

lations of his personal life. She draws on his personal correspondence, but mainly relies on interviews with Harlow's former students and colleagues. She follows Harlow from his rural Iowa upbringing, to his graduate work at Stanford, through the stages of his career at the University of Wisconsin. We see him turn from studying rats to studying primates, then from measuring primate intelligence to exploring primate emotions, particularly the love between mother and offspring. Blum describes Harlow's interest in tracing the whole arc of love, from healthy maternal response to neglect and rejection. She follows Harlow through two marriages and a divorce, his troubled relationships with his children, the death of his second wife, his alcoholism and depression. Two final chapters consider his legacy for present-day research and the objections of animal ethicists to his work, the latter an interest of Blum's from her 1992 Pulitzer Prize-winning book *The Monkey Wars* (Oxford).

In *Love at Goon Park* Blum is interested in two things—Harlow's experiments and Harlow's personality—and not in setting his science in its broader context, intellectual or otherwise. Harlow's ideas occur to him *de novo* (on an airplane, for instance) or else come out of his own personal experience or troubles: he starts to study depression in macaques when he himself falls into a depression. If there was a broader community interested in the biology of emotions at this time, Blum doesn't let on, and she certainly doesn't connect Harlow to it. Harlow is portrayed as opposing current trends—behaviorism, Gestalt psychology—but not as emerging from any scientific or social traditions himself. Behaviorism is presented as drowning out all interest in animal intelligence until Harlow appeared on the scene to revive it. Blum does reveal that Harlow corresponded and sympathized with John Bowlby, the British Darwinian psychiatrist, but does not explore his connections with ethology, Konrad Lorenz, or the mid-century revival of interest in evolutionary approaches to mind and behavior that led to sociobiology. Harlow is instead portrayed as an isolated genius; the striking similarity of his ideas to Bowlby's is attributed to coincidence.

Blum uses historical explanation inconsistently, as it suits her purposes. Her final chapter, on the opposition of animal ethics activists to Harlow's experiments, defends Harlow by explaining that standards for the ethical treatment of animals were established in later decades, after Harlow's death. We shouldn't expect researchers of the mid-twentieth century to "share

our social consciousness," she tells us (p. 302). Yet throughout the book Blum updates Harlow's idea about mother love to include fathers and caretakers, though it's far from clear that Harlow himself had any such liberalizing impulse. Why focus on *mother* love? Where did the idea of a maternal instinct for baby-tending come from? Why did it gain such currency at midcentury? How did the science of instinct gain such an important gender aspect? Those who are looking for answers to such historical questions as these will not find them in Blum's book. She includes telling historical details about the treatment of women in psychology in the 1950s (including both of Harlow's wives). But she dismisses Harlow's fixation on mothers with a wave of the hand: "this was the 1950s" (p. 149)—hardly a historical explanation. Blum spends half a chapter discussing the negative response of feminists to Harlow's work and to his habit of telling lewd jokes in lectures. But she seems concerned mainly to excuse and defend his behavior, not to historicize it. Harlow's purposeful baiting of feminists can be explained by his odd sense of humor, his scientific integrity, his personal unhappiness, or his political tin ear. If there is a broader theme here about the interaction between psychology and the women's movement at midcentury, Blum does not pursue it.

Blum has certainly captured the drama and fascination of Harlow's life and work, although she could have done with less purple prose ("smoke coiling like dreams" [p. 139] and the like). Psychologists and students of psychology who would like to have one of their heroes fleshed out will probably enjoy *Love at Goon Park*. But historians are likely to be disappointed by it.

NADINE WEIDMAN

**Frederic Lawrence Holmes.** *Meselson, Stahl, and the Replication of DNA: A History of "The Most Beautiful Experiment in Biology."* xii + 503 pp., illus., figs., index. New Haven, Conn.: Yale University Press, 2001. \$40 (cloth).

