants offering French cuisine and many other types. For example, Moroccan restaurants offering couscous were a novelty for us. The gourmets thought these restaurants were all bad, but I went to several of them, and some I rather liked. At least they were in my price range. Typically in the evening someone would come round selling flowers, which I didn't need; musicians also went from one restaurant to the next to busk, and that was preferable.

I had learned French at school, but found it was quite inadequate for real conversation, so at work would usually speak English. Only the lab secretary Nicole was sympathetic to my efforts at speaking French. The Parisians speak fast, using many key phrases absent from school French. I tried to improve, by reading the newspaper almost daily, and listening to the radio. Some of the more academic discussions that were broadcast were initially quite interesting. However, much of what is said has very little content, consisting rather of platitudes, like "How one should think about this issue depends on the way one defines it." It was more difficult to pick up popular culture – perhaps I should have tried harder. At some point I met someone who knew how to swear in fifty languages, but this wasn't my style.

It was ultimately frustrating not to be fluent in French. Once I had a date with a rather young cellist whom I had met, and she found it odd that my French was worse in some ways than what a 3-year old can manage. So that got nowhere.

Early during my time at ENS I had to apply for a new postdoc position, and applied again to the places in the US that I'd tried before. This time I was more successful, and received an offer from the Center for Theoretical Physics at MIT, as well as at least one more offer, but not from Harvard. It probably helped my case that Jeffrey Goldstone had moved to MIT from DAMTP, and knew me; also by then I had four sole-author papers, and had completed my PhD. People had also got to know me at CERN. Partly because of the social problem of not being fluent in French, I was pleased to have a job to go to in the US at the end of the year, and declined an offer of an extension at ENS.

I made a few trips outside Paris. The first, in early December, was by train to Brussels for a few days to see Mike Lowe; Mike was then a postdoc at the Université Libre de Bruxelles (ULB), and his parents were also living in Brussels. I recall the colourful art at the metro stations there. To get to Mike's flat in the suburbs we had to take the metro, and then cross a car park by the station. That evening there had been drizzle followed by a sharp frost, and I have never experienced such a treacherous, slippery surface. Mike still liked beer, and introduced me to the fascinating, strong, typically Belgian beers like the cherry-flavoured Kriek. He also showed me his office at ULB. The facilities weren't very good compared either to the rather shabby but busy DAMTP, nor to the swish ENS. That day there was a seminar at Louvain, and we went along, as Belgium is small enough for just about all Belgian theoretical physicists to gather for seminars in one of their universities. At the weekend just before I returned to Paris, Mike, his parents and I had a pleasant walk in the Forêt de Soignes. The following March, I visited Cambridge briefly to collect my PhD, joined by family and friends, and later in the spring went on a day trip to Fontainebleau, and was impressed by the palace, which bears witness to the arrival of the Renaissance in France.

In Paris, I got involved in some music, and joined an orchestra at the UNESCO building. It was quite small, and semi-professional, as some of the players were political refugees from South America who were paid for rehearsals and concerts. A few of them were rather good. The conductor was also from the Spanish-speaking world. We played unambitious pieces, including one or two of the Four Seasons by Vivaldi. The leader was a good soloist in Winter, and we repeated this a few times, partly for diplomatic gatherings at UNESCO. In summer we played once at Versailles – that was the first time I saw the palace and gardens. I also went to other concerts. Occasionally, Nicole at ENS had a spare, complimentary ticket for a fine event to give me, and several times I went to Radio France where concerts were free. I enjoyed these, but felt that French audiences for classical music were polite rather than enthusiastic. A highlight on the radio was a series of programmes playing and discussing recordings made by Wilhelm Furtwängler years back. I was particularly struck by a performance of his with the Berlin Phiharmonic of Beethoven's 7th, recorded in Cairo, and managed to purchase the record of this which I still treasure today.

On many Saturday mornings, I walked to the library next to the Jardin des Plantes to borrow records. In addition to classical music, I liked albums of Jacques Brel songs that Gisele had recommended. Once, in the library lift, there was a man wearing a St John's College scarf, and I asked in English if he'd been at St John's? He said "Quoi?" and obviously didn't understand. Probably someone had visited Cambridge and brought him the scarf as a present.

Another event at UNESCO, nothing to do with the orchestra, was a lecture by Paul Dirac on the occasion he got some award. That was the only time I saw him, because he had retired from Cambridge just before I arrived as an undergraduate, and I didn't meet him when he occasionally visited St John's thereafter, for dinner with the Fellows. His lecture was a rather standard one for him, where he spoke about the difficulty of the infinities of quantum field theory. He expected that the ultimate theory would not have them, but it's still a challenge to find such a theory. Some theories, like supergravity, almost avoid infinities, but they don't closely describe the particles we observe.

Near the end of my ENS stay I went to a conference at CERN, travelling by train from Paris. That was the last visit I made to CERN for many years. The conference was disappointing. One talk I remember was by Alan Irving, a physicist from Liverpool. He'd been asked to review Regge phenomenology as applied to strange mesons and baryons, something he was expert in. His talk

was fine, but he also made clear that this approach, avoiding explicit quarks, was not making much progress any more. Afterwards, someone asked him if he was still working on this and he replied "Good God, no! I'm doing lattice QCD." Much more interesting was a summer institute at Durham shortly afterwards, one of the first bringing together mathematicians and physicists with common interests in geometry, Yang–Mills theories and solitons. There were inspiring talks by Atiyah, Chern and Coleman. I learned that Yang and Chern had interacted in China many years earlier, which explained the relationship between Chern's theory of connections, and Yang's rediscovery and reinterpretation of them (with Mills) as physical gauge potentials.

I didn't follow French politics or current affairs very much, despite reading the newspaper Libération regularly (an easier read than either Le Monde or Figaro). But noticeable was French pride in their technological progress, which had taken off with the Concorde project a decade earlier, and included further development of their aircraft industry in Toulouse, nuclear power plant construction all over the country, and electrification of the railways. The high-speed, TGV rail network was also being developed, and its first passenger service started a couple of years later. As a souvenir of my year in Paris, I bought a book called La France Maintenant (France Now). It celebrated this progress.

18 Supersymmetry

Rather than continuing chronologically, this section will focus on one topic, describing my interaction with the subject of supersymmetry (SUSY) and with some of the people involved in its study.

I was aware of supersymmetry while a graduate student at DAMTP. The idea had emerged in the early 1970s, as an ingredient of certain field theories. It also appeared at about the same time in the development of string theory. As string theory incorporates both Einstein gravity and gauge field theory in its low-energy limit, there is a supersymmetric version of gravity, called supergravity (SUGRA), although it is also possible to define and investigate this independently of string theory. By the late 1970s, when I was a postdoc, the study of all these things was a major part of theoretical physics.

SUSY is a type of symmetry that relates the bosonic fields of a theory, including gauge fields, gravitational fields and scalar fields, to fermionic fields, the type associated with quarks and leptons (electrons, neutrinos and the like). Another way of saying this is that SUSY is a symmetry unifying particles with integer and half-integer spins. But early attempts to interpret SUSY as directly relating particles we know didn't work. This is usually because there is a mismatch between the charges. A pair of particles where a close link was plausible was the photon-neutrino pair, since both appeared to have zero charge and zero mass, but there is only one photon and several neutrino types, and their weak interactions are very different, so that didn't work. (Neutrinos in fact appear now to have very small nonzero masses, while the photon is still massless.) This example was investigated by Pierre Fayet, who had been at ENS before me and returned there later.

SUSY is still attractive as a unifying idea, but to use it one needs to take a theory with observed particles, like QCD, and extend it in a supersymmetric way. This is possible, though sometimes technically hard. The resulting theory has the particles we know together with an equal number of new, SUSY partner particles. The partners have fancy names like gluinos (paired with gluons) and squarks (paired with quarks).

An important feature of a SUSY theory, apart from its enhanced symmetry, is that there are

potential cancellations of the awkward infinities occurring in quantum field theory calculations. A simple example of cancellation occurs in the SUSY harmonic oscillator, a non-relativistic quantum mechanical system. Here, the ground state energy is naturally zero, unlike in the standard harmonic oscillator, where there is a positive energy ground state, due to zero-point fluctuations. In the SUSY oscillator this positive energy is cancelled by a negative contribution from the fermion. One can also see this cancellation algebraically. Simple free field theories have infinitely many harmonic oscillator modes, so such field theories have a zero-energy ground state if they are supersymmetric.

Another attraction of SUSY is the way gravity can be combined with field theory. The quantum mechanical unification of gravity and particle physics is a holy grail for many theoretical physicists, and SUGRA is one step in this direction. SUSY string theory (superstrings) goes even further, because it has a single dynamical principle, has some of the attractive finiteness of SUSY, has a graviton state (a quantized gravitational wave state), and it incorporates many particle types that are similar to what we have experimentally observed, like photons. Superstrings can really only be consistently defined in 10-dimensional spacetime, but there are many ideas for how to interpret its results from a four-dimensional perspective, the familiar perspective of standard, relativistic physics in Minkowski space.

What then are the problems of SUSY. Well, many! When one extends a known theory like QCD to be supersymmetric, the theory predicts that all the SUSY partner particles have the same masses as the known particles. But such particles are not seen, so one needs some mechanism that breaks the supersymmetry. There are several proposals for how to do this, but none is very elegant. Using one or other of these mechanisms, it can be arranged that the SUSY partner particles have much higher masses than the standard particles, but the constraints on their masses are not strong, so almost any mass pattern is possible. SUSY in this broken form is much less predictive than one might hope for. Vast efforts have been made by the experimental particle physics groups to search for these new particles at ever higher energies, but they have never been detected. This has gone on for over 40 years. I recall on a visit to CERN seeing a rack of experimental preprints; most of the titles were "Search for supersymmetry ...," meaning that in yet another reaction or at a new energy frontier, SUSY was not seen. When new accelerators were proposed, one of their main scientific goals was to uncover SUSY. Ed Witten and others said that the goal was to discover the 'fermionic nature of spacetime', which sounded good. Another goal was to discover the Higgs boson, the 'origin of mass', and this, fortunately, has recently been achieved.

Another problem is the issue of mathematical elegance. The enhanced symmetry of SUSY suggests that spacetime can be extended geometrically to combine bosonic and fermionic coordinates. This is a variant of the idea that, for example, the Lorentz symmetry of special relativity allows one to consider Minkowski spacetime, which unifies space and time coordinates into one geometry. There are, in fact, successful constructions of superspace (more precisely, superspacetime), and in this arena the basic dynamical objects are superfields, but there are all kinds of technical stumbling blocks with this. SUSY comes in several variants, labelled by the number $\mathcal{N}$ of SUSY symmetry generators, and although the $\mathcal{N} = 1$ version has a nice superfield description, the $\mathcal{N} = 2$ version of superfields requires what is known as harmonic superspace, something understood by rather few people. Finally the $\mathcal{N} = 4$ version of SUSY has no accepted superspace or superfield description, and one has to use $\mathcal{N} = 1$ or $\mathcal{N} = 2$ tools. That makes the potentially very appealing $\mathcal{N} = 4$ theories hard to work with. Supergravity in its superspace versions is also inelegant, in my view, because the Riemannian geometry in the fermionic directions is subject to

a number of tricky constraints. It was curious to hear once, at a conference, back-to-back reviews by Julius Wess and Bruno Zumino on supersymmetry. They had pioneered the subject of SUSY field theory in a famous paper, and had collaborated frequently. Wess's talk used superspace and Zumino's talk did not, showing that the utility of superspace remains controversial.

These mathematical difficulties mean that calculations in SUSY theories can be algebraically very messy. Extending an existing theory to have SUSY is almost always possible (it is said that one can even supersymmetrise one's grandmother), but the construction can require pages and pages of hard algebraic manipulations, and the result is a Lagrangian with a very large number of terms, and many random-looking coefficients (although they all need to have definite values). Such calculations appealed to the most active protangonists, but hardly to me. The calculations often took weeks, but could be enjoyable. Peter van Nieuwenhuizen said how much he liked doing them, but he needed to leave the noise and distractions of New York for the quieter Stony Brook to make progress. I felt that in these kinds of calculations there was no deep geometrical insight underpinning the algebra.

Mathematicians have the view that superspace geometry is not elegant as a geometrical theory, in the way that they see it, and that explains the algebraic complications. There is a long history of axiomatising geometries, and getting beautiful mathematical results concerning them, but that hasn't really worked for superspace geometry. Superspace simply can't be visualised. The fermionic coordinates have something to do with spinors on the background of a standard manifold, but there isn't a true geometrical unification of the bosonic (ordinary) and fermionic geometric ingredients. The same applies to physicists' descriptions of SUSY theories. The bosonic part is often written down explicitly and has interesting nonlinear and geometric structure; it could be a variant of gauge theory, or of Einstein gravity. But the fermions are then just tacked on, and don't have an important effect on the structure. For example, classical solutions are often found for the bosonic fields, and could represent monopoles or black holes. But then the fermions just come along for the ride, and don't change these solutions. The only role of the fermions is to control some of the quantum corrections, and avoid infinities. In the nicest cases, the classical energy of a classical solution is quantum mechanically exact, because all quantum corrections cancel out. This is only possible in a SUSY theory, and is one of the attractions of SUSY, but it means that mathematicians prefer just to think about the classical bosonic field theory, and ignore fermions and quantum effects altogether. I should add that one strength of SUSY is that the energy or mass of a particle in a supersymmetric theory can sometimes be calculated purely algebraically in terms of its charges; this is a quantum mechanical version of the Bogomolny argument relating the classical mass of a soliton to its topological charge.

Michael Atiyah had a very specific criticism of SUSY. For him the Dirac operator is a beautiful and mysterious object, as more generally is a spinor on a curved manifold. The Dirac operator has both negative and positive energy levels. In the quantum theory of fermions, the negative energy levels (the states in the Dirac sea) are filled, and this leads to remarkable topological effects like spectral flow that are related to the index theorem in mathematics and anomalies in physics. In SUSY one tries to unify bosonic and fermionic fields, and so one loses these interesting aspects of the fermions. Technically, this appears to happen because SUSY theories often require the fermions to be of Majorana type (satisfying a reality condition), and Majorana fermions couple to other fields in such a way as to suppress topological features like spectral flow. I planned to study in detail the role of the Dirac sea and topology for Majorana fermions, but never got round to it. Michael thought that it is desirable to have a Dirac sea and spectral flow in our physical theories, and concluded that SUSY is unlikely to be physically plausible.

Where do I come into all this? Throughout my research career, from about 1975 onwards, a large fraction of the theoretical particle physics community has been working on SUSY; there's also been a major experimental effort to uncover SUSY. A consequence has been that members of the community, like myself, have been under some pressure to join in. There have been countless seminars and papers on SUSY ideas, and at times it was very difficult to be seriously considered for an academic post if you were not active in this area. Also, from time to time, it seemed that discovery of SUSY was just around the corner. Fortunately, there were always SUSY sceptics, and I was particularly influenced by those who were sceptical about the mathematical elegance of the idea. So for much of my career I resisted the siren calls.

This got more difficult following the superstring revolution. In 1984, Mike Green and John Schwarz discovered anomaly cancellation in selected versions of superstring theory. This made superstrings very attractive to theoretical physicists, and there was great whoop-la in popular science circles. The anomaly-free superstring theories in ten dimensions were some of the best-behaved quantum theories incorporating gravity and particles, and almost unique. Many people piled in to study these 'Theories of Everything'. There was a lot to do because there are many schemes (virtually infinite in number) for interpreting the theories in four spacetime dimensions, and for trying to connect them with gravity and particle physics as we know it. However, my first research project had been on strings, and I had successfully left this behind, in parallel with Peter Goddard moving into monopole research. This had been successful, so I didn't want to re-enter string theory as a minor player. Nevertheless there was pressure, because in the early 1990s, Ed Witten, who became a major string theorist, announced publically that if people were not working on superstrings they were essentially lost from the mainstream of theoretical physics.

There was a second superstring revolution later in the '90s, when Juan Maldacena showed that in certain SUSY theories, derived from string theory, one could replace quantum field theory calculations by simpler calculations involving gravity. That was because quantum gravity in a spacetime corresponds to quantum field theory on its boundary, and sometimes the quantum gravity can be well approximated by classical Einstein gravity. This idea initially worked in very restricted contexts, but further research showed that it was more broadly valid. The idea is not really rigorously proved, but is believed to work, and there's considerable evidence supporting it. It has more recently been applied in non-SUSY contexts. This is exciting, because it is now thought that certain properties of particles, like the viscosity of the quark-gluon plasma, or properties of strongly interacting electrons in condensed matter systems, can be calculated by studying particular black hole spacetimes, analytically or numerically. Also, in the other direction, the quantum properties of black holes are coming under better control.

I still didn't get involved, because now I wasn't expert enough in gravity theory and black holes, which often appear here in other than four dimensions.

So what did I do in SUSY, if anything? My first contribution was actually very useful. It was to work out a consistent way to couple 10-dimensional supergravity theory to a nonabelian SUSY Yang–Mills gauge theory. This was part of the low-energy structure of 10-dimensional superstring theory, although I didn't know the details of the latter. The project arose from a visit in 1982 by George Chapline, whom I had met earlier in Santa Barbara, to Stony Brook, where he had a discussion with Peter van Nieuwenhuizen (PvN). PvN and collaborators had found the way to couple 10-dimensional supergravity to an abelian gauge theory (just having photons). Their paper implied it was simple to make the nonabelian extension. George was more sceptical, and brought the problem to me at Aspen, the summer institute in the Colorado Rockies where we had agreed to meet for a week or two. I recognised that part of the abelian

theory involved a Chern–Simons term, something that is gauge invariant but not in a trivial way. I was familiar with the nonabelian version of a Chern–Simons term, and thought we should use this. But here things are a bit more complicated algebraically, and initially our construction had no supersymmetry. Our breakthrough was to combine the available fields in the right way, including the rather mysterious 2-form field B. This went slightly beyond what PvN and collaborators were aware of, and we found an interesting nonabelian form of the gauge transformations and supersymmetry transformations. The transformation for the 2-form field B was rather unexpected. After a week's work, we could write a paper. Our paper also discussed the fermionic fields, closely following the earlier work, but here we made a small mistake. PvN found later that we needed one additional, quartic fermionic interaction term to ensure full SUSY and gauge invariance.

George and I were interested in theories like this, because earlier in Santa Barbara we had together developed the formalism for using symmetry to reduce a 10-dimensional theory to four dimensions, building on my earlier work in Paris on symmetries in pure gauge theories. So we thought the dimensional reduction of our 10-dimensional SUGRA coupled to SUSY Yang–Mills was worth investigating. I looked into the details of dimensional reduction in the context of SUGRA later that year, while visiting Oxford for a term, but nothing very interesting emerged. Instead, the impact of our paper was that Green and Schwarz found that they could do an anomaly calculation in 10-dimensional superstring theory by exploiting our SUGRA–Yang–Mills theory. The coupling we had found between the B-field and the other fields, and the Chern–Simons structure, was exactly right for them to prove anomaly cancellation in superstring theory, provided the Yang–Mills gauge group was one of just two possibilities, SO(32) or $E_8 \times E_8$. We had had no reason to focus on these particular gauge groups ourselves. Their stunning result, referred to as the first superstring revolution, appeared a couple of years after our paper, and I think they were influenced by having heard us talk about it at Aspen. They realised then that what we were saying was interesting. Publication of my paper on dimensional reduction of SUGRA was significantly delayed, so in the final version I could look at the particular example of SUGRA coupled to an $E_8 \times E_8$ gauge theory, one of the Green–Schwarz anomaly-free examples, and this was more interesting than the general case.

