

Comments and Replies

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Eliane Silverman and Margaret J. Osler (Vol. 2, No. 2, Part 1, Spring 1977) criticize my review of Anne Sayre's study of Rosalind Franklin (Vol. 2, No. 1, Fall 1976) on the ground that I failed to "provide an adequate historical analysis of the images and realities of science." I don't know whether they intended the emphasis to be on "adequate" or "historical," but in any case the examples they gave of questions I should have raised, e.g., the differences between France and England, do not impress me as adding anything conceptually to the existing literature. I hope they will undertake such national comparisons, but I doubt if they will find that the differences among countries in Western Europe are anything more than trivial differences in degree. I reserve judgment and wish them well.

In addition they attribute to me a naive interpretation of science which I did not put forth and was, indeed, critical of Anne Sayre for holding. It is she, not I, who thinks that Franklin's case was special, a violation of the universalistic norms of professional behaviour. The tragedy is, as I pointed out, that Rosalind Franklin probably shared the same belief, and might have personalized a problem which I regard as a structural

phenomenon. Dorothy Wrinch, another woman scientist who was also engaged in research on protein structure in England and the U.S., had an even more embittering experience with hostile male colleagues and male dominated foundations. As a consequence she had fewer illusions about the ethics and rationality of her scientific co-workers. And she, too, missed the Nobel prize.*

Finally, Silverman and Osler suggest that my discussion does not contribute to a new feminist historiography of science.

Perhaps it would clarify matters if I summarized the points I was making in my review, and readers can judge for themselves whether their charges are justified.

1. The classical picture of science and science-making which one finds in philosophies of science obscures the social realities of science. This accounts for the popularity of Watson's informal description of how it really happened with DNA in The Double Helix. But this new approach can be deceptive if insiders like Watson consciously or unconsciously substitute other myths,

* I am indebted to Alison Prentice, Dept. of History and Philosophy, OISE, who after reading my review called my attention to the case of Dorothy Wrinch and provided me with an article

e.g., the spurious competition with Pauling. The Watson image of Rosalind Franklin panders to a typical stereotype of women professionals and serves to justify his reality. My point is that the new sociology and psychology of science should be scrutinized carefully by feminist scholars for it opens the way to a range of sexist biases.

A good example of this is C.P. Snow's review of the Sayre book. Snow recognizes some of the self-serving distortions in the Watson account, but he does Franklin and women generally an even greater disservice by perpetuating the image of women as good but not good enough, lacking the kind of sustained commitment required for greatness. Watson, after all, may have been a liar and a thief, but C.P. Snow's prejudices are more subtle and more insidious.

2. In my comments on the Snow review I also wanted to draw attention to the fact that in the new historiography of science (cum Kuhn) women are put down for lack of intuition, whereas in the older historiographies of science which emphasized cognitive intellectual skills, women were put down for relying too heavily on intuition; a

about her by Marjorie Senechal entitled "A Prophet without Honor, Dorothy Wrinch, Scientist, 1894-1976" which appeared in the Smith Alumnae Quarterly, April 1977, 18-23.

zero-sum game. Therefore, I suggested in my review that those qualities of mind or personality that allegedly handicap women are neither innate nor acquired, but are a function of the exclusion of women from the informal information networks within institutions. As I pointed out, Watson and Crick knew more about Franklin's work than she knew about theirs.

3. Both of the above points illustrate my more general thesis that as long as science enjoys the mystique and power it does, women will be discouraged from entering it, discriminated against within it, and tolerated only as long as they replicate their subordinate status in the larger society: socializing the young (teaching science in elementary and secondary schools) keeping house (doing routine and repetitive lab chores) and supporting the egos of men (remaining assistants to, or employees of, the great man who gets the grants.) Thus the struggle for equality in science must begin with a more radical move toward the demystification of science itself.

4. Finally, in discussing Franklin's career I wanted to make the point that the theory of the dual labour market which has been applied to unskilled women and other minorities applies with equal force to high status, professional women, traditional or liberated, the opportunity structure is, in the last analysis, a better

predictor of the numbers and status of women at any point in the occupational hierarchy. The kind of socialization of women study that Silverman and

Osler recommend would be interesting but it makes us victims of our parents and teachers, of our childhood, not of the larger economic power structure.

