

The J. H. B. Bookshelf

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Jan Sapp, *Evolution by Association: A History of Symbiosis* (New York: Oxford University Press, 1994), xvii + 255 pp., \$55, \$29.95 (paper).

In an 1890 address before a gathering of fellow biologists at the Marine Biological Laboratory in Woods Hole, Massachusetts, University of Chicago biologist and MBL director Charles Otis Whitman announced that “on the same grounds that the sociologists affirm that a society is an organism, the biologist declares that an organism is a society” (p. 37). Whitman’s inversion of the “society as organism” metaphor made the biological social. His origin story of metazoan evolution, in which individual cells of the *Volvox* colony sacrificed their “personal independence for physiological union,” was a moral fable to remind colleagues of the importance of professional community in a period when the centrifugal forces of specialization were fragmenting the discipline of biology as a whole. This vignette captures one of the central and most telling themes of *Evolution by Association*: how ever-increasing specialization affects the life sciences. By situating the history of symbiosis within the interstices of biological specialization, Sapp has written a compelling historical narrative that transcends disciplinary allegiances. In this important book, the history of symbiosis becomes a powerful means to reassess the predominant conceptions of life in the twentieth century, bolstered by concepts, techniques, disciplinary boundaries, and the metanarratives centered around Darwinian evolution, Mendelian genetics, and the germ theory of disease constructed by historians and biologists alike. *Evolution by Association* is required reading for all historians of twentieth-century life science.

To classify life and to define biological autonomy are both acts that inscribe a set of social relations upon the biological realm. Accepted biological

categories dim these historical relationships, but where the boundaries of the organism are not clearly defined, social dissensions can come into full view. Lichens represented one such problem organism. When the Swiss botanist Simon Schwendener suggested in 1866 that lichens were a parasitic relationship between a fungus and algae, he threatened, in Sapp's words, "not only existing methods of classification but the hard-won autonomy of lichenists themselves" (p. 6). Many botanists questioned Schwendener's portrayal of the fungus-algae association as a master-slave relationship. Lichens were but one example of a wide range of complex associations, from antagonistic to mutualistic, that existed between unlike organisms. In 1878, the German botanist Anton de Bary introduced the neutral term "symbiosis" to describe this range of associations. But, as Sapp demonstrates, symbiosis never remained a neutral category. Symbiosis researchers, such as the University of Edinburgh biologist Patrick Geddes and the University of Illinois lichenologist Albert Schneider, equated these associations with mutualism in response to the emphasis on conflict and competition within Darwinian evolution. Indeed, symbiosis offered a creative source for evolution in a period when natural selection was viewed primarily as a destructive force.

Sapp argues that interpretations of symbiosis as mutualism found a supportive climate in Gilded Age opposition to Social Darwinism, during the First and Second World Wars, in the politically tense period of the Cold War, and during the rise of environmentalism in the 1970s. In an early chapter on "The Meanings of Mutualism," Sapp elucidates the inherently social nature of biology in exploring key words such as "mutualism" and "inheritance," tracing the roots of such metaphors to political movements and jurisprudence. But the social relations that he brings to the foreground in his analysis are largely those of priority disputes and disciplinary contestations found within the professional sphere. As his story becomes entwined with the increasing specialization of biology in the twentieth century, his analysis of the deeper political and cultural meanings becomes more superficial, less attentive to the changing meanings of key words such as "cooperation" and "individuality" within particular sociopolitical contexts. For instance, a number of American biologists took issue with symbiosis concepts during the First World War, not because they did not support mutualism, but because the nature of individuality implicit in symbiosis accounts conflicted with American ideals of community and the common good. This is evident in the criticisms that William Emerson Ritter, director of the Scripps Institution for Biological Research, launched against Samuel Jackson Holmes's theory of the organism as a symbiotic aggregation in 1919. Ritter argued that because each component of the symbiotic association reproduces itself, individuality never extends

beyond the constituent parts. He expressed a vision of democracy, commonly upheld during the Progressive period, that the common good stood prior to and supportive of individual identity. In a book that covers 150 years and a wide array of national contexts, Sapp's uneven cultural analysis is forgivable and serves to illustrate the daunting task facing the historian who tries to integrate the social relations of science studies with those of the cultural historian. Nevertheless, Sapp's work opens an expansive terrain by illuminating how human conceptions of individuality, derived in part from political and legal theory, helped shape definitions of individuality within the biological realm.