The Green–Schwarz superstring work made them famous, and they were awarded substantial prizes later. Michael Green became Lucasian professor for a few years before he retired, following Stephen Hawking. George and I also received numerous citations for our paper, and it is one of my most cited, years later.

My first serious interaction with SUSY ideas was actually earlier, around the time I was finishing my PhD, and it was a bit indirect. As I mentioned before, David Olive, with Peter Goddard and other collaborators, had established results on the possible charges of monopoles, generalising Dirac's old result on magnetic charge quantization. Just as ordinary particles have electric-type charges forming a multiplet determined by a representation of a gauge group, so magnetic monopoles have charges classified by a representation of a dual group. The dual group and original group may be quite similar, having for example the same Lie algebra, but differing topologically and having different centres. David Olive extended the duality idea, working at CERN with Claus Montonen, who was Finnish but had done graduate studies in DAMTP a bit before my time there. David wanted the dual theory of monopoles with its dual gauge group to be a full dynamical theory of monopoles. This required the spins of monopoles to match the spins of gauge particles in the original theory. Naïvely this didn't work, because gauge particles have spin one, and most basic monopoles are spherically symmetric and have spin zero. The forces between

monopoles should also match what one expected in a gauge theory. Here things worked better. My classical monopole force calculation could be matched to Feynman diagram calculations for scattering of electrically charged particles, from which one can derive the forces between them. The Higgs particle is massless in the case of interest, so just as I found cancellations between forces for magnetically charged monopoles, so similarly the forces cancel among the electrically charged particles. The duality seemed to work here. This was the particular reason David Olive found my work valuable.

A little later, Hugh Osborn resolved the problem of the spins. He calculated that if monopoles interact with fermions of spin half, then some monopole states acquire spin half or spin one too. This works best in a SUSY version of the field theory. Here one finds the same multiplet of different spins among the monopoles as for the original, electrically-charged particles in the theory. All the evidence points to the Montonen–Olive duality working perfectly in this SUSY context, at least in the best cases, where charges are abelian and there is $\mathcal{N} = 4$ SUSY. SUSY also gives a very symmetrical formula for the masses of all the particles including the monopoles, respecting the duality. The mass formula agrees with what one would calculate classically for monopoles satisfying a Bogomolny equation. So it was realised that the classical monopole solutions of the type first found by Prasad and Sommerfield, and understood more deeply by Bogomolny, fit most naturally into quantum field theory when one is in a fully supersymmetric framework (most simply with $\mathcal{N} = 4$ SUSY). From then on, low-energy quantum states of monopoles and all kinds of other particles in SUSY theories were called BPS states. BPS states occur in a wide variety of contexts that have nothing directly to do with what B, P and S did, for example, for certain types of black hole. In fact, rather few people who mention BPS know who these physicists are, but I met all three. Prasad became a postdoc at MIT while I was there, Sommerfield was his professor at Yale during his graduate student days, whom I met once when giving a seminar at Yale, and Bogomolny attended a conference on monopoles at Trieste that I went to, although he moved later into quantum chaos theory, and lived in France rather than Moscow.

An extension of the duality predictions was made by Ashok Sen. He noticed that certain dyons, which are particles combining magnetic and electric charge, could be treated as fundamental. The duality was not just an exchange symmetry between electric and magnetic phenomena, but was a whole SL(2,Z) group of transformations mixing magnetic and electric charges. Again, SUSY is essential for this to work. To verify the duality, it was necessary for Sen to show that certain dyons were stable particles. In particular, a dyon with two units of magnetic charge and one unit of electric charge needed to be stable. The interaction of two monopoles of unit charge could be studied using the two-monopole moduli space, something that I had investigated (this is discussed further below). Sen's stable dyon arose as a quantum state on this moduli space – its wavefunction was a localised harmonic 2-form that he could calculate, giving the right charge and spin. Sen's SL(2,Z) duality idea had wide ramifications in SUSY field theory and superstring theory. It was given the name of S-duality, and led to many predictions in these theories. Rigorous proof of these duality ideas is hard, but they are supported by evidence from many special, nontrivial situations. The subject has generated many research problems for geometers and physicists, concerning the geometry and topology of moduli spaces. Although I have not worked on these ideas much, I have been over the years to several workshops and seminars where they have been discussed. One starting point for investigating dualities is the notion that the Riemannian geometry of moduli spaces of solitons is important, and my original paper on BPS monopole moduli spaces has become another of my most cited.

After returning to Cambridge and St John's College in 1985 my research was on non-supersymmetric monopoles, partly in collaboration with Gary Gibbons, but SUSY was still on my mind. This was the time of greatest enthusiasm for the superstring revolution, and Fellows at St John's and others I met there were keen to discuss it.

There was a very interesting paper by Ed Witten that had appeared a few years earlier, called Supersymmetry and Morse Theory. Morse theory is a topic in differential topology leading to relations between the stationary points of a function f on a manifold, and the topological structure of that manifold. Ed Witten had simplified the Morse theory proofs using SUSY ideas. His work also gave physical insight into what was going on. The supersymmetric viewpoint meant that one had to consider the quantum mechanics of a particle moving on the manifold, and stationary points of the function f became stable locations for the particle. (The squared gradient of f was the potential energy of the particle.) This work was appreciated by mathematicians more than SUSY field theory was, because the geometrical content was deeper. A little later, SUSY was used to give new, not quite rigorous proofs of the Atiyah–Singer index theorem and similar results. Again the mathematicians were impressed, and the results were made more rigorous subsequently. This part of SUSY theory was a development of the notion of supersymmetric quantum mechanics, which I found rather appealing.

To understand Witten's work better, I encouraged my graduate student Nick Mee to look at special cases of SUSY quantum mechanics and Morse theory, where the quantum wavefunctions could be calculated explicitly. He eventually found particularly good examples to study, where the underlying manifold is a complex projective space. Here there is a lot of geometrical symmetry, and the wavefunctions can be presented either explicitly, or graphically after some fairly simple numerics. The results illuminated Witten's results, but weren't very novel, so although Nick got his PhD, it wasn't possible for him to get a paper published. I maintained sporadic contact with Nick over the years after that, hearing about his work in educational mathematical software, his association with the sculptor John Robinson, some of whose work is outside the Isaac Newton Institute, and receiving a copy of his book Higgs Force – Cosmic Symmetry Shattered. More than two decades after Nick received his PhD, we met again, and agreed to collaborate on writing another book. That book is a survey of physics called The Physical World.

SUSY quantum mechanics is obtained by a fairly standard quantization procedure applied to a classical supersymmetric mechanics. This matches the idea that SUSY quantum field theory is obtained by quantizing a classical field theory with a supersymmetric Lagrangian. In classical SUSY mechanics, the variables are a particle position and some fermionic partners of this. This sounds simple, but is a conceptual morass. For consistency, all the variables have to take values in a Grassmann algebra, with the position variables being even and the fermionic parts odd. The classical equations of motion couple these together. Now, a subsector of the even part of a Grassmann algebra consists of ordinary real numbers, and if all the odd (fermionic) variables are zero, a consistent solution just involves these real numbers. This is ordinary mechanics for a particle. But when the odd variables are nonzero, they usually generate nonzero parts for the even variables, outside the real numbers. Such quantities are physically mysterious. What do they mean? It is similar in SUSY field theory. The bosonic fields are not just real-valued, as one is used to in classical field theory (in electromagnetic theory, for example), but they are even Grassmann-valued.

I wanted to understand this classical version of SUSY better, and worked on it with my student Roger Heumann. We made our life simpler by assuming the Grassmann algebra is finitely generated. That meant the coupled equations of motion close, forming a finite system. It

was argued that for formal reasons, the algebra should be infinitely generated in a field theory, but that argument does not apply to SUSY mechanics. We found examples, like the 1-dimensional harmonic oscillator, and particle motion in a Coulomb potential, where all the equations could be solved. It was still difficult to interpret the results, and they didn't seem too closely related to any physically familiar system. The interpretation seems to be that the real even part is a genuine classical motion, and the odd part adds some fermionic character to the motion, sometimes related to particle spin. The non-real even parts represent a deviation away from the real trajectory. It is as if a solution of the SUSY equations of motion represents a whole family of real trajectories at the same time, treated via a formal perturbative expansion in Grassmann-odd parameters. This is somewhat like the fuzziness associated with classical interpretations of quantum mechanics, but we could never make this precise. Statistical mechanics also treats all classical trajectories at the same time, so maybe there is a link between this and SUSY, but we never investigated in detail.

Roger progressed with this project rather slowly, and unfortunately we made a technical error setting up the example of the Coulomb potential. We found a SUSY superpotential that worked, but there was a simpler one available, and our approach was much more complicated and less symmetric than necessary. In our approach there was no conserved Runge–Lenz vector, normally one of the charming features of Coulomb systems. Roger and I managed to publish a paper or two on these ideas, but he found the slow progress depressing, and decided to give up. We had hoped to use our results to understand the classical limit of SUSY quantum mechanics, including fermionic variables, but we couldn't ever get to grips with this. Roger was my only graduate student not to submit a PhD thesis. After some years, he became a high-school physics teacher in Wuppertal, Germany and we occasionally met again in Cambridge. He offered to proofread my book with Nick Mee, The Physical World, but this wasn't in the end needed. Others did similar work to us on SUSY mechanics, at least formally. They called the finitely generated Grassmann algebra a skeleton, and there are infinitely many of these. I think we were alone in finding explicit dynamical solutions using such a skeleton.

My conclusion is that SUSY makes more sense in its quantum than classical version, but the large community of SUSY theorists just pushes this problem under the rug. They write down endless examples of Lagrangians defining SUSY classical systems, in field theory, supergravity and string theory, without ever really considering where these Lagrangians and associated equations of motion are living mathematically. For the larger community, a Lagrangian is just a formal symbolic object on which one performs formal symmetry transformations. It provides a code for how to do quantum calculations, and these appear to make more sense than the classical dynamical solutions. The lack of a clear interpretation of classical SUSY systems is one reason that mathematicians are sceptical about SUSY.

Quantum SUSY systems, on the other hand, can be interpreted simply as special examples of ordinary quantum mechanical systems. The SUSY systems have some special properties, of which the best known is that states are classified into pairs, one bosonic and one fermionic, with equal energy. Also, in numerous cases, but not all, the ground state has exactly zero energy. One would imagine that it is easy to find physical examples where SUSY quantum mechanics is realised, at least approximately, but this is not true. For example, both the quantum harmonic oscillator and a quantized particle in a Coulomb potential have elegant SUSY versions, but it is the original non-SUSY versions that have applications.

The reason may be as follows. Long ago, Darboux found that operators sometimes occur in pairs, where the eigenvalues of both operators are the same, or possibly the same except for a

single extra zero eigenvalue for one of them. Schrödinger also considered such operator pairs. A physical quantum system can then be modelled by a Hamiltonian (energy operator) that is *either* one of these operators *or* the other, with very similar results. Using transformations between the operators one can often find all the energy eigenvalues algebraically, a nice piece of mathematical physics. The SUSY version sets up a quantum system where *both* operators appear, as diagonal entries in a matrix Hamiltonian. The energy eigenvalues are as before, as are other properties of the states, but now all the nonzero eigenvalues occur twice. This is a kind of pairing that is not frequent in nature.

A good example, that I looked at carefully, was considered by Andreas Wipf and collaborators. They studied the SUSY Coulomb system (actually in any dimension, but the three-dimensional case is enough here). The SUSY system incorporates a whole octet of Hamiltonians whose energy spectrum is related to, but not identical to, the standard spectrum for a spinless electron in an atom. (Spinless electrons work quite accurately in atomic physics, even though electrons really have spin half.) One of these Hamiltonians describes spinless electrons, and another describes spin-1 vector particles. The Darboux interpretation is that there could be a physical atomic system involving these vector particles, and if so, the spectrum would be closely related to that of spinless electrons. This is an interesting observation, even though such a system is exotic, and occurs rarely or not at all. But at least the standard electron system is physical. The SUSY interpretation requires there to be a physical atomic system with electrons and vector particles present *simultaneously*. No atoms are like this, so the SUSY Coulomb system, though interesting as mathematics, is not realised at all. It is not even a good approximation for the study of normal electrons in atoms, because there is an unwanted doubling of states.

I thought about this further, and came up with a point of view half-way between that of Darboux–Schrödinger, and SUSY. Rather than use the diagonal matrix SUSY Hamiltonian, with its degeneracy, I replaced this by an off-diagonal operator, whose square was the Hamiltonian. This was something like the trick leading to the Dirac equation as a square root of the wave equation. The spectrum of the new operator was the same as before, but states that had previously been independent now had linked amplitudes. The degeneracy was therefore reduced. I found a time-dependent analogue of the Schrödinger equation within my scheme. I called this Superevolution. That was more original than just finding the off-diagonal operators.

Unfortunately this scheme didn't progress further. I didn't extend it to SUSY field theory, although this might be possible if one understood better how to formulate SUSY field theory from the Hamiltonian perspective. A prerequisite is to understand Majorana fermion states better, and not just the free ones. One consequence of my scheme is that what appear to be distinct states in SUSY, one a fermion and one a boson, now appear linked together. So fermions are equivalent to bosons, and one can't really tell them apart. SUSY transformations become rather like gauge transformations or coordinate transformations that have no physical effect, but just change the mathematical description. I didn't feel this was ridiculous, although it is not the standard interpretation of SUSY. After all, SUSY predicts similar behaviour for bosons with integer spin and fermions with half-integer spin, so it makes sense to say that you can't tell the two kinds of particle apart. When I presented a seminar on this in DAMTP, I was much criticised. It was as if I were saying that experimenters were unable to decide if a particle had spin half or spin one. But this measurement is done routinely in the lab. (There was another criticism, to do with particle pair creation and unitarity. This was beyond the kind of calculations I was doing in my simple SUSY quantum mechanical model, so I was never sure if this criticism was valid.)

The resolution of this paradox seems to be that if SUSY is relevant to fundamental physics,

then it is significantly broken. The fields and particles we currently know are effectively not supersymmetric, and we know their spins for sure. For example a photon has spin one and an electron has spin half. Using these as tools in experiments, one can measure the spins of new SUSY partner particles that might be found, and there is no spin ambiguity. But if SUSY were exact, then, I claim, there could be a spin ambiguity.

In summary, my scheme didn't fly, and the resulting paper has very few citations. But if SUSY is ever confirmed experimentally, some of the deep issues with SUSY that I have grappled with will probably resurface. What is precisely the right formulation of the dynamics of SUSY quantum theory? Does spacetime have fermionic coordinates? Which Grassmann algebra, if any, should one work with? What is the classical limit of a SUSY theory, and does it have a fermionic part?

There was one more project to do with SUSY that was explored by my student Anne Schunck – a Gates scholar attached to St John's College – in collaboration with another student, Chris Wainwright. SUSY in curved space is not easy to think about. Usually one works in Minkowski spacetime, or else in a SUGRA context, where the background spacetime is itself dynamical. But there do exist SUSY field theories in fixed, curved spacetime backgrounds, provided they have some symmetry. They are quite tricky to study, so with my encouragement, Anne and Chris looked at simple examples, and in particular the simplest field theory on a sphere. Usually, SUSY would make use of some differential geometry intrinsic to the sphere, including its metric, and some spinors on the sphere. I suggested they try instead to deal with the issue algebraically. A sphere is a surface in three-dimensional Euclidean space defined by a quadratic equation. They found that a supersphere could similarly be defined in terms of an algebraic constraint on the even (vectorial) and odd (spinorial) coordinates of a flat superspace. The superfields could be defined using constraints in flat superspace too. This was nice work, leading to a joint paper, and Anne got her PhD for this and earlier results. Just before that, she married a Research Fellow of St John's, Ingo Kleppe, so she graduated as Anne Kleppe, and they moved together to Jena in Germany after her PhD. He stayed in science, but she got a job in banking, working mainly from her flat in Jena, and occasionally visiting the bank's office in Hamburg (I think). I met them and their growing family a couple of times when visiting Jena a bit later for a seminar at the university, and for a summer workshop. They had a good address, a flat in Am Planetarium, close to the historic Zeiss planetarium, where I went once to a show.

It is worth noting that my three students who worked on SUSY didn't have great success academically. Perhaps my viewpoints on SUSY were too far away from the mainstream, and my work and theirs was not appreciated. On balance, I have reached the conclusion that SUSY will probably not be discovered in the form that most physicists are searching for, if at all.

19 Two-Year Postdoc at MIT

In autumn (fall) of 1979 I took up my postdoc position at MIT, in the Center for Theoretical Physics (CTP). I needed some documents from MIT in advance, so as to get my J1 visa. That involved some queuing at the US embassy in London. It was economical to get a standby flight ticket at the TWA office in central London on the day I planned to travel, or possibly a day before. Then, with my Mum, we took a bus from Victoria to Heathrow. There I found that my luggage weighed too much. I had packed various treasured souvenirs, including L'Amérique, a book of Haas photos of America acquired in France, and my photo book of Slovakia, as well as some maths and physics books. Mum had to take some of these home, but I managed to take

them to the US on subsequent trips.

The flight was on a Lockheed Tristar, not a Boeing 747 as I expected. Other airlines flew McDonnell-Douglas DC10s, but a bit earlier and a bit later there were a few crashes of these. At Boston Logan airport, the immigration officer saw that I was going to work at MIT and said "That's a good school!". I was amused by the different words that Americans use, compared to British English. Some were difficult to pick up at first, like "route" pronounced like "rout" and "momentarily" meaning "in a moment". The difference in accent is a small issue by comparison. Some people in the US said my English accent was cute.

I had a bank account by then, with the First National Bank of Boston. Setting this up also taught me a difference about America, because I had first thought of opening an account at Chase Manhattan Bank, a bank I'd heard of, but then learned from someone at MIT that Chase Manhattan had no branches outside New York state. Banks were very localised in the US because of 20th century financial scandals, and there were very many of them as a result.

At first I had nowhere to live, but fortunately Graham Shore had an apartment I could share for a while, off Garden Street in Cambridge (Cambridge MA, of course.). He had started a postdoc at Harvard the year I had started in Paris. I thought I'd find something of my own quickly, but in the end it took over a month. I nearly rented an apartment on the Boston side of the Charles River from MIT, on Commonwealth Avenue; the dishy agent almost persuaded me, but it was rather too expensive. Finally I found an apartment quite close to where Graham lived, ten minutes walk to the north-west of Harvard at 20 Concord Avenue, Apartment 9. That was near the attractions of Harvard Square – shops, restaurants, newsstands, and Harvard University itself – but I needed to take the Red Line subway two stops to Kendall Square to get to MIT.

I imagined Harvard Square would look like the spacious Trocadéro in Paris, with grand institutional buildings on one side, and a lively city scene of café terraces opposite. In fact, it is more homely and rather suburban, because Harvard, by American standards, is very old, with some buildings dating back to the 18th century, and the more modern developments are of limited scale so as not to dwarf them. And curiously, Harvard Square is rather narrow and not square in shape at all.

An interesting event on TV in the first weeks, while I was still at Graham's, was the 1979 World Series. I had not watched baseball before, and the rules and language of the game were initially mystifying. The game is not at all like cricket, despite the batters, innings and runs. The teams playing were the Baltimore Orioles and Pittsburg Pirates. I don't think I've ever seen an oriole in the wild (or a pirate). In the first inning of the first game, Baltimore scored five runs. This was an exciting game! It soon became clear, however, that a scoring rate like this was quite exceptional, and the final score after nine innings was 5-4 to Baltimore. One has to be patient waiting for any kind of score. That's the charm of baseball – its slow development, and the way the pitchers slowly tire and lose accuracy, unlike the hectic pace of games like ice hockey and basketball. I watched quite a lot of baseball after this first experience, live and on TV. The TV commercials (ads) are not so annoying as in other American sports and other types of programmes, because they fit rather well into the gaps between innings, where some relief is welcome.

