Book Reviews

The Molecular Vision of Life: Caltech, The Rockefeller Foundation, and the Rise of the New Biology by Lily E. Kay

Oxford University Press, New York and Oxford, 1993. 304 pages. $49.95

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Science has advanced so rapidly in our century that histories of science are often written while participants in the events described are still living, some still active. This book is an example. It deals with genetics at the California Institute of Technology from 1928, when Thomas Hunt Morgan, founder of the Drosophila school of genetics, became first chairman of the Division of Biology, to 1953, when the molecular structure of DNA was revealed by Watson and Crick (at Cambridge University). People like me who lived through the events are hard for historians to please. In my view, Kay has done a good job of describing the events, but a surprisingly bad one of understanding them. Her book is, it turns out, an ideological assault on molecular biology.

From its start, Morgan's department was an important international center for research and training in genetics—classical genetics at first, molecular later—with the support of the Rockefeller Foundation. This support is one of the book's major themes. Kay contends that molecular genetics is the product of a collusion between Caltech and the Rockefeller Foundation undertaken for the purpose of acquiring "social control." The phrase "social control" was popularized around the turn of the century, Kay tells us, by an American sociologist concerned with some of the problems of his time. It is clear that the words did not have the sinister sound then that they have today; for us, it would seem that the phrase might better be translated "social self-control." In any case, it was favored by the trustees of the Rockefeller Foundation (founded in 1913), a group Kay describes as composed of successful Nordic, Protestant businessmen who desired to shape American society along lines they approved of. In time, we are told, "social control" became the watchword of the program under which the foundation would fund research on molecular biology (a term coined in 1938 by Warren Weaver, the foundation's director of natural sciences). It also becomes the mark by which Kay demonizes this foundation, which has been one of the most progressive forces of the 20th century.

The foundation funded a molecular approach to biology, presumably because it realized that this approach was the best way to attain an understanding of what was perceived to be, in essence, chemistry. Kay disapproves of this decision for two reasons. First, she insists that what the foundation and its complaisant client scientists really wanted was "control." Thus, she tells us, "The program expressed the perception that mechanisms of upward causation were necessary and sufficient explanations of life and the most productive path to biological and social control."

Second, Kay condemns the idea that life can be explained by "upward causation." She is an antireductionist—a position sometimes adopted by those who dislike what they perceive to be the direction of modern genetics. It must be understood that antireductionism is not a scientific position, but a political one, just as Lysenkoism was before it. Like Lysenkoism, which also claimed to be scientific, it is actually antiscience. Its adherents do not perceive that science *must* be reductionist—that natural systems can be said to be understood only after they have been reduced to and reassembled from their components. On the contrary, Kay claims that a variety of nonreductive biologies exist, any of which would have served better than molecular biology as a major theme for understanding life. She says: "The abundance of rigorous quantitative antireductionist models that have developed during the second half of the twentieth century attests to the limits of the mechanistic and physicochemical approach for solving problems of biological organization.[21]" Needless to say, neither Kay nor the cited reference describes even one such model.

The foregoing beliefs are advanced in Kay's early chapters. The rest of the book is a history of genetics at Caltech up to about 1953, as already stated. Considering the handicap that Kay labors under—an antireductionist discussing the history of science is very hard to take seriously—she does a creditable job. Her account centers on the four best-known Caltech contributors to genetics: Thomas Hunt Morgan, George Beadle, Linus Pauling, and Max Delbrück—all Nobel laureates. Her attitude is that these are great scientists, but not necessarily great men. She likes Delbrück the best and gives him favored treatment, apparently because she thinks he was an antireductionist. This was true in a sense, but not Kay's sense. Delbück thought an ambiguity—a paradox—would be found at the end of biology, like the wave-particle of physics. The Watson-Crick double helix destroyed this notion, however: life is reducible to chemistry, and so far no paradox.

The other three scientists are treated less well. Morgan, whom I knew in my graduate years, emerges from Kay's archival sources as a two-dimensional, anti-Semitic figure. Morgan grew up in Kentucky in the 19th century, and no doubt he made anti-Semitic remarks on occasion, although I never heard one. But he was a complicated man, and whatever he may have said, he was not anti-Semitic in his conduct. As a graduate student, I regularly worked at the Kerckhoff Marine Laboratory on weekends with Morgan and Albert Tyler, one of the Jewish members of his faculty. (Kay misstates the nature of Morgan's work in those years.) Morgan never displayed the slightest anti-Semitism toward Tyler or me, but was always his rational, dignified self. Later he was chairman of my Ph.D. Oral Exam, asked me an interesting question, and, I have always believed, secured the National Research Council Fellowship I was awarded in 1939. I regret that Kay did not choose to discuss Morgan—Beadle also, whom she slurs—with me while she was working at Caltech, but instead uses some quoted remarks of the great man to further her political attack on molecular biology.

Much of the book deals with the relations between Caltech and the Rockefeller Foundation. Scientifically, its major question—one that Kay handles well, given the limited scope of the treatment—is that of the biological role of proteins. Pauling's work on sickle-cell hemoglobin and the α-helix is clearly presented. At the time, it was commonly believed that genes are nucleoproteins. This view was accepted by many at Caltech even after the 1944 paper of Avery, MacLeod, and McCarty gave strong evidence that genes are nucleic acids. Kay treats the scientific issues here broadly, for the most part. This is well, because where she goes into detail, she tends to err, sometimes badly. Thus, she seems to think that the genetic code contains 124 codons.

A lot of effort went into this book, and it contains much of interest, more than I can comment on here. With more open-mindedness on the author's part and a good editor it could have been an important contribution to the historiography of science. What a pity that it turned out otherwise.

Electrical Properties of Mammalian Tissues: An Introduction by B. J. Northover

Chapman and Hall, London, 1992. 109 pages. $29.95

Reviewed by Kenneth R. Foster, Department of Bioengineering, University of Pennsylvania

This brief text aims at providing "...a student of biology having little background knowledge of physics with a fairly painless, but rational, entry into the vast and detailed literature. ..on biological electricity." Northover, a cardiovascular pharmacologist at Leicester Polytechnic (UK), summarizes standard topics in electrophysiology—the behavior of electrically excitable cells, ion currents through membranes, cable theory, and so on—with special emphasis on the electrophysiology of the heart and kidney.

The natural audience for this book consists of advanced biology students with considerable knowledge of biology but an aversion to mathematics. The book is not an "introduction" as the term is used in the American textbook market. It includes a 30-page discussion on the electrophysiology of cardiac arrhythmias, but not an introduction to the electrophysiology of the normal heart of the sort presented in introductory physiology texts. Some of the discussion is very brief—the chapter entitled "Models and Mechanisms of Ion Channels" covers gating mechanisms, patch clamping, and the modulated-receptor hypothesis—all in five pages. Nevertheless, many instructors and students will find the book useful and interesting as a supplementary text.

I recently reviewed an electrophysiology text for engineers, in which the choice of biological topics was skewed by what is mathematically interesting. This book has the opposite problem: a sketchy and at times confusing theoretical analysis. Northover pulls the Nernst equation literally out of thin air, by calculating the work needed to compress an ideal gas and abruptly substituting the chloride ion concentration in a cell for the gas pressure. Elsewhere, he declines to solve a simple differential equation, stating (incorrectly) that integral transform techniques are needed. If this book goes into a second edition, it would benefit by a more careful and comprehensive discussion of this large field.