The proliferation of experimental work in cytology and embryology in the late nineteenth century offered another venue in which symbiotic concepts were entertained. If, as theories of the cell state implied, the organism was a community of individual cells bound together by mutual dependence and interaction, perhaps the cell itself was a symbiotic association of individual organelles. In the 1890s, Gottlieb Haberlandt suggested that chloroplasts originated as symbionts, while Shosaburo Watasé viewed the cytoplasm and the nucleus as a symbiosis of phylogenetically distinct organisms. Sapp suggests that the increasing specialization of the biological sciences and nucleocentrism, reinforced by the Mendelian theory of inheritance, lessened interest in the potential role of symbiosis in evolution. Techniques for staining chromosomes, for instance, precluded the investigation of important cell organelles such as mitochondria that biologists had pointed to as potential symbionts.

While nucleocentrism and the technique-ladenness of observation proved obstacles to symbiotic theories of the cell, the germ theory of disease offered another source of opposition. Sapp keenly demonstrates how the Pasteurian dogma of aseptic healthy tissue and the medical perspective of bacteria as disease helped to undermine the unifying theory of French biologist Paul Portier that all organisms, except bacteria, were symbiotes. In the interwar years, symbiotic researchers often ignored one another, working in a climate of competition rather than one of solidarity. Experimental studies of symbiosis fell between the cracks of specialized disciplines such as ecology and evolution, which, Sapp maintains, treated symbiosis as an exception. However, he may overstate the extent to which ecologists in the interwar years excluded interspecific symbiotic associations from research on mutualism. The ecologist Warder Clyde Allee in his 1931 book *Animal Aggregations*, for example, wrote favorably of Ivan Wallin's theory of symbiointicism – a theory that was, according to Sapp, virtually ignored.

The visibility of symbiotic research changed after World War II, Sapp reveals, as the study of cytoplasmic genetics expanded both the "definition of heredity to include infectious entities and the concept of the organism to include symbiotic complexes" (p. 209). By the 1980s, the symbiotic theory of

the eukaryotic cell had developed into a respectable research program, had led to the establishment of an international society, and was accompanied by the founding of a journal – all indicators of a discipline in formation. However, Sapp warns his reader, contemporary studies of cell evolution that confirm the symbiotic origins of mitochondria and chloroplasts differ significantly from late-nineteenth-century symbiosis theories: narrowly circumscribed, contemporary accounts offer no challenge to bacteriology, no claims for the role of symbiosis in the origin of species. Regarded as a rare phenomenon by neo-Darwinian evolutionists and ecologists, symbiosis has not altered the consensus about the nature of evolutionary change. Studies of symbiosis have found a niche in the organization of life in the twentieth century; they have not fundamentally restructured the economy of nature.

In 1952, Joshua Lederberg suggested that “fixed conceptions of the scope of the organism” contributed to the barriers that existed between the biological disciplines (p. 148). Sapp’s history of symbiosis is powerful testimony to the ways in which the cultural, political, and professional meanings invested in definitions of the organism have fundamentally shaped and limited the nature of biological science in the twentieth century.

Gregg Mitman

Evelyn Fox Keller, *Refiguring Life: Metaphors of Twentieth-Century Biology* (Columbia University Press, 1995), xix + 134 pp., \$20.

This is a case in which one can indeed judge a book by its cover. Pixellated electric chromosomes spread out over the book’s surface, while a line runs horizontally across the middle of this slim and elegant volume. At the line the background changes from black to white, with the chromosomes reversed out; the back cover flips the pattern. In the pages between, Keller is writing on the line, that infinitesimal border where momentous changes occur and black becomes white, internal becomes external, language becomes material, machines become messages, genes become “a parallel computing network embedded in the global geometrical and biochemical structure of the cell,” (p. 28), and intellectual history becomes social history, literary theory, philosophy, and cultural analysis. And, of course, vice versa. In short, the book is an artfully crafted assemblage of both borders and border crossings as they occur in history and in disciplinary methods, and a lyrical examination of how limits and their transgression are each productive in their own ways. It should become a vital line in any course syllabus in the history of the life sciences, or in feminist and cultural studies of science.

The three essays collected here were presented originally as the Wellek Library Lectures at the University of California at Irvine, deservedly placing Keller in a lineage that includes Jacques Derrida, Jean-François Lyotard, Hélène Cixous, and Fredric Jameson. Like the chromosomes on the cover, with their sporadic bright spots signaling where a fluorescent probe has hybridized in situ to the genetic material, Keller probes with these essays a number of strategic sites of writing and experimentation in the convoluted histories of modern genetics, developmental biology, and cybernetics. In “Language and Science: Genetics, Embryology, and the Discourse of Gene Action,” she tracks the material, social, and linguistic forces that kept the dominant conception of gene as “part physicist’s atom and part Platonic soul” (p. 9) so productive (and, by necessity, so limiting) for so long, as well as how its most recent successful expression in the Human Genome Project may ironically be one of the factors leading to its own dissolution and to “the long-awaited rapprochement of genetics and embryology” (pp. 24–25). Why Erwin Schrödinger’s collapse of the organism into a codescript resembles the mirage of subjectivity analyzed by Lacan, and, in another ironic result of the crossing of lines that defined both “life” and “thought,” how “Thought and Life both have been thoroughly dispersed on the winds of information” (p. 78) – these are some of the subjects broached in “Molecules, Messages, and Memory: Life and the Second Law.” Finally, in “The Body of a New Machine: Situating the Organism between Telegraphs and Computers,” Keller discusses how the interplay of machines and their metaphors over time have brought us to where

