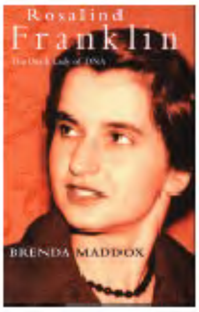

Rosalind Franklin: The Dark Lady of DNA. Brenda Maddox. Harper Collins. London. 2002. 380 pp. Price: US $ 20.

Nearly two years ago, I reviewed for *Current Science*, Georgina Ferry's biography of Dorothy Hodgkin[1]. When I recently came upon Brenda Maddox's biography of Rosalind Franklin (I understand from hearing Maddox on the radio that the pronunciation is *Roz-lind*), the temptation to write another review was overwhelming – not in small part because the lives of these two famous chemist-crystallographers; Dorothy only ten years the senior, were so different. In addition, 2003 marks the 50th anniversary of the Watson–Crick model for the double-helical structure of DNA, and it is appropriate to examine the lives of other scientists, contemporaneous with Watson and Crick, who contributed to understanding DNA structure.

The late Anne Sayre, a close friend of Rosalind, had written the previous biography, *Rosalind Franklin and DNA* (W.W. Norton, New York). Sayre's book was rather limited in its scope, providing in effect, a rebuttal to the outrageous portrayal of Rosalind in James Watson's *Double Helix* (Atheneum Press, New York). Maddox, with all the maturity that time provides, has achieved a much more complete description of Rosalind's life and work.

Rosalind was born in 1920 into a wealthy Anglo-Jewish banking family in London. The early chapters are a description of what it meant to be Jewish in England in the late 19th and early 20th centuries. Quite by coincidence, I read Oliver Sacks' autobiographical account of his fascinating childhood, *Uncle Tungsten* (Alfred A. Knopf, New York) nearly simultaneously with *Dark Lady*. Sacks had a doctor father and a surgeon mother. When he was five-years-old in pre-Second World War London, his mother told him about the 'crying' of zinc and tin when bent: 'It's due to a deformation of the crystal structure'. Like Sacks, Rosalind was the product of a social class steeped in intellectualism in the best sense of the word. This is vividly portrayed by Maddox. Trained in science, Rosalind's father was obliged to abandon a possible career thereof and instead run the family bank. He compensated by teaching, on a voluntary basis, electricity and magnetism to working men in an evening college. Despite the enlightened milieu, Rosalind's precocious intelligence was cause for some concern. 'Rosalind is alarmingly clever', reports an aunt of the six-year old, 'she spends all her time doing arithmetic for pleasure, and invariably gets her sums right'.

Maddox then goes on to describe Rosalind's schooling. A picture emerges of an affectionate child – a very good correspondent, and unusually gifted at various crafts. At the age of eleven, Rosalind was admitted to a secular London girls school known for its academic rigour. The school was an obvious prelude to college, and possibly a career. Rosalind's letters are indicative of an early interest in science, but not at the expense of her all-round development. She made it into the school's cricket, tennis and hockey teams, as well as the debating society. In 1938, she entered Newnham College in Cambridge, a year ahead of her contemporaries. We are told of Rosalind's budding interest in crystallography. As an undergraduate, Rosalind managed only a high second class degree due to exam nerves. This was clearly cause for great disappointment. She was nevertheless offered in 1941, the chance to start a Ph d in physical chemistry with Ronald Norrish (later to share the 1967 Chemistry Nobel Prize with George Porter and Manfred Eigen). They did not get along very well, and their relationship seems to have been characterized by flaming rows. She worked on problems in chemical kinetics, but was not very successful; we are told due to no fault of hers.