*Meselson, Stahl, and the Replication of DNA* is a book about scientific experimentation in the second half of the twentieth century. It is also, in itself, an experiment in historiography. Frederic L. Holmes, until his recent death a professor of history of medicine at Yale University, has published widely in the history of experimental sciences. His works on Antoine Lavoisier and Claude Bernard and, more recently, his two-volume biography of Hans Krebs are rightly considered essential contributions to the history of

science. This new book represents an extension of his previous studies toward more recent history and increasingly fine-grained historical analysis.

In 1953 James Watson and Francis Crick published their double-helix model of DNA. The most exciting feature of this structure was not the helix but the fact that it was double and that each of the DNA strands was complementary to the other. Watson and Crick stressed that their structure “immediately suggests a possible copying mechanism for the genetic material” (*Nature*, 1953, 171:737). This is approximately where Robert Olby’s now-classic *Path to the Double Helix* (Washington, 1974) ends and Holmes’s latest book begins. Already in 1953 scientists such as the physicist-turned-biologist Max Delbrück at Caltech pointed to the difficulties of separating two long and interwoven threads of DNA, a necessary step in making semi-conservative DNA copying possible. This came to be known as the “replication problem.” Delbrück suggested several alternative models for DNA replication that did not entail disentangling the DNA threads. Determining which one was correct became an important challenge.

Holmes explores how two young researchers, Matthew Meselson and Franklin Stahl, became involved with this problem. Meselson was a Ph.D. student under Linus Pauling, working in crystallography, and Stahl was finishing his Ph.D. in phage genetics under August G. Doermann. Their initial collaboration considered the mechanism of mutagenesis, with the replication problem only a side issue. Holmes follows both paths “symmetrically,” without giving priority to the one that resulted in the famous experimental denouement. Indeed, he has long been interested in reconstructing the far from linear “investigative pathways” that scientists have traced, changing directions, methods, problems, and organisms along the way. In October 1957 Meselson performed the ultracentrifuge experiment that provided the first clear-cut result indicating that bacterial DNA replicated in a semi-conservative way. This result was published a year later and soon made its way into most molecular biology and biochemistry textbooks. For many, the Meselson-Stahl experiment became an icon of experimental science—the “most beautiful experiment in biology,” according to a molecular biologist. Holmes goes beyond this apparent simplicity and makes the complex intellectual and experimental arguments involved clear to the reader.

Particularly fascinating is Holmes’s account of Meselson’s and Stahl’s ultracentrifuge work.

He describes in detail not only this “formidable machine” (p. 186) but also everything that goes into operating it. He highlights how much tacit knowledge, blind tinkering, and uncertainty was involved in carrying out these experiments. Holmes also underlines the difficulties of planning experiments on an expensive machine that has to be shared with other researchers, leading to the necessity to “borrow time” (p. 291) from colleagues. Finally, he shows how the ultracentrifuge methodology that Meselson and Stahl had first developed to solve an intellectual question became a driving force in its own right, orienting researchers in new directions.

Holmes also explores the “literary technologies” employed to convey experimental results to the public. Meselson and Stahl chose to present their work not as a test of Watson and Crick’s and alternative models of DNA replication but as an experiment that stood on its own. They wrote their paper “from the experimental results outward” (p. 375). Holmes then follows how this experiment has been described in successive textbooks and discusses the consequences of its different visual representations.

Holmes’s book is itself an experiment in historiography. He has done for experimental practice what Martin Rudwick did for scientific controversies in his classic volume *The Great Devonian Controversy* (Chicago, 1985): he presents the most meticulous microanalysis one could imagine. Holmes has gone further than most historians in exploring the micro level of experimental practice. His book on Krebs followed the day-to-day experimental life of that great scientist. With Meselson and Stahl, we are on an hour-to-hour basis. In the crucial moments almost every gesture and detail of experimental protocol is described. Such reconstruction was made possible only through the survival of an exceptional set of written archives and laboratory notebooks, as well as Holmes’s unique way of handling these documents and discussing them with their authors.

What are the results of Holmes’s experiment in historiography at this micro scale? He presents a very convincing argument that investigative pathways are “not linear,” that the “form and nature” of an experiment can change over time, and that a given objective is often “meshed in several other investigative goals” (pp. 437–438). Holmes is also able to convey effectively a sense of what it is like to tinker with complex experiments around the clock, without always knowing if one is moving toward a solution. Finally, along with the disciples of performative historiography, he has compellingly shown that experimen-

tation crucially depends on a set of embodied skills and what Otto Sibum calls “gestural knowledge.”

