



# Autobiography

An autobiography published a few years ago\* records many of the salient events in my life which led me to become a scientist and my Nobel Lecture covers the intellectual background and consequences of the research work for which the Prize has been awarded.

In this somewhat compressed version, I start with my birth on the 13th January 1927 in a small town, Germiston, in South Africa. My parents were Jewish immigrants from Eastern Europe; my father came to South Africa from Lithuania in 1910, my mother, from Latvia, in 1922. My father was a shoe repairer and our first home was in some rooms at the back of his shop. He never learnt to read or write but, in addition to English, Yiddish and Russian, he learnt to speak Afrikaans and Zulu. I learnt to read at an early age, and a customer of my father, Miss Walkinshaw, persuaded my father to allow me to go at the age of five without charge to her kindergarten. I completed the first three years of primary school in one year and was admitted to the local school the age of six directly into the fourth year, some two years younger than all my contemporaries. After 4 years in primary school, I went to Germiston High School where I matriculated in December 1941, just before turning 15.

During this time I discovered the Public Library in Germiston, one of the many libraries set up all over the world with funding from Andrew Carnegie's endowment. It was here that I found a source of knowledge and the means to acquire it by reading, a habit of learning which I still follow to this day. I also became interested in chemistry and gradually accumulated enough test tubes and other glassware to do chemical experiments, using small quantities of chemicals purchased from a pharmacy supply house. I soon graduated to biochemistry and tried to discover what gave flowers their distinctive colours. I made the (to me) astounding discovery that the pigments I extracted changed their colours when I changed the pH of the solution.

I was fortunate in that the Town Council of Germiston gave me a bursary of 60 pounds per year that allowed me to go to the University of the Witwatersrand in Johannesburg to study medicine and, in 1942, at the age of fifteen, I began the course studying Physics, Chemistry, Botany and Zoology. I lived at home and I cycled every morning to the railway station to travel by train to Johannesburg followed by a walk to the University, carrying sandwiches for my lunch and returning in the evening the same way. My Uncle Harry had given me a microscope as a present which allowed me to continue my personal explorations of the living world. This was the beginning of my contacts with the real science. In my second year, after moving to the Medical School, I began the courses of Anatomy and Physiology. I had begun to see that I was interested in cells and their functions.

It was noted then that I would be too young to qualify for the practice of medicine at the conclusion of my six year medical course and I was allowed to deviate and spend one year in a Medical B.Sc. course in Anatomy and Physiology. This was heaven. The small group of about a dozen students inhabited a small room in the Anatomy Department where we each had a small laboratory bench. We learnt how to do research by working in small groups with more advanced researchers in the Department. I learnt physical chemistry with Joel Mandelstam (later Professor at Oxford University), microscopy with Alfred Oettle, and neurology with Harold Daitz who became a close friend and who died at a very early age in Oxford. Raymond Dart and Robert Broom taught me anthropology and paleontology, and the man who inspired all this activity was Joseph Gillman, a histologist who had created a centre of research in that isolated place. He invited me (I needed no persuasion) to continue with research and I stayed on for two more years doing an Honours degree and then an M.Sc., supporting myself by working part-time as a laboratory technician. I read many books and taught myself many subjects during this period, learnt how to build equipment and how to do experiments, and had many arguments and discussions with Joe Gillman. I also began to publish papers. My scientific bibliography begins in 1945 with a paper published with Joe Gillman and his brother, Teddy, but my first paper as sole author appeared in 1946. This paper dealt with a histochemical reaction, and it was the first of several which reflected my growing interest in a subject which I later called cell physiology. My M.Sc. thesis was in the field of cytogenetics, another self-taught subject, and this was the beginning of my research in genetics. This background was to serve me well in later years when I became a molecular biologist.

