Molecular Biologists and Their World

The Eighth Day of Creation. Makers of the Revolution in Biology. HORACE FREELAND JUDSON. Simon and Schuster, New York, 1979. 686 pp. + plates. $15.95.

When a book takes nearly a decade to conceive and write, it ought to command a certain respect. Horace Judson has spent this length of time on the composition of his historical account of molecular biology, and the result is, in its way, very impressive. This long and detailed book is clearly based on a great deal of research, which has been classified, interpreted, and arranged into a very readable narrative. As Judson says, it acknowledges the specialist historian while being written for the general reader, from conversations with over a hundred scientists and study of personal and scientific papers. The wealth of detail in the book will certainly be of enduring interest and value to historians of science, and I would also expect the book to be widely read and discussed in laboratories around the world.

Manifestly the major problems that face anyone who seeks to describe or analyze a field of science like molecular biology are those of scale, complexity, and diversity. There is a great deal to read and comprehend, and it is difficult to know which patterns of conceptual, technical, and institutional development to emphasize and which to ignore. On this question Judson has received much sensible advice from some of the key figures concerned, Francis Crick, Matthew Meselson, François Jacob, Sydney Brenner, and Max Perutz among others. The narrative falls into three main sections. The first recounts the research on the structure and function of DNA, leading up to the double-helical model in 1953 and some of the subsequent confirmatory work. This certainly goes beyond what has already been published on this issue. The provision of a dramatis personae to this section was perhaps a mistake. The second section deals with the work on the genetic code, the discovery of messenger RNA, and the theory of the operon. This is a complicated story, and the narrative depicts convincingly the considerable uncertainty and confusion in molecular biology in the 1950's. The third section describes the extended studies of protein structure and the recent successes in explaining physiological function at the molecular level. There is an interlude between the first two sections, on the reductionist strategy in mid-20th-century biology that allowed molecular biology to develop. This section does not seem to me successful in this position. There is also an afterword containing some interesting ideas that I shall discuss below. Judson has therefore covered a thematically complicated set of scientific developments over the period roughly from 1945 to 1970 by making three synchronic passes through the historical material. In this he has gone further in time then did Robert Olby in *The Path to the Double Helix* and further in scope than Franklin Portugal and Jack Cohen in *A Century of DNA*, the books with which this invites comparison. This book will complement these accounts but will not or should not displace them. Judson's history is brighter and less recondite than Olby's; scientists are more carefully introduced and more fully characterized in *The Eighth Day of Creation*, so that one can usefully turn from that book, inspired by Judson's lively sense of the dramatic in science, to check it against the measured detail in *The Path to the Double Helix*.

It would be fatuous to attempt to summarize the historical narrative in this book, and the most useful method of review seems to me to look at Judson's way of working, his mode of exposition, and some of the less obvious results of his synthesis and then to turn to some of the more general issues raised by his account. The book is constructed around a series of interviews with about 130 people, many of them distinguished molecular biologists, and their verbatim comments appear in profusion in the text. In a number of cases these people were interviewed several times, having read sections of the book, on which they commented in general and on matters of detail. This transcribed material has now been deposited in the library of the American Philosophical Society, and it must be accounted a valuable resource in its own right. Judson has also drawn upon the growing body of secondary sources in the history of molecular biology, explaining emphases and interpretations and pointing out textual errors that have crept in through inaccurate reproduction. For example, he points out that Oswald Avery's famous letter to his brother Roy describing the experiments on the transforming substance has been rendered imperfectly and that sections of Rosalind Franklin's notebooks that bear upon the important controversy about the significance of her crystallographic studies of DNA have also been wrongly quoted.

The use of interview material has had at least two significant consequences. First, there are some fascinating comments by working scientists on the cognitive processes of science. Crick, for example, talks at various points about the construction of theoretical models and hypotheses in biology. Walter Gilbert describes the difficulties of experimental design in certain contexts; Roger Kornberg offers some insights on the possible emotional attachments to unsolved scientific problems; Erwin Chargaff makes an interesting comment, reminiscent of the philosopher Paul Feyerabend, on the danger to creativity of theoretical systems that seem capable of explaining anything. There are many, more specific examples in the book. Second, the interview procedure does allow the depiction of a particular subculture, since in talking about their world the scientists concerned use very specific codes, conventions, evaluations, and stereotypes. We hear of legendary periods of collaboration and canonical experiments that have shaped a research tradition and are offered perceptions of close colleagues that register a very specific set of habits or tastes or preferences. Judson brings the quality of lived experience to this science of life. But the method has its risks, among them that one might stretch out the anecdotal material to cover an institutional generalization.

Moreover, Judson's mode of exposition is rather unusual in a study that has a good claim to be a serious work of history. While the style adds to the readability of a very long book, it is in my view not really successful. Judson combines detailed technical exposition, verbatim quotation from interview, contextual information about the interview, and secondary elaboration of both historical and contemporary events in a way that endlessly changes focus and perspective. Sometimes it works well, both stylistically and historically, illuminating a scientist's past work and activity by showing what he or she has become. Sometimes it is confusing or trivializing; thus one does tire of learning of the meals that Judson ate while talking to the next big name on his interview program. Moreover, one notes that this journalistic technique displays only cer-

tain rather special aspects of scientific life; we are shown very little of the managerial and political roles that scientists play, very little about obtaining grant support, or planning new projects, or interacting with subordinates, or doing business at conferences. Having said that, I should make clear that Judson does not provide an idealized picture of science: some people's faults are plain to see, as are the virtues of others, and there is ample reference to intense competition, lasting enmity, biased appraisal of work from low-status laboratories, and collective error and confusion. This is post-*Double Helix* science writing.