In 1942, she broke-off her doctoral research, and joined, as a part of the ongoing war effort, the British Coal Utilization Research Association (BCURA) in Kingston, working under D. H. Bangham. There, she was set to the task of using helium adsorption measurements to characterize the porosity of different coal samples. Charcoal being an essential component in gas masks, the project was important and current in the 1940s. In 1945, Cambridge accepted her Ph D thesis on the physical chemistry of coal. During her undergraduate years in Cambridge, she had come under the influence of the French physicist Adrienne Weill, who was, as a sophisticated senior woman in science, a sort of mentor to the young Rosalind. Weill turned out to be quite the fairy godmother, when in 1947, she helped Rosalind find a position in a French government laboratory in Paris. There, Rosalind could continue her analysis of coal, but using X-rays instead of adsorption measurements as she had used in the BCURA.

Scientifically, Paris was extremely fruitful and she seems to have thoroughly enjoyed living there and speaking French. Some of her letters to her parents from this period argue her preference for things French over things English. Rosalind also formed some scientific friendships (with Jacques Meering and Vittorio Luzatti, in particular) that would last through her final days. Hiking in the French Alps was something that Rosalind was passionately fond of and it seems to have provided her much-deserved breaks from her X-ray camera.

In 1951, through the offices of Charles Coulson, the famous theoretical chemist, Rosalind was offered a fellowship to work in the Physics and Biophysics Department of King's College, London, with the understanding that she would shift her attention to biological molecules. Here, she was expected to establish the X-ray techniques that she had become so comfortable with. The next two years, which would catapult her to eventual fame, were made miserable for her for a number of reasons: King's did not permit women in the faculty common rooms, her upper-class accent was despised by most of her co-workers, and she was obliged to work with Maurice Wilkins, for whom she seemed to have formed an unaccountable dislike almost on sight. It did not help that she was presented to Maurice Wilkins as a fait accompli, and he in turn resented her for this. They shared a student, Raymond Gosling, who worked with both of them on DNA, a project that Maurice Wilkins had initiated.

Maddox describes the enormous contributions Rosalind made over a two-year period in King's to the crystallography of DNA. She found ways of keeping the fibres hydrated, so that the material remained stable over the time it took to record images (that Bernal described later in her obituary as '. . . amongst the most beautiful X-ray photographs of any substance ever taken . . .'). She showed that there are two forms, A and B, distinguished by their polycrystallinity. She used Patterson functions to determine that the phosphorus atoms sit on the outside. And off course, she recorded with Gosling, the famous image 51.

In 1953, Rosalind moved to Birkbeck where she could work under the benevolent yet roving eye of the famous John Desmond Bernal. She started working on the crystallography of tobacco mosaic virus (TMV), and started Aaron Klug on the path that led him to his Chemistry Nobel in 1982 (Klug has this to say in his Nobel lecture: '. . . I can claim no credit for the choice of my first subject. It was the late Rosalind Franklin who introduced me to the study of viruses and whom I was lucky to meet when I joined J.D. Bernal's department. . . . It was Rosalind Franklin who set me the example of tackling large and difficult problems. Had her life not been tragically cut short, she might well have stood in this place on an earlier occasion.'). Klug and his wife became close friends of Rosalind, and in fact, were important benefactors from her will when she died.

Maddox describes Rosalind's growing fame for her important work on graphite, her trips to the US, and her pioneering foray into virus crystallography. By the time she died of ovarian cancer at the age of 37, Rosalind had therefore made fundamental contributions to the structures of three different classes of materials, all of which are crucially important in their respective domains. Instead of ruing over the fact that she missed out on the double helix, Maddox rightly celebrates *all* of Rosalind's achievements in their totality. Maddox points out that Rosalind's tragedy was not that she died without her contribution to DNA being properly acknowledged, but that she died so early, when she was just beginning to blossom as one of the most important chemist-crystallographers of her time.

Dark Lady is the magnificent life story of a fascinating person. One needs no agenda in order to read the book and enjoy it. I finished it saddened that such a talented and clearly likable person had her life cut short, and yet rejoicing at the richness of her short life. Buy the book!

Having said that, one wishes that Maddox did not harp so much on Rosalind having remained single. Also, one wished that there was more detail to the science presented. There is a reference to 'face-centred monoclinic' in one of the early chapters, which, if it is not young Rosalind's error, should not have gotten past the editors. Also, the reader might wish to know a little more about the kind of Ph d problem Norrish set for Rosalind.