today’s biological organism bears little resemblance to the traditionally maternal guarantor of vital integrity, the source of nurture and sustenance; it is no longer even the passive material substrata of classical genetics. The body of modern biology, like the DNA molecule – and also like the modern corporate or political body – has become just another part of an informational network, now machine, now message, always ready for exchange, each for the other. (pp. 117–118)

Keller summarizes her previous book, *Secrets of Life, secrets of Death* (Routledge, 1992), by stating that “Scientists muddle through with staggering success. Our task . . . is to make sense of the successes of science in terms of the particular linguistic and material conventions that scientists have forged for their sorts of muddling through” (p. 181). The current volume provides further essential tools for all of us life scientists – whether we work in laboratories or libraries – to answer the question, “How are we going to write (about) life now?” in the most open way possible: Go figure.

Michael Fortun

Janet Browne, *Charles Darwin: A Biography*, vol. I, *Voyaging* (New York: Knopf, 1995), xiii + 605 pp., \$35.

[The idea for this review began when an evolutionary biologist sent the book to a historian friend along with a copy of a letter to the author saying that he had opened the book thinking he could learn nothing new about Darwin, but had shortly learned otherwise. Indeed, he added, “you make the characters I have read about since I was a boy suddenly come alive. Never before had I really understood Fitzroy, or Lyell, or Dr. Darwin or Emma and many others. Especially I realized that I had never quite understood Darwin himself.” The historian, who has no special knowledge of zoology but a long interest in biography, was intrigued. She was especially intrigued by the biologist’s suggestion that the book could only have been written by a women. . . .]

Type “Darwin, Charles” into the computer catalog of any good university library and – whether you ask for subject, author, or title – hundreds of entries will appear. More than a century after his death biologists debate, elaborate, refine, and discuss; biographers puzzle.

The driving force behind much biographical writing is the question, What does the life (whether of artist, political figure, or scientist) have to do with the work? And since human beings are more complex and present more variables than the most gifted biographer can encompass, few biographies can be considered definitive once and for all. There are always new questions to ask, new answers to seek.

Janet Browne worked for years editing Darwin’s correspondence. In the process of such immersion she came to feel that despite the millions of words already in print there was more to be said about Darwin than had yet been said. In her introduction she observes that “remarkably little attention has been paid to the way he lived out his life” as a member of Victorian society; few of his biographers, she said, “attempt to paint a picture his wife or friends might recognize” (p. xi).

This, then, is what Browne set out to accomplish. No review can fully describe a work of this length and complexity, but the high points may be noted. Inevitably, given the great interest in the subject, parts of the story are well known, although not always well understood. The man who emerges from this work is not quite the one that most biologists thought they knew. Browne does not neglect Darwin’s development as a scientist, but she goes much beyond that to deal with his whole milieu – shaped as it was by childhood experiences, especially the two traumas of his mother’s death and his departure for boarding school, by the constant oversight of his older sisters, and by the close relationship with his brother, Erasmus, with whom

he conducted his first scientific experiments. His search for scientific data always reached beyond trained scientists to people whose daily experience might be relevant: beekeepers, horse breeders, and market gardeners were all sources of information. He benefited from the connections provided by a large and prominent family, especially connections to the small intermarried circle that constituted British science of the time.

Browne is blessed with a gift for writing easy, luminous English prose. Trained in the history of science, she brings to her study a sure understanding of the scientific issues Darwin faced. Along the way she provides a dazzling introduction to life in a certain stratum of Victorian English society. She understands – as she thinks Darwin himself did not – that figures like him “were the products of a complex interweaving of personality and opportunity with the movements of the times. . . . The Darwin brothers,” she writes “felt themselves part of a larger concern with the fundamental powers of matter that gripped Europe in the first decades of the nineteenth century” (p. 32). She speculates that the social and economic environment in which Darwin grew up, one in which competition was seen as the principal engine of progress, may have provided one clue that led to the idea of the survival of the fittest.