On the day after arriving, I went to MIT. MIT looks grander than Harvard, at least at the front, with its colonnaded portico on Massachusetts Avenue, and substantial buildings from around 1916. The CTP is in this older part, but one needs to go along an almost endless corridor and upstairs to get there. The professors were on the third floor, with offices off a rather swish lobby, hung with original prints of Ansel Adams photos (these were later too valuable to display,

and were replaced). The postdocs and graduate students had offices upstairs, which were still better fitted out than rooms in neighbouring departments. Notably there was air-conditioning, which made CTP a much more pleasant place to work in summer than many other parts of MIT, or my apartment in Concord Avenue.

My main contact was with Roman Jackiw, then a fairly young professor, who had done various important things in field theory, including work on solitons. He was interested in my work on symmetric gauge fields, and was more familiar than me with the general idea that if a physical system of particles or fields interacts with a fixed background with symmetry, then the system's dynamics has conserved quantities given by Noether's theorem. For example, in a spherically symmetric background, there is a conserved angular momentum.

Flat space is spherically symmetric around a chosen origin, and one can add to this basic geometry a spherically symmetric gauge field. In both backgrounds, a system has a conserved angular momentum, but the formula for this changes when the gauge field is added. Roman knew a very general way of finding the conserved quantity in any symmetric background, a method I have taught to Part III students in more recent years. My formalism with Peter Forgács for symmetric gauge fields was also very general, so Roman and I could combine our insights to find conserved quantities of a very general form, which could be applied to many special cases, for example, to particles interacting with background monopoles. There are two variants of the conserved quantities when there is a background gauge field. One is 'canonical', which is what directly follows from the standard version of Noether's theorem, and the other is 'covariant', meaning that it has a more manifestly gauge invariant appearance. Both versions use quantities that were in my paper with Forgács. In the covariant version, this is the Higgs field that we had discussed at length.

Roman worked quickly on this. We had discussions one day in his office, and by the next morning he had already written a draft section of a paper. I contributed too, making technical and conceptual improvements. One result was to make the covariant version look more like the canonical version, by more carefully defining the field variations required for the application of Noether's theorem. These variations were themselves gauge dependent. I also learned from Roman more about the stress-energy tensor of field theories during this project. This has canonical and covariant versions too.

After about two months we had a finalised paper, which we submitted to the locally edited journal, Annals of Physics. They particularly welcomed papers that had some originality, but also a pedagogical flavour. I imagined working further with Roman after this, and we quite often had further discussions in his office, including with visitors or his students. But nothing was promising, and I ended up working more on my own.

MIT had a cafeteria where I often had a hot lunch, but there was also a popular sandwich counter. This was my first experience of the splendid, loaded American sandwiches. Locals would be asked to "shoot" their complicated orders. There was a choice of bread, of meat or cheese slices or both, of lettuce, tomato and sprouts, of butter, mayo, pickles etc. The server making the sandwiches had an amazing memory for what had been requested. I liked ham and swiss on rye most of the time, but tried other variants too. Sandwiches could be taken back to the CTP where a group of the established professors would get together for lunchtime chat. This group included Francis Low, Herman Feshbach, Vicky Weisskopf, Jeffrey Goldstone, Felix Villars, Bob Jaffe and Arthur Kerman. They were nuclear physicists as much as particle physicists, and several were well connected with government, because they had been close to those involved with the Manhattan project (Francis Low and Vicky Weisskopf had been directly involved) and had

benefitted from Office of Naval Research contracts that supported pure science after the war. Most of them were peaceniks, seriously concerned about nuclear proliferation, and supporting attempts to control it. They opposed the American hawks elsewhere in the country, some of whom had advocated using nuclear weapons during the Vietnam War a few years earlier. I noticed that here at MIT, but also at Harvard and among visiting seminar speakers, there was a much larger percentage of Jewish physicists than in the UK.

Near my office were two larger offices for graduate students. Among the students were Andy Strominger, Joe Lykken, Lawrence Krauss, Antti Niemi and Manu Paranjape, all of whom had successful academic careers later. I got to know them reasonably well and met them again on various occasions over the years. The postdoc in the office next to me was Paolo Rossi from Scuola Normale in Pisa, who had an interest in monopoles. Other postdocs were Manoj Prasad, who had earlier found the exact 1-monopole solution in the case of a massless Higgs field, and Luc Vinet from French-speaking Canada, who much later became rector of the Université de Montréal.

The Prasad–Sommerfield 1-monopole solution was known to solve the first-order Bogomolny equation, and it was realised that static multi-monopole solutions were most likely to be found by investigating this Bogomolny equation further. The existence of such solutions was first established by Clifford Taubes in collaboration with Arthur Jaffe, both at Harvard. They used analytic methods to show that several monopoles at large but arbitrary separations can be glued together exactly to solve the Bogomolny equation. This was a profound extension of my formal result showing that monopoles have no forces between them. But Taubes' results gave no explicit formulae, and no insight into solutions with monopoles close together.

In fact, the first new, precise solution was found by Richard Ward. This was an axially symmetric solution of charge 2, with the monopoles as close together as they can be. He had found this by using some of the subtle geometry derived from his understanding of instantons and twistors. The results of Ward were not very explicit – he had transformed the problem of finding monopole solutions into another problem, where there was an explicit solution, but that gave rather limited information about the gauge and Higgs fields. Manoj and Paolo decided to try to develop this further. (Paolo also worked with Claudio Rebbi on this.) I was involved in the discussions briefly, but found I had nothing really to contribute. They exploited a different reformulation of the monopole equations, something called the Yang equations, and managed to rederive Ward's solution. They found further information about the fields, and also found analogous solutions with higher charges. All these were axially symmetric, and surprisingly, they were unique apart from the possibility of rigid shifts. I had imagined in my earlier studies that axially symmetric monopoles would look like chains of basic monopoles, with arbitrary parameters representing the monopole separations along the axis of symmetry, but the actual solutions are best thought of as rings of magnetic charge density. If I had been smarter, I could have anticipated this, because my description of axially symmetric monopole fields involved some singularities along the axis in most cases. I thought these were just Dirac strings that could be removed by a gauge transformation, but they were worse, and only disappeared for the ring-like configurations, where all zeros of the Higgs field coincide at the origin.

Working by myself, I looked with more care at spherically symmetric monopoles. My work with Forgács implied that, when the gauge group is SU(2), there are infinitely many types of spherically symmetric fields, labelled by an integer that is related to a Dirac monopole charge. For the monopoles of 't Hooft–Polyakov type, this integer has to be 1, so the only spherically symmetric monopole has unit charge. Why was this? I discovered the reason was that the

action of the symmetry group has to extend smoothly to the origin in this case. Here the orbit of the rotation group is not a sphere but just one point, so the isotropy group is larger than on a generic orbit, and the algebraic constraints that need to be satisfied are stronger. This clarified why spherical symmetry in the context of smooth monopoles is more restrictive than for Dirac monopoles, and explained how an analysis of spherically symmetric fields by Romanov, Schwarz and Tyupkin fitted with the results I had obtained with Forgács. I also understood in some generality how discrete, parity symmetry works for monopoles. This symmetry, together with appropriate gauge fixing, reduces the number of radial functions describing the unit charge monopole from four to two. I wrote an Annals of Physics paper on all this, but it wasn't very important.

Fortunately, I also had a lucky break at this time. We went fortnightly to Harvard for seminars, usually followed by an outing to a Chinese restaurant led by Sidney Coleman, who was an amusing and brilliant theoretical physicist. In the alternate weeks there was a joint seminar at MIT. At Harvard, Michael Creutz gave a seminar on his pioneering work in the area of lattice gauge theory, the numerical study of quantum Yang–Mills theory. He presented his lattice version of the Yang–Mills Lagrangian, which involves a Lie group valued field on the lattice links and plaquettes, rather than the usual Lie algebra valued gauge potential of the continuum theory. An action (four-dimensional energy) is constructed from the plaquette variables, and this involves taking a matrix trace using a choice of representation. Creutz may have been studying differences between different choices of representation, or the effect of taking linear combinations of these possibilities – not the most elegant part of the lattice gauge theory construction. I realised that a more geometrical viewpoint was possible. Knowing something about the geometry of Lie groups from my studies in Paris, I knew that there was just one (bi-)invariant notion of distance in a (connected) Lie group between a general Lie group element and the identity element. This was particularly simple for SU(2). I proposed using this distance instead of matrix traces for defining the action. This approach agreed with standard Yang–Mills theory when one took a continuum limit, and it had some other nice features. I wrote a short paper on this, and my approach got called the Manton action for lattice gauge theory. For some time afterwards, the specialists in the field would compare the different choices for lattice actions, and mine was one that needed to be considered. For some purposes it was an improvement over others, but eventually this geometrical approach was replaced by more sophisticated constructions of improved lattice actions using renormalisation ideas.

This was confirmation that it helps to go to seminars on a range of topics, and to maintain a critical mind. Even without being an expert, one can sometimes contribute a good idea to some area of research. However, there are limits. If mathematicians try to use sophisticated mathematics in a research area where this is not the norm, then the result can easily be of little scientific value. In any case, the work is unlikely to be understood or appreciated, and the effort wasted.

Let me now go back to describe other aspects of my life in Cambridge MA. My apartment in Concord Avenue was unfurnished, unlike earlier places where I had lived, so it was necessary to acquire furniture quickly. Here, the efficiency of American commerce helped. I found a bed frame and mattress, a couple of armchairs, one of which rocked, and a chest of drawers that I varnished a rather orangey brown. They were delivered quickly. I also purchased bedding and things for the kitchen. It took a bit longer to get a good table. This I purchased second-hand from someone leaving Harvard, together with hard, really uncomfortable dining chairs. Later I shipped some of these things to Santa Barbara, and then back to Cambridge, England, so we

still have the table, the chest of drawers and the rocking chair. I didn't get a TV at that time, but did acquire a radio and record player.

There were beautiful autumn colours (fall colors) to admire from mid-October to early November. People recommended going to New Hampshire or Vermont to see them, but I didn't have a car so that was not practical. But just a couple of weeks after settling in, I took the local train from North Station in Boston to Concord, which is a picture-postcard New England town with many buildings from before the Revolution. Here, and also in historic Salem, the colours were particularly fine. By mid-December it was much colder, with clear skies and overnight temperatures of 15 degrees (Fahrenheit); the views of the Boston cityscape were beautiful, being much clearer in the dry air than anything one experiences in London. My apartment was very well heated in winter by a large furnace in the basement, close to where one could dry laundry, and occasionally I needed to open windows in the coldest weather to cool the place.

I found the winters really cold, from mid-December till early February. My British clothes including a duffel coat were not really adequate, and it was essential to walk really fast to the Red Line stations at the Harvard Square and MIT stops. By this time I had joined the Belmont Orchestra that rehearsed about three miles away, out along Concord Avenue. That meant waiting for a bus, and when it was really cold my violin got cold too. Fortunately George Hein, one of the violinists who had a car, was friendly and offered to give me lifts (rides) at least one way, and sometimes both. I made friends with one girl, with whom I went out a couple of times, but we didn't have enough in common to continue the friendship. Once she took me on a daytrip for cross-country skiing, to Concord or maybe Lexington, but I wasn't much good.

Summer was different. The apartment had no air conditioning, which was not unusual in New England, and it got stiflingly hot and humid in July and August. I bought a large fan that rested in the window opening, but several evenings I still had to walk the streets to get some relief. Fortunately, there was a fine ice cream parlour to visit that was air-conditioned.

During the year, I went to museums like the Isabella Gardner, the Fine Arts, and also to the historical ship, the USS Constitution. The Museum of Fine Arts hosted an exhibition of Korean art, which I had not appreciated before. There were some exquisite ceramic vases with unfamiliar shapes. Occasionally I'd go to a concert, and maybe once to the Boston Symphony Orchestra at Symphony Hall, although tickets there were sold by subscription and were very hard to come by. I did get in to a piano recital at Symphony Hall by Maurizio Pollini, playing the Chopin Etudes. This was the first time I'd heard them, and I didn't really appreciate then how good they are as music, nor Pollini's brilliance. I later bought his record of these, and of the Chopin Preludes too, and they became among my firm favourites.

After several months I was getting a bit bored living in Cambridge. To have another activity, I went to a stamp auction, and bought one sheet with a mixture of mainly Canadian stamps from the 19th and early 20th century. This was the first time I'd seriously got interested in stamp collecting since the age of 16. The auction was a bit of a racket, because there were only a handful of dealers there, probably selling stamps to each other, and I was one of the few outsiders. Rather than go to later auctions, I found there was a Boston stamp club that met fortnightly at Boston University. Here, mainly amateur collectors would sell and exchange stamps among themselves. Most were collectors of US stamps, and I started to acquire quite a lot of these, but my main interest was boosting my British Commonwealth collection. This was quite successful, at fairly modest cost. I got more Canadian stamps, and some from East Africa and India. The club wasn't very social, but that was mainly my fault for concentrating too hard on looking through the stamp books. Before going there, I would almost always have a supper

of clam chowder and cheesecake at a nearby diner, both very American dishes.

The stamp club was not far from Fenway Park, home of the Boston Red Sox baseball team. In summer, it got so hot in my apartment in the evenings that I experimented with going to the baseball. Tickets were easily available for most games, because there is a game every day when the team is at home and not on a road trip. Tickets also get discounted after the game has started. I enjoyed the game, and went once more, but was surprised to see that there were few away supporters. It's also a rather middle class game (family entertainment) where everyone sits, without the rowdiness and drunkenness associated with some live sports in England at that time. I politely applauded when the away team scored, but got frowns from neighbours, so stopped that. It isn't so much fun to watch a game when the home team is losing, because the whole crowd loses interest and starts to drift away before the end. The TV coverage is more unbiassed. Quite fun is the 7th inning stretch, between the top and bottom halves of the 7th inning, when everyone stands up to stretch their arms and legs.

The fielders are very skilled to carry out the various plays, including double plays to get two batters out. The batters, during practice, can hit balls easily, but during real games the pitchers make hitting really hard, and this keeps the scoring low. One thing that surprised me tactically was that pitchers are often changed by the manager's decision, although it's usually done too late, after the pitcher has got tired and given away a run or two. Why not earlier? I think it's because the teams only have a limited number of pitchers, and the relief pitcher who comes on near the end can't be brought on earlier, otherwise he couldn't pitch again game after game, day after day. Possibly the reason is that pitchers are some kind of heroes, like gladiators, who have to be tested to their limit, otherwise the crowd would be disappointed. Obviously a starting pitcher who holds on to win a game without being relieved is some kind of hero.

People in England, and foreigners I got to know in the US, were quite surprised at my captivation with baseball, and how I got to learn some of the jargon of the game.

During my two years at MIT, I had several opportunities to travel to other places in the US, mostly to give seminars, and I got used to the idea of taking flights every few months. An early trip was to the University of Chicago in December '79. I saw something of the city, including the tallest skyscraper – the Sears Tower – and was amused there by the company of Peter Freund. I suggested jokingly that he should write down his anecdotes about science and scientists for posterity, and many years later he did so, acknowledging my suggestion. I also went to the Museum of Science and Industry near the university, with its submarine, but didn't like it that the university is surrounded by a neighbourhood where it is very dangerous to walk.

Just before Xmas, I made a brief first visit to New York with Mike Gunn, and I think we went up the Empire State Building. Then, in January '80, I travelled to Montréal by bus from Boston, an 8-hour trip through New Hampshire and Vermont. This was again to give a talk, and was my first trip to Canada. It was striking how the landscape opened out into farmland just across the Canadian border, after endless forests. It may be because of the change in geology as one approaches the St Lawrence river, but more likely, farming on the southern edge of Canada is profitable, and on the northern edge of the US is not. At the border north of Burlington, the US citizens were dealt with quickly, but I, with my UK passport, was given a much harder time. They wanted to know how long I was going to stay and whether I would earn any money; they were suspicious because I'd told them I'd been offered expenses. This was ironic, because just behind the immigration officer's head, on the wall, was a picture of my Queen Elizabeth II, so I felt I should have been more welcome. In the end they let me in on a visitor visa for precisely the length of my planned stay.

I liked the look of Canada. That part of Québec was well inhabited, and it had some of the smaller scale charms of Britain or France. But Montréal was considerably colder than Boston, and I still relied on my inadequate duffel coat, finding it really hard to make my way from the hotel to the department at the Université de Montréal. One day it felt a bit warmer in the middle of the day, so I took the metro to St Helen's Island in the middle of the St Lawrence where Expo 67 had been held more than a decade previously. The site was now an amusement park, only operating in the summer, however. Here it felt colder still because it was windy, and I only stayed out for about five minutes. It was nevertheless impressive to see the river flowing quite quickly past, full of floating ice blocks. I had seen small frozen rivers in England, including the Cam, but never with moving ice blocks like these.

My hosts included John Harnad, a Hungarian who had settled in Canada, and Jiri Patera, a Czech who was expert on Lie groups and their representations. John was studying symmetric gauge fields from the perspective of fibre bundle theory applied to physics, so we had a lot to discuss. One thing I learned was that even before you consider a connection (a gauge potential) on a bundle, there is a notion of a lift of the action of the symmetry group from the base manifold to the bundle over it. This gave a reinterpretation of some of the formulae I had established with Peter Forgács. When working with bundles, you have to define them carefully, and pin down their topology early on. In my formalism with Forgács, we seldom needed to worry about this, and still we ended up with monopole fields and the like with their nontrivial topology. I thought of a motto to describe this: Look after the symmetry, and the topology looks after itself.

Near the end of February '80 I gave a talk at the University of Michigan in Ann Arbor. To get there I flew from Boston to Detroit but didn't see that city. The campus at Ann Arbor has a mixture of interesting buildings from the first half of the 20th century, but it was still bitingly cold there, when Boston was getting milder. A little later I made a trip by train to New Haven CT to give a seminar at Yale. There I met Charlie Sommerfield who had found the exact monopole solution with Prasad. At Yale there is an interesting museum of British art.

My Mum and Puttel also visited me in the spring of 1980; this was their first time in the US. They travelled first to New York, with Rita and Jack Davis, and stayed at the Empire Hotel, then one of the few modestly-priced decent hotels near Lincoln Center and Central Park. I went to meet them there, and we enjoyed a boat trip on the Circle Line all the way around Manhattan. The four of them continued to Washington while I returned to MIT, but a few days later my Mum and Puttel came by train to Boston. I met them at South Station and settled them into a hotel near Harvard Square. We visited the historic sights of Boston, and I also showed them Harvard, MIT and my apartment.

Earlier that spring, Roman Jackiw had been for a period to California, where he had contacts at UCLA and at the new Institute for Theoretical Physics (ITP) at UCSB, the University of California at Santa Barbara. He generously arranged for his students and postdocs, about four of us, to spend a month at ITP. At that time, ITP was not very well known, and they had space to welcome visitors.

