BOOK REVIEWS

■ **Feature**

Michael Hunter. *Robert Boyle (1627–91): Scrupulosity and Science*. x + 293 pp., frontis., app., bibl., index. Woodbridge, U.K./Rochester, N.Y.: Boydell Press, 2000. $90 (cloth).

A new biography of one of the founding fathers of the Scientific Revolution, Robert Boyle, is no easy undertaking, but no scholar is better poised to give us a revisionist view of this iconic figure than Michael Hunter. For fourteen years Hunter, together with Edward Davis, supervised the definitive fourteen-volume edition of Boyle's complete works, published and unpublished. This was the first such undertaking since the 1744 edition compiled by the cleric and antiquary Thomas Birch. (See the Essay Review of this work by Margaret Osler, *Isis,* 2001, *92*:351–353.) Almost no Boyle scholar has been as privileged—or as dogged—in probing every bit of Boyle's legendary scribblings as Hunter. With his new biography, which gestated for decades and reflects the accumulating wisdom gained only with disciplined, persistent, and wide-ranging reading, Hunter has shed new light on Boyle's life and character. Specialists will delight in having Hunter's path-breaking articles gathered together in one volume from journals as far ranging as the *Journal of Ecclesiastical History* and *Medical History* to the more standard venues of the profession.

Which Boyle have historians of early modern science more recently encountered, and how does Hunter's book enhance or change our understanding? Richard S. Westfall and Marie Boas Hall produced portraits of Boyle as a bona fide, uncomplicated mechanist devoted to testing his view of the universe by frequent experimentation. Steven Shapin and Simon Schaffer in their *Leviathan and the Air-Pump* (1985) set the stage for a more complex version of Boyle. They portrayed him as a consummate strategist who craftily used his experimental philosophy to serve his own conservative political and social agendas. Shapin's more recent *A Social History of Truth* (1994) claimed that Boyle relied heavily on his social status to carefully craft his public image as a gentleman (rather than as a professional) in order to gain credibility and trust from his audiences. Lawrence Principe in his *Aspiring Adept: Robert Boyle and His Alchemical Quest* (1998) has shown us a Boyle who was passionately committed to alchemy and fascinated by, if fearful of, magic. Barbara Kaplan in her *Divulging of Useful Truths in Physick* (1993) emphasized Boyle's long-standing interests in medical issues and medical practices.

What sort of Boyle does Hunter offer his audience, and how has his high place in Boyle scholarship enabled him to present such a novel interpretation? Hunter shows us a Boyle whose fully complex personality directly contributed to, and thwarted, his scientific production as an experimenter. Hunter's analysis is not deeply seated in psychoanalytical theory but, rather, emerges from a fine-grained analysis of Boyle's intense religious preoccupations. In the case of Boyle, Hunter argues, earlier eighteenth-century biographers—including Gilbert Burnet, Roger North, Henry Miles, and William Wooton—produced adulation for a figure whose deep religiosity enhanced his scientific endeavors. Rather than casting his lot with contemporaries who sought a conservative solution to church and state relations after 1660, Boyle had troubling religious preoccupations so that things for him were not so simple, as Hunter shows. Indeed, Boyle befriended and associated with men who were openly acknowledged as mystics and enthusiasts and never allied himself exclusively—or even primarily—with Church of England spokesmen or ecclesiastics. Moreover, Hunter argues, Boyle felt more acutely aware of the dangers of excessive rationalism than he did of the claims of direct divine inspiration. Boyle's religious anxieties, far from mere youthful preoccupations, plagued him throughout his lifetime. Hunter reproduces Boyle's notes on two full confessional interviews he had with two prominent churchmen toward the end of his life. The two churchmen, Gilbert Burnet and Edward Stillingfleet, appeared unsympathetic with the great man's scruples over the legitimacy of his ownership of considerable lands once belonging to the Church. But for Boyle this was no trivial matter; throughout his life he was similarly plagued about the moral rectitude of inheritances from his wealthy father, about constant incursions of blasphemous thoughts, and about the moral propriety of taking oaths. Indeed, for Hunter Boyle's obsessive concern in salving his conscience induced his refusal to accept the presidency of the Royal Society in 1680 because of the oath-taking necessitated by the position. His tortuous, obsessive religious preoccupations,

Hunter argues, find their parallels in Boyle's equally exacting procedures related to his punctilious scientific experiments. Thus, while some historians might see the scrupulous religiosity of Boyle as "dysfunctional," the same instincts and personality features made him a highly "functional" scientist.

Hunter's careful probing of Boyle's defensive apologies, frequently found as prefatory material in his published treatises, reveals another side of Boyle, hitherto largely ignored by his biographers. Excessively concerned about his reputation for establishing new empirical findings, Boyle reveals himself to Hunter in several instances as a masterful dissembler or plagiarist, careful in the extreme to conceal his debts to empirics, compilers, and less learned or less socially prominent men, including George Starkey, William Salmon, Johann Glauber, J. J. Becher, and Jean-Baptiste DuHamel. What emerges from Hunter's depiction is the antithesis of the decisive, manipulative, socially prepossessing Boyle that Shapin has depicted. What Hunter furnishes is a Boyle who is perhaps far more insecure than the one historians have allowed themselves to envision and who struggled to hide his own vulnerability and inadequacy.

In a satisfying exploration of Boyle's connections with medical circles, Hunter searches to explain why Boyle, well known among his contemporaries for enjoying a constant state of ill health and for his dissatisfaction with learned Galenic medicine passed down by the esteemed physicians of his day, was, nevertheless, so loath to publish his stinging criticisms of the medical profession. Historians have long recognized that Boyle evinced strong interest in medical matters from the time of his early researches with physicians in the 1650s in Oxford and that he continued to write about medical issues for the next several decades of his life. But Boyle was acutely aware—as his prefaces attest—that he was not a member of the profession and that his access to sick bodies was severely constricted by his status as a gentleman. Hunter suggests that Boyle's reticence in publishing his attacks on the Galenists owed something to his social position—he was, after all, on good social terms with many eminent physicians. His deference, Hunter argues, owed not just to his hesitation to attack a well-respected profession, but also to his diffidence about proclaiming universal truths on matters as changeable—and ultimately untestable—as sick human bodies. Hunter further probes another paradox of Boyle's life. We know that Boyle freely gave out his medical advice and medicines (also without charge) that were produced in his private laboratory to friends and acquaintances and that, furthermore, he saw this as a moral duty and Christian charity. Yet Boyle, the champion of open communication in matters of natural philosophy, steadfastly refused to publish his medicine recipes (although some of these appeared posthumously in his *Medicinal Experiments*). His unpublished drafts reveal a Boyle who was sharply committed to widening the access of his poor countrymen to chemically prepared and effective medicines. But Boyle stopped well short of making these recipes available for wider dissemination through the medium of print. Why? Again Hunter presents no simple answers but suggests a medley of motives. Exquisitely sensitive to his own reputation, Boyle, in part, may have censored himself lest he be seen as the equal of empirics, apothecaries, and quacksalvers, all abundant and thriving in London in the 1670s. Despite his high-minded sense of philanthropy, Boyle was particularly concerned about his public persona, so much so that his private passions at times took second seat. Hunter suggests a similar explanation for Boyle's refusal to forthrightly publish his investigations of supernatural phenomena, including witchcraft, visions, angelic apparitions, and second sight. In addition to his strong religious scruples that such investigations might be tainted by demonic participation, Boyle censored himself out of his fear of the charge of easy credulity. As popular satires of experimenters were beginning to pour forth from the pens of Thomas Shadwell and Samuel Butler, Boyle must have smarted at the thought of being classified among those indulging in magical pursuits unbecoming to a genteel experimenter. Hunter concludes by revealing a Boyle far from being a confident showman and expositor of the new science. Instead he shows us a conflicted, ambivalent man who suffered from his high social status, even as he manipulated it for his own aggrandizement or for that of the Royal Society and its experimental program.