Browne, as Darwin biographers must be, is aware of what seems to be a mystery: how could a man who seemed to be so conventional, so ordinary, write a book that shook up not only the scientific world, but people everywhere, including many devout souls who had only the vaguest idea of the nature of scientific hypotheses or of scientific proof? By paying close attention to such disparate sources as Darwin’s meticulously kept account books – every penny accounted for through every year of his post-Beagle life – his private musings on subjects not scientific, letters from his family, and patterns of behavior, she achieves a three-dimensional portrait of the man. In a marvelously amusing section she describes the pragmatic analysis “to marry or not to marry” that led the young scientist to conclude that he really must have a wife. Darwin’s experience reveals an unexplored version of the Victorian marriage market: one that focused not on money but on brains, as leading scientists educated their daughters with an eye to attracting the most promising younger men into their families. The zoologist Leonard Horner, who had succeeded in marrying one daughter to Charles Lyell, very much had his eye on Darwin – his single daughters were encouraged to read science and natural history to prepare for such an eventuality. Darwin, however, was not in search of an intellectual companion; preferring a traditional Victorian wife, he proceeded to a matter-of-fact courtship of his cousin Emma Wedgwood, whom both families had long since decided he should marry. After a brief huff the Horners forgave him; if he was not to be a son-in-law, at least he could be a friend of the family.

Darwin loved solitude, but required a complex support system for its maintenance. He was not, Browne writes, a man who could ever stand completely alone. He depended on his father for financial support, on friends for moral support as well as for all kinds of help in various scientific explorations, and on his wife to manage a growing family and an increasingly complex household. He was a most un-Victorian father, loving and expressive with his children, whom he often mobilized as assistants in his research. He was a fond, if preoccupied, husband, but one who never expected, or apparently desired, his wife to understand his work.

Darwin's lifelong ill health has been the subject of steady interest and considerable analysis. In Browne's view it was partly an organic problem, partly a response to circumstance. (He was, for example, one of these men who experienced labor pains when his wife gave birth.) Nor was ill health altogether without use for his intellectual development, since it provided him with a good reason to avoid spending time on things he did not value. It justified long periods of working alone that might otherwise have been seen as simply selfish. Ill health did not prevent him from procreation: at one point there were seven children under age eleven.

Janet Browne's careful picture shows a man who, despite outward appearances, in essential ways was anything but ordinary. People of unquenchable curiosity are far from common; so are people who can truly say "My love of natural science has been steady and ardent." Even less common are those who, given these characteristics, are also willing to address questions of great range and magnitude. Combined with his mental adventurousness, however, there was a kind of caution – whether in deciding to marry, or in deciding to publish his ideas about the struggle for existence and natural selection.

After more than five hundred pages Browne reaches the moment when – after long delay and much soul searching – Charles Darwin, approaching age fifty, at last decided to publish his ideas about evolution and began to write what would become *The Origin of Species*. At this point, the volume ends. One can only hope that volume II is soon to be published.

[Could this book only have been written by a woman? One can only say that it *was* and that none of the biographers before her had managed so believable a picture of the man. She noticed many things that had not been noticed before, and this in part accounts for her success. Perhaps women from their own experience notice things that men, especially scientists focused on Darwin as scientist, do not observe.]

Anne Firor Scott

W. Conner Sorensen, *Brethren of the Net: American Entomology, 1840–1880* (Tuscaloosa and London: University of Alabama Press, 1995), xvi + 360 pp., illus., \$59.95.

The science of entomology, or the study of insects, has been largely neglected by historians of biology. This condition may have arisen from perceptions of the strictly taxonomic or utilitarian labors conducted by entomologists, along with a purported absence of theoretical investigations. In *Brethren of the Net*, independent scholar W. Conner Sorensen conclusively demonstrates how and why those assumptions are flawed, and argues convincingly that American entomologists achieved world leadership in their discipline through social as well as scientific developments.

Previous literature on the history of entomology was written chiefly by practicing scientists, and embodied encyclopedic compilations on the specialty's "great men" or chronicled numerous battles waged against insect pests. From an exhaustive survey of entomologists' correspondence and published writings, Sorensen has constructed a thorough and accurate portrait of the leading issues affecting patronage, professionalization, and the divergence of entomological styles. His analyses of the debates waged over natural selection – based on entomologists' observations and experiments on mimicry, seasonal dimorphism, and coevolution of insect-plant relationships – are masterfully presented. But the major strength and significance of Sorensen's contribution rest upon its offering the first truly synthetic, *community*-level study of American entomology.