I went out there in May 1980, flying first to Los Angeles and seeing for the first time the colourful, rocky landscape of the American south-west, as well as Los Angeles and the Pacific Ocean. At Los Angeles' LAX airport I changed to a small plane of the airline Golden West, with two propellers. I had never flown before on a plane like this. It had only about a dozen passengers, together with two pilots and a cabin crew of one. It took off quietly after a very short acceleration on the giant LAX runway. The flight was about half-an-hour along the coast to Santa Barbara, and arriving there was a memorable experience. There was a pretty, Spanish-

style terminal building to which one had to walk, and a powerful, very attractive smell of blossom mixed into the sea breeze. The many nearby Eucalyptus trees probably contributed to the smell. Despite some aircraft noise, the atmosphere there seemed very peaceful.

I moved into a small house quite near the airport and also near the university, shared with the others from MIT. It was in Los Carneros Road on the edge of Isla Vista, the student dormitory area of UCSB. I found the Spanish names that abound in California cute, but soon realised that the culture is dominantly English-language based and American, despite the many Mexicans and other Spanish speakers living there. In the evening, there was a lot of noise from frogs in the marshy areas near the house.

I could walk to ITP, and spent most of my time in the UCSB neighbourhood during that month. ITP was on the sixth (top) floor of Ellison Hall, one of the main academic buildings, and the offices had great views either towards the ocean and the islands beyond, or inland towards the quite high mountains just a few miles behind Santa Barbara, running parallel to the coast. UCSB opened just after the war, the site having previously been part of a Marine Corps Air Station. Its buildings are mainly patterned concrete on steel frames, and rather stylish. The tallest is the Storke bell tower. The site is close to a lagoon that is almost connected to the ocean, and beyond it is an almost endless sandy beach. I took some walks around there, but only surfers in their wet suits could enjoy going into the water at that time of year. Tar on the beach, that seeps out from the oil reserves in the Santa Barbara Channel, is also a hazard.

Another visitor to ITP, who arrived slightly later, was George Chapline. George had a staff position at Livermore, the US Atomic Weapons lab, and he did some hush-hush work involving lasers, recognised later by President Ronald Reagan. George also wanted to keep up his interest in pure physics research, and got some time off from Livermore for this. He was rather an interesting character, a real American, who could trace his roots back to the American colonies in the 1700s. His mother had been successful in the movies in Hollywood. Not surprisingly for someone working at Livermore, he was patriotic and strongly anti-communist, though he also admired Soviet technological achievement. It was a bit difficult for me to swallow his anti-liberal views, and he was close to the school of thought that the Soviet threat to the West should be destroyed by a preemptive nuclear strike. Fortunately, the Soviet empire disintegrated by itself a little later, but we didn't know that would happen in 1980.

George was interested in my work on symmetries and dimensional reduction, with its application to the Weinberg–Salam theory. Together, we started to investigate extensions of these ideas to grand unified gauge theories (GUTs) with larger gauge groups. Such theories were fashionable as providing a possible unification of all the forces other than gravity. They were popular among students and postdocs at Harvard, where Sheldon (Shelly) Glashow and Howard Georgi, who had invented GUTs, were based. Large experiments were starting up to search for proton decay, a striking prediction of GUTs. A feature of GUTs that I was aware of was that, unlike the Weinberg–Salam theory, they have (very) massive monopole solutions, although I was only slightly interested in these. George and I hoped to give some geometric, higher-dimensional explanation for the rather complicated and ad hoc Higgs symmetry breaking that is needed in GUTs. We made some progress, and began to think about dimensional reduction applied to fermions as well as gauge fields, following up some discussions I had had on this with Peter Forgács.

I certainly liked being in Santa Barbara. One could reach downtown quite easily by bus 24 from the campus. That was about 10 miles away and took about half-an-hour on an express bus going along the freeway (route 101). Downtown is mostly very pretty. Santa Barbara had been

flourishing because of the early movie industry, but was largely destroyed by an earthquake in 1925. The city decided on a rigorously controlled rebuilding in the Spanish colonial (Andalusian) style, including Moorish decorations. The courthouse is the masterpiece in this style. The architecture is enhanced by beautiful gardening, relying on plentiful irrigation. The only substantial building genuinely from the Spanish colonial time is the Mission, slightly uphill from downtown, and this also had to be repaired after the earthquake.

Santa Barbara continued to impose strong constraints on development, for example on the size of commercial signs beside the roads and on buildings, so shopping malls and suburban housing are all low-rise and quite pretty. The blue blossoms of the jacaranda trees and the agapanthus flowers were new to me. (Agapanthus is now available from garden centres in the UK, but not then.) I never tired of admiring them during the flowering season. Also there are pavements (sidewalks) everywhere, and grass verges, and it is pleasant to walk almost anywhere, unlike in most of America. Locals quite often walked or jogged, despite most of them also having cars. There was a culture of prettiness, and people felt an urge to keep their cars clean too.

There is no doubt that Santa Barbara is one of the most beautiful towns in the US. It is mostly very prosperous, except perhaps in its small hispanic neighbourhoods. With its mild climate, it is a popular place for people to retire, and while I was there was secure enough not to need barbed wire exclusiveness. But unlike similar seaside towns in England, Santa Barbara had a flourishing industrial and commercial life, as well as the large university, and younger people were mostly working. Some people thought the well-organised life and environment in Santa Barbara, with its predictable weather, rather boring, but I didn't really find this. It's true that many Californians, who couldn't imagine living anywhere else, are somewhat complacent and provincial, but this wasn't a problem for me because I had been to all kinds of other places, and there was plenty of stimulating culture available if one wanted it. Also, most of the people that I knew had moved to Santa Barbara from other places in the US or elsewhere around the world.

From Santa Barbara I made a trip to Pasadena to give a seminar at Caltech, with Richard Feynman in the audience. He asked a question near the start, but then his thoughts drifted away. I thought it would be straightforward to go from Santa Barbara to Los Angeles by train, and from there to Pasadena, but there was only one passenger train a day to LA, and the Union Station in LA was not connected by train to Pasadena (since 2003 it has been). Instead I took a flight and minibus, possibly also visiting UCLA on this trip.

After returning to MIT, through contacts with the Harvard theoretical physicists at the joint seminar and also via my friend Graham Shore, I met Ian Affleck, a Canadian who was then a Harvard Junior Fellow – a prestigious kind of postdoc. He was interested in monopoles and instantons, following in the footsteps of Sidney Coleman. Together, we realised that we could study monopole-antimonopole pair production in a strong magnetic field. This would be dual to the well-known study of electron-positron pair production in a strong electric field by Julian Schwinger. Both are a kind of quantum tunnelling process, involving an instanton. The reason that tunnelling is required is that it costs a positive amount of energy to create the monopole-antimonopole pair close together, but as the pair separate in the external field the energy decreases again. So there is an energy barrier to cross. But no-one knew the precise nature of the instanton, the classical solution in four-dimensional Euclidean spacetime that dominates the tunnelling amplitude. I discovered that the required instanton has the form of a circular monopole loop in four dimensions. This is because a constant magnetic field pointing along the spatial 3-axis corresponds to an electromagnetic field in Euclidean spacetime with circular symmetry in the plane spanned by the time-axis and spatial 3-axis. It was straightforward to find

the radius of the circular loop by stationarising the total instanton action, whose contributions come partly from the monopole mass integrated around the loop, and partly from the coupling of the monopole to the external magnetic field. This circular instanton was very closely analogous to one that Mike Stone had talked about in DAMTP some years earlier, related to particle pair production in some other context. Fortunately I had remembered this. However, unlike the instanton of Schwinger, and the one Mike Stone discussed, our instanton involved smooth solitons of finite size and not point particles.

Ian was more expert than I was on how to use our instanton to do a proper quantum mechanical calculation of the pair production rate. The calculation involves a quantum 1-loop correction that takes into account fluctuations of the field away from the classical instanton. Progress was quite tricky, and eventually reached an impasse. After some months, we realised that what we had missed was that simultaneously with monopole pair production, there can also be dyon pair production. The dyon's electric charge arises from the monopole twisting an integer number of times as it goes round the circular loop of the instanton. We needed to sum over the contributions of all these twists. The sum converges because dyons are more massive than monopoles, their mass increasing with their electric charge.

There was some dispute at that time about the exact dependence of mass on electric charge for a dyon. One formula was based on the original classical dyon solution found by Bernard Julia and Tony Zee (work Bernard had done at Princeton before I met him in Paris). Another formula was due to Roman Jackiw and Jeffrey Goldstone, who had carefully quantized the monopole to allow for the field fluctuations that can give a magnetic monopole an electric charge. The electric charge is a quantum effect, subject to the Dirac quantization condition. Ian and I knew that these formulae should all agree, and we had our own insight into the right answer that relied on the SUSY properties of Bogomolny monopoles and dyons studied by Olive, Osborn, Sen and others. This answer is simple, and allowed us to carry out our sum over dyon charges in an elegant way, using the Poisson summation formula. We went to discuss this with Roman, suggesting that his mass formula needed some correction because of a technical issue to do with gauge fixing and Gauss's law. Roman, disappointingly, got angry, saying that his formula was almost certainly right because Jeffrey had calculated it, but he couldn't remember the details. We talked to Jeffrey independently, who was more conciliatory. He also admitted to forgetting the details, and said we were probably right. Rather than trying to persuade Roman further, we never raised the matter with him again, and proceeded to write up our paper. The issue was important, because I was relying on Roman for job references. This was one of the hardest projects I had ever worked on; it was truly a quantum mechanical calculation involving solitons, and it took until nearly the end of my second year at MIT to complete. Without Ian, I could never have done it. There was a follow-up paper that Ian wrote in collaboration with Orlando Alvarez. I was minimally involved, but did talk a little to Orlando and Ian, and agreed, rather reluctantly, that my name could be added to that second paper.

It was useful, later, to have clarified what the mass of a dyon is. The simple square root formula that emerges from SUSY is correct. It has the right quadratic behaviour for modest electric charges, and is consistent with the Julia–Zee formula, provided one imposes Dirac quantization of charge. Our conclusion was that a dyon is a quantized version of a monopole, which takes into account the internal twisting of the monopole. The complications of quantum field theory are not really needed.

Let me now go back to the time just after my one-month visit to Santa Barbara. Summer set in and it began to be hot. Some old friends that I knew met up in Boston, including John Bishop,

who had been one of the residents at Devonshire Road, and maybe also Mike Lowe. John had returned home to Fredericton in New Brunswick by then, and had a car. We made a tour from Boston west to Buffalo and Niagara Falls and then east through Toronto to Fredericton, stopping overnight in Montréal. We must have also bypassed Québec City. Niagara is of course impressive, and I recall the big difference between the Canadian and American sides. On the US side there are just elegant stainless steel barriers where you overlook the falls, but on the Canadian side there are hotels and parks laid out with flowers, and the barriers are decorated cast iron (or painted steel that mimics this), as one would find in a park in Europe. Montréal was much busier with tourists in the summer than I had seen it in winter; it was really quite a different city. Fredericton was curious. It is not much more than a village, despite being the capital of the province of New Brunswick. All the institutions of government – parliament, courts, police, armoury, federal and provincial administrative departments – are close together along the main street. There I met John's family, including his sister, whom I invited to visit Boston. From Fredericton we went to see a pond with a beaver nest, and after a day or two continued to a village on an inlet from the Bay of Fundy, with its enormous tides. As the tide went out, it looked like a bath being emptied. I took a Greyhound bus back to Boston overnight from the Maine border town of Calais. That was uncomfortable, and I vowed not to take overnight buses again. John stayed in touch by letter, and told me about various jobs he had, and mostly disliked, in Canada. Mike Lowe moved back from Brussels to live at Buckingham Terrace in Edinburgh and to do some research at Edinburgh University in condensed matter physics, but that didn't go very well, and he moved into industry.

Fortunately, I could escape Boston again for a few weeks, this time to the Aspen Center for Physics, another attractive destination. Aspen is at 8000 feet, in the Colorado Rockies on the western side of the Continental Divide. To get there, I flew to Denver, the mile-high city that is still surrounded by the flat Colorado plains, and then took a small plane to the Aspen airport. I was warned that the flight over the mountains can be bumpy and alarming during the late afternoons when there are often thunder clouds, so I ensured I got to Denver by lunchtime and on a plane to Aspen by about 2 pm. It takes a week to acclimatise (acclimate) to the high altitude. That was especially noticable when I borrowed a bike and was going slightly uphill. Only after more than a week did I venture higher into the mountains, taking the chair lift and then hiking a little.

Aspen is a former silver-mining town, but had become a premier ski resort, and had numerous apartment blocks. A few physicists from Los Alamos NM and California started to spend time there in winter and summer, and bought property while it was still cheap. They started a small summer physics institute in purpose-built wooden huts. (Americans are hard-working – they don't just want to put their feet up!) This institute became popular, because so much of the East Coast of the US becomes stifling for a couple of months, and participants could stay relatively cheaply in the apartments built for winter skiers. Near to the physics institute was a large tent hosting a music festival. Rehearsals often took place in the morning, and could easily be heard by the physicists as they studied their equations. I got to hear a concert by the famous folk singer John Denver (or possibly just the dress rehearsal); he lived near Aspen and eulogised it in his songs.

I met up again with George Chapline in Aspen, and we made further progress on our work on dimensional reduction and GUTs. We completed our paper a few months later in December '80, and during the following months at MIT I completed a paper on dimensionally reduced fermions, while working at the same time with Ian Affleck on the monopole pair production

project. Ronald Reagan became President in January '81, after his landslide win in November.

During that second year at MIT, I needed to apply for a new postdoc position. Nearly a year earlier, David Olive and Tom Kibble at Imperial College had encouraged me to apply for a lectureship there. I made it to a long shortlist but was thought to be too young and inexperienced to be appointed. But Tom wrote to say I had a good chance of getting a Science Research Council 5-year fellowship at Imperial if that was of interest, so I didn't need to make many postdoc applications. As I had liked Santa Barbara, and was impressed by the scientific atmosphere at the ITP, that was one obvious place to apply, and the other places I applied were elsewhere in the US. Fortunately, I received postdoc offers from Caltech and SLAC (but not Harvard), also an offer of an untenured teaching position at Stony Brook, but best of all was an offer of a three-year Assistant Research Physicist position at Santa Barbara to start in autumn 1981. The last was the most attractive. I accepted, and in the end stayed for three years and three months. That long stay in Santa Barbara was going to change my life.

During the spring of 1981 my Mum visited Boston again for a couple of weeks, I worked part of the time while she went sightseeing and visited the museums. For the first two days we were invited to a wedding celebration in the family of Hanni Curland, a friend of my Mum's from her childhood in Königsberg. The Curlands were very hospitable, despite not knowing us well, and having other guests to entertain. They met us at New London CT and pointed out the large submarine-building facility at Groton where one of them worked. New London and Groton are separated by the Thames – pronounced Thames, not Temms. Their home was in Norwich – pronounced Norwich, not Norridge. We didn't really fit comfortably into this American Jewish middle-class society. They weren't much interested in my being at MIT, which seemed to them rather distant. My Mum was surprised at the absence of bookshelves in any room of their large comfortable house. Most Jewish households in the UK, in the circles we knew, were crammed with books, and music too.

A week or so later we went to Canada. That must have been my third time in Montréal, and I recall we were shown around the city by car by Luc Vinet. Luc had been at MIT, but by now had returned to his home city. My Mum and I also visited Québec City. We were surprised seeing the changing of the guard at the Citadelle, rather like outside Buckingham Palace, but with the orders being given in French. I wonder how local loyalties to the UK and to France have evolved since 1759, when Québec was captured by the British – maybe the loyalties are now entirely to Canada. It was still cold, but we enjoyed sitting on a south-facing bench in the sun, with a historical stone wall behind us and the St Lawrence in front.

I got invited to give some further seminars that year. One was at Stony Brook, where I saw Fred Goldhaber again. To get there I flew from Boston to the small airport at Islip NY on Long Island, and got a view from the air of the circular ring of the particle accelerator ISABELLE that was being built at Brookhaven. ISABELLE was cancelled soon after, but revived as the heavy-ion collider RHIC a decade later. A further trip was in late January '81 and early February to SLAC, the particle physics lab attached to Stanford University. I flew to San Francisco on a United Airlines DC-10 with only 50 passengers on board (probably because of the aircraft's poor reputation by then), and stayed at Menlo Park, not seeing Stanford proper, nor much of San Francisco. But I do recall seeing part of the San Andreas Fault, a shallow valley with ducks swimming in a pond along the fault line. On the way back, we flew over the Yosemite Valley, and one of the stewardesses went to the window, saying "Wow!" and telling people around how much she loved Yosemite. I decided I must visit sometime.

Yet another seminar invitation was to the University of North Carolina (UNC) in Chapel Hill,

later in February. This was an opportunity to visit Washington DC, where I had not been before. I took the train from Boston through New York, and stayed a couple of nights in Washington near the main station. There was time to visit the Capitol on a guided tour and the Smithsonian Museum with its planes and spacecraft, and to stroll along the Mall, to see the White House and some of the monuments. In the evening I went to a concert at the Kennedy Center. I was impressed by the layout of Washington and its architecture, although it is a bit antiseptic. The dome of the Capitol is so much more elegant, and larger, than the several smaller domes of State Capitol buildings that I had seen on my travels. I continued by bus to Chapel Hill, passing the gothic splendour of the chapel at Duke University, and seeing strange-looking large leaves growing in the fields. These were of course tobacco leaves. At UNC my hosts were Louise Dolan, whom I had met in Paris, and Paul Frampton, a British particle and string theorist who had settled there, although he cultivated contacts at Harvard and elsewhere, including with Shelly Glashow, and visited Worcestershire regularly to see his mother. Paul much later got into serious difficulties, ended up in prison in Argentina, and lost his job at UNC. But since his release he has done some physics research again, and has one paper in collaboration with George Chapline – small world!

In April I visited New York again, and maybe visited the Metropolitan Museum on this occasion. I gave a seminar at Columbia University, high up the west side of Manhattan, with Erick Weinberg, T.D. Lee and Norman Christ as hosts. T.D. Lee took speakers for lunch to his favourite Chinese restaurant. The hot and sour soup and spicy green beans were memorable, although similar dishes were typical in Cambridge MA too. Another trip was to Cornell University, in Ithaca NY, by bus. I needed to change at Syracuse, from where I had received the Letter from the Syracusans some years back, but didn't stop there any length of time. This trip solved a problem for me! Fresh milk and yogurts (which I found delicious) were plentiful in Boston, but on travels through Massachusetts and neighbouring New England states I had never seen a cow. However here, near Interstate 90 in upstate New York, there were lots of dairy farms, and plenty of milk trucks heading towards New York City and Boston. I met up with Graham Shore at Cornell. He had moved there from Harvard, and was finding the 7-month-long winters hard going. He managed to return to Europe from Cornell, and after several years working at various locations in Switzerland and in London, eventually settled in Swansea, where he later became Head of Physics, a successor to Ian Halliday.

There were of course regular seminars at MIT, including a colloquium that was followed by a reception with fine French cheeses and biscuits. A colloquium every month or so, followed by wine and cheese, was something I introduced to the Centre for Mathematical Sciences in Cambridge UK much later, modelled on the one at MIT. There were also lunchtime seminars and discussions at Harvard, organised by Steven Weinberg and Shelly Glashow for their 'family' groups. I didn't get to know Weinberg personally in the way I did Sidney Coleman, but admired Weinberg for his scientific insights. I acquired his best-selling book The First Three Minutes, and eventually got to understand General Relativity from his clear book Gravitation and Cosmology.