Hunter's depiction of a tortured man—hitherto most often conceived as an iconic devotee of experimental science or as a consummate, self-fashioning, smooth-talking gentleman—will not sit well with some historians. Nor will his characterization of Boyle's lifelong religious scruples sit well. Just as Lawrence Principe presented readers with a disturbing vision of a Boyle duped by alchemical operators or conversing with angelic voices, Hunter offers up a tentative, hesitant, and uncertain man who does not seem to fit with earlier portraits. With ex-

Cartoon from Punch, *1883, depicting politicians as microbes (reprinted from Peter Whitfield,* Landmarks in Western Science: From Prehistory to the Atomic Age, *p. 219).*

tremely convincing and documented evidence, Hunter draws a complex, strong personality that left its stamp on his scientific agenda. Hunter's most piercing insight is to see Boyle's deep-seated diffidence not as an obstacle to his science but as a driving force behind his commitment to experimentalism. In the end Hunter's view will outlast earlier, less complex versions.

MARTHA BALDWIN

■ General

Peter Whitfield. *Landmarks in Western Science: From Prehistory to the Atomic Age*. 256 pp., frontis., illus., figs., bibl., index. New York: Routledge, 1999. $35, Can $50.

Peter Whitfield's *Landmarks in Western Science* is a truly handsome book—thoughtfully designed, lavishly illustrated, and clearly written—but also a disappointing one for its historical naïveté. Whitfield, a cartographic historian who "searched for years for a book like this" and never found one, wrote *Landmarks in Western Science* to fill the gap. It is an ambitious book, marching at a breathtaking pace across the development of science "from prehistory to the atomic age" in eight chapters and 245 pages.

For successfully consolidating his material in such a space, Whitfield must be commended. His enthusiasm for the subject carries the reader through some of the more technical discussions, and his overarching framework that places the history of science in dramatic contrast to religious thought helps to keep the reader engaged in the midst of a torrent of discoveries, inventions, and discrete facts about the development of scientific ideas. Whitfield's research is good, his facts usually interesting and illuminating. He has read the standard histories by well-regarded scholars.

Whitfield promises to present a view into the nature of science itself: the history of science, he

says, is "conditioned by historical circumstances" (p. 7). Such a perspective is an important starting point for someone attempting to utilize the research of the last several decades in the history of science. Whitfield partially fulfills this promise by sometimes placing science in a broad social context. He covers astrology in ancient and medieval times. A long two-part chapter on science in religious cultures deals with the medieval Islamic and Christian worlds. His chapter on the eighteenth century portrays that period's salon culture, with its infatuation with scientific demonstrations. The book's illustrations, even more than the textual discussion, illuminate the complexity of the scientific enterprise: a page out of a ninth-century Arabic copy of Euclid's *Elements;* a painting of William Harvey dissecting a deer in front of Charles I; an editorial cartoon portraying the French and English priority dispute over the discovery of Neptune; Ernst Haeckel's embryological demonstrations of his theory of recapitulation.

Unfortunately, Whitfield's narrative doesn't go far enough in explaining science in historical context. The professional historian will be let down—indeed, occasionally embarrassed—by the gradual appearance of the giant of whig history and its eventual conquest over any historically contextualist narrative. The book ends up being a simple chronology of those who "got it right."

Whitfield praises mavericks for their foresight, but unthinkingly. He never gives credit to people who rigorously defended ideas that were later overturned, even when contemporary evidence was in their favor. Francis Bacon, for instance, presents a problem: he was "unimpressed by the major advances of his day such as Copernicanism" (p. 130), so Whitfield must lament his blindness. And Kepler especially puzzles him. He acknowledges the astronomer to have been a passionately religious man but presents this quality as a barrier to his science rather than integral to it, as so many historians argue. Kepler's laws of the universe, Whitfield apologetically remarks, "had an element of mysticism" (p. 123) in them—one could likewise say that the ocean has an element of moisture in it, so misleading is this portrayal.

Why did the author fall into this trap? He is the captive of Comtian-style positivism. The chapter epigraphs, from Bergson to Democritus to Diderot to Tyndall, announce the triumph of reason over superstition. So the history of science becomes the chronicling of the discovery of the positive facts, with little concern for the nature of the struggle to understand the world. Science, for Whitfield, is a simple combination of skepticism and empiricism. As a contextual history of science, this book fails—too many opportunities are missed. This is unfortunate, for it had the potential to do so much more. For the casual reader, the book has much to commend it—the illustrations and design, superb; the writing, accessible and clear—but one should not look for thoughtful history here.

STEPHEN P. WELDON

Toby A. Appel. *Shaping Biology: The National Science Foundation and American Biological Research, 1945–1975*. xiv + 393 pp., tables, apps., bibl., index. Baltimore: Johns Hopkins University Press, 2000. $42.50.

The National Science Foundation has special significance for historians of science. A large literature has identified the creation and evolution of NSF as an important part of the dramatic story of transformation and growth in American science and the national science establishment since World War II. Created in 1950 after several years of national debate, the foundation also became a major patron of the history and philosophy of science in the United States, providing support for numerous studies, including Toby Appel's *Shaping Biology.* The central themes and controversies in the foundation's history—like the relationship between basic and applied research, the role of politics and other social factors in shaping scientific inquiry, the tension between elitist and democratic ideals in science, the existence of competing visions of science as a unified or a pluralistic intellectual enterprise, the virtues of big versus little science—have been central matters for historians of science.