Within Holmes’s own framework, this book poses many stimulating questions. For example, How can the “investigative pathway” be articulated with other attempts to understand scientific experimentation, including Hans-Jörg Rheinberger’s “experimental systems” and Andrew Pickering’s “mangle of practice”? After Holmes’s masterly study, some will question whether this scale of analysis and the exclusive emphasis on individual scientists can still offer new insight into the nature of experimental life. In focusing not only on intellectual arguments but also on experimental practices, Holmes’s book is obviously of central importance to recent science studies. However, this “new internalism” completely shuts out the broader institutional, social, and cultural contexts. Holmes tactfully alludes to the human price paid by those who run an ultracentrifuge seven days a week—especially the costs for home and family life—but since he takes scientific creativity to lie with individuals, he leaves aside broader community relationships. Couldn’t we gain additional insights into Meselson’s and Stahl’s experimental practices by considering how they were able to build on the material and social resources of two exceptional communities (the phage group and the physical chemists around Pauling) at Caltech? Robert Kohler, in his study of *Drosophila* genetics, proposed one of the most convincing attempts to connect experimental systems to their broader cultural and social contexts. Holmes has now offered an irreplaceable starting point for those who would like to give it a try for more recent experimental life sciences.

BRUNO J. STRASSER

**Helen Rozwadowski.** *The Sea Knows No Boundaries: A Century of Marine Science under ICES.* ix + 410 pp., illus., bibl., index. Seattle: University of Washington Press, 2002. \$50, £37.95 (cloth).

The title of this book suggests the familiar theme that marine science is inherently international because its object of study exists outside national borders. The International Council for the Exploration of the Sea (ICES) promoted international cooperation with similar reasoning throughout the twentieth century, weathering the political strains of two world wars and a cold war. Helen Rozwadowski has written a thoroughly researched and accessible book that takes stock of ICES after its first one hundred years.

ICES began simply as the International Council, and its goals were practical. Northern Europeans wanted a scientific basis for sensible fishing regulations, to prevent the depletion of plaice, cod, and herring in neighboring seas. Many founding members were Scandinavians, whose economies depended heavily on fishing. Throughout its life, the council prioritized the rational, controlled exploitation of the sea as its first aim.

The council considered itself a scientific body, and in 1926 it established the *Journal du Conseil* in part to reaffirm its scientific foundations. But it made no pretense of promoting research for its own sake. Scientists such as Johan Hjort of Norway and Michael Graham of Britain provided theoretical tools to determine optimum catches, and fisheries scientists developed a sense of responsibility for keeping fisheries from becoming unprofitable. The most visible problem was the rapid growth of Antarctic whaling in the 1920s. Although it lacked the power to create restrictions, Rozwadowski credits the council for stimulating thought and action on whaling regulation from the 1930s onward.

The council’s pragmatism steered it through the stormy seas of the twentieth century. National animosities during the Boer War were set aside in favor of national desires to exploit the seas. The council barred Germany from membership after World War I, as did other international scientific bodies, but worked toward its return to ensure adherence to conservation recommendations. World War II halted work virtually everywhere, and the council’s general secretary died of dysentery in a prison camp. Yet the council saw opportunity amidst tragedy: it could study the effects of the wartime closure of fisheries on fish stocks.

In 1955 the International Council officially became ICES, another victim of the acronym plague that struck international science in the 1950s and 1960s. It organized the Polar Front Survey during the International Geophysical Year (1957–1958). But the IGY also stimulated the formation of global bodies that increasingly placed ICES on the periphery. Nevertheless, ICES benefited from the new climate of cooperation through new members, such as the Soviet Union, enabling ambitious regional studies such as the Overflow expeditions. The involvement of ICES in other research trends, such as investigations of environmental challenges and global climate change, is hardly surprising given the funds available for such research.

This book demonstrates that the strength of ICES was its ability to adapt. It was not bound