Related to the arguably inadequate coverage of Rosalind's science are a few additional points that could be made. I use as an excuse, the historical importance of this year, 2003.

Some carbonaceous materials turn into highly crystalline graphites on heating, while others do not. Rosalind used small and wide angle X-ray diffraction to come up with a coherent picture of what makes carbons from different sources so different in their tendencies to *graphitize*. This work continues to have industrial importance. Indeed, I find that her 1951 paper[2] is well cited even in 2002. Anyone familiar with graphite will know how messy its crystallography can be. The paper is an unequivocal manifestation of Rosalind's ability to do systematic experiments on very difficult systems, and to knit myriad facts into sophisticated models.

The X-ray images of DNA that Rosalind then proceeded to record were not patterns from single crystals, but from fibres. Fibre diffraction is very different from, for example, the kind of diffraction images that Dorothy Hodgkin was acquiring concurrently on insulin and B_{12}, and that Kendrew and Perutz were acquiring on globular proteins. Fibre patterns, like the turbostratic graphite X-ray patterns that Rosalind had earlier worked on, are much more limited in the information they provide when compared with the complete single-crystal patterns of crystalline proteins. Their interpretation therefore, can be far less routine. Rosalind, descended as she was from a French school of crystallography, (following people like André Guinier) famous for work on disordered and large-period structures, was very well trained to examine patterns from DNA. In fact, she later extended the same facility to analysing diffraction from rods of TMV.

It is in this light that we should examine Rosalind's relation to Watson and Crick. Maddox answers some important points and throws new light on the controversy. First, she makes it clear that Gosling gave Wilkins image 51, and he in turn showed it to Watson and Crick. The image was not stolen, as has often been suggested. The big surprise is that Watson and Crick never let on to Rosalind that they had seen the image, and that it had given them crucial clues. This despite their considerable shared correspondence in later years on virus structures, and in the case of Crick, a close personal friendship.

Post-1953, it became fashionable to describe Rosalind as having been overcautious in interpretation of her data, and perhaps even too stupid – that it needed the brilliant Watson and Crick to see what others did not. Maddox ascribed Rosalind's caution to her upper-class Anglo-Jewish upbringing, where doing things correctly and never exposing yourself to ridicule were all-important. I proffer a different view:

> Rosalind did experiments. She waited till she had the data before she performed the analysis. Her work on graphite had already demonstrated her ability to be imaginative in her interpretation of complex diffraction patterns, and her ability to relate them to physical phenomena. Neither did she lack the mathematical tools – in fact, she had authored a *Nature* paper on diffraction theory. Clearly her intellectual isolation at King's contributed to her not appreciating the urgency of the DNA problem. However, I think the primary reason for her reluctance to jump to conclusions was that *she was doing the experiments*. Experimentalists, unlike people who model-build/speculate (*ergo* Watson and Crick) are not *permitted* to make mistakes. Their careers and reputations depend on thoroughness and on being correct. Not so theorists, model-builders, speculators. . . .
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> Let me take the example of Bragg and Pauling. Bragg collected a lot of crystallographic data on silicate minerals. Yet, it was Pauling who (to the annoyance of Bragg) rationalized their structures into a simple set of rules. But Bragg, who was doing the experiments, could not afford to be wrong, while Pauling could. History does not use this story to detract from the achievements of either Bragg or Pauling. Indeed, Pauling was often wrong – DNA, nuclear structure and quasicrystals come to mind. We do not remember him for his mistakes, since we knew all along that he was making leaps in the dark, and that too, with someone else's data. And of course, he was often gloriously right.

1. Ferry, Georgina, *Dorothy Hodgkin – A Life*, Granta Books, London, 1998.
2. Franklin, Rosalind, *Proc. R. Soc. London, Ser. A*, 1951, **209**, 196.

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