By focusing on public patronage for biology, *Shaping Biology* makes an important contribution. In the last dozen years or so, a minor scholarly industry has substantially enlarged our understanding of American science during World War II and the Cold War, in the process raising troubling questions about the extent to which national politics and federal patronage shaped scientific inquiry, careers, expertise, and institutions. Overwhelmingly, this scholarship has concentrated on military patronage of the physical sciences. Yet the evolution of the biological sciences since the middle of the twentieth century is surely among the most important stories we need to tell. By focusing on NSF, Appel illuminates an interesting piece of that larger story, as she shows how developments within the foundation reflected and contributed to broader

trends in American biology and national science policy.

Appel pays special attention to the problem of unity in biology. Those responsible for biology at NSF during its early years assumed (or hoped) that there was "one biology," that biology was a unified, integrated scientific enterprise. Support for this view came from biologists who found the notion of "one biology" appealing intellectually, but it also had strategic importance when explaining the value of the foundation's Division for Biological and Medical Sciences to nonbiologists. Within the division, the emphasis on unity was accompanied by an innovative organizational structure, with programs devoted to functional lines of inquiry like regulatory biology or systematics rather than to disciplinary programs like botany or zoology. Over time, however, support for "one biology" eroded. Within biology, the wide variety of topics and levels of analysis, from the molecule to the ecosystem, and the many methods of study, from laboratory experiments to field research, helped to undermine any assumed unity. So did the drive within NSF for political support, which often meant singling out specific lines of study as especially relevant to changing national concerns—for example, the contaminated environment or inadequate energy resources.

In other ways as well, Appel explains, NSF's leaders struggled to define and redefine its biology program as the American political and intellectual setting itself changed. For example, the foundation was continually under pressure to define and defend a special mission for itself in relation to other federal patrons for biology, including the Atomic Energy Commission, the Office of Naval Research, and, most important, the National Institutes of Health. NSF was also constantly explaining the value of basic research in biology (and other sciences) to national leaders who were often more interested in practical payoffs. By the 1960s, national pressures led the foundation for the first time into the arena of applied research, a venture followed by a period of soul searching and sometimes bitter controversy within the foundation as some worried that support for applied studies would drive out support for basic studies.

Shaping Biology ends with a major reorganization at NSF in the mid 1970s that closed the biology division. Subsequently, support for biological studies was divided among multiple new organizational structures, thus bringing to an unhappy end the notion of "one biology." Appel's timely and important study is clearly written and well researched. It should be of interest to historians, biologists, science journalists, and national science policy makers.

MARK SOLOVEY

Marina Frasca-Spada; **Nick Jardine** (Editors). *Books and the Sciences in History*. xiv + 438 pp., illus., fig., index. Cambridge/New York: Cambridge University Press, 2000. $85 (cloth); $29.95 (paper).

The fertile intellectual field of Cambridge has produced another collection of essays that, like the earlier *Cultures of Natural History* (1996), brings together the fruits of new research directions in the history of the sciences. Twenty essays are organized into three chronological sections that focus, first, on texts from the eighth century into the seventeenth century, then on the long eighteenth century, and finally on early nineteenth-century print culture. Geographical sites include a Carolingian monastery in Picardy, the fifteenth-century Ottoman empire, early modern English alchemical sites, New Spain in the 1570s, seventeenth-century French salons, eighteenth-century German research libraries, and early nineteenth-century independent Spanish America. The substantial number of illustrations indicates that the visual cultures of science are part of this history too, and a few essays address the topic.

Distinguished scholars from book history and the history of science take up issues about audience, genre, and technology as well as authorship, production, distribution, and reception, and each essay carries suggestions for further reading. The book's coherence is in its goal of exploring cultural and material intersections between the "sciences," broadly construed, and "books," also broadly construed. Readers will find a sampler of current interest in literary forms of scientific communication in times past, including astrological publications, encyclopedias, periodicals, children's books, and textbooks. The benefits of knowledge of bookish paratexts are made apparent in essays that explore interpretive and critical uses of scholarly footnotes, glosses, quotations, and commentaries found in writings about the sciences.

The methodological agenda of the volume is an approach to scholarship through specificities of time and place. Mary Terrall, writing about packaging natural philosophy for polite audiences, compares French and English uses and resonances of Fontenelle's *Conversations on the Plurality of Worlds* (1686). Aileen Fyfe, in an essay about young readers and the sciences in England, compares "rational" and "religious"

children's books through a careful reading of the conversational form in Sarah Trimmer's *Easy Introduction to the Knowledge of Nature and the Holy Scripture* (1780) and John Aikin and Anna Barbauld's *Evenings at Home* (1792–1796). Other contributors place similarly high value on textual particulars and close readings of historical moments and bring to life early science books and their complex cultural grids. Essays also clearly show the relevance of past practices for contemporary understanding. In particular, afterwords by Nick Jardine and Adrian Johns point book history outward to the history of knowledge making more broadly and to issues in electronic communication today.

Various essays take up the theme of books and their multiple audiences and examine how early science books often served disparate constituencies. Richard Yeo makes the point, when discussing Ephraim Chambers's *Cyclopaedia of the Arts and Sciences,* that this foundational eighteenth-century book likely had several types of readers, some scholarly and others less learned, each group using the book for its own purposes. The volume under review mirrors that same historical pattern. The editors present the collection as introductory, "a work of first resort for all those interested in the history of the sciences in relation to the history of the book" (p. 8). Yet the nuanced analyses will carry more specialized readers toward deeper levels of understanding. The essays are accessible in language and levels of information while at the same time reflecting rigorous scholarship, and the essayists wear their learning lightly. Such a recipe should succeed in recruiting new readers for the topics presented here. A welcome model in the dissemination of ideas, the collection shows students and researchers how to shape their own inquiries and present the fruits of their intellectual labors to others.

ANN B. SHTEIR

Mary Midgley. *Science and Poetry*. 207 pp., bibl., index. London/New York: Routledge Publishing, 2001. $30 (cloth).

Mary Midgley's *Science and Poetry* tackles so many topics of importance that one wants it to be very good. Yet Midgley, a moral philosopher, makes one idea the measure of all things, so that the book is just good enough.

Her topic is not really "science and poetry" but the failure of neurobiological reductionism to understand the human mind. That poets understand the mind better than scientists is the subtext of this collection of essays, but the poetic theories Midgley quotes are too general to make much headway against science's prejudices about the arts' naive handling of the serious matters of consciousness, the operation of the brain, and connections between mind and body. Midgley needs to dig deeper into poetry and to provide better analyses of works like Wordsworth's "Tintern Abbey" and Shelley's *A Defence of Poetry.* She also needs to update her poetic references to include contemporaries such as A. R. Ammons, Patti Ann Rodgers, and Alison Deming, who make the science-based topics that preoccupy Midgley the overt focus of their work.