From only a handful of researchers active in the 1840s, entomologists grew in number to encompass a community of roughly 900 practitioners by the 1880s, including a core of 100 central figures. Sorensen attributes the remarkable growth of entomology during this period to three primary factors: (1) the establishment of scientific institutions and collections equal to the best European representatives; (2) the pursuit of entomological research after 1860 to provide confirmation of Darwinian evolutionary theory; and, most importantly, (3) the commercialization and mechanization of agriculture, which fostered mutually supportive relationships between professional entomologists and agriculturalists (pp. 256–257).

Using the devastation wrought by the Rocky Mountain locust plagues as a springboard, Sorensen skillfully characterizes the social as well as scientific struggles that entomologists faced in trying to achieve recognition and reforms within the U.S. Department of Agriculture. His research fully supports the notion of A. Hunter Dupree, who first identified this "problem-centered" approach with the establishment of federal scientific bureaus. The U.S.

Entomological Commission became a model upon which later organizations were structured and funded.

Sorensen has amassed extensive numerical data on which to base a solid reconstruction of the entomological community around 1870. All relevant parameters concerning the background and training of these scientists are compared with other quantitative studies performed on the American scientific enterprise. A novel feature of Sorensen's assessment is the numerical ranking he assigns to its publishing elite, using the priority-grid methodology of applied psychology. He concludes that entomology was among the last of the scientific disciplines to erect boundaries between "amateur" and "professional" workers: much valuable data concerning insect life histories were gathered through the cooperative efforts of talented nonprofessionals, including notable women investigators, who reported findings to state and federal leaders.

However, Sorensen does not adequately convey the lasting influence that Paleyan natural theology exerted upon British entomology, from its formalization in 1833 to the years beyond the publication of Darwin's *Origin*. He asserts that American entomologists paid little if any attention to the design argument, apart from its application to the "balance of nature" ideology. If so, then we might glimpse a more convincing explanation for the rapid acceptance of evolutionary thought by the Americans. Sorensen attributes this distinction to other social and cultural factors, including greater field orientation, the prominence of *agricultural* entomology, and freedom from class distinctions (pp. 210–211). Those reasons appear questionable, because Sorensen's "typical" American entomologists arose from families who possessed strong Protestant backgrounds and received apprenticeship-style training from tutors with British entomological experience (pp. 182–183). Sorensen also originates an error concerning the zoological illustrations of Thomas Say's wife, Lucy: he states (p. 191) that Lucy drew at least some of the figures from which the plates for Say's *American Entomology* were engraved, but in actuality, Lucy only provided illustrations for Say's *American Conchology*.

Despite these minor complaints, Sorensen is to be congratulated for having assembled such a comprehensive, yet fascinating, account of the maturation of this neglected American scientific community. The standards of scholarship and interpretation employed are generally excellent. This work will remain a valuable reference and model for all future historians of American entomology.

Jordan D. Marché II

Tim D. Smith, *Scaling Fisheries: The Science of Measuring the Effects of Fishing, 1855–1955* (Cambridge: Cambridge University Press, 1994), xii + 392 pp., illus., maps, \$74.95.

For scholars interested in the history of natural resource management, fishery biology, or population ecology, Tim Smith has crafted a useful and suggestive study. He traces the historical development of scientific methods for assessing and explaining the condition of fish populations, and he concentrates on organizations and individuals involved in managing North Atlantic and Northeast Pacific fisheries. Luminaries such as Georg Sars, Spencer Baird, Johan Hjort, William F. Thompson, and William Ricker figure prominently, but Smith rarely flirts with hagiography; for this is ultimately a story of failure.

In the second half of the nineteenth century, concern about the perceived decline of various North Atlantic fish populations led European and North American governments to commission scientific investigations. These inquiries revealed a gaping ignorance of even the most basic aspects of marine biology. The findings became springboards for launching government-sponsored agencies to investigate, quantify, and manage contested fisheries. By 1900 the U.S. Fish Commission and the International Commission for the Exploration of the Sea had grown considerably, but they still could not measure population fluctuations accurately, let alone assign causation with certainty. Mounds of empirical data had not illumined what Spencer Baird had hoped would be the “general principles” (p. 197) of population fluctuations that everyone recognized but no one could explain persuasively.

Fishery scientists needed a method for understanding these changes without having to make interminable measurements – or, to paraphrase William Thompson, they needed a way of “tunnelling the mountain” without “removing it in its entirety” (p. 181). Thus biologists turned to a priori methods. They adapted mathematical models to fishery questions, and by the 1950s three partial theories had emerged to help explain population dynamics: *surplus production theory*, *spawner and recruit theory*, and *yield per recruit theory*. Biologists used these theories as heuristic tools, but managers misconstrued them as reality, overemphasized their predictive value, and implemented disastrous harvest policies (pp. 266–267). The collapse of California’s sardine fishery during the 1950s is a classic example of this fallacy.

