

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.

HELEN BERMAN

*Institute for Cancer Research,
Philadelphia, Pennsylvania*

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

Academy, who earlier was a member of the board of directors of a company that markets an artificial sweetener. This made Handler's presidential opinion on the banning of cyclamates the view of "in fact, a somewhat biased witness," not because Handler had any direct connection with, or stood to profit from, the marketing of an artificial sweetener but because "he presumably retains the conditioning of a corporate director who would tend to regard the products of his industry as beneficial and efforts to ban them as unreasonable" (p. 184).

Even though hedged about by "even-handed and scholarly" words like "in fact," "somewhat," "presumably," and "would tend," Boffey's unfriendliness to Handler here seems to me to take him too far. More typical of the book are direct imputations of bad faith on the part of Academy committee members based upon the bad company they keep: "A DuPont scientist was allowed to draft key sections of a report on lead pollution; a group of industrial toxicologists was allowed to draft guidelines for determining insignificant levels of chemicals in food; a subcommittee on dog and cat food standards was headed . . . by an official of . . . a major pet food manufacturer" (pp. 76-77).

Consider how such a standard might be used in the gentle art of book reviewing. Boffey's employer for the writing of this book was Ralph Nader (identified as "consumer champion Ralph Nader" on p. 186), who of late has gotten rather heavily into the business of sponsoring exposés of establishment-type establishments (see, for example, M. J. Green, J. M. Fallows, and D. R. Zwick, *Who Runs Congress?*, Bantam, 1972, and M. J. Green, *The Other Government*, Grossman-Viking, 1975). Under these circumstances of employment, could Boffey have done other than to produce an attack, no matter how flimsily founded, on the Academy?

For someone who reasons in this fashion, there is support in the opening pages of the book, where we learn that American society "genueflects" before experts, that their advice "comes out strutting." A member of the Academy was "characteristically arrogant" in declining to be "investigated." These conclusions all appear in the preface.

Soon thereafter, Boffey notes, in criticizing a National Academy of Engineering committee on pollution abatement, that it "was no more qualified than any other group of citizens to judge what would be 'wise' public policy" (p. 72). Sound doctrine, and yet Boffey criticizes other of the Academies' committees for taking on an assignment pertinent to a naval communications project that did not include eval-

uating its "desirability" (p. 63) and for not venturing to raise "questions as to the basic worth" of the space shuttle program (p. 76).

Thus, the Academy is damned if it does pronounce on the overall wisdom of public policies, and damned if it doesn't. Such an attempt to have it both ways, and in the space of only a few pages, would give at least superficial credence to a belief that from somewhere Boffey has acquired a certain antipathy to the Academy that he is not altogether successful in explaining straightforwardly.

This, as I have mentioned, is Boffey's own primary method of demonstration: a glance at somebody's background gives a "motive" for selected characteristics of his performance. It is only good for so much mileage. In defending Boffey's book from being traduced by his own method, we might point out that Boffey is a highly reputable and skilled journalist who has covered national science politics for some years now, and whose work deserves a more impersonal evaluation on its merits. No doubt the scientists whose connections he has found wanting feel the same way about themselves. Can we be sure that in every case they would be mistaken in this view?

I do not see any satisfying way of settling the issue on a wholesale basis. Arbitrarily imposing the symmetrical assumption, for example, that Boffey and the Academy are both fatally incapacitated by conflict of interest has the effect of condemning both the Academy and the book out of hand. The contrary symmetrical assumption, that both the book and the Academy are to be taken at face value, vitiates much of the point of the book. Either way, Boffey's argument suffers.

A more discriminating approach is available to persons having intimate knowledge of the political dynamics of Academy activity, as I do not. They will have to evaluate, for example, the fairness and completeness of Boffey's account of six cases in which Academy advice evidently failed in some way to conduce to an end desired by Boffey. In each case, strong political currents were running in the society at large, and these provided a point of entry from which Boffey could criticize Academy performance with respect to advice on the SST, defoliation, pesticides, radioactive waste disposal, airborne lead, and food protection.

These are important issues, and if Boffey is right in his characterization of Academy performance with respect to them, he is doubtless justified in his jaundiced view of Academy activity as a whole. Yet it is far from trivial to ask just what proportion of the whole these cases represent, how

typical these cases were of the work of the "network of hundreds of [Academy] advisory committees which serve the middle levels of the federal bureaucracy" (p. 3). The book is silent on this question.

Are there instances known to Boffey where the Academy functioned in a satisfactory way? And if so, can we draw some conclusions about those conditions in which Academy participation is especially helpful and those where the Academy contribution is less useful? Judging from the kinds of issues Boffey tends to raise and the kinds he ignores, it may be that whenever there is a serious disagreement about facts by experts the weight of the Academy's judgment ought to be withheld unless choices can be made on scientific grounds. This means that Boffey's occasional plea that the Academy enlist on the side of the angels ought not to be heeded; on the other hand such a self-denying ordinance might have kept the Academy out of a good bit of the hot water Boffey claims it has gotten into in the past.

Another curious omission concerns the treatment of the Academy's staff, 1107 persons strong as of mid-1974, which rates only a single page of the book (p. 43). Yet on that page we learn that "the staff plays the major role in formulating proposals for projects and in selecting the experts to carry them out." The rest of the page gives a smattering of opinions more or less in aid of Boffey's rather opaque conclusion that the staff is "weak."

A broad-brush condemnation such as Boffey's, no matter how adroitly worded, naturally raises the question: Do we need a national academy of science? This is a question of the sort asked on many university campuses where there are plenty of convenient places to have lunch: Do we need a faculty club? On balance I come down on the favorable side for that portion of the Academy's activity that is purely honorific, since it performs the indispensable task of helping deans figure out who, if anyone, on the faculty is smart. After reading *The Brain Bank of America* I do not know what to think about the Academy as an organization for evaluating the state of scientific knowledge on a wide range of topics, for checking the veracity of purportedly scientific claims, for sponsoring disinterested inquiry, or for blessing one or more alternative courses of action by various and sundry government agencies. Perhaps some day a book will be written that throws enough light on this cluster of problems that it will deserve to be called "an important contribution to the science policy literature."

NELSON W. POLSBY
Department of Political Science,
University of California, Berkeley