The valuable first section of the book presents capsule histories of science and its march toward dualism, from the Greeks to the Enlightenment, interspersed with poets' contrary views. Midgley succinctly traces the evolution of the theories of the mind/body split that such a campaign produced. She drops the "science and poetry" focus in the second section for her major concern—showing that contemporary scientists themselves, in pursuing scientific understanding, employ choice, intention, and cognition, the very factors denied by strict scientific materialist explanations of how the mind works. This is the neatest feature of the book—its witty turning of scientific discourse about the purely mechanistic operations of the mind back on the scientists themselves, who violate their own theories in stating them. Her favorite scientists to tweak in this way are the sociobiologist E. O. Wilson and the geneticist Richard Dawkins. When she needs to, Midgley lets a philosopher of language like John Searle handle others, such as Daniel Dennett and his claim that cultural ideas are simply genetically produced "memes." Midgley's summary, however, does not do justice to the complexity of the debate about the nature of language as a window to the operation of the mind.

Midgley's strength is in testing fundamental theoretical assumptions by applying commonsense reasoning to them. For example, Descartes's mind/body dualism fails on the face of it because he uses socially constructed language, a product of mind, as he asserts the complete independence and autonomy of individual minds. Midgley's deft use of simple but revealing thought-problems also helps make her case, and she is particularly good at alerting us to ordinary words that need clarification, such as "cause." She's like a poet when she sensitizes us to the power of simple words in shaping scientific discourse.

In the miscellaneous last section of the book Midgley provides examples of the contemporary dangers of the reductivist way of thinking, but

the first two parts could have used livelier, more pointed dramatizations of the consequences of splitting the world into subjects and objects. Universal human rights and the right of all creatures to live in cooperation are what Midgley believes science ultimately sacrifices in its vision of competitive, reductive materialism. Scientists and social scientists such as Freud and Marx who use that "vision" have led us down disastrous paths. That science should learn to see the world as a cooperating, interacting, and living whole is her wish—and the subject of many of her other books.

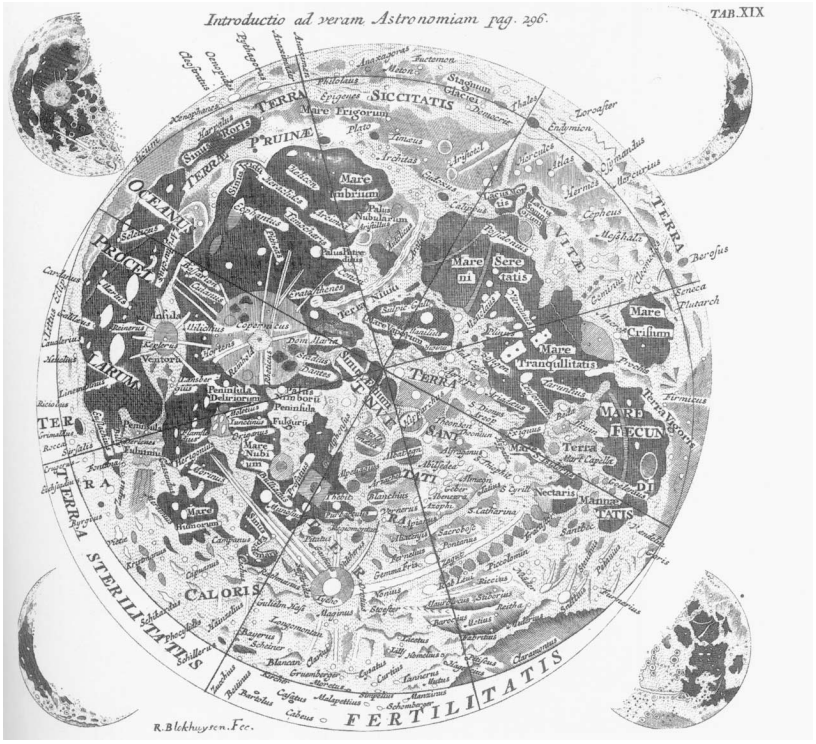
Science and Poetry would be more effective if condensed into a single, focused essay that concentrated on convincing Midgley's presumed audience—scientists who may have found themselves at the dead end of materialism as their own disciplines discover their limits and branch out into territory more familiar to poets. The book should interest students of literature who want an introduction to the many topics it treats, but they will find little to convince them that poets do indeed have important ideas about the human mind.

ROBERT CHIANESE

Ewen A. Whitaker. *Mapping and Naming the Moon: A History of Lunar Cartography and Nomenclature*. xx + 242 pp., frontis., illus., tables, apps., index. New York: Cambridge University Press, 1999. $59.

It is understandable that Ewen Whitaker developed an interest in the history of mapping and naming the moon. As a participant in the Apollo missions and a member of the Task Group of Lunar Nomenclature of the International Astronomical Union, he was himself directly involved in conflicts between representatives of different countries over naming newly discovered lunar features. In an effort to understand the passions surrounding current controversies more completely, his book examines their origin and development from the seventeenth century to the present.

Whitaker's account, written without footnotes



Early eighteenth-century map of the moon from Keill (reprinted from Ewen A. Whitaker, Mapping and Naming the Moon: A History of Lunar Cartography and Nomenclature, *p. 89.)*

for the general reader, proceeds in a strictly chronological fashion. As he makes clear, the key event in the history of lunar mapping and nomenclature was the discovery and application of the telescope to increasing knowledge of the moon's features. Mapping and naming thus became important issues in the wake of Galileo's observations. Accordingly, the first part of Whitaker's study, which deals with the period from prehistory to 1651, is the longest and most important of its four sections. By 1651, with the publication of Francesco Grimaldi's map and the application of Giovanni Riccioli's nomenclature, a fairly detailed and accurate picture of the moon's surface features had become available.

The second part of Whitaker's account carries the story to the beginning of the Victorian period and features the creation of a new lunar map based on the work of Johann Mädler and Wilhelm Beer. In Parts 3 and 4, which together are shorter than either of the first two sections, Whitaker focuses on such key developments as the emergence of photography and the creation of international organizations to settle disputes over nomenclature. By 1935, a committee of the International Astronomical Union had reached agreement on a map and the naming of surface features. However, the further explorations of the space age dramatically increased the work of mappers by providing the first photographs of the far side of the moon and revealing new features on the near side.