An exciting event was a visit by Richard Feynman to give a seminar at MIT. Feynman had recently published a paper on pure, quantum Yang–Mills gauge theory, in which he gave arguments why there is no massless particle in the theory. The lowest energy particle would be a positive mass glueball rather than a massless gluon, and one says that the theory has a mass gap. Gluons would exist as particles in a limited sense, but like quarks they would be confined. His paper was mainly about the case of Yang–Mills theory in two space dimensions, where on dimensional grounds, the arguments are simpler than in three space dimensions. There was no

rigorous result in the paper, but the ideas were stimulating. Roman Jackiw knew that Feynman came to Boston fairly frequently to visit a group that was pioneering quantum computers, and persuaded Feynman to take time off to speak about his gauge theory work at MIT. A small group of us went with Roman by car to collect him, and that was an opportunity to discuss his work. He was lively, but we couldn't get him much interested in our research. Roman arranged that Feynman would first give a general seminar open to all, to be followed by a technical session where we would discuss ideas on how to make his results rigorous. The first session was packed out. Feynman included a few technical details, and spoke for well over an hour. At the end, Jackiw asked non-experts who'd heard enough, and not interested in further details to leave, but hardly anyone did so. The seminar restarted, and Feynman was asked to go through some of his arguments carefully again. But I think he felt he had said just about enough, and didn't have a clear idea how to develop his ideas further. So the seminar ended. Even today, there is no proof of Feynman's claim of a mass gap, though the numerical evidence is convincing.

Quite a few seminars, especially at Harvard, were on GUTs and on technicolor. Technicolor is the idea that the scalar, spin zero Higgs boson is a bound state of two fermions. It extends the notion of color in QCD, and the name is intended as a joke. Just as pions are bound states of quarks, so the Higgs boson would be a bound state of techniquarks. This appealed to theorists who were suspicious of fundamental spin zero particles, the reason being that they fail to satisfy some technical notion of 'naturalness'. However, the initial technicolor models had problems of their own, and a seminar that we heard proposed a baroque variant, with lots of new particles and lots of unknown parameters. This wasn't convincing.

The difficulties with GUTs and technicolor – both of them requiring many new particles and parameters, an unsuccessful prediction of proton decay, the lack of discovery of the Higgs boson by then – meant that many particle theorists in the US felt that the whole subject was in some kind of malaise. I heard a really dreadful talk by the great Murray Gell-Mann a couple of years later at Santa Barbara on an absurdly complicated grand unified theory based on the group SO(22). Such GUTs are not grand or unified, because very complicated hierarchies of symmetry breaking steps are needed to get anywhere close to the Standard Model with its much smaller gauge group $SU(3) \times SU(2) \times U(1)$. Something really new was needed to go beyond the Standard Model. That made the field ripe for the superstring revolution in 1984. In defence of Gell-Mann, I should say that he supported John Schwarz at Caltech for many years when string theory was unfashionable, and John, with Mike Green, was the instigator of the superstring revolution.

Returning again to life outside physics, at some point I left the Belmont Orchestra and joined the better Civic Symphony Orchestra of Boston. As one of its members, I briefly joined the orchestra of the Boston Conservatoire of Music as a guest player for their Commencement Concert in May 1981, where they performed Beethoven's 9th Symphony. This was recorded, and I have a copy of the LP records. It's not bad at all, although the horn fluffs the counting once in the slow movement.

I also had a visit from Richard Antrich in June. That was important, as we rented a car, an automatic, and I drove again for the first time since about 1970. Richard gave me some good instruction, and fortunately the driving wasn't difficult on the wide American roads. A bit later I rented a similar car by myself for a weekend, and drove from Boston to the start of Cape Cod and back. On a particularly hot summer day, Ian Affleck and his wife offered to take me on an outing to the sea, and we went almost to the end of Cape Cod, passing the large cranberry bogs. I sat on a beach wearing a teeshirt and trunks, with legs just below the water to keep cool. That

was not wise, as I was badly sunburnt. I had an agonising night back in my apartment trying to ease the pain with wet towels and my fan on. It took a couple of days to recover. Ian's wife was a Canadian from Timmins, a town towards the north of Ontario. Her family were involved with the gold mining there. It sounded rather exotic and interesting, and I managed to visit Timmins and its gold mine many years later with Anneli and Ben.

I escaped the Boston heat for part of that summer, spending some time back at the Ecole Normale during their summer institute, and participating as a tutor at BUSSTEPP in St Andrews. St Andrews, with its coastline and famous golf courses, was a charming place to spend a couple of weeks, and we enjoyed the challenge of the 'Himalayas' putting course.

During my second year at MIT I had made friends with Linda and Steve Pope, a couple who worked there. She was the editorial secretary of Annals of Physics, with whom I had had some dealings previously over my papers. When I had first visited her office, she recognised me as British before I spoke, because I was wearing a Marks and Spencer pullover. She knew about Britain because her husband, a faculty member in the engineering department, was from there too. They invited me for dinner to their house, rather frequently. I was a bit embarrassed to accept too often and tried to reciprocate by inviting them to my rather bare apartment, or to a restaurant. Anyway, they didn't have children and seemed to like my company. I also enjoyed their company and talking about the UK, though we didn't have much in common scientifically. At this time, I realised that my social life had changed. Rather than having single, drinking pals of my own age, I was now moving into circles of married couples. In America, people didn't in fact go out drinking much. I felt I didn't want to go on like this for ever, and with the move to California coming up, resolved to try to find a partner to share my life.

20 Three-Year Postdoc in Santa Barbara

In late September 1981 I flew out to Santa Barbara and started my postdoc there. As I was now comfortable with driving, I rented a car for a couple of weeks, a Toyota Corolla, and planned to buy my own car as soon as possible. Practical, economical cars like the Corolla were plentiful in California by then, and the large American gas-guzzlers were going out of fashion. After a few days I found a place to live, about halfway between the university and downtown Santa Barbara, at 4002 Via Lucero, Apartment 8. Santa Barbara was familiar from my one-month visit in 1980, but renting an apartment in suburban surroundings and driving about were novelties.

The apartment had two floors and was part of a three-sided low-rise complex, which surrounded a partly roofed-over car park with a pool at its centre. My apartment had a living room downstairs and bedroom upstairs with nice carpeting but was otherwise unfurnished. I swam a little the first week or two, but the pool was very small and filled up with insects and leaves. It was maintained regularly, but I soon got the impression that such pools were regarded more as decorative status symbols than useful. Occasionally, the Space Shuttle would pass overhead, emitting a pair of sonic booms as it came in to land at Edwards Air Force base about 100 miles from Santa Barbara.

I had some furniture delivered from Cambridge MA, but needed a new bed, so I quickly got a queen-size bed from Sears department store. It was a bit big for me alone, but turned out to be just right later. I also bought a TV, and had access to a wide range of cable channels. A highlight was the series Brideshead Revisited, shown in the US early in 1982, which had a stellar cast, beautiful locations in Oxford, Venice and at Castle Howard (Brideshead itself), and poignant music.

During the first two weeks I toured the used-car showrooms around Santa Barbara, but in the end took a bus to Ventura, down the coast towards Los Angeles, and the Toyota dealership there. They had a new, white Toyota Carina for about 6000 dollars. I bought that and drove back to Santa Barbara. It was clean and looked good compared with what most postdocs had, but was rather minimal, with visible wheel nuts and no hub caps for example. and synthetic materials inside in an attractive blue. I had a UK driving licence, but needed to get a California licence after a few weeks, which meant passing a written test, including questions about whether it was legal to turn right on red, and how far behind school buses and fire trucks one needed to stop.

At the ITP, the director was Walter Kohn, assisted by his deputy Bob Sugar who was also attached to the physics department nearby. Frank Wilczek had joined the physics faculty too since my previous visit, but was more-or-less full-time at the Institute. Walter Kohn was welcoming, and seemed to have broad interests in physics. I didn't know then that his main claim to fame was the density functional theory of multi-electron atoms, most appreciated by chemists. This later won him a Nobel Prize in Chemistry. Bob Sugar led a group doing lattice field theory. Frank Wilczek was interested in solitons, and particularly in the interplay of topology and quantum phenomena in field theory. This included his work on anyons, and on anomalies and fractional charge. I discussed some of these things with him, and tried to learn more, but didn't make any real progress in this area till a year or two later.

The ITP was a friendly place. Most people there were not Californians, with the exception of one postdoc. There were several British postdocs, including Tony Kennedy and Nigel Goldenfeld at ITP, and Denis Nicole, Brian Pendleton and Neil Turok based in the physics department, some of whom arrived at different times from me. Non-Brits included Doug Toussaint, Rob Pisarski and Emil Mottola. Alan Newell, originally from Ireland but now a professor in Arizona, was a visitor, and he arranged Friday evening drinking outings to a pub in Goleta, followed by dinner in a restaurant (seafood I think). Goleta was the district north of the airport, and closer to the university than Santa Barbara. There was still a small gap between Goleta and Santa Barbara with lemon groves, but these had once been much more extensive. Goleta was not such a prestigious and prosperous place as the city of Santa Barbara, but a local brochure described it as a place "where the elite retreat." That amused those who lived there, like Tony.

One weekend early on, ITP arranged for a group to go on a boat trip from Ventura to Anacapa Island, the only easily accessible offshore island. I also visited Berkeley, where I met Edgar Knobloch again after many years. He was on the faculty of the physics department there, and applying ideas of dynamical systems to astrophysical problems. To keep my violin playing going was less easy, but I joined a string quartet at UCSB after enquiries at the music department. That didn't last long, however.

Shortly after I arrived at ITP there was a visit from Iz (Isadore) Singer. He was a real mathematician, but seriously interested in theoretical physics. He was then based at Berkeley, and well known as a good friend and collaborator of Michael Atiyah. He spoke about his view of quantum gauge theory, a more geometrical version of what Feynman had spoken about at MIT. The space of gauge potentials is an infinite-dimensional linear space, but the true configuration space (still infinite-dimensional) is the quotient of this by the group of gauge transformations, which is highly nonlinear and topologically rich. So the true configuration space is topologically complicated. It inherits both a Riemannian metric from the gauge theory kinetic energy, depending on the nonabelian electric fields, and a potential energy, depending on the nonabelian magnetic fields. The quantum Hamiltonian, as in most other field theories, is the sum of the

negative of the Laplacian on the configuration space, which is constructed from the Riemannian metric, and the potential energy. What one wants to prove is that the energy spectrum has a gap above its vacuum energy – a mass gap. Singer's idea was that in the directions where the potential is weak (long wavelength modes) the Riemannian geometry is as on a compact manifold, leading to a discrete spectrum even without a potential. In the directions where the potential is strong (short wavelength modes), the potential alone creates a discrete spectrum, as in the harmonic oscillator, even though the Riemannian geometry is becoming non-compact. These were nice ideas that made intuitive sense to me. Unfortunately, Singer and his friends could never prove the ideas rigorously, because of analytical difficulties.

Singer's picture had some antecedents that I was familiar with. Babelon and Viallet had discussed the Riemannian metric on the true configuration space of a gauge theory as a projection of a degenerate metric, where the null directions correspond to gauge transformations. This was consistent with the requirement that wavefunctions had to be constant along the orbits of the group of gauge transformations. Physicists had found a local version of the true configuration space by imposing the Coulomb gauge condition, but Gribov had shown that this was not a linear condition as it naïvely appeared, because there exist large gauge transformations in nonabelian theories that preserve the Coulomb gauge condition, mapping a gauge potential to a physically equivalent copy, and one needs to quotient these transformations out. The existence of these Gribov copies could be interpreted as saying that the true configuration space can be covered by one (linear) coordinate chart, but the chart has a complicated boundary. Large gauge transformations map the exterior of the chart to the interior. I found Singer's picture of the true configuration space as being a smooth Riemannian manifold much more illuminating.

I didn't directly work on this geometrical picture of quantum gauge theory, but thought of an interesting application to monopole dynamics. In pure gauge theory, the lowest energy field configuration, the classical vacuum, is a unique configuration after quotienting out by gauge transformations. It is a point in the true configuration space. But in a Yang–Mills–Higgs gauge theory with BPS monopoles, the space of lowest energy field configurations with fixed non-zero monopole charge N is instead a manifold of finite, positive dimension. This submanifold of configuration space is known as the N-monopole moduli space. In the theory with gauge group SU(2), its dimension is $4N$. (For example, the 1-monopole moduli space has three coordinates specifying the position of the monopole, and one internal phase angle. When the internal phase is time-varying, the monopole acquires an electric charge and becomes a dyon. This kind of phase twisting was important in my study of monopole pair production with Ian Affleck.) On each moduli space, the potential energy is constant, and minimal, but the Riemannian geometry is likely to be nontrivial. This is what I learned from Singer's insight. I then realised that N-monopole dynamics at slow speeds is approximately a drifting or adiabatic motion through moduli space. The potential energy constrains the dynamics to be close to the moduli space, but is otherwise unimportant. BPS monopole dynamics is therefore like free motion in a finite-dimensional valley that may be curved, but has a flat bottom. The dynamics is dominated by the kinetic energy term while the motion remains tangent to the moduli space. And since the kinetic energy is quadratic in velocities, it simply encodes the Riemannian metric on the moduli space. Motion through the moduli space should therefore follow geodesics.

This is the geodesic approximation to monopole dynamics, but one needs to know the metric in order to find monopole trajectories explicitly. For one monopole this approximation is simple to apply, as the metric on the 1-monopole moduli space has no curvature, and the result is that a monopole has a constant velocity and a constant electric charge. However, at the time I didn't

know at all what the Riemannian metric on the 2-monopole moduli space was like. Later, this metric was constructed explicitly by Atiyah and Hitchin, using the insight that the metric is of a special kind known as hyperkähler, and it was then possible to study in detail the motion and scattering of two monopoles by finding the geodesics. The interactions of various other types of soliton could be investigated similarly.

The reason this approach is an approximation is because it ignores the small deviations of the motion away from the moduli space. These occur for the same reason that a bobsleigh going around a corner on a flat-bottomed track climbs up the side of the constraining wall. However, such deviations are small at slow speeds, and in the context of solitons seem to be negligible whenever the motion is non-relativistic.

Just before Xmas in 1981 there was a conference in Trieste on Monopoles in Quantum Field Theory. This was to celebrate the 50th anniversary of Dirac's seminal paper on magnetic monopoles in quantum mechanics. I was invited to speak, and planned my trip so as to spend a few days beforehand in Pisa visiting Paolo Rossi, who had returned there from MIT. The flight was tiring, from LA to New York, then to Rome, and after a wait, a third flight on to Pisa. I got hardly any sleep and was badly jet-lagged. The trans-Atlantic plane, descending into Rome airport, got too close to a smaller plane in front, and close to landing had to suddenly and alarmingly climb and turn to approach the airport a second time. I was worried that there was insufficient fuel left for this manoever.

In Pisa, after two or three days, I gave a talk but was so tired I nearly fell asleep on my feet. I asked for a coffee, guessing that it might be easily available, but it took about 15 minutes to make, and was then delivered in a fine pot to the seminar room. Somehow I kept going but that was probably the worst talk I ever gave. Nevertheless, it was charming to see Italy in a fairly mild, rainy December with few tourists. I went sightseeing from about 7 in the morning, and then needed to sleep in the afternoon. There was a physics outing to Florence for a seminar and I enjoyed being there again, eight years after the visit with my parents during the cholera crisis.

Paolo and I took a train to Trieste after five days, and I recall being for the first time completely recovered from jet-lag that afternoon. Trieste was considerably colder than Pisa, despite being on the Adriatic. The conference was at the International Centre for Theoretical Physics, hosted by its Director, Abdus Salam. This was probably the largest scientific meeting ever devoted solely to monopoles, with well over a hundred participants.

Dirac himself declined his invitation, writing "I am inclined now to believe that monopoles do not exist. So many years have gone by without any encouragement from the experimental side." However, there had been a lot of recent, theoretical progress on monopoles. In addition to the work of Prasad and Rossi that I had followed closely at MIT, similar results on explicit solutions of the Bogomolny equation for monopoles had now been obtained by Peter Forgács and his Hungarian colleagues, and Peter Goddard and Ed Corrigan had developed a general construction of monopole fields that reduced the problem to satisfying some nontrivial contour integral constraints, extending special cases that appeared in Ward's work. Another development was due to Werner Nahm. He had considered the ADHM construction reducing the PDEs defining instantons to algebraic conditions, and found an analogous construction for monopoles, exploiting the notion that monopoles are time-independent instantons of infinite charge. Nahm's construction reduced the problem to solving a set of ODEs (ordinary differential equations) – the Nahm equations. The Nahm equations are formally integrable, because they can be expressed as a Lax pair with a spectral parameter. Monopoles therefore have a spectral curve, and this fundamental object connects the various constructions of explicit solutions.

My own talk was mainly on the work I had done with Ian Affleck on monopole pair production, but at the start I spent ten minutes outlining the idea of monopole dynamics as geodesic motion on moduli space, which I had just written up as a paper before leaving Santa Barbara. Afterwards, members of the audience including Atiyah and Nahm said that the geodesic idea was much more interesting than the pair production calculations. That was nice, but I felt it ironic that an idea that had been written up after a fortnight's work, and was hardly developed, was more appreciated than the whole year's work on pair production. There were several other talks, on monopoles in SUSY theories and in quantum field theories more generally, and on dualities. There was also a summary of (unsuccessful) experimental searches for monopoles. All this appeared in a book of proceedings.

I spent Christmas in England with my Mum, and returned to Santa Barbara a few days into 1982. The flight went far to the north over Greenland and Canada, in the dark, and it was so cold that the water pipes froze for a while, so there was no tea or coffee. I was alarmed again, this time thinking the fuel might freeze up, but it didn't.

The winter season in Santa Barbara was a good time to explore the back country; the native vegetation there is quite different from the lush planting in the city gardens and streets. I took part in some hikes along the several wooded valleys with small streams that descend from the mountains behind the city. Here there was shade, and sometimes small pools where one could have a dip. Above the highest spring the landscape was dryer, with little shade, and on one occasion I got mild heat exhaustion. It had been a cool morning and I had taken a pullover and jacket, expecting the temperature to drop as one climbed up, as it does on the hills of England and Wales. But in California it gets quite hot around mid-day as one goes away from the coast, even at modest altitudes in January. Fortunately I found a bush with a bit of shade, drank some of my water, and after half-an-hour had recovered. At least some of the party came down the same route, so I had company when I rejoined them. As an alternative to hiking, I got some exercise by playing soccer. One of the postdocs, Emil Mottola, was keen to arrange a kick-about on Saturdays; it was on a real grass pitch, but the grass was tough and sharp-edged to cope with the strong sun. Two of the other players were Turkish, non-identical twin sisters from the physics department, one of whom was called Atiye, easy to mix up with Atiyah. Another interesting place to go in winter was a wood near the coast, a few miles west of Goleta, where monarch butterflies hibernated. There were many thousands of them, mostly resting against the tree trunks and branches, but some flying about.

My Mum visited Santa Barbara with her friend Marlis Woodward in April. We went to Los Angeles for a day or two, to Disneyland and also to see Verdi's Falstaff. I particularly like the music of Falstaff, but Marlis focussed more on the storyline and found it a bit childish. At about this time the Falklands War started, and relations between the British postdocs at ITP and the one Argentinian postdoc got frosty.

From the start of 1982, the ITP had hosted a new programme on dynamical systems. It was typical that ITP had shorter-term visitors for particular research programmes, as well as permanent staff and longer-term postdocs. Dynamical systems was a flourishing and fashionable subject, with 'chaos' as the catchphrase. The main interest was the chaotic and long-term unpredictable behaviour of solutions of differential equations, but chaos in the behaviour of mappings was also being studied. These topics are related, because a differential evolution equation with cyclic behaviour can be investigated through the return map that occurs after each cycle. A differential equation in three dimensions, say, defines a return map on a two-dimensional section. I didn't fully appreciate the relation, because in most cases one cannot

explicitly evaluate the map from the differential equation. However, it was claimed that the details of this aren't important, because what matters is the universal character of the chaos, and how that sets in as one varies parameters.

