Book Reviews

A Restitution

Rosalind Franklin and DNA. ANNE SAYRE. Norton, New York, 1975. 222 pp. $8.95.

It is not really surprising or unusual that the credits for some aspects of a discovery as significant as the structure of DNA are often muddled; that often happens in science. Standard textbook accounts tell us that Watson and Crick proposed the structure on the basis of model building, Chargaff's discovery of base equivalence, and x-ray diffraction data obtained (variously in these accounts) by Wilkins, by Wilkins's group, or by Wilkins and Franklin. Rosalind Franklin is, perhaps, a dimly remembered figure in this episode. But with the publication of Watson's *The Double Helix*, she was indelibly characterized to us as "Wilkins's assistant," someone with a less than pleasant personality whose excellent set of x-ray photographs Watson had to obtain surreptitiously. Anne Sayre, a friend of Franklin's, has been compelled to correct this impression and tells us, with considerable scholarship, about Rosalind Franklin the scientist and the person.

From the point of view of scientific documentation and history, the most important feature of *Rosalind Franklin and DNA* is the description of Franklin's research into the structure of DNA. Not only did she take "beautiful pictures," she also carefully interpreted the diffraction patterns. In a recorded oral presentation, which Watson attended in 1951, she described the structure of the B form of DNA as "helical with the phosphates near the outside." She was cautious and refused to commit herself to a helical structure for the more crystalline and less easily interpretable A form of DNA, but she was not, according to Sayre, "antihelical," as Wilkins called her. Franklin's stature as a scientist is evident from a remark by Crick that left on her own she would probably have arrived at the structure of DNA in "perhaps three weeks. Three months is likelier."

There is much more to this book than a description of Franklin's research. Sayre in an attempt to replace the personality which she claims Watson "stole" reveals something about the family background and personal life of Franklin. We are told about a woman from a socially and politically active Jewish family who was educated at Cambridge during wartime and who had to face considerable challenge to establish and maintain her career in science. The book was not intended to be a biography, but what we are told is so fascinating that one wishes Sayre had given an even more complete picture.

Franklin was an intense person who expressed herself strongly; her home experience and her experience in Luzzati's laboratory in Paris encouraged that sort of behavior. Wilkins, a reserved person, who could have had several reasons to resent her, described her as "fierce." This "personality clash" was only one of Franklin's problems in the King's College laboratory. Before she came to London she had been in a congenial laboratory in Paris where she had learned diffraction after becoming an expert in the chemistry of carbon in England. She arrived at King's to find an ill-defined laboratory setup; the relationship that was meant to exist between Wilkins's and Franklin's research efforts on DNA in Randall's department was never clearly outlined to either of them. Moreover, she was not allowed to partake of the informal interchange at mealtimes because the men and women had to eat separately and she had no female colleagues in the laboratory. It is not so surprising that, while she applied all her intelligence and characteristic dedication to the problem of DNA, very little of what she had to say about the results was heeded; there was a blank wall of "noncommunication." This is in marked contrast with her later experience in Birkbeck College, where she collaborated effectively with her colleagues on structural studies of tobacco mosaic virus.

In the last pages of her account, Sayre questions the effects of Watson's book on the morality of budding scientists. While it is true that the book may have served to perpetuate an overzealous competitiveness, it could also be said that Watson was simply reflecting what some of us regard as less desirable trends in science. It is unlikely that a serious student of science would change his or her way of approaching a research problem on the basis of a reading of Watson's book. The damaging aspect of Watson's book was the case he built against a person who figured prominently in a scientific discovery. His epilogue did not really correct the negative impression he left about Rosalind Franklin. Sayre has repaired the damage and has produced a book remarkable both for its content and for its readability.

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Questions of Bias

The Brain Bank of America. An Inquiry into the Politics of Science. PHILLIP M. BOFFEY. McGraw-Hill, New York, 1975. xxiv, 312 pp. $10.95.

The Brain Bank of America, the report of an "investigation" of the National Academy of Sciences sponsored by Ralph Nader's Center for Study of Responsive Law, has already been the subject of an admiring *Science* news story (13 June 1975, p. 1094). Thus, readers of *Science* have been told—as it happens, by a friend of author Phillip M. Boffey's—that the book is "evenhanded and scholarly" and "an important contribution to the science policy literature." It is an amusing coincidence that so much of *The Brain Bank of America* consists of complaints about the Academy's propensity for the inside job.

The book asks why the Academy, chartered by Congress to provide scientific advice to the United States government, gives advice that is for various reasons no good. Sometimes, says Boffey, this advice is contaminated by possible conflicts of interest, sometimes by insufficient independence from government clients, and sometimes by being on the wrong side, according to Boffey's view of the merits.

If this book has a central thesis, it is that the results of Academy studies tend to reflect the biases of the organizations that paid for them. This is established in a number of ways, for example by describing the purported interest of the government agency that commissioned an Academy report and establishing a correspondence between that interest and the report's conclusions, or, more frequently, by listing past or current affiliations of members of Academy committees (many of whom, incidentally, are not members of the Academy). Occasionally a more tortuous inferential path is traveled, as in the case of Philip Handler, since 1969 the full-time president of the