The most compelling facet of Whitaker's study are the 112 reproductions of drawings and photographs of the moon from the seventeenth century to the present, which, taken together, constitute a rich visual history of efforts to portray lunar features for the public and for scientists. In addition, detailed appendixes, arranged in chronological order, provide lists of new names of lunar features as assigned by the most influential mappers of the lunar surface from Michiel Van Langren to the *NASA Catalogue of Lunar Nomenclature.*

While Whitaker limits himself strictly to matters of mapping and nomenclature, his study has already been useful to other historians of lunar exploration, including Scott Montgomery (*The Moon and the Western Imagination* [Arizona, 1999]) and William Sheehan and Thomas Dobbins (*Epic Moon: A History of Lunar Exploration in the Age of the Telescope* [Willmann-Bell, 2001]). Montgomery seeks to explain how political and aesthetic factors shaped the portrayal of lunar features and thus the drawing of maps and the naming of surface features up to 1651, while Sheehan and Dobbins move beyond mapping and nomenclature to consider controversies over the possible existence of life on the moon and the conflict surrounding the origin of lunar craters by volcanic activity or meteor impact. Whitaker's study, however, provides a unique record of the development of our image of the moon's surface over a period of more than three centuries.

DAVID STRAUSS

John Gribbin. *The Birth of Time: How Astronomers Measured the Age of the Universe*. x + 237 pp., illus., bibl., index. New Haven, Conn./ London: Yale University Press, 1999. $22.50.

The cover page of John Gribbin's *The Birth of Time,* listing more than thirty books he has written on astronomy, physics, and general science, shows the success this author has had in making these subjects interesting to and understandable by the general public. The eight chapters of *The Birth of Time,* ending with a useful list of books for further reading and a well-compiled index, do indeed present a readable account of a difficult subject: man's attempts, from the time of the ancient Greeks onward, to understand what is out in the sky and our place in that world. The book contains no mathematics and can be enjoyed by general readers, high school students, and teachers.

The first chapter is historical: an account of the progression from Arabic science at the end of the first millennium, including the discovery and recognition of fossils as indicators of a geological history of the earth, through the seventeenth century's beginnings of an attempt to understand this history. The second chapter provides an account of the twentieth century's growing knowledge of the physics of stars, with nuclear energy as the source of their outpouring radiation. The fundamental work of Sir Arthur Eddington at the University of Cambridge is described here, and a simple account is given of the evolution of stars, from their formation from clouds of gas and dust to their eventual death as spectacular explosions or as cold, inert lumps of mass.

Only in Chapter 3 does the author get to the real basic techniques of distance measurement, starting with triangulation by surveyors on the earth's surface and proceeding with ever-increasing baselines, as Copernicus, Tycho Brahe, and Galileo made their impact on our understanding of the solar system and measurement of the earth's distance from the sun made this baseline the tool for distance measurements of stars.

The concept of "standard candles"—stars

whose intrinsic luminosities could be determined and whose apparent brightness could be measured, thus yielding their distances—is described next. A fair description is given of the epochal discovery in 1912 by Henrietta Leavitt, working at the Harvard College Observatory on routine measurement of variable stars on photographic plates, of our nearest neighbor galaxy, the Small Magellanic Cloud. She discovered a tight relationship between the periods of variation of a certain class of variable stars, the Cepheids, with their overall brightness, thus giving posterity the most important starting tool for distance measurements of galaxies outside the Milky Way.

As Gribbin takes readers through the history of the gradual understanding of the place of our home galaxy, the Milky Way, in the universe at large, the work of various heroes of these progressive discoveries and the pitfalls encountered by others are described, somewhat unevenly. The astronomer Vesto Slipher, observing the spectra of galaxies with the twenty-four-inch telescope at Lowell Observatory in northern Arizona, receives due credit for the first measurement of line-of-sight recession velocities of galaxies, from the Doppler shift of atomic lines in their spectra toward the red end of the spectrum. The role of Milton Humason, former mule driver, janitor, and eventually the famous associate of Edwin Hubble, in the discovery from these redshifts of the expansion of spacetime is well described. Allan Sandage, a former Hubble pupil who is clearly Gribbin's hero, might himself demur at the enthusiasm with which his work is described; while Gerard de Vaucouleurs, were he still with us, would not be pleased with the account of his work that led to a value for the Hubble Constant—the quantity that converts measured recession velocities of galaxies to their distances from us—twice that determined by Sandage.

The description of Gribbin's own recent research (Ch. 8), which he describes as "just one minor brick in the scientific edifice" (p. 198), based as it was on his belief that our sun and solar system exist in a very ordinary average-sized spiral galaxy, is quite interesting. It follows a chapter giving an account of recent additions to the arsenal of astronomers' tools for distance measurement in the universe—and also the nasty systematic biases that can slant the result away from what one hopes is correct.

The book is illustrated by eight nicely printed pages of images obtained by the Hubble Space Telescope. It is easy to read and tells its story in a friendly, chatty style. Though it concludes by suggesting a value of some 13 billion years ago as the epoch of "the birth of time," the discerning reader will realize that the story is still evolving and that surprises can be expected.

MARGARET BURBIDGE

Helaine Selin (Editor). *Astronomy across Cultures: The History of Non-Western Astronomy*. (Science across Cultures: The History of Non-Western Science, 1.) xxiv + 665 pp., illus., figs., tables, index. Dordrecht: Kluwer Academic Publishers, 2000. $345, £215, NLG 650.

This is the first installment in the series Science across Cultures: The History of Non-Western Science; subsequent volumes will treat mathematics, medicine, nature and the environment, chemistry, and physics and optics. The aim of the series is to "rectify the lack of scholarly attention paid to most of the world's science" by providing scholarly readers with "factual information about the practices and practitioners of the sciences as well as insights into the world views and philosophies of the cultures that produced them." The hope is that "readers will achieve a new respect for the accomplishments of ancient civilizations and a deeper understanding of the relationship between science and culture" (p. vi). In terms of focus, "non-Western" is here a cultural rather than a geographic designation, denoting peoples outside the Euro-American sphere, including the native cultures of the Americas. Accordingly, for the purposes of this series "science" is broadly conceived to mean ways of "defining, controling [*sic*], and predicting events in the natural world" (p. vi), which every culture is presumed to possess.

Perhaps as a sign that cultural astronomy has come into its own as a discipline, barely twenty years after the First International Conference on Ethnoastronomy was successfully convened at the Smithsonian Institution in 1983, it is now possible to assemble highly informative essays on the astronomy of a variety of non-Western cultures. The astronomy, astrology, cosmology, and cosmography of the non-Western world, far from being out of our scholarly reach, are now on full display in the pages of this excellent collection of articles surveying the richness of the cultural roles played by the first of the exact sciences.