The book then is a careful description of the formative years of molecular biology that makes fascinating reading. It is a book that contains a great deal of information; it offers a lot of clues to the historian, suggests lines to follow up, themes to develop, and models that could be tested against its data, but in general it does not operate on this second-order level itself. Judson does make one general historical claim, that the history of molecular biology can be regarded as the development of the concept of biological specificity, apprehended from the convergent points of view of genetics, biochemistry, microbiology, physical chemistry, and x-ray crystallography. This is an interesting idea, and it immediately raises questions about the nature of the process through which this concept was deepened or elaborated and given new levels of meaning. I can best explain this by discussing briefly three issues raised by Judson's book.

One of the recurring ideas in *The Eighth Day of Creation* is that of a style of reasoning specific to molecular biology, a style that requires one to make simplifying assumptions, to exercise a certain boldness of supposition, in advance or in defiance of the data, and to reason theoretically. At several points Francis Crick talks of the difficulties and satisfactions of pursuing this type of argument while evading the clutter and distraction of chemical detail. Brenner mentions a cult of minimal experimentation. Certain experiments are presented as models of elegance and intellectual parsimony, although on occasions the method broke down and led molecular biologists astray. Now such a style can be maintained only if it works, that is, if it gives results or models to test; and if it can be practiced, that is, if institutional conditions permit this innovation and the admission of this mode of argument into the domain of biology. This raises interesting questions about the development of molecular biology, about how the conceptual ground was prepared for such theoretical reasoning and how such unconventional styles of argument gained and sustained professional legitimacy.

This leads to a second issue, which one might call the paradox of information theory. Judson argues that the postwar information sciences (cybernetics, control theory, cryptography, the theory of programming, and others) played no direct role, with a few minor exceptions, in the formation of the concepts of molecular biology. In this he is persuasive, but the problem still remains of how the ubiquitous presence of terms like "code," "message," "feedback," "information," "reading head," and "program" within its contemporary discourse is to be explained. This system of terms is not a mere façon de parler; it is the outward manifestation of a set of deeply rooted rules of thought. How and why was such a transformation effected and which groups and individuals brought it about? Clearly we need to know more here.

The third issue is a more general one, concerning patterns of professional interaction in science, particularly as a new field develops. Judson offers us comments, capable of generalization, on the way the phage group was set up or the manner in which molecular biology was established at the Institut Pasteur and on the international network of communication and collaboration that developed. He also describes instances of noncommunication where one might have expected more interaction, and in the final chapter he discusses the coalescence of five approaches to establish this new science of life. I wish he had gone further here and worked at the creation of some organizing model, about how the new groups formed, how the changing approaches to theory and experiment were classified and an institutional identity established. This remains for others to do, making use of the fascinating material assembled in *The Eighth Day of Creation*.

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SALT: Questions and Interpretations

The SALT Experience. THOMAS W. WOLFE. Ballinger (Harper and Row), Cambridge, Mass., 1979. xviii, 406 pp. $18.50.

Arms Control and SALT II. W. K. H. PANOFSKY. University of Washington Press, Seattle, 1979. viii, 76 pp. Cloth, $6.95; paper, $2.95. Jessie and John Danz Lecture Series.

The United States Senate is now debating the ratification of the treaty resulting from the second phase of the strategic arms limitation talks (SALT II) with the Soviet Union. One might assume that such agreements are of overwhelming importance, for they concern the missiles and bombers by which the United States and the Soviet Union threaten and deter attacks on each other, and perhaps keep peace in the world. In the face of worries that technology will make land-based missiles vulnerable to attack in the 1980's, or that weak American bargaining may have given the Soviets too many advantages since SALT I, one might have expected a great public interest in the issues discussed in the books under review.

Yet the level of public information on the SALT talks is remarkably low. In polls taken this year as few as one quarter of Americans could correctly identify the United States and the Soviet Union as the two parties to the negotiations, and fewer still could answer questions about the details of the proposed agreements. In light of the importance attached to the negotiations, both by those favoring the SALT II agreements and by those opposing them, how can one explain this apparent indifference?

One reason for indifference might simply be that we have reached such a level of mutual assured destruction, of mutually reliable second-strike strategic force capabilities, that development of additional weapons by either side makes no difference. The world, in this view, is not in danger of a Soviet sneak attack, or a World War III, regardless of whether SALT fails or succeeds, and any such danger is not significantly increased by new Soviet missile developments or by any Soviet hard bargaining about the terms of a treaty. If the public seems to care less about the details of the strategic arms balance today than it did in the days of Sputnik and the "missile gap," in this view, it is because the dangers are indeed less real today.

In another view of the situation the lack of public awareness would be seen either as representing foolish optimism