Michael Nauenberg had moved to ITP from UC Santa Cruz at this time, and became acting director of ITP for a while. Other visitors for this programme included Mitchell Feigenbaum, whose work on the universal behaviour of one-dimensional maps had had a big impact, Mike Cross, David Ruelle, Pierre Hohenberg and Eric Siggia. I noted then and later that it was typical of American science that many researchers in leading institutions would move into fashionable areas quickly, more so than in the UK, but the key breakthroughs were often made by more isolated individuals working in less prestigious institutions. Feigenbaum, at Los Alamos and subsequently at Rockefeller University, and Robert Laughlin, working a little later on the Fractional Quantum Hall Effect at Livermore, were good examples of such individuals.

I got interested in these maps, through hearing a few seminars. They are iterated maps, because each cycle of the dynamical system gives one operation of the map, and it then repeats arbitrarily often. A mathematically elegant type is an iterated complex map, a holomorphic map from the complex plane to itself. The behaviour of such maps had been considered for decades previously, going back to work by Pierre Fatou and Gaston Julia in Paris in the early 20th century. Gaston was Bernard Julia's grandfather, I think. The simplest interesting maps are quadratic, with just one complex parameter after the map is normalised. These were the maps considered by Benoit Mandelbrot, and his famous set is a region of their parameter space.

The newer work on maps, including that of Feigenbaum, suggested new questions. I discussed this with Michael Nauenberg and we started to work on just one specific map, a quadratic complex map where the linearised behaviour near the origin is a rotation by an irrational multiple of 2π. Specifically, we assumed the multiple was the golden ratio 0.618... as this number is maximally far away from its rational approximations, as one sees from its continued fraction representation. We could study the behaviour of our map numerically, something that Fatou and Julia couldn't do. Michael knew how to do the numerics, and print out graphs, which was most helpful. We found that the iterated map near the origin traces out points that eventually fill in smooth, slightly deformed circles, but there is an outermost one of these continuous curves that is fractal in shape, and nowhere smooth. This outermost curve passes through the unique point where the map has a stationary point. We studied its properties numerically, and explored how things change as one varies the map a little. This was interesting, and we discovered some universal behaviour, but we couldn't prove anything with our numerical techniques. After a few months we wrote a paper with several conjectures that we hoped others could prove. The region we had mapped out was called a Siegel disc, and we had a good idea of what it really looked like, including its fractal boundary.

That was my brief foray into dynamical systems, and I moved back to working on theoretical physics problems afterwards. But our work made some impact, and I attended a few dynamical systems meetings that year and the next, sometimes being invited to speak. The first opportunity was to attend a conference at Los Alamos in May. This was my first visit to the American southwest. I flew to Albuquerque in New Mexico and rented a car to drive north to Los Alamos. In Albuquerque, I was struck by a familiar looking view. It was one of the Haas photos in the book L'Amérique that I had bought in Paris. The caption in the book didn't mention Albuquerque; the photo was just illustrating a typical Main Street USA, with lots of commercial signs for motels and fast food joints, and a reasonably tall bank building at the end. At the conference, Mandelbrot gave an extended talk, or maybe he just went over his allotted time. After some perfunctory

mathematics, he showed many computer-generated images of fractal landscapes created by his assistants. This was supposed to prove that real landscapes, tree shapes, cloud shapes etc. can be modelled by rather simple fractal structures generated by even simpler algorithms. The idea is neat, and the pictures are pretty, but the details are not very illuminating mathematically. In retrospect, it seems that there is no universal fractal dynamics, comparable with Newton's laws, explaining the complicated appearance of nature. The fractal idea, implying scale invariance over an infinite range of length scales, doesn't seem to work very well. Instead, one law works over a fairly modest range of length scales, and is then replaced by a different law at the next length scale, and so on. The lack of a wide range of length scales undermines the notion that there is a fractal dimension. Stated more bluntly, the coastline of the UK doesn't have an infinite length, because the notion of a single coastal boundary curve breaks down at a scale of a few metres.

The lab at Los Alamos had a part 'behind the fence' to which we had no access, so there wasn't much to do there. At the conference I met a mathematician who worked in San Francisco, Gerianne Krause, and we got on well. We joined a group that went down the hill to Santa Fe, one of the most historic towns in the US. After a few days we were good friends, and almost intimate. But she had relationships with people in Berkeley, and was not interested in continuing our short-lived friendship, although we did arrange to meet for a coffee when I went to Berkeley a month or so later. It was disappointing for me to find that someone I rather liked was already attached.

Back in Santa Barbara I became involved in another string quartet, mentioned earlier in the section on Music. The cellist was June Kambach, who lived in a fine house on Hope Ranch, a private estate. Her husband, a businessman, was often away, and June was keen to play music on most days. She had friends, Mary and Edgar (Bump) Hoover, who played viola and violin and lived at a pleasant retirement complex, Valle Verde. They had their own apartment that was rather similar to mine, but could also take meals communally if they wished. I was willing to play once every week or two, and became the first violinist. There was never any problem for me arranging quartet evenings, because they had more spare time than I did, and I think they enjoyed our quartet playing more than their other music. That made a pleasant change from my experience over many years in England and in Boston, where the best musician in a group usually plays hard to get. During the more than two years that we played together, we tackled some Haydn, most of early Beethoven and the ten famous Mozart quartets, and I especially remember our reasonably successful attempts to play the late Beethoven quartets. I knew these well from records, and practised them, so I could keep the quartet together when the rhythms and notes got tricky. We never played these in public, but we did play movements from a Beethoven Opus 18 quartet at a local concert, and once we were invited to play background music at a wedding reception, although there we were outside and no-one really heard us.

Shortly after returning from the Los Alamos trip I was off again to the American south-west, this time on a geological tour arranged by the geology department of the Santa Barbara Community College. This was something that had run in previous years, and I was attracted by an advert. It was very well organised, with a bus and driver, the head of the department as guide, and a separate van carrying tents and a field kitchen. The van went ahead each day to set up camp, while the bus visited the interesting geological sites. The whole trip was 11 days in mid-June and there were about twenty paying people on the tour. It was supposed to make some money for the department, but unfortunately didn't and the year I went was I think the last. I can't describe the whole itinerary, but it was a really fantastic trip, one of the best of my life.

Not only did we see many of the iconic places, but we were also given professional insight into the landscape and rocks. We went through the Mohave desert to Cottonwood in Arizona, and through spectacular Sedona to Sunset Crater and the Grand Canyon, then on into Utah past Monument Valley, stopping to see a large coal mine in Navajo territory. After that, we visited most of the National Parks in southern Utah – Arches, the edge of Canyonlands, Zion, Bryce – before returning via Las Vegas to Santa Barbara. Particularly memorable were the columbine flowers in a wet spot in Zion, and the quite unexpected splendour and strangeness of Bryce Canyon. I have a good collection of slide photos from the trip, and even kept a diary with quite a lot of detail. The night before Las Vegas, in the Valley of Fire at fairly low altitude, it was so warm we could sleep under the stars, but earlier, up on the Colorado plateau, the nights got quite cold and tents were definitely needed. We only spent about an hour one morning in Las Vegas, but in that time I managed to make a profit of 65 cents at one of the casinos.

I was keen to go again one day to places that we had rushed through, and on a sabbatical trip to Arizona in 2014, Anneli and I revisited some of them. We stayed a few days in Sedona, at the Grand Canyon, and in Monument Valley, and I also showed Anneli the Goosenecks on the San Juan River, which I remembered well.

I didn't spend much time back in Santa Barbara before going off yet again, this time to Aspen. Usefully, there was a daily direct flight by jet plane from Santa Barbara to Denver, and from there I drove a rental car via Copper Mountain, Leadville, and over Independence Pass to Aspen. At the top of the pass, at over 12000 feet, I thought there might not be enough oxygen for the car, but more of a problem was that I myself was a bit short of breath, so soon descended. It is quite interesting to visit towns named after the elements, often mining towns, but I haven't particularly sought them out. I've been recently to Carbon (in Alberta), but missed Boron (in California). In Aspen I worked with George Chapline on 10-dimensional supergravity, as described earlier. George also introduced me to some friends of his at a party. This wasn't quite my scene, with some cocaine sniffing in addition to beer drinking. I was offered a go in a float tank, an enclosed bath with some salts in the water to increase buoyancy. It was a nice experience but didn't quite work for me as my legs tended to sink. I just couldn't stay completely still.

After a week or two, I was pleased to go on a hiking trip with a few others, including Sidney Coleman and his wife Diana. They were considerably older, and not too athletic, so I thought this wouldn't be too demanding. We started at 10000 feet, and climbed together to the foot of the famous Maroon Bells mountains, at about 12000 feet. Here we had lunch, and a few of us including Sidney continued up to a nearby peak covered with boulders at about 13000 feet, with great views of the more rocky mountains round about, at heights over 14000 feet. That's probably the highest climb I've ever done. Sidney was diabetic by this time, and had a low-sugar crisis at some point on the trip. I'm glad I could help out with the raisins I continually carried along with me in case I had a hypoglycemic episode.

On the way back to Denver, I took the main road through Glenwood Springs, and enjoyed a break at the large, naturally warm swimming pool in the town. Back in Santa Barbara I polished up the paper with George and started to think about dimensional reduction of supergravity. It was hot in Santa Barbara in the later part of summer, so I went a few times to the beach at about 4 in the afternoon, when the sun was no longer too strong. At that time of year, it was warm enough to swim in the ocean without a wet suit.

Near the end of September I returned to the UK for an extended period. The ITP was not too keen on my taking leave but agreed to this. Michael Atiyah, during the Trieste conference

or just after, had invited me to spend a term in Oxford at the Mathematical Institute, and had arranged for me to share a large house with Cliff Taubes in Northmoor Road in north Oxford, owned by his college St Catherine's. Cliff was delayed and only arrived two or three weeks later. He was depressed by the perpetually dreary, cloudy weather during that Autumn, and left after six weeks to return to colder but sunnier Cambridge MA. I stayed a further two weeks into December, when there were several days of cold, lingering fog in Oxford.

Despite the poor weather, I enjoyed my time in Oxford. Several of the students were doing great work on the mathematics of gauge theories. The most striking results were by Simon Donaldson on 4-manifold geometry, using the instanton equations and instanton moduli spaces as tools; Donaldson was awarded the Fields Medal not long afterwards for his results. I tried to understand the basic ideas, and gave some lectures on this when back in Santa Barbara in January. Others I met in Oxford were Frances Kirwan and Michael Murray. Michael stayed in touch when he settled back in Australia, and invited me for a sabbatical visit to Adelaide in the early '90s.

Also interesting were the powerful methods Nigel Hitchin had developed to understand monopoles. He had established that for the Bogomolny monopole equation, the set of all straight lines in three dimensions forms an appropriate twistor space. This is a space of two complex dimensions, and is simpler than Penrose's original twistor space of planes in four dimensions, which has three complex dimensions. The monopole fields define an equation along each line, and thinking about solutions of this equation (now known as the Hitchin equation), one could recover most of the previously known results about exact monopole solutions. Hitchin's formalism was not easy for me to understand, because it involved holomorphic bundle theory over twistor space, but it gave good intuition into the spectral curve of a monopole – a monopole's most basic gauge-invariant feature. Hitchin's formalism did not break any of the symmetries of three-dimensional Euclidean space, which was a real advantage over other approaches.

A further development in monopole theory occurred a little later. Donaldson showed, using Hitchin's equation along lines, that there is a one-to-one correspondence between SU(2) monopoles and rational maps. Rational maps are really functions. They involve a single complex variable, and are ratios of two polynomials. The degree of the rational map (equivalently, the degree of the polynomials, or the larger one of these if they are not the same) equals the monopole charge. This meant that the moduli space of N-monopoles, which was not well understood in detail, was the same as the better understood moduli space of rational maps of degree N. I was particularly interested in the phenomenon that there are very special rational maps with given symmetry, and these correspond one-to-one with monopoles having the same symmetry. For example, there is a unique centred rational map with axial symmetry and degree 2, so there is a unique centred monopole with axial symmetry and charge 2. This was an incredibly simple way to evade the difficulties that had been overcome earlier, by Ward and others, to establish the existence of such a charge 2 monopole. This particular monopole was somehow entirely encoded in the simple map $z \to z^2$. Donaldson's formalism actually broke full rotational symmetry, but later an alternative version of the correspondence between monopoles and rational maps was found by Atiyah's student Stuart Jarvis. In the Jarvis formulation, rotational symmetry is preserved but translation symmetry is lost, so it is best applied to monopoles with their centres at the origin. Using Jarvis maps, it is fairly easy to understand the charges for which a monopole can have various platonic symmetries, like tetrahedral or icosahedral symmetry.

While in Oxford, I myself was working on dimensional reduction of supergravity, but Michael Atiyah thought that this was a rather uninteresting project. He clarified for me the essential

triviality of some of my thinking about symmetric fields, including gauge fields. He pointed out that on each orbit of the symmetry group, one just needs to ensure symmetry under the action of the isotropy group at one point. This reduces to linear algebra, because the isotropy group doesn't move the point, and acts linearly on the tangent space there. Having imposed the symmetry at one point, the fields elsewhere on the orbit are determined by acting transitively with the symmetry group. Actual formulae then follow automatically, and aren't very interesting. I knew most of this – including the importance of constraints resulting from the action of the isotropy group, but Atiyah had explained how this was just about everything, because it was trivial to construct the fields globally after satisfying the constraints.

I didn't discuss things very much with Cliff Taubes, but we did talk a little about his discovery in the previous year of a solution of the SU(2) Yang–Mills–Higgs equations representing a monopole and antimonopole in unstable equilibrium. This solution is in the vacuum sector of the theory, with zero net monopole number, and it doesn't satisfy the first order Bogomolny equation. Cliff had proved that such a solution exists by using a topological argument. His idea was that by inputting some energy, one can create a monopole-antimonopole pair from the vacuum and separate the pair a little. Then one can twist the antimonopole around by 2π, bring the pair back together again, and annihilate them, extracting some energy and returning the field to the vacuum. Importantly, he showed that this vacuum to vacuum path is non-contractible, meaning that one cannot smoothly reduce the energy everywhere along the path so that the fields are arbitrarily close to the vacuum. Instead, the top of the path gets trapped at a saddle point of the potential energy. More heuristically, if the antimonopole twist is stopped half-way, after a rotation by π, then the fields are close to an unstable equilibrium solution that has quite high energy, and also a magnetic dipole moment. Here, the antimonopole doesn't know whether to twist forwards or backwards to annihilate with the monopole. It is like being perched on a mountain pass – a locally flat saddle point in the landscape – from which one can descend by going either forwards or backwards.

After Cliff left Oxford, I mulled over this argument and wondered if it could apply to the standard electroweak theory. That theory also has an SU(2) gauge group (together with a U(1)), but it has a different type of Higgs field, a complex doublet, and therefore no monopoles. However, the topological construction could still work. I had a Eureka moment while walking home along the Banbury Road one evening. I realised that the topology of the electroweak theory should indeed allow the existence of an unstable but static solution of the field equations, despite there being no stable monopoles, and this solution would have a localised character, rather like a kink soliton or a monopole. No-one had found a solution of exactly this type before, but I was convinced it should exist. I planned to investigate this further after returning to Santa Barbara.

I spent Christmas at home in London and returned to the US very early in 1983. I flew directly to San Diego, to participate in a Dynamical Days workshop at a large hotel in La Jolla, and give a talk. I remember the mornings were chilly, but otherwise the weather was pleasant for early January. However, shortly afterwards, when I was back in Santa Barbara, there were several storms, and one of these caused serious mudslides in Malibu, burying the coast road. Storms in Southern California don't involve fierce winds, just a lot of rain. There is usually at least a couple of days warning before a storm bears down, as a deep depression moves along the coast from Alaska. The locals thought these storms were terrible, because they prefer unbroken sunshine, but I thought that was boring, and welcomed some grey skies and rain. There was no talk of drought while I was in Santa Barbara. For that, you need rain.

In February, Emil Mottola and another postdoc, Eshel Ben-Jacob, proposed a short skiing

trip to Lake Tahoe, on the California-Nevada border. I went along, and so did the ITP secretary Sharon Krieg. I described this eventful trip and Emil's injured leg earlier, in the section on Sport. I once went on a date with Sharon. She was gentle-voiced, but underneath was a strong, sporty woman. We weren't well suited to each other, and she in fact developed a close relationship with Emil at about that time.

The rains continued intermittently into March, when the Queen visited Southern California. She was supposed to travel from Long Beach to Santa Barbara by yacht, but flew instead. I went to the airport on 1st March to get a glimpse of her arrival. She was met by the President, Ronald Reagan, who took her party to his ranch above Santa Barbara for lunch. The newspapers said that the rain turned the Ladies in Waiting into Ladies in Wading.

For fun, I acquired a book parodying the Southern California lifestyle, called The Valley Girl's Guide to Life (referring to the San Fernando Valley in suburban LA). The ideal day involves dressing and making up, more than once, shopping at the Glendale Galleria, spending the afternoon at the beach watching the dudes surf, and then hanging out with one of them in the evening. A storm can really spoil such a day! Emil thought I bought this book for my education, to understand Californian girls better. He was partly right, but I was a bit too old to join in, and never tried surfing. To extend my education, I also acquired Real Men Don't Eat Quiche!

Workwise, after finishing writing up the paper on dimensional reduction of supergravity in January, I delved deeper into my topological argument that the electroweak theory supported a static, but unstable solution. The basic idea was to use a variant of Morse theory, which I knew something about from Taubes' work, also from a classic book by Milnor on the subject, and finally from Witten's paper Supersymmetry and Morse Theory, which was published the previous year. I needed to construct a non-contractible loop of finite energy field configurations. The topology in the electroweak theory is entirely captured by the behaviour of the asymptotic Higgs field near infinity. The field there has to lie on a 3-sphere as this is the manifold of vacua for the complex Higgs doublet. The sphere at infinity in physical space is an ordinary 2-sphere, so one needs to construct a loop of maps from a 2-sphere to a 3-sphere, that in total covers the 3-sphere once, and cannot be contracted. This is not difficult. It is like taking a short elastic band resting on an ordinary sphere, stretching it and swinging it across the whole surface of the sphere. The band starts and finishes with essentially zero length, and at the half-way point goes round the equator. The required asymptotic Higgs field is similar but in one dimension higher. Having found a formula for this, depending on one parameter along the loop, I had to flesh it out to include the gauge field and extend both the Higgs field and gauge field smoothly over all of space. This could be done, and in a fairly symmetrical way. If there is just an SU(2) gauge field, the fields can be truly spherically symmetric. The additional U(1) gauge field of the electroweak theory breaks this, and allows only a residual axial symmetry, but its effect can be neglected in the first approximation. I wrote this up by May, and included an upper bound for the energy of the static, unstable solution, analogous to the energy of the stretched rubber band going round the equator.