In 1946, W. Le Gros Clark visited South Africa and invited me to come to his Department of Anatomy in Oxford, but I was advised by everybody to finish my medical course, because it was believed then that a medical qualification would be essential for a research position later. I did go back to my medical studies but I continued working in the Department of Anatomy and moved to the Department of Physiology when Joe Gillman became Professor there. I was not a good medical student and had an erratic career, brilliant in some subjects, absolutely dismal in others. In my final year I failed Medicine, scraped through Surgery but got a First Class in the third subject, Obstetrics and Gynecology. I had to go back and repeat Medicine and Surgery and six months later, in July 1951, I finally received the degrees of MB BCh. I had already decided that I would do research and that I needed to go abroad. CH Waddington, who had earlier visited South Africa had advised me to go to Cambridge. I applied to the Department of Biochemistry and never even received a reply. I had decided that the subject I was interested in was molecular biology which, of course, did not exist at the time, and when I was awarded a scholarship by the Commission for the Royal Exhibition of 1851, H. Raikes, head of the University of Witwatersrand, who was originally an Oxford trained chemist, advised me to write to [C.N. Hinshelwood](#), the Professor of Physical Chemistry at Oxford University, who had interests in the applications of physical chemistry to biology. That sounded closer to what I wanted to do. Hinshelwood had written a book called the "Chemical Kinetics of the Bacterial Cell" which I read and thought was in the direction I wanted to go. He accepted me and suggested I work on bacteriophage

resistance in bacteria. I immediately began to read about bacterial viruses and in October 1952 I arrived in Oxford to do a Ph.D. in the Physical Chemistry Laboratory.

There was still food rationing in England and life was difficult all through my 2 year stay in Oxford. In addition, I and the others were outsiders three times over; we were scientists, we were research students and we were colonials. Many of my friends in Oxford shared these stigmata and the only compensation was the opportunity to join Halifax House and lunch there. This was where I met Jack Dunitz, a crystallographer, and through him Leslie Orgel, a theoretical chemist, both of whom have remained lifelong friends and colleagues.

We had many discussions on DNA, for I had come to Oxford with two half ideas both of which were more than half wrong. One was a way of working out the structure of DNA using dyes and the other was how nucleic acids could participate in the synthesis of proteins. I can remember in November 1952 Jack telling me about two fellows in Cambridge who were going to solve the structure of DNA. When in April 1953, Jack told us that these two fellows in Cambridge, [Francis Crick and Jim Watson](#), had indeed solved the structure of DNA, Jack, Leslie and I drove to Cambridge on a day between the 16 and 18th April to see the model.

This was the watershed in my scientific life. The moment I saw the model and heard about the complementing base pairs I realized that it was the key to understanding all the problems in biology we had found intractable - it was the birth of molecular biology. It was a revelation reinforced by conversing with Jim Watson at greater length during a walk we took together, when I realized that working with bacteriophage had put me on the right road to enter this exciting new field, even though what I was doing was trivial.

My wife and I were married in London in December 1952 and she was also engaged in doing a Ph.D. in Psychology in London. She was allowed to move to Oxford and, until June 1954, we lived in a flat in Woodstock Road working on our theses, had a child in addition to my stepson, Jonathan, dreaming all the time of food and the warm climate of our native South Africa.

An opportunity to visit the United States came about when Dr. M. Demerec, the director of the Carnegie Institution Laboratory at Cold Spring Harbor visited Sir Cyril Hinshelwood. It was Demerec who invited me to Cold Spring Harbor and who helped me to obtain a Carnegie Corporation Travelling Fellowship which enabled me at the end of the summer in Cold Spring Harbor to make a trip across the United States to visit other laboratories. I drove across America with Jim Watson to Cal Tech and I then had a period of research at the Virus Laboratory in Berkeley, working with Gunther Stent. I also had made a short visit to Washington, D.C.

My visit to America was important because through it I met many of the then important workers of the Phage School, Seymour Benzer, who became a lifelong colleague and friend, [Max Delbrück](#), the founder of the phage school, [Salvador Luria](#) and many others

who were destined to play important roles in the development of the new science of molecular biology.

I had also met Francis Crick again in Woods Hole and Cold Spring Harbor and visited him in Cambridge in December 1954 on my way back to South Africa. At this meeting we discussed how I might join him in the future and I returned to South Africa at the end of 1954 as I was committed to do so by the terms of the Carnegie Corporation Fellowship.

