minists find themselves in a corner. The amount of tinkering necessary to draw the needed causal connections between ideology and the content of particular scientific theories is so extensive that their position "degenerates into absurdity." With enough tinkering "virtually *any* theory can be used to justify *any* social policy" (p. 155). But Bowler also finds himself in a corner because Darwin was not always able to resist the attractions of the progressionism so popular in his day. Although Bowler admits that it may seem "rather silly" to think of Darwin as betraying one of his most important insights, that is precisely what he did on occasion with respect to progressionism.

Although Bowler directs his book at Darwin scholars, it can be read with profit by anyone interested in Darwinism. Just as most of us can recall the secret enjoyment we felt in school when one of our classmates was being punished and not us, we are liable to get vicarious pleasure out of Bowler's chastising the Darwin industry. I have complaints on only two matters, one that could be remedied, the other not. Although the notions of Darwinian, pseudo-Darwinian, non-Darwinian, and anti-Darwinian are central to Bowler's analysis, I could not always follow his usage. He defines the terms but does not manage to stick to his definitions, in part because such categories have two dimensions—conceptual and social—and the two do not always go together. Scientists who disagree with each other over fundamentals can nevertheless cooperate. Although Huxley supported Darwin in his attempt to reorient biology, he disagreed profoundly with Darwin on evolution and contributed little conceptually to the development of Darwin's research program. Huxley was not socially anti-Darwinian, but he differed conceptually hardly at all from several of Darwin's opponents who were.

More important, Bowler is frustrated by the continuing emphasis on Darwin in histories of evolutionary biology, but in his own attempt to counter this bias he himself is forced to pay too much attention to Darwin. I see no way out for Bowler in this book. In order to show Darwin's actual role in evolutionary biology, he is forced to talk a lot about Darwin. If other Darwin scholars are convinced by his radical conclusions, then future historical works may rectify this pervasive imbalance, but I doubt this will happen. The myth of Darwinism has become too much a part of our worldview. Bowler may convince Darwin scholars that Darwin was really a minor figure in the history of evolutionary biology from the middle of the 19th century until the modern synthesis, but future works will still revolve around Darwin. Non-Darwinian theories will be classified first and foremost in relation to Darwinism and only then evaluated in their own right. After all, Bowler did not title his book "The Developmental Revolution." I doubt that very many authors in the future will be able to resist including the name "Darwin" in their titles any more than Bowler was.

DAVID L. HULL
Department of Philosophy,
Northwestern University,
Evanston, IL 60208

Max Delbrück

Thinking About Science. Max Delbrück and the Origins of Molecular Biology. ERNST PETER FISCHER and CAROL LIPSON. Norton, New York, 1988. 334 pp., illus. $19.95.

Max Delbrück was one of the most influential biologists of our era, a leader in the conjunction of microbial genetics and macromolecular chemistry that led to the field we now call molecular biology.

Delbrück was originally trained as a theoretical physicist by and among those scientists who were at the center of the European physics community just as the first wave of excitement from the development of quantum theory was subsiding. The authors of this new biography document, from his own publications and letters, that Delbrück felt frustrated in physics since the great paradoxes had already been resolved. He wanted desperately to make an important discovery and expected that biology might be fertile ground. More specifically, he felt that by finding the ideal simple system for a particular problem and by mounting an all-out assault, a situation might be found in which the known understanding of the natural world would be insufficient to explain the results; new laws of physics would be necessary.

In 1937 Delbrück arrived in the United States from Germany, looking for just such a system. He held a Rockefeller Foundation fellowship to visit several of the most important centers of genetics research. At the Biology Division of the California Institute of Technology he found out about bacterial viruses, also called phage (short for bacteriophage), which seemed perfect for the study of replication. Because of the small size and rapid replication of both the virus and the host this was an ideal system for the use of quantitative methods that came naturally to a physicist.

Delbrück remained in the United States during the war and continued his work with phage, moving to Vanderbilt University as a physics instructor at the end of two years of support from the Rockefeller Foundation. In addition to his research he used all the opportunities available to spread his enthusiasm for phage work and to invite others, particularly physicists, to join him. Perhaps the most important event in this "advertising" campaign was the establishment in 1945 of the phage course at Cold Spring Harbor Laboratory, a small private institution on the north shore of Long Island that had long specialized in genetics. This intense three-week course was designed to give its students, who were highly trained in some other field, enough knowledge, both theoretical and hands-on experimental, that they could begin doing phage research in their own laboratories.

In 1969 Delbrück, who had been invited back to be a professor at Caltech in 1946, was awarded the Nobel Prize together with Salvador Luria and Alfred Hershey. Delbrück was honored for his career as a leader in the development of this experimental system that was crucial to our understanding of gene action, rather than for any particular experiment.