Though the companion discipline of archaeoastronomy is also in evidence here, the majority of articles are broader in scope and historically more wide ranging than the reconstruction of astronomical alignments of sacred structures and their significance. Essays trace the influence of

astronomy in religion and ceremony, cosmology, and political ideology, as well as practical applications in calendar making, naked-eye celestial navigation, astrology, and mathematics. Cultures and people represented include aboriginal Australians; ancient Polynesians from Hawaii to the Maori of New Zealand; ancient Mesoamericans, the Inca, and Plains Indians; Indian astronomy and its hybrid offspring in Tibet and Indonesia; China, Japan, and Korea; the folk astronomy of Sub-Saharan African peoples; ancient Egypt and Babylonia; and Hebrew and Islamic astronomy. Two excellent introductory essays, "Sky Tales and Why We Tell Them" (Edwin C. Krupp) and "Astronomy and Prehistory" (Lawrence H. Robbins), perform an important service in setting the stage for what follows; indeed, both are indispensable reading for anyone seeking a general orientation in cultural astronomy and its extraordinarily ancient roots.

The articles are nearly all engagingly written, amply illustrated, and thoroughly accessible, not only to the scholarly audience to which the series as a whole is directed but also to educated readers in general. In keeping with the stated purpose of the series, the authors in most cases strive to provide an overview of the state of the field in the cultural domain under discussion. Following each article, all authors provide copious bibliographic references to assist readers interested in probing deeper. *Astronomy across Cultures* is handsomely produced and meticulously edited. Though readers will find themselves thirsting for more information about "non-mainstream" peoples and cultures than is to be found here, one can hardly quibble with the selection of subjects treated in this first volume of its kind. My chief lament is that this auspicious beginning to the Science across Cultures series is so expensive that it will certainly be out of reach for most—even at 665 pages a retail price of $345 seems hard to justify.

DAVID W. PANKENIER

Paul Brouzeng; **Suzanne Débarbat** (Editors). *Sur les traces des Cassini: Astronomes et observatoires du sud de la France*. (Based on papers presented at the 121st Congrès National des Sociétés Historiques et Scientifiques, Nice, 26–31 October 1996.) 370 pp., illus., figs., tables, index. Paris: Editions du Comité des Travaux Historiques et Scientifiques, 2001. €31.

An outdated geography supplies the bond among the thirty-one articles in *Sur les traces des Cassini.* In the seventeenth century, when the Italians Gian Domenico Cassini (known familiarly to historians as Cassini I) and his nephew Giacomo Filippo Maraldi (Maraldi I) were born in Perinaldo, north of Genoa, their birthplace belonged to the County of Nice. Hence the rationale of building a set of papers on astronomy in the south of France around Cassini I and his family, which for four generations ran the Royal Observatory in Paris.

Over half the articles concern the Cassinis, mostly Cassini I and his great-great-grandson and namesake Cassini IV. There was also a Cassini V, Henri de Cassini, who countered the family genius and stamina by preferring botany and dying early, of the same outbreak of cholera that took the life of Sadi Carnot, without having created Cassini VI. He had already entered the Academy of Sciences with a push from his father. "I dare to beg of you [Cassini IV wrote to his fellow academician A. M. Ampère] to consider whether this unique situation in the history of letters, [a family's] devotion to the sciences for five successive generations and 170 years, ought not add some weight to the scientific credentials of my son." It is hard to refuse the children of important alumni.

The portion of *Traces* dealing more directly with astronomy in the south of France gets off to a distant start. Pytheas of Marseilles, who lived about 350 B.C., sailed to the Orkneys and the Baltic and earned himself the reputation of a liar back home for his stories of midnight suns and frozen lakes. He measured the latitude of Marseilles, the obliquity of the ecliptic, and the size of the earth. Ptolemy praised him. Strabo did not: "Pytheas lied about everything and covered it up with his knowledge of astronomy and numbers."

No traces worth following up were laid down for just under two thousand years. Then, in 1580, Nicolas Fabri de Peiresc first saw the light of day. He lived in Italy for four years as a very young man, deepening his knowledge of astronomy and human nature and meeting the main future actors in the Galileo affair: Galileo himself, Bellarmine, and Matteo Barberini (Urban VIII). At his center in Aix-la-Provence, Peiresc made many useful astronomical observations, some in collaboration with Pierre Gassendi. He died in harness, worrying about the change of the obliquity since the days of Pytheas.

There follow articles on Provençal astronomers who determined longitude and latitude at sea, on neglected observers in Languedoc who assisted the cause of the Enlightenment, and on modern observatories in the south of France. The political circumstances after the defeat of 1870–1871 favored decentralization of astronomy

away from the Paris Observatory. In Italy, too, recent political events—the unification of 1870—made a restructuring of astronomical institutions desirable and possible. But whereas France had too few observatories, Italy had too many. Georges Rayet and Pietro Tacchini, both astrophysicists, compared the circumstances in their countries and made mutually reinforcing proposals to their colleagues and governments. Their respective proposals, most of which were enacted, called for reassigning some Italian observatories to meteorological work, building new observatories in Besançon, Bordeaux, and Lyon, and refurbishing older ones at Marseilles and Toulouse. Once again, as in the days of Cassini I, Italy made a decisive contribution to the practice of astronomy in France.

Sur les traces des Cassini mixes slight and weighty work, admits antiquarian and broader approaches, offers new documentation, displays pertinent illustrations, and does it all at a high level of scholarship. Since, because of its title, the book's primary audience probably will be people interested in the Cassinis, its fullest articles about them may usefully be mentioned here: Anna Cassini (the author of an excellent but hard-to-get biography, *Gio: Domenico Cassini* [Perinaldo, 1994]) on Cassini's brief return to Italy, 1694–1696; Claude Teillet on the provincial life and poetry of Cassini IV; Christiane Demeulenaere-Douyère on the Cassinis and the Académie des Sciences; Fabrizio Bonoli and Alessandro Braccesi on Cassini I's astronomical work in Bologna, with full bibliography; and Monique Pelletier on the Cassini map of France, on which she has written a book (*La carte de Cassini* [Paris, 1990]). Pytheas and Peiresc are the subjects of collaborative articles by Simone Arzano and Yvon Georgelin.

J. L. HEILBRON

Jean-Claude Pont; **Jan Lacki** (Editors). *Un cordée originale: Histoire des relations entre science et montagne*. xii + 434 pp., illus., index. Chêne-Bourg/Geneva: Georg Editeur, 2000.