At some point while in Santa Barbara I renewed my interest in stamp collecting, but not much, as there were other things to do, especially the string quartet. I tried to join the local Santa Barbara Symphony Orchestra, which I thought would be a good quality civic, amateur orchestra. I was invited for an audition, and was asked to bring something prepared – the start of Mozart's 40th symphony, I recall. There was also some sight-reading. Interestingly, in the audition I was screened off; this is to avoid discrimination, but it was the only time it happened

to me. The audition didn't go very well, but neither was it a disaster. They asked who my teacher was, or had been. That was trickier. I hadn't had violin lessons for more than ten years, but fortunately remembered a few lessons I'd taken back then from Mr Good, a professional in one of the London opera orchestras. They'd never heard of him, of course. I didn't get into the orchestra, but shortly afterwards went to one of their concerts. It turned out that they were very good and professional, really just one step down from the LA Philharmonic. I wish I'd known earlier.

In May I visited Berkeley briefly, and afterwards went for a couple of days with Edgar to Yosemite National Park. I was keen to visit Yosemite, having seen photos of it, and heard its virtues extolled by the stewardess on that earlier flight from San Francisco. We camped for a day or two in the Yosemite Valley, where it was still very cold at night. Edgar by this time was a strong mountaineer, and had climbed high volcanoes in Mexico, carrying packs up to 45 kilos on the way. He wanted to climb to some high point above the Valley, but I went on a more gentle climb to Vernal Falls. The scenery is spectacular, and there's nowhere else similar. Even the vegetation near the Merced River along the bottom is beautiful. The Park Service has allowed essentially no development or traffic, which helps a lot, and at that time of year there were few hikers. I also found beautiful the many trees with wispy lichen on their branches that one sees by the roadside when approaching Yosemite.

In late June I was off again, to London and then Copenhagen. There was a new lectureship vacancy at Imperial College, so I applied there once more and got invited for an interview. That went quite well, but soon afterwards Tom Kibble told me that the job had been offered to Mike Duff. Mike had been a research council fellow at Imperial for several years, and although he had taken considerable time off at CERN, Imperial were obviously committed to him. I was really disappointed this time, as I had been a postdoc for five years now, and jobs in the UK and elsewhere were rather scarce. Since the Thatcher government had come to power in 1979 there was a freeze on university expansion, and because many academics had been appointed in the 1960s, few retirements were coming up. I got ill for a few days, perhaps through the stress and disappointment, and had to postpone my planned two-week trip to Copenhagen.

In Copenhagen there was a summer research programme on dynamical systems at Nordita. I got there a couple of days later than planned, after rebooking my flight. That had to be in business class, so I sat near the front and got a free champagne. Despite the additional cost, this cheered me up. My Mum said I should enjoy my time in Copenhagen, and not dwell on not getting the Imperial job – "Wer weiss wo's gut ist?" The programme was run mainly by Predrag Cvitanović, whom I had met previously. Like me, he had worked mainly in quantum field theory, especially on its group theoretic aspects, but had found the new ideas in dynamical systems fascinating. I knew a few others at Nordita and the neighbouring Niels Bohr Institute, especially Jan Ambjørn, Holger Nielsen and Poul Olesen.

After a day or two I was introduced to another participant, Anneli Aitta, who was a PhD student from Helsinki. She spoke a little English, although she hadn't previously travelled beyond Stockholm and Uppsala. I knew not one word of Finnish, except sauna, but she seemed to understand some things I said. I had learnt after many trips to Europe and even the US to speak English straightforwardly, without elaborate colloquialisms. At the weekend, several of us, including Anneli and I, went with one of the locals to the seaside at Tisvilde, and I think it was on the Monday following that Predrag said to me that Anneli didn't know too many people, and might like it if I suggested an evening out with her. So I did. That went well, and we had a couple more evenings out, once to Tivoli, where we took a ride on the world's oldest

and least scary roller coaster. I felt a bit queasy after that and was worried that Anneli would think I was not too healthy, but I soon recovered. I also gave a talk, and Anneli tells me that she was very impressed by this, although it was not anything very special for me. Somehow my fluency in English and enthusiasm made the mathematics sound interesting, and I had some nice pictures from my collaboration with Mike Nauenberg. Anneli also gave a talk, on her work in Helsinki on the Leidenfrost effect – the way a water droplet floats and oscillates when heated by a hot plate underneath. The various kinds of dynamical instabilities of the spherical droplet are interesting, and hard to model mathematically. A number of shape-changing modes, with different symmetries, can be excited.

Anneli had plans to spend some time abroad working towards her PhD, and had made a note of four professors in the US whose research was related to what she was doing, and which she found interesting. She had been asked to find out more about these professors and their work while in Copenhagen. Finland was keen at that time for students to have some experience of work abroad before submitting their PhD thesis. One of the professors was Guenter Ahlers, whom Anneli thought was at Bell Labs in New Jersey, but had in fact moved to Santa Barbara a few years earlier. I knew of him indirectly, because his research had been discussed by some of the visitors at the ITP, notably Pierre Hohenberg who was one of his collaborators. Anneli was very pleased to hear that Guenter was at the same university where I currently was.

The atmosphere in a warm summer in Copenhagen is quite sexually charged. Girls sunbathe topless in the parks. There was noisy lovemaking in my hotel, easily audible through open windows. Anneli, at her residence, was disturbed by noisy homosexual neighbours, and had to move to another floor. Anneli and I didn't have an intimate relationship, but by the end of the two weeks we were there, which fortunately coincided despite my delayed arrival, we were rather in love. Anneli was feeling low when we said farewell, because she thought we might not meet again. I liked her, and decided to make a real effort to see her another time, even though I had no plan to go to Finland.

Back in Santa Barbara, I visited Guenter Ahlers in the physics department and enquired whether Anneli could be a visiting student there, to progress with her PhD work. He told me that this was possible if she got funding. He was interested in the Leidenfrost phenomena, and thought that the oscillation dynamics, though complicated, could be studied in his lab.

I wrote to Anneli saying I'd like to see her again, and that there was a possibility to spend time in Santa Barbara working with Ahlers and his group. She was thrilled at the possibility to travel to California, a place she had only previously read and dreamed about. It was a big step for her to go so far from home. She wrote to Ahlers and made an application to the Academy of Finland for a studentship to support a period of research work at UCSB, and after some months got this. Anneli and I exchanged a few letters, and looked forward to meeting once more in early 1984.

During the rest of the summer, I think I visited San Diego briefly, perhaps with Tony Kennedy, and went to Sea World. Another visit was to the Huntingdon Library and the Simon Norton Museum, near Caltech. The Huntingdon has beautiful gardens, with an exceptional display of cacti, and inside is a brilliant collection of British artworks that sadly are not in the UK any more. Outstanding are Gainsborough's Blue Boy, and Lawrence's Pinkie, and there are numerous first quarto editions of Shakespeare's plays. Also, at some point, I made a trip to the East Coast, to Harvard and to the Institute for Advanced Study at Princeton, but don't remember this well. Eric and Raya came on a visit to Santa Barbara for a few days at the end of September, and enjoyed their stay. They heard about my meeting Anneli, and hoped that she would be able to

come to Santa Barbara later.

When term started at the university, and I had a bit of time to wander around the campus, I occasionally sat in on lectures in other subjects. One was on music appreciation. Another was on human behaviour and politics, in which students were told that certain behaviours which they knew to be bad and unnatural through their education were in fact determined to be so by a political or religious elite, and should be reconsidered critically. In particular, monkeys quite often engage in homosexual behaviour even while pursuing heterosexual relations, with no detriment to monkey society. In both these lectures I saw the value of the liberal education offered by US universities. In the UK we learned these kinds of things at school, at a younger age, when we were less mature, and at university our lectures were all subject-specific and specialised. In compensation, we had opportunities through university and college societies to develop our interests more broadly. I realised that in the US, the agendas of newspapers and TV were pretty conservative, but the antidote available to college students was the liberal education provided by most university professors across the country.

Another event where I spied on an activity with no analogue in the UK took place in the main hall of UCSB. Busloads of senior high school students from all over LA and surroundings arrived to hear a motivational talk by a 'leadership trainer'. His spiel galvanised the participants, and they stood up in their groups to cheer and wave banners, and boo groups from the opposite side of town. It was very tribal. Then the trainer reminded the students in the hall how they had been specially selected, because of their academic ability and ambition, and their notable contributions and commitment to their schools. They were the elite, and they were going to be the leaders of tomorrow – all of them. To me this was odd, because it seemed that here were about a thousand kids all acting the same, and difficult to tell apart. I thought that leaders were isolated individuals who stood apart from the crowd, but were influential despite this.

This is an American paradox, how to give people the impression of freedom and individuality, and yet create a homogeneous society where some things (e.g. the hamburgers, and even the many newspapers) hardly differ from coast to coast. Each shift in a hamburger joint has a 'team leader', who needs to use their initiative as well as obey instructions from the regional office. In turn, executives in the regional offices have to follow company rules. It's like the hierarchy in the army, so are the people 'free'? This paradox was encapsulated in a box of Girl Scout cookies that I bought at UCSB (my contribution to a fund-raising drive). It had on the front a close-up of a Girl Scout (a Brownie in UK language) with the caption "I'm not like anyone else" and on the back a picture of the same Girl Scout in a larger group with the caption "We have a lot in common." I think that Americans are under more overt pressure than people in other western countries to conform, yet the society manages to be more creative than others, partly because of its wealth and size, but also because of its open door to talented immigrants.

Back to research: My work on the topology in the Weinberg–Salam electroweak theory only involved the bosonic, classical fields, i.e. the gauge and Higgs fields. But the complete theory has fermions too, both leptons like the electron, and quarks. The fermions are influenced by the topologically nontrivial gauge and Higgs fields. In fact, the net number of these fermionic particles can change, due to what are called 'anomalies'. An anomaly denotes the anomalous breaking of a conservation law that naïvely would be present as a consequence of Noether's theorem. In the Weinberg–Salam theory, the naïve lepton and baryon number conservation laws are anomalously broken. Lepton and baryon number can therefore potentially change in time-varying background fields (although this has never actually been seen to happen), and they would do so continuously, so these 'charges' can have fractional values. 't Hooft had worked out the

details of the extent of the breaking, and how the fermionic charges change in a related theory, and I could use these results.

I felt I needed to understand anomalies better, and in a more fundamental way. The existing calculations by Fujikawa, Jackiw and others relied on subtle, rather formal manipulations of path integrals, or on the difficulty of regularising the infinities in certain special Feynman diagrams. Topology didn't seem relevant to the calculations, but strangely, the final formulae had a topological interpretation. Jackiw was one of those who considered the consequences of this, and with Rebbi found a model field theory where the fermionic charge in a soliton background was one half. I was influenced more by Singer's picture of the topology of the true configuration space of a gauge theory, and wanted to understand the effect of this on fermions. At first, rather than study an anomaly in a field theory, with its need for regulators of formally infinite quantities like the baryon number of a Dirac sea of quarks, I found a nice quantum mechanical example, without infinities, where a conservation law that is naïvely present is broken by topologically interesting effects. This example is a charged particle moving on a torus in a background magnetic field.

For a particle moving on either a plane or torus, there is a conserved momentum vector, because of the translation invariance of the background geometry. If a uniform magnetic field is present, then for a particle in a plane there is still translation invariance, and therefore a conserved momentum vector, but it is an altered quantity, and the two components of the quantum mechanical momentum operator no longer commute. The quantum states in this situation are known as Landau level states. On a torus, things are more weird. Here, there is a topological, Dirac constraint on the strength of a uniform magnetic field, but even if this is satisfied, the momentum of quantum states is no longer conserved. In fact, momentum cannot even be defined consistently. This is because of the nontrivial fibre bundle structure associated with the magnetic field. One consequence is that states with definite energy are always mixtures of what would naïvely appear to be states with definite momentum. This is forced by the periodicity conditions on the wavefunction as one goes round the torus, in either direction. An alternative way to understand the result is to realise that the magnetic field is not really translation invariant. There is a gauge potential that is more fundamental than the magnetic field, whose holonomy along each circular loop around the torus varies with the position of the loop. There are a number of circles in both directions along which the holonomy is unity, and the intersection points of these circles are special, and break the translational symmetry. This explains the lack of a conserved momentum. There is an anomaly.

I wrote a short paper on this phenomenon in October '83, but it was rather a small thing that was probably already known to experts, so I didn't try to publish it. The paper also outlined how the system of a particle on a torus in a magnetic field has some analogy with a field theory of fermions coupled to Yang–Mills fields, and I promised a further paper on the Schwinger model, a tractable special case, where fermions are coupled to an electromagnetic field in one space dimension. This is what I worked on until well into the following year.

That same October, there was a workshop at Oregon State University in Corvallis, entitled Asymptotic Behaviour of Mass and Spacetime Geometry, and I was tempted into going even though not working on gravity. Speakers included Roger Penrose and Malcolm Perry. After the workshop I took two days off, along with someone I had known in England, to explore the volcanic Oregon mountains. We saw the Three Sisters peaks, and I particularly remember driving around a very fresh-looking area of contorted black lava, with little growing on it. Just a few small juniper bushes or baby conifers had taken hold in cracks. I was surprised to learn that the lava flow was not recent, but over a thousand years old. My flight back was memorable, as it

involved six take-offs and landings, more than I was used to, with a change at San Francisco.

That year, just before Christmas, I flew to San Francisco to meet my Mum, who arrived from London. We stayed at the Beresford Hotel, explored the city and Berkeley, and took a minibus trip to Muir Woods with its impressively tall, coastal redwood trees. We then returned by Greyhound bus to Santa Barbara for Christmas itself, and were invited by my quartet friends Mary and Bump to the communal lunch at Valle Verde. My Mum enjoyed meeting them and their family, and stayed in contact by letter for some years, long after I had gone back to live in the UK. During the following days, we installed and tried out an oven at my apartment.

I have some special memories of San Francisco from various visits. Most obviously there are the steep hills, the trams, and the cool fog that rolls in under the Golden Gate bridge and spreads over the city. I also recall an old-fashioned exhibit by the prune industry at the Embarcadero, the old arrivals and departures building for ocean-going liners that was by now hardly used. It displayed numerous posters for Californian prunes, and the very many ways they were packaged and sold, either loose, in boxes, in candy, as juice, or in tins. This creative marketing has more recently been applied to cranberries. Finally, I remember a visit to the Exploratorium. This was a pioneering, imaginative science exhibition, with lots of hands-on displays. Unlike the Science Museum in London, or the Deutsches Museum in Munich at the time I had been there, it concentrated on science rather than on technology. I particularly recall a display of electrical discharges through metallic vapours, whose bright spectral lines could be seen by looking through a diffraction grating. Additionally, and this was the exciting bit, there was a strong horseshoe magnet hanging nearby that one could bring up close. The spectral lines split into two, three or more lines, depending on the vapour, and the split lines moved apart as the magnet came closer. I knew from my undergraduate physics that this was the quantum Zeeman effect, and felt for the first time that I was directly influencing the quantum world by moving the magnet. Of course, we interact all the time with the quantum world, through breathing, eating and digesting, flicking light switches, and in all kinds of other ways, but not so consciously.

From the start of 1984, and with my ITP postdoc ending in September, I needed to think about finding a new job. I was now looking for junior faculty positions rather than yet another postdoc. My Mum advised me that I should not continue for ever as a 'wandering minstrel'. There was a possible position at the University of Michigan, but I hadn't much liked being in Ann Arbor, so that wasn't promising. Stony Brook was somewhere I had good contacts, but I don't think they had a vacancy at that time. Another possibility that I pursued was in the mathematics department at UCSB, where I had met some of the faculty. But most promising was an unsolicited phone call I received from Alan Chodos at Yale. There was the possibility there of a 3-year fellowship that could be renewed and turn into a physics faculty position. I expressed interest, and asked if he could find out more about the salary and so on. Should I apply formally? I expected some response reasonably quickly and perhaps an invitation to give a talk and meet people, as I only knew Yale from one brief visit. I heard nothing and after about six weeks phoned Alan. His response was that I hadn't really been keen enough. Apparently, jobs like this have to be created after arguments within the faculty, and they want the candidate to commit themselves pretty firmly first.

It's different in the US with superstars. They solicit a few offers from lesser universities, requiring the faculty there to make quite a big and ultimately wasted effort. Then when the candidate has a few offers in the bag, and the rumour mill has been active, the big hitters like Harvard, Princeton and Stanford may make an offer. All this takes quite a long time. I prefer the British system, where vacancies arise and are advertised. Then, fairly quickly, many people

apply, shortlisted candidates are interviewed and an offer made.

My fallback was to stay longer at ITP, which turned out to be possible, and to hope for some vacancy in the UK. There had been very few of these because of the jobs freeze, but the government was relenting, and came up with a 'New Blood' lectureship scheme. At about the same time, the Royal Society launched its own scheme of long-term University Research Fellowships for younger scientists, where the host university was expected to make some commitment to offer a permanent lectureship at the end.

The most important thing at this time was Anneli's arrival, on 5th February '84. She flew from Helsinki to Los Angeles, with a stopover for US immigration at Seattle. I drove to the airport to meet her, and we embraced. On the way back to Santa Barbara we stopped at a diner for a meal, and I asked whether we should find somewhere for her to stay temporarily or whether she liked to stay at my apartment. She said that, after our expressions of love in our letters, she wouldn't have come such a long way without wanting to live with me. I too, wanted her to come and live with me. So, informally, this was the moment that we got married. We soon got back to my place in Via Lucero, and did what newly married couples usually do. That was great for us both, because we were both hungry for some intimate relationship. It was useful that I had my comfortable queen-sized bed, and I soon got used to being usually on the left hand side, near the window.

Next day there were some practical matters to deal with, to register Anneli as a visiting student at UCSB, and introduce her to Guenter Ahlers and the physics department. After a short time, she started to plan an experiment in consultation with Ahlers. Anneli had very recently learned to drive in Finland, but she didn't need a separate car, and most of the time we went to and from the university together.

I had done some shopping for us and cooked one of my usual dinners, a spaghetti bolognese, but she wasn't too keen on the nutritional value of this, and we had to start shopping for things we both liked. Apart from going to the nearby supermarket Vons, my favourite, we went regularly to the German Bakery closer to downtown for their continental bread and cakes. We also ate out, once going to the Skandibuffet, which I thought would make Anneli feel closer to home. A place we didn't patronise often, but liked, was the Jewish-style restaurant Einstein. Anneli thought their chicken was the best she'd ever had.

I had an invitation to visit the University of British Columbia (UBC) in Vancouver, in late February. By then, Ian Affleck had settled at UBC, and I also knew Gordon Semenoff there. I left Anneli to fend for herself in Santa Barbara, which she did well, using the bus to get about. Vancouver is a beautiful city to visit, and attractive to Canadians because of its mild climate. More recently it's become very expensive. I gave a seminar, and also visited the museum on the UBC campus of the art of the Haida and other peoples native to the north-west Pacific coast. This combined historic totem poles from the 19th century with modern art having a similar inspiration. Downtown, near my hotel, I went to a concert where Yehudi Menuhin played. It wasn't too well attended on a damp February evening, and I was surprised that Menuhin still continued touring like this.

Back in Santa Barbara, I was developing my own understanding of the Schwinger model. This had been essentially solved by Julian Schwinger more than two decades earlier, in a paper just a few pages long. He studied the exact Green's functions of the field theory, and went on to show that the physical particle states were not the massless fermions that one starts with. Instead, the interaction of the fermions with the electromagnetic field in one dimension creates a free scalar boson, and there is a simple formula for the boson's mass. All this is remarkable,

but also mysterious. It wasn't clear to me at all, even after reading the paper more than once, what was going on.