Shaping the development of these methods was a political economy that forced biologists to render hurried answers within a management framework unwilling or unable to restrict economic activity. Smith mentions these forces in the introduction and conclusion, but his treatment is superficial. He does not adequately document the impact that management priorities had on the vector of research and institutional development in specific instances. The result is

a sometimes skewed depiction of the development of science. For example, Smith credits Baird with realizing the need for a coordinated research program to find “general principles,” but he never reconciles Baird’s words with Dean Allard’s observation that Baird often diluted Commission activities to further Smithsonian agendas. Instead, Smith attributes the eclipse of science in the Commission to the appointment of fish culturist Marshall McDonald in 1887 (p. 66) – a reasonable observation, but not wholly explanatory.

Smith’s reliance on published material during his research contributes to these gaps, but it is a limitation he readily acknowledges in the introduction (pp. 6–7). The resulting lacunae are significant but hardly fatal. Smith possesses an admirable ability to explain the development and significance of scientific methods. *Scaling Fisheries* adds considerably to our understanding of the historical development of fishery biology.

Joseph E. Taylor III

Edward J. Larson, *Sex, Race, and Science: Eugenics in the Deep South* (Baltimore: Johns Hopkins University Press, 1995), ix + 251 pp., illus., \$35.

Given southerners’ long-standing obsession with racial purity, one might have reasonably expected the region to have embraced the American eugenics movement from its birth at the dawn of this century. In particular, it is not too hard to imagine eugenic sterilization being promoted as yet another weapon in an already well-stocked arsenal aimed at controlling the region’s large African American population. However, as Edward Larson points out in this fascinating book, the story of eugenics in the South is far more complex than might first appear. For example, until the postwar civil rights movement, eugenic enthusiasts in the states of South Carolina, Georgia, Alabama, Mississippi, Louisiana, and Florida – the so-called Deep South on which Larson focuses his attention – seemed much more concerned about protecting and purifying the Caucasian race than about enforcing eugenic policies on African Americans. Larson argues that as far as southern eugenicists were concerned, Jim Crow segregation, strong social sanctions against interracial marriage, and strict miscegenation laws effectively neutralized any threat that southern blacks might pose to the white germ plasm.

The spread of eugenic policies in the Deep South was also slower and implementation less thorough than might be initially expected. In the first decade of the twentieth century, when “hyperactive experts” (p. 30) in the North began lobbying for immigration restriction, marriage laws, sexual segregation in state-supported institutions, and compulsory sterilization to protect America

from the “menace of the feeble-minded,” discussion of eugenics in Dixie was rare. Eugenic arguments for immigration restriction never gained a foothold among the relatively homogeneous white population of the region, though several southern congressmen did advocate the complete suspension of all immigration to this country, usually on more traditional nativist grounds. Nor did eugenic marriage laws ever make headway in the region, even though more than half the states in the rest of the nation had enacted such restrictions by 1914. According to Larson, when it came to proposals to regulate who was fit to marry, eugenic enthusiasts failed to surmount strong southern feelings about the sanctity of the family and the “private right of men and women” (p. 98) to join together in holy matrimony.

Despite these setbacks, eugenicists in the Deep South did eventually succeed on two fronts. By 1920, mental health officials, private physicians, civic leaders, and members of politically active women’s organizations from every state in the region joined together to secure the creation of sexually segregated public institutions for the mentally retarded. Yet, even this victory was only partial: at no time in the succeeding decade did the commitment rate at these newly established institutions exceed one-half the national average. By the early 1930s, when two dozen states across the nation initiated eugenic sterilization programs in the wake of *Buck v. Bell*, only one jurisdiction in the Deep South – Mississippi – enacted a comprehensive sterilization statute. Not until 1935 and 1937 did South Carolina and Georgia become the final two states in the nation to pass eugenic sterilization laws.

What eugenicists in the Deep South lacked in speed, they soon made up for in enthusiasm. After World War II, when national eugenic sterilization rates began to decline (dramatically so by the 1950s), several states in the region finally overcame objections to the controversial practice based on religious arguments and concern for individual rights. By the late 1950s, Georgia, North Carolina, and Virginia were responsible for three-fourths of all compulsory sterilizations performed in the United States.