I set up a laboratory in the Department of Physiology in the Medical School in South Africa and begin to try to find a bacteriophage system which we might use to solve the genetic code. I also continued to work on some theoretical aspects of the genetic code and during this period was able to prove the impossibility of all overlapping triplet codes which was circulated in an RNA Tie Club note and later communicated to the Proceedings of the National Academy of Sciences by George Gamow. Francis worked hard to get me an appointment at the Medical Research Council Unit in Cambridge and in December 1956 we left South Africa for a new career in England. I did all of my work on molecular genetics in the Cavendish Unit and its successor, the MRC Laboratory of Molecular Biology, where the work on *C. elegans* was initiated and developed. I spent 20 years sharing an office with Francis Crick and many new and exciting ideas (both right and wrong) were generated from our conversations. The centre point of our interests had begun to diverge and whereas we were both interested in the nervous system, I was far more interested in finding a simple experimental system which might tell me how brains were constructed, whereas Francis wanted to know about the complex activities of higher nervous systems. He left Cambridge in 1976 to join the Salk Institute where he pursued an entirely new career in neuroscience.

In 1977, I was appointed proleptic Director of the MRC Laboratory to succeed [Max Perutz](#) on his retirement in 1979. However, I immediately took over the financial management of the laboratory and spent several years in trying to get the finances on a proper basis. These were times of hyperinflation in Britain and not enough attention had been given to the rising costs of our research. I often referred to myself as the epileptic Director. During this period, I became interested in how the new techniques of cloning and sequencing DNA could influence the study of genetics and I was an early and active proponent of the Human Genome Sequencing Project. By 1985, I found that the administrative load of the Director was becoming tiresome and interfering with what I still wanted to do in research and so when I was asked whether I wanted to continue as Director after 1987 when my term of appointment ended, I jumped at the opportunity and left the laboratory in 1986 when my successor took office. The MRC gave me a small Unit and, with some added resources, I set up a Unit of Molecular Genetics based in the Department of Medicine where space was provided for me by Professor Keith Peters. It was in this Unit that the pufferfish project began. The Unit was closed by the MRC in 1992 when I was 65 but I continued the laboratory with other support for some years thereafter. In the meantime, it became imperative for health reasons that I spend the winter months in a warmer climate and Richard Lerner made this possible by giving me a part time appointment in The Scripps Research Institute in La Jolla, California. Here I

found I could pursue new interests in chemistry and especially in the interface between chemistry and biology. I also became involved in a company in the San Francisco Bay Area called Lynx where together with another friend, Sam Eletr, we developed a new massively parallel method for sequencing DNA. In 1995, I founded The Molecular Sciences Institute with a gift from the Philip Morris Company where I hoped that we could create an environment where young people could pursue science in an atmosphere of harmonious purpose and high intellectual challenge. I retired from the Institute in 2000 and in 2001 was appointed a Distinguished Professor in the Salk Institute in La Jolla where I rejoined Francis Crick.

I owe a great debt to the many people who have helped me in my life. My parents would have preferred me to become a surgeon or a physician but were most understanding of the ambitions of their son. My wife and family have borne the burden of a preoccupied husband and father for fifty years. Living most of the time in a world created mostly in one's head, does not make for an easy passage in the real world. Throughout my scientific life and in all my projects I have been joined by many scientists, young and old, whose work was absolutely essential for the success of our scientific endeavours. Many have gone on to do important scientific work but all remember those wonderful times when we and our science were young and our excitement in meeting new challenges knew no bounds. I believe that a scientist should be judged by the quality of the people he has helped to produce and not by prizes or other honours bestowed on him. Let my works speak for themselves.

I am still, at the age of 76, excited by scientific research and the prospect of what can be done in biology. Science is something one is tied to for life and one should never retire from anything until one has secured one's next job. The endless quest for knowledge will continue as long as humans exist.

From [Les Prix Nobel](#). *The Nobel Prizes 2002*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 2003

This autobiography/biography was written at the time of the award and later published in the book series [Les Prix Nobel/Nobel Lectures](#). The information is sometimes updated with an addendum submitted by the Laureate. To cite this document, always state the source as shown above.

---