To make contact with my work on the particle on a torus, I found it helpful to study the Schwinger model on a circle. Fermions moving on a circle have a discrete Dirac energy spectrum, which is convenient in calculations. A background gauge potential shifts this spectrum up or down, and in opposite directions for left- and right-handed fermions. A filled Dirac sea can lose one left-handed fermion, and gain one right-handed fermion as the gauge potential is turned up from zero. But when the gauge potential is turned up enough, it becomes equivalent to being zero again, by a gauge transformation. By this stage, however, the fermionic state has changed, and has higher energy. The gauge potential by itself is a circular, not linear, variable because of such gauge transformations, but because of the way it couples to the fermions, this circle unwinds, and the difference between the number of left- and right-handed fermions (the axial charge) is not well-defined in the vacuum state, or in any other state. There is therefore an axial charge anomaly, and the mathematics of the spatial circle and the circle of gauge potentials is very close to my earlier model of an anomaly for a particle on a torus.

This explained one aspect of the Schwinger model, but to go further I had to consider the Hamiltonian in detail, evaluate the energy of the Dirac sea as the gauge potential varied, and also consider the energetics of particle-hole excitations. All this was interesting, and here I could use some previously-introduced bosonisation ideas that related fermion bilinear operators to bosonic, scalar field operators. It was my treatment of the circular gauge field dynamics that was novel. At the end, I reproduced Schwinger's result that the whole model can be re-expressed as the dynamics of a free bosonic particle, and the mass of the boson is determined by the anomaly equation. I wrote this up very carefully, aiming to be as clear as possible. The paper has three appendices, one being about the particle on a torus, as I had not published my earlier note on this, and one on the fractional charge of solitons. The paper was appreciated and got lots of citations, even though it has essentially no novel result. I think it greatly clarified the Schwinger model, viewed from the Hamiltonian perspective.

Incidentally, some months before this, Roman Jackiw visited ITP. When he was in Southern California he always tried to have lunch with Julian Schwinger in Westwood, close to UCLA. Schwinger was on the faculty of UCLA but quite reclusive, and could only be persuaded to meet to discuss physics over lunch, rather than in his office. Jackiw often took along his more junior associates, and I went to one of these 'lunches with Julian'. Schwinger was the winner of a Nobel Prize, along with Feynman and Tomonaga, but he was somehow still bitter that his contributions to field theory were under-recognised, and his work overshadowed by that of Feynman. The main reason was that the methods he expounded in his magnum opus on quantum field theory were less intuitive than Feynman's diagrams. At the lunch, we talked a little about his work on monopoles, and on electron-positron pair creation, but I don't think he had followed the more recent developments. I hadn't yet finished my work on the Schwinger model, but I doubt that he would have been impressed by it, because he had solved the model his own way much earlier.

Simultaneously with working on the Schwinger model, I collaborated with Frans Klinkhamer, who had started a one-year postdoc at the ITP in January '84. Frans was a rather outspoken Dutchman, who didn't mince his words. He knew about my paper on the topology of the Weinberg–Salam electroweak theory, and told me that no-one would take seriously its claim about the existence of a static, unstable solution unless I actually constructed the solution. He agreed to join me in doing that.

I explained the form the fields should have. While the gauge group is purely SU(2), the fields

are spherically symmetric in a certain sense, and the static field equations reduce to coupled radial ODEs. We discovered that these equations had in fact been studied earlier, by Dashen, Hasslacher and Neveu (DHN), and more recently by John Boguta at Berkeley. Both groups had in mind some application to strong interaction physics, whereas for the electroweak application the length and energy scales were different. We could nevertheless use the existing solutions. This looked helpful, because neither Frans nor I had much experience of numerics. DHN had not presented their solution in much detail, but had given estimates of its properties. Boguta gave more details, but we discovered that his work was not very accurate in the way the boundary conditions were imposed. We invited him to Santa Barbara, and he agreed in principle to calculate his solution more carefully, but after a month or two there was no progress. We managed to find an approximate solution ourselves, using a variational approach, and at some point Frans bought a fancy scientific pocket calculator that could solve differential equations, and proceeded to find a more accurate solution. There is one dimensionless parameter to vary, the ratio of the Higgs boson mass to the W and Z gauge boson masses (the W and Z masses are the same in the pure SU(2) theory), and we could find how the energy of our new, static solution depended on this parameter, which hadn't been done before. The energy ranges between 8 TeV and 14 TeV as the Higgs mass increases.

There were several aspects of our work that were distinct from what had been done earlier. We knew that the solution was unstable, and we knew that it occurred in the electroweak sector of the Standard Model. The dimensionless parameter had a definite physical value related to the Higgs boson mass, although that particle had yet to be discovered and its mass was not known. (Now that the Higgs boson mass is known to be 125 GeV, the energy of our solution can be estimated to be about 9 TeV.) We also knew that there was an additional U(1) gauge field to consider. We couldn't find the exact solution that included this field, because it only has axial symmetry, but we could investigate the U(1) field perturbatively, while keeping the SU(2) gauge field and Higgs field spherically symmetric. The dominant effect is that the U(1) field has a dipole structure relative to some axis, and this gives the solution a magnetic dipole moment with a strength proportional to the U(1) coupling strength. This coupling strength is known, because it is related to the Weinberg mixing angle that accounts for the mass difference between the Z and W bosons, and it is fairly small. Finally, we had some understanding of the relationship between the solution and fermions. The solution has half a unit of baryonic and leptonic charge for each Standard Model generation of fermions.

Next, we thought it important to give this object a name. Frans and I discussed this, and Anneli was involved too. The solution is like a soliton, but unstable. Anneli knew from her physics studies the relevant Finnish word 'labiili', related to the rather rarely used English word 'labile', so Frans proposed the word 'labilum' for our solution, or possibly 'labilon'. I wasn't keen on this mixture of Latin and Greek parts. Most particles, including protons and electrons, have names based on Greek, so I went to the UCSB library and found that the ancient Greek word meaning unstable, or ready to fall, is 'sphaleros' ($\sigma\phi\alpha\lambda\epsilon\rho o\varsigma$) and proposed for our solution the word 'sphaleron', to be pronounced with a short 'a'. Frans agreed, and this was introduced in our paper. It has stuck, and there is by now a Wikipedia entry on sphalerons. I was a bit annoyed that some US physicists give the word a long 'a' sound, but most use a short 'a'.

Our paper was completed in April, and during the summer there was a workshop at Berkeley on Electroweak Symmetry Breaking, where we had a chance to present a summary of our work on the sphaleron, and contribute to the proceedings. The sphaleron is something that could be produced, briefly, in a high energy particle collision, though it was argued by most people that

the probability of this happening would be negligibly small. The most striking signal would be its decay into multiple Z, W and Higgs bosons. A more important role for the sphaleron is probably in the high temperature conditions of the early universe. This was noted and investigated in detail by various physicists, including Rubakov, Shaposhnikov, McLerran and their collaborators. Here, the high density of particles and energy favour sphaleron production and decay. Fermion numbers change as the fields oscillate backwards and forwards through the sphaleron barrier, and there is a slight directional asymmetry in this due to CP violation. This affects the net baryon number and the net lepton number that remain at the time when the temperature is lower, and sphaleron processes are effectively turned off. The sphaleron energy sets the scale for this freeze out. It is currently hoped that by combining knowledge about the sphaleron with knowledge of various possible sources of CP violation, one can understand the matter-antimatter asymmetry of the universe. The ultimate goal is to understand the ratio of protons to photons in the cosmos, which appears to be about 10^{-9}. Big Bang cosmology has not yet explained this ratio.

Frans Klinkhamer invited Anneli and me one evening to dinner at his apartment. He prepared a fine halibut of generous size, baked in milk sauce. This actually wasn't a big deal for him. He came from a family of restaurateurs, and an uncle on his French mother's side was a chef at La Tour d'Argent, the famous restaurant in Paris. Frans wasn't keen on haute cuisine himself, and advised people that it was preferable to have a diet rich in fresh vegetables. Frans, Anneli and I had a meal at a Thai restaurant on another occasion. His soup contained a small, bright green object that might have been a piece of green onion, but was actually a very hot chili. Frans chewed on this and nearly had a heart attack. He survived, but was very angry with the staff for not warning him. Frans also maintained an interest in French history, and read scholarly books and journals on the medieval and Renaissance periods. He was surprised that Anneli knew little about France and its language. He imagined that all Europeans knew more, but in Finland the focus was on Sweden and the English-speaking world, although before the war there had been quite close relations between Finland and Germany. Most educated, older Finns spoke some German, and this was useful for me once when I was there and couldn't converse effectively in Finnish. Though he was interested in physics, Frans thought that concentrating on this alone was too narrow a life. He was certainly critical of some of the more speculative ideas in physics, and didn't trouble to investigate them in detail.

Anneli and I did many things together in Santa Barbara, and some separately. We went together to a concert by the LA Philharmonic conducted by Carlo Maria Giulini at the downtown Santa Barbara theatre that is designed to look like a Spanish village square. I continued my quartet playing – my quartet partners were most welcoming to Anneli, so long as she gave me some evenings off for music! At Easter time we went on a car trip to Cambria and Hearst Castle. The road further north along the Californian coast was closed at this time because of landslips. Hearst Castle has some beautiful parts, including the Neptune pool surrounded by columns and statues, but much of it is a rather overwhelming hotch-potch of items acquired from Europe and North Africa. We visited LA a few times, and on one of these occasions met David and Etain Antrich at their hotel while they were also in LA. They were the first of my friends and relations to meet Anneli, although Eric and Raya also wrote to say that they looked forward to meeting her. I think we also went on this occasion to Caltech, to the Huntingdon Museum and Library (for me this was the second time), and to see the ocean liner Queen Mary in Long Beach. On the way to Long Beach we ventured into south LA, but that got scary, and we instead drove further west and past dozens of nodding oil derricks. Seeing the Queen Mary was a thrill for me because I had made a model of the ship when a teenager, and could now see the parts full-size,

including fiddly bits like the lifeboats and their supports, and the ventilation outlet grills.

Soon after her arrival, Anneli started work on the Leidenfrost effect. She set up a device to measure the frequencies of oscillation of a droplet, but after a short time this broke down and couldn't be repaired in a hurry. Fortunately, just then Ingo Rehberg, a member of Ahlers' group, gave a short talk about a recent paper on the Taylor–Couette system. This system consists of viscous fluid between two coaxial cylinders, with one or the other rotating. Above a small rotation frequency, the purely azimuthal Couette flow is unstable, and the flow consists of vortices in cells, equally spaced along the cylinder. For a finite cylinder length there are corrections to this picture, because of the modified flow near the endplates. Of particular interest at the time was the flow in a short cylinder, of comparable length and radius. Such a cylinder has room for just two vortices. The recent research had been about the bifurcations that can occur between a flow where the two vortices are the same size and there is reflection symmetry, and a flow with broken symmetry where one vortex is larger than the other. Ideally, the symmetry breaking can go either way. This short system was being studied experimentally, also theoretically by solving the full Navier–Stokes equations for a viscous fluid in this geometry, and finally by a simple modelling of the bifurcation behaviour with one order parameter quantifying the symmetry breaking. The results were not very precise, and Ingo thought that a more accurate experiment could be done in Ahlers' lab. Ahlers and his colleague David Cannell agreed, and this became Anneli's project. She had a system made with a motor to control the rotation speed of the inner cylinder, and she could adjust the length of the cylinder over a modest range. The transparent fluid inside was charged with reflecting tracer particles so that she could observe the flow and measure its velocity.

The order parameter used to quantify the symmetry breaking was the integral over the length of the cylinder of the component of velocity parallel to the cylinder axis. This was more robust than just the velocity at the mid-point, or an estimate of the position of the separation surface between the two vortex cells. Anneli could plot this order parameter as a function of the two control parameters, the cylinder rotation speed, and cylinder length. The behaviour was modelled by Landau theory, where the order parameter obeys a first order time-evolution equation involving the derivative of a potential depending on the order parameter. The normal form of the potential is sixth-order and symmetric, because of the reflection symmetry, and the two control parameters are encoded in the coefficients of the quadratic and quartic terms. This model usefully describes the equilibria of the flow; they correspond to the minima of the potential, where its derivative is zero. The number of minima can be three, two or one. In the case that there are three, the values of the potential at the minima determine whether the equilibria are locally or globally stable. As the coefficients are varied, there are bifurcations in the number and relative stability of the minima.

For this model to work accurately, there needs to be a careful understanding of exactly the relationship between the control parameters and the coefficients of the potential. Also, to take account of the imperfections of the apparatus that break the reflection symmetry, it is important to add a linear, symmetry-breaking term to the potential with a small coefficient.

Anneli observed the various equilibria predicted by this kind of model. There are continuous symmetry-breaking bifurcations, and discontinuous bifurcations with hysteresis, analogous to second-order and first-order phase transitions. At the point where the coefficients of both the quadratic and quartic terms vanish, the three types of bifurcation curve merge, and this is a tricritical point. The model predicts the magnitude of the order parameter in a sizable region of the plane of control parameters, around the tricritical point. Anneli showed that here, the

model works extremely well, confirming the existence of the tricritical point. This was quite an achievement, because no other experimental dynamical system had exhibited this so clearly, and it is not a foregone conclusion mathematically that the infinite-dimensional Navier–Stokes equations can be reduced to a one-dimensional dynamics for an order parameter. A further test was to look at the time dependence of the order parameter as the system relaxed to its equilibria. Was it sufficient to simply have an equation where the time derivative of the order parameter is (minus) the gradient of the potential?

Anneli worked hard at this over several months, essentially the whole time she was in Santa Barbara. She found this the most exciting and stimulating research project she had ever worked on, and went into the lab at weekends as well as during the week. Occasionally I saw the apparatus. Several plans we had for relaxing activities, and trips around California, were put on hold. For example, I had proposed a tour to show Anneli the Yosemite Valley, and visit other parts of Yosemite National Park and the Sequioia and Kings Canyon National Parks where I hadn't been. But we never did this.

She completed a paper with Ahlers and Cannell towards the end of the year, presenting the basic observations of the tricritical phenomena. This was published in Physical Review Letters. She later completed three more papers on the details of the Landau potential modelling and on her observations of the time dependence of the order parameter. But those further papers involved considerably more analysis and were only completed after Anneli had departed from Santa Barbara.

By this time, there had been some change in personnel at the ITP. Bob Schrieffer had become the director, and Jim Langer had joined too, becoming director himself some years later. I hoped that Schrieffer could give some lectures on the BCS theory of superconductivity, which I had not comprehended during Part II Physics in Cambridge, but he was not willing to go over this early, Nobel Prize winning research of his. He focussed more on leading the institute, and arranged at least one cocktail party at his home. His wife was Danish, and I learned that from the time of Niels Bohr, many (male) physicists had met their wives in Copenhagen. The only difference for me was that Anneli was Finnish and not Danish.

In the middle of summer '84, the problem of my future job was resolved. There was a vacant lectureship in Durham (UK) for which I applied. The Durham faculty members were well acquainted with me, and I thought the Department of Mathematical Sciences would be a good place to work. At the interview I was successful, and immediately afterwards they offered me the job. David Fairlie in particular was keen for me to accept and arranged a lunch outing to the pub. I was of course pleased to have got the offer, but had some doubts about living in Durham, and whether Anneli would have a chance to pursue her research there. They had nothing similar to Ahlers' lab. From Durham I went to St John's College in Cambridge for a visit. Here Peter Goddard told me that St John's were willing to set up a position similar to a Royal Society University Research Fellowship, for five years with the possibility of extension for three and if necessary two more years. It wouldn't be a College Fellowship, but would have some of a Fellow's privileges and also a limited requirement to give supervisions. Peter thought it very likely that within ten years I could get a lectureship at Cambridge if my research continued to progress well, as it had done recently. This was very tempting, and the tasty dinner in College at High Table was a bonus. So I declined Durham, and accepted the St John's offer, which was confirmed shortly afterwards. I thought that Anneli could find a group to work with in Cambridge, because there was fluids research in DAMTP, and also in the Engineering and Chemical Engineering departments at the time.

At the interview in Durham, I had been asked to suggest a project suitable for a starting PhD student that I might supervise. I proposed an extension of my Schwinger model work to study the interaction of fermions with nonabelian gauge fields in one dimension, i.e. one-dimensional QCD, and its possible anomalies. This is an interesting problem, because the gauge field dynamics alone is a finite-dimensional quantum mechanics on a Lie group, but the problem of coupling this to fermions remains open today, at least from the Hamiltonian perspective that I had applied to the Schwinger model.

Something Anneli and I looked forward to was the Olympic Games, taking place in LA in August 1984. We thought about going, and heard through a Finnish organisation in Southern California that they had tickets available that had been earmarked for supporters travelling from Finland. Many of these were unsold and we obtained two pairs of tickets for the athletics in the main stadium. We enjoyed this so much that we bought another pair on the street outside the stadium, so in the end we went to three sessions on 5th and 6th August – two evening sessions with important events, and one morning session with qualifying events. I described earlier some details of what we saw, in the section on Sport. The colour scheme used to decorate the stadium, the brochures and the tickets was really nice. It didn't use primary colours much, but instead bright splashes of gold, orange, cyan, and mauve.

Later in August there was a workshop on Solvable Models at ITP. Here I met Nicholas Ercolani, who with his collaborator Sinha had developed some new insight into monopoles and their spectral curves. This reawakened my interest in monopoles, and later led to some research by me and my graduate students when I was back in Cambridge.

Despite Anneli being busy with her experiments, we did find time for a few other trips. Nearly two months after the Olympics we went to Riverside, 50 miles east of downtown LA and with its own University of California campus, to see some Finnish friends of Anneli. It was a terribly warm afternoon, and got hotter into the evening, making driving difficult. My car didn't have air conditioning, only a fan, and I often had trouble driving on hot, sunny afternoons. But such a stifling evening was unusual, and the weather broke the next day. In November, Anneli and I flew to the East Coast, where she had not been before. We first went to Stony Brook, but there I was ill for a few days and she and Fred Goldhaber had to nurse me back to health. We went next to New York, and stayed at Rockefeller University where I gave a talk. We also visited some of the tourist sights, including the Museum Of Modern Art and the viewing deck at the top of the World Trade Center, and enjoyed the musical Chorus Line. After this, we made a brief visit to Boston and Harvard, and were invited by Sidney Coleman to his home. Late in the year, we also made it to San Francisco by car, and returned via Monterey and the Big Sur coast road, which had now reopened.

At the end of December my postdoc at Santa Barbara finished. Those three and a bit years had been the most productive and varied of my research career. We sold a few things at a Garage Sale at Via Lucero, and shipped some furniture and smaller items to England. I sold my car to one of the new ITP secretaries. Anneli's original funding from Finland had been for six months, but Ahlers found a further six months of funding, so Anneli arranged to stay in Santa Barbara till the end of January '85. She moved in with a Swiss family for a month, while I returned to England. Early that January I moved into 'married accommodation' in Cambridge, a flat at 23 Cockcroft Place in a complex that St John's College had helped develop.

At the start of February, Anneli returned to her flat in Kivenlahti in the suburbs of Helsinki, and a few days later I joined her there, and experienced the cold Finnish winter for the first time. From Helsinki we made a train trip to Utajärvi, far to the north, for me to meet her

parents. Then in mid-February we travelled together to Cambridge. Thus started a new life for us, focussed on Cambridge, where she had not been before, and Finland, where I had not been before. We were both 33, about half our ages now. My research changed direction, though not dramatically, and so did hers. The story will I hope be continued in Part II of this memoir – A Career in Cambridge.

Acknowledgements

I am grateful to Anneli for carefully reading through this memoir and suggesting several corrections and improvements. Colin Vout and my cousin Anthony also read through parts of this, and I thank them for their comments and corrections, many of which have been incorporated. I also thank Janet Balment (née Antrich) for comments and corrections, especially about her father David's background.