Not only did the number of eugenic sterilizations in the Deep South increase during this period, but in several states the target of those operations also began to change. According to Larson, it was during this postwar period, when “the civil rights movement began dismantling the machinery by which southern whites controlled local blacks” (p. 2), that eugenic practices were finally turned against the blacks. By far the most extreme example is South Carolina, where, in the years between 1949 and 1960, 102 out of 104 enforced sterilizations were performed on African Americans. Yet in other southern states many, if not most, of the victims of eugenic sterilizations continued to be white. In the end Larson is less thorough and less convincing in his

explanation of the region's growing interest in eugenic sterilization than he is in accounting for the strong initial resistance to the idea.

Larson has mined a number of rich sources – including newspaper accounts, records of legislative proceedings, journals of medical societies, and annual reports from state institutions – to reconstruct the history of eugenics in the unreconstructed Deep South. However, given the limited geographic scope of his study, he has passed up a golden opportunity to find out more about who the victims of eugenic sterilization laws were. With the exception of Carrie Buck, we still know almost nothing about the more than 60,000 American citizens who fell prey to experts claiming the authority of science for their dubious practices. Yet this is hardly a fatal omission. *Sex, Race, and Science* represents a valuable contribution to our understanding of the eugenics movement, particularly as it relates to policymaking and implementation in the Deep South.

Mark V. Barrow, Jr.

Gerald L. Geison, *The Private Science of Louis Pasteur* (Princeton: Princeton University Press, 1995), xiv + 378 pp., illus., \$29.95.

In the first lines of his recent book on Louis Pasteur, Gerald Geison asks the question that instructs every chapter in what follows: Why, other than to mark the 100th anniversary of his death, should another biography of Pasteur and his contributions to science and medicine appear at this time? The simple answer is that Pasteur's laboratory notebooks were made available for scholarly research just ten years ago.

For many years the standard English reference work on Pasteur was *Louis Pasteur: Free Lance of Science* (1950) by the French émigré and eminent scientist René Dubos, but with the publication of Geison's essay in *The Dictionary of Scientific Biography* (1974), scholars relied on his scrupulously researched sixty-six-page examination of Pasteur's scientific work and influence that was grounded in the collections of all Pasteur's publications and virtually all his professional and private correspondence. More recently, new lines of historical inquiry have challenged the usual accounts of the historic circumstances that lodged Pasteur, the resolute scientist with his swan-necked flasks, front and center in the pantheon of scientific idols. The sociologist of science Bruno Latour focused on how nineteenth-century French sanitarians or hygienists, modestly camouflaged as Pasteurians, first overcame their own political impotence and then gave Pasteur and his assistants the opportunity to spread their science. But as Geison points out, we still know very little of

how Pasteur's professional and public conduct itself formed and perpetuated the image and reputation that inspired succeeding generations outside France.

Although the heroic legend of Pasteur's powers has been tempered by changing times and historiography, only now is it possible to systematically study his laboratory notebooks, the records of what Geison calls Pasteur's "private science." Throughout his life Pasteur sequestered these journals of observations made daily in the laboratory and their implications for further experiments, plans, and projects, even taking the current notebook with him whenever he was away from Paris. He then instructed his family to prevent their examination after his death, and he certainly intended to exclude the content of his working notes from publication. Pasteur's grandson, who edited the scientific papers, books and correspondence for publication, retained the notebooks until, in anticipation of his own death, they were deposited in the Bibliothèque Nationale and were eventually catalogued in 1985. It is these laboratory notebooks that provide the main additional resource and the *raison d'être* for Geison's new examination of the history that generated "the myth of Pasteur, [that] like all myths, embodies important elements of the truth" (p. 277).

Historians sometimes describe their research strategy as "reading other people's mail," savoring the implications of passing for insiders, and access to Pasteur's laboratory notebooks – unique and every bit as enticing as correspondence – might easily excite similar illusions of penetrating privacy. Geison is careful to explain that these notebooks are not "privileged" in the sense that the text breaks through the barriers of Pasteur's sensibility and culture, but rather that they entitle Pasteur to speak in a voice different from that heard in public. Although Geison periodically reminds us that scientists and scientific knowledge reflect their own times as well as the stamp of a particular scientist or scientific discovery, the way in which Pasteur, the man who "revolutionized science," was incorporated into the history of modern science often confounds his efforts. Just imagine how perverse it would seem to organize the story of Pasteur's science around the history of animal experimentation in France.