Alpinists often use "une cordée" to rope themselves together on dangerous slopes. The editors of this book have linked together an interesting series of twenty-nine articles on the intersections of mountains and science. As might be expected from such a compendium, there is no one peak to conquer, no single message. Rather, the reader is faced with many options, and this review will not, alas, highlight every article. After Jean-Claude Pont's introduction concerning human attempts to understand mountains, four large areas are considered, as they relate to mountains: natural sciences, medicine and physiology, ethnology, and geography. The temporal terrain centers on, but is not limited to, the eighteenth and nineteenth centuries. As might be expected for a product conceived by the History and Philosophy of Sciences Group at the University of Geneva, the book has a Swiss and Alpine flavor. Other areas receive their due, however. A noteworthy example is Ezio Vaccari's valuable overview of scientific travelers in the Apennines during the seventeenth and eighteenth centuries. Tuscany, the Pyrenees, the French Alps, and even the Caucasus all are treated in separate articles.

A key attribute of the book is the rich integration of historical and scientific topics with literature, theology, philosophy, mountain myth, geography, and medicine. Natural theology is a good example of the book's integrative theme, as Gabriel Gohau shows how eighteenth-century scientists argued that God's beneficence could be seen in the gifts of mountains and Nadine Sommer considers Elie Bertrand's (1713–1797) "Essai sur les usages des montagnes" (1754) to substantiate the point. Historians of science will appreciate those general interweavings, as well as discussions about the evolution of specific disciplines, such as botany, glaciology, tectonics, and mountain exploration itself. Heinrich Zoller, for example, presents a summary of "the discovery of the Alps" from Petrarch (1304–1374) to Konrad Gessner (1516–1565). Mountains interacted with medical practice when iodine-rich thermal waters were used to cure goiters, high-altitude sanitariums were built for sufferers of tuberculosis, or "cretins" were sent to Abendberg, Johan Jakob Guggenbühl's (1816–1863) Swiss refuge for mentally impaired children. Individuals are also illuminated, a particular example being Horace-Bénédict de Saussure (1740–1799). Albert Carozzi uses Saussure's unpublished manuscripts to develop an in-depth portrait of the man, his methodologies, and his attitudes. Marguerite Carozzi then presents an interesting analysis of how James Hutton (1726–1797) responded to Saussure's vision of Alpine geology.

An example of an integrated study with strikingly modern overtones is Patrick Matagne's commentary on "la montagne, une autre nature." He notes that nineteenth-century interest in mountains often combined scientific elements within a Romantic literary base. Victor Hugo (1802–1885), for example, used metaphors such as pyramids and cathedrals in discussing mountains, and he fostered an ideology of preservation

of natural vistas and resources that has contemporary echoes. Various writers recognized a linkage of forests with global climate, and Charles Flahaut (1852–1935) contended that deforestation was a sign of the decadence of nations.

It is a subjective comment, but there is considerable variability in the substance and tone of the articles. All have intrinsic interest, especially for the specialist in a given field, and most are presented in straightforward French, but a few exhibit a degree of what might be termed academic murkiness. Typographical miscues are few (Alexander von Humboldt's dates are stated as "1869–1859" on p. 19; "Gueyttard" appears on p. 361). Printing, binding, and illustrations are excellent. Libraries with francophone patrons should probably own the book; potentially interested individuals might wish to peruse the table of contents before purchase. Those who enjoy smorgasbords will be rewarded.

KENNARD B. BORK

Brian Fagan. *The Little Ice Age: How Climate Made History, 1300–1850.* xxii + 246 pp., illus., figs., index. New York: Basic Books, 2001. $26, Can $39.50.

From approximately 900 to 1300 A.D., a period known to climatologists and historians of science as the Little Climatic Optimum, temperatures in the Northern Hemisphere averaged one or two degrees Fahrenheit above normal—this according to ice-core samples, tree ring analysis, the calculation of sea levels, and other standards of measurement. In what would become known as the Four Corners region of the United States, the culture of the Anasazi flourished as never before in the now-famous ruins of such magnificent cliff dwellings as Chaco Canyon. Meanwhile, far to the north, the Viking raider and convicted killer Eric the Red brought European civilization by open boat to Greenland, so named to attract wary settlers whose common sense dictated that these harsh surroundings might spell their doom. Yet both cultures would flourish for upward of three hundred years.

Things began to change for the worse in the latter half of the thirteenth century, as shifts in wind and weather ushered in the so-called Little Ice Age, which lasted from about 1300 to 1850. Brian Fagan, a professor of archaeology at the University of California at Santa Barbara and the author of more than a dozen books, most of which are directed at the general reader, observes this period through the lens of climate change, with particular emphasis on what transpired in an anxious and often turbulent Europe.

From 1300 onward the climate grew cooler, more stormy, and subject to sporadic extremes—in a word, unpredictable. Glaciers and pack ice advanced; rivers never known to freeze over became natural skating rinks; blizzards enveloped and erased the frozen landscape; and people starved and froze to death in the seemingly endless winters depicted by the Flemish master Peter Brueghel. At other times the climatic seesaw drove temperatures up, replacing bitter cold with blazing summers, drought, or torrential rains, changes that could take place in a single season or across decades. In between there were long periods of mild winters and warm summers that saw the harvesting of bountiful crops. But overall temperatures in the Northern Hemisphere declined, boosting the misery index of Europeans left to wonder what they had done to so offend an angry God.

Unlike the current greenhouse warming ushered in by the Industrial Revolution, the climatic variability of the Little Ice Age remains, in Fagan's words, "a conundrum." He speculates that it may have had much to do with a shift in the great ocean conveyor belt that rushes, like a thousand Amazons, from Southern Hemisphere to Northern and back again. Or it may be associated with the virtual disappearance of sunspots during much of the period under scrutiny, or with some other phenomenon that we know much too little about.

The scientific causes aside, the author takes great pains to disassociate himself from what he calls "environmental determinism," the notion that climate change has been a primary trigger for major historical events or that it has been the single most important cause of any civilization's decline and fall. Rather, he sees it as one of many factors, including politics, war, religion, and population shifts, that scholars must fold into their teaching of history and related subjects. This may be so in many instances, but how else to explain the literal overnight disappearance of the Anasazi from the Chaco, where the ashes from their fires of six hundred years ago lie scattered on the floor? Then there are the haunting words of Ivar Bardarson, a seafaring Norwegian priest who, in 1361, sailed up Greenland's west coast, hoping to make contact with his fellow Christians, from whom nothing had been heard for several years because of the ice. Search though they did, Bardarson and his companions found no survivors, causing the priest to remark: "There was never a man."

This well-written and copiously illustrated

book will make excellent reading for generalist and professional alike. And for those wishing to introduce the subject of climate change to undergraduates by placing it within a historical context, *The Little Ice Age* offers an excellent place to start.