In 1966 the Cold Spring Harbor Laboratory published a book of great importance in recording the history of the young field of molecular biology. *Phage and the Origins of Molecular Biology* (traditionally abbreviated *PATOOMB*) was published as a festschrift for the occasion of Delbrück's 60th birthday. *PATOOMB* was different from most such volumes. Most of the contributions were reviews of the influences, personal and intellectual, that enabled the authors to make the important discoveries that constituted the new field, and the book became a classic because of the depth of feeling shown by the authors for Delbrück and the crucial influence he had on their work.

Clearly *PATOOMB* was a marvelous resource for the authors of this biography; it also presented a problem. Although many of the best stories of Delbrück's legendary intellectual dominance, love of practical jokes, and unique methods of motivating his coworkers are told again and may influence today's students, they do not have the same impact as when told by those who were actually there.

In contrast, a strong point of this new biography is that it takes us beyond a description of the famous phage years and presents the wide range of Delbrück's experimental and theoretical contributions. As early as 1950, he had begun to search out new problems that could be approached by the kind of concerted effort on a single simple experimental system that had worked so well with phage. Although the other experimental systems he chose never approached the popularity of the phage system, he continued to have a strong influence on a wide circle of molecular biologists—perhaps most of all because of his insistence on accuracy and thoroughness in scientific proof. I was a graduate student in Delbrück's lab from 1965 to 1972, a period during which he was using the fungus *Phycomyces* as a simple system to study sensory transducer physiology. Unfortunately, I believe the presentation of some of the scientific developments of those years is inaccurate and undermines the picture of Delbrück as a powerful leader who would search out the right expert when he needed a new approach.

Furthermore, the narrative at many points throughout the book is quite difficult to follow. In many cases stories are told that do not really have a point in the context of the events described. Just as they seem on the verge of some deeper analysis, the authors often move on to the next anecdote. I am disappointed that they do not take us further into an analysis of the rich emotional dynamics of Delbrück's life. They provide an inadequate description of his family of origin, and we learn even less about his relationship with his wife and four children. There is no discussion of the highs and lows that must have come together with the driving ambition. We learn little of any rivalries or even disagreements with colleagues. The one or two that are alluded to are not explored in any depth. The tremendous intensity with which Delbrück tore into problems is not here. Rather, he seems to move through his whole life in an exalted state, enjoying his work, teaching others, spreading the word about his latest choice of experimental material. Only in the all too brief description of his last days, as he struggles with his imminent death, do we finally gain a glimpse of his humanity.

In 1980 after he was diagnosed with a terminal cancer, multiple myeloma, Delbrück began an autobiography. At the time he explained that the project was an attempt to make it easier for science historians to understand his life. Since he knew little time was left, he did what he had done so many times before—influenced another scientist to devote his time and energy to the project. Peter Fischer met the requirements: he had received his Ph.D. degree with Delbrück in 1977, so could understand the science; he was German so could understand the language and culture of the important early years; and, finally, he had shown interest in and talent for writing about science. Following Delbrück's death in 1981 Fischer continued the project, which he has now finished with the help of Carol Lipson, a professor of English at Syracuse University who is also the wife of a former Delbrück research associate. Fischer also received financial help from Delbrück's wife, Manny, and institutional support from Cold Spring Harbor Laboratory. Thus this biography must be considered, as Delbrück himself would have said, an "inside job."

In *PATOOMB*, Jean Weigle, an associate of Delbrück's who had already had a distinguished career as a physicist before joining the "phage group," where he made several important discoveries, confesses how he has "told the story of the permeating influence of a questioning mind, producing in those near it another sort of questioning attitude which could be expressed this way: 'What will Max think of it, if he does think about it?' " In the case of this biography there is still an additional burden: What will all those who loved Max think of it? I think that this burden would fall too heavily on any insider attempting to write this biography. The close family feeling that made for such good science has made for entertaining but superficial science history.

KOSTIA BERGMAN
*Department of Biology,
Northeastern University, Boston, MA 02115*

Delbrück "welcomed at Copenhagen Harbor as he arrives for the Polio Congress in September 1951." Left to right, Gunther Stent, Ole Maaloe, Delbrück, C. Bresdi, and James Watson. [From *Thinking About Science*; photo courtesy of Gunther Stent]

Genetic Rearrangements

Transposition. A. J. KINGSMAN, K. F. CHATER, and S. M. KINGSMAN, Eds. Published for the Society for General Microbiology by Cambridge University Press, New York, 1988. xvi, 375 pp., illus. $75. Symposia of the Society for General Microbiology, vol. 43. From a symposium, Coventry, U.K., April 1988.

The role of transposable genetic elements in reshaping the structure of prokaryotic and eukaryotic genomes is now generally recognized. The phenomena observed by McClintock in her early studies on chromosome rearrangements in maize are now understood as the effects of a family of transposable elements that includes *Ac* and *Ds*. The insertion sequences, IS elements, first identi-