Accordingly, in seven of his ten chapters Geison follows Pasteur through crucial episodes of his scientific career, in much the same way as has every earlier biographer (beginning with his son-in-law). A combination of the chronology of crucial experiments, scientific triumphs, and public recognition is apparently hard-wired into the Pasteur story, and given the sheer volume of laboratory notebooks (102 in the archive, plus one that has disappeared but is available on microfilm), Geison could hardly afford to stray from the established path. First come Pasteur's investigations of the relationship between optical activity, crystalline structure, and organic compounds, work

begun as a student of physics and chemistry, which led him to assert the connection between optical activity and life. Within the next decade he took up the study of fermentation, and again reported experimental evidence of biological processes – microbial activity that contested Liebig’s virtually unchallenged work ascribing fermentation to chemical activity. Next Geison evokes the key symbols of Pasteur’s enduring fame: the demonstrations that evidence of spontaneous generation was the result of faulty experiments and tortured reasoning, the consummating drama that revealed the vulnerability of disease carrying microbes to scientific mastery – Geison concentrates on the public trials of protective anthrax vaccine – and the obligatory finale of rabies treatment.

In important respects the notebooks vividly corroborate Dubos’s and Geison’s learned accounts of the “public science” and the rising tide of Pasteur’s ambitions, as well as his self-portraits of a life totally committed to science. But while Dubos and Geison documented the magnitude of Pasteur’s scientific achievement and public reputation, they were less convinced that his investigations were chained to the “inflexible logic” that Pasteur claimed to have discovered in the relationship between optical activity, life, fermentation, and disease. They celebrated his distinctions and credited some of his genius in the laboratory to his technical superiority, his visual acuity (and occasionally his nearsightedness), and his superb intuition. Nonetheless, both Dubos the scientist and Geison the historian of science stood far enough back from the workbench to see the occasional contribution of the hopeful experimenter to the discovery of what was expected.

This background makes it less than surprising to learn that Pasteur’s laboratory notebooks give implicit (and sometimes explicit) evidence of conflicts with published accounts of his scientific discoveries and contributions to medicine. Geison writes that he is only one of several Pasteur scholars who have found that the notebooks expose versions of experiments and demonstrations that differ from what was reported in public. When Geison compared celebrated scientific reports and the laboratory notebooks covering the relevant research, Pasteur’s record of his “private science” revealed unacknowledged debts to colleagues and competitors, misrepresentation of experimental results, and instances of what Geison believes was personal conduct that violated the public trust that Pasteur solicited (and that protected him). In this account, Pasteur’s pursuit of recognition and of support for his scientific work and his self-promoting public discourse, even if unavoidable, accentuate a picture of iniquitous complicity rather than unfortunate negligence. Geison is the leading American Pasteur scholar, and as such he reports on the laboratory notebooks with understandable feeling and a dramatic flair that conveys the sense that he is unraveling the evidence in front of the reader,

focusing the eye on connecting events. As is true for all historic documents, establishing these notebooks' significance is the important and problematic task.

Geison's research on the conduct and impact of Pasteur's laboratory experiments addressed questions that had already challenged him when he wrote, for the *DSB*, that Pasteur's "contributions to basic science were extensive and very significant, but less revolutionary than his reputation suggests. The most profound and original contributions are also the least famous" (*Dict. Sci. Biog.*, 10: 351). The notebooks gave vivid evidence of different circumstances and practices that compromised Pasteur's claims to originality and credibility over a lifetime; nonetheless, read in the context of his "public life" they confirmed the bases of his extraordinary scientific success.

Historians have found it useful to "complicate" the received historical narrative, Geison's reading of Pasteur's laboratory notebooks leads to a peculiarly decontextualized history. Absorbed in fitting together the pieces of an immensely productive scientist's life, he slights or simplifies the circumstances that give history deep meaning. He notes the intensely competitive scientific environment and the role of patronage in establishing the conditions of Pasteur's work, but he gives not enough attention to the particulars of intellectual and cultural rivalry. With the exception of a concise and carefully referenced review of the cultural wars identified with debates over spontaneous generation, Geison plays out the drama with a cast largely limited to immediate participants. The revelation of resentment among Pasteur's assistants, for instance, leads back to speculation about how Émile Roux's clinical sensitivities conflicted with Pasteur's insistence on validating research in the laboratory, and ignores Roux's early work on diphtheria antitoxin and research connections outside the Pasteur Institute.

At the end of the final chapter, Geison briefly compares the questions that informed his and others' readings of the laboratory notebooks. For the Italian scholar Antonio Cadeddu, Geison suggests, "the basic lesson is ultimately an epistemological one," but for himself "the central point has to do with Pasteur's public presentation of self, and is thus closely related to the historical myth of Pasteur" (p. 277). Geison argues that his reading of the notebooks as evidence of Pasteur's "private science" makes it possible to "deconstruct" the mythological proportions of Pasteur's achievement, to separate Pasteur's heroic inventions from his remarkable and clearly consequential scientific contributions. But this dismemberment of Pasteur's life in the interest of revealing truth distorts history, and paradoxically places the work of scientists outside history's realm.