GALE E. CHRISTIANSON

Mark Monmonier. *Air Apparent: How Meteorologists Learned to Map, Predict, and Dramatize Weather*. xiv + 309 pp., illus., figs., tables, app., index. Chicago/London: University of Chicago Press, 1999. $27.50, £21.95.

Air Apparent explores the history of meteorological cartography in its relationship to meteorological theory, numerical weather prediction, and its public and media presentation. Mark Monmonier's book is richly documented—with copious footnotes, plenty of well-placed figures, and attractive color plates—aimed to redress the "perverse ignorance" among historians of cartography about meteorological mapping and, in the author's view, the parallel "cartographic inattentiveness" of historians of meteorology. Essentially the book traces the development of the weather map from Edmund Halley's late seventeenth-century representations of trade winds to the satellite and radar imagery on weather websites and television. Monmonier follows the growth of observation networks (mostly in the United States), the theoretical formulations explaining large-scale weather systems, and the rise of twentieth-century numerical prediction—allowing readers a view of the extraordinary importance of the visual in shaping (and being shaped by) these processes.

The practices of visual representation of weather are the most intriguing moments in Monmonier's volume, which addresses such questions as: What prompted Halley in the early 1680s to draw his map of trade winds and monsoons, why the observations of the members of the eighteenth-century Meteorological Society of the Palatine never resulted in a weather map, and why was the weather map invented no earlier than in 1816 by the German meteorologist Heinrich Wilhelm Brandes? Pre-nineteenth-century meteorologists, it is argued, sought to understand climate, not to forecast weather, and thus lacked the notion of the atmosphere "as geographical phenomenon." With the growing popularity of geographically oriented Humboldtian sciences, research on the paths and circulation of storms, and medical topography and climatology (curiously absent from the book), weather cartography gradually became an established scientific practice. Joseph Henry in 1858 said that weather maps offered a view of weather changes "at a glance"—Halley termed it "at one view"—and in 1917 George Bliss thought that the "special faculty" for intuitively understanding forces depicted on the weather map and then predicting their evolution was the result of a long experience in maps, *not* "a profound study of atmospheric physics" (p. 10). Statements like these made terms in a growing visual inventory—such as isobar, isotherm, isalobar, wind-direction arrow—not only descriptive but also heuristic.

With the Norwegian "Bergen" school the weather map came to represent "model" atmospheric situations and embodied the notion of atmospheric fronts. But where the Bergen-school people stressed hydrodynamics, their American counterparts favored a "cartographic fundamentalism." Monmonier here tells an important story of how the situation changed in 1933 when Roosevelt's Science Advisory Board issued a statement endorsing Bergen's approach. During the second half of the twentieth century, computers increasingly determined numerical weather prediction. Computer-based weather modeling is outlined, as well as methods used in environmental mapping, satellite imagery, radar observation, and media presentation. Monmonier's analysis here is particularly worth reading: his panache in dissecting the aesthetics of the newspaper weather map is one of the best features of the book. But, as in other places, the many details would likely intimidate an uninitiated reader.

While *Air Apparent* provides solid coverage of cartographic history, historians will want to hear a bottom line: What is the meaning of this history, and what does it tell us about the nature of meteorological (and scientific) knowledge? I am not sure that these concerns are answered by Monmonier's use of Jacques Bertin's semiology of visual variables (p. 223), or that the example of Bergen-school-laden "frontal" maps exhausts the problematics. It is also arguable whether the premodern absence of a cartographic "gaze" was due to the lack of a geographically oriented perception of the atmosphere. Weather (if not the "atmosphere") *was* of geographical concern, as the republic of "meteorological" letters proves in its interest in weather of different places. For instance, Daniel Defoe thought that the Great Storm of 1703 originated in Virginia and moved over the Atlantic.

Nor was the dramatization of weather a prerogative of modern science. Today's weather depictions may be even less dramatic than the early modern ones: the lack of knowledge to anticipate

its threat to agriculture, economy, and health of past generations caused tangible atmos-*fear* unknown to modern societies. It is also important to keep in mind that a history of weather forecasting cannot be exhausted in numerical prediction, as *Air Apparent* makes implicit by neglecting alternative methods involving weather rules, weather cycles, prophecy, and astrometeorology. Understanding how the new scientific meteorologists "learned to predict" weather is to understand how practitioners of these largely supplanted traditions lost much of their credibility since Victorian times. While Monmonier's study abounds in valuable empirical information, issues like these are not fully addressed, and *Air Apparent* will not satisfy those looking for a sustained social history of the weather map.

VLADIMIR JANKOVIĆ

Peter Douglas Ward. *Rivers in Time: The Search for Clues to Earth's Mass Extinctions*. x + 315 pp., illus., bibl., index. 1994. New York/Chichester, West Sussex: Columbia University Press, 2000. $29.95.

Here is the problem. If the message is important, is it fair to write negatively concerning the format, let alone the messenger?

In 1994 Peter Douglas Ward published *The End of Evolution: On Mass Extinctions and the Preservation of Biodiversity.* The paper trade edition carried the subtitle "A Journey in Search of Clues to the Third Mass Extinction Facing Planet Earth."

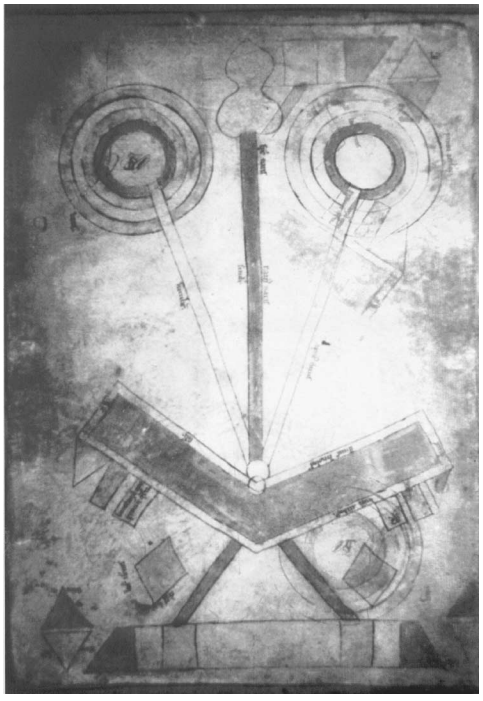
There are changes between *The End of Evolution* and *Rivers in Time,* but one has to search diligently to find them and they are trivial. The photographs are larger but no more informative. One new chapter is mainly about collecting under difficult conditions. The final short chapter, more or less new, weakens the alleged reason for writing this book. The prinicpal change is in the title. To quote another's review of Ward's earlier book: "Though his thesis is clear—sometimes overwhelmingly so—interesting, if tangential, digressions occasionally mar the story's flow." Some readers may characterize the style as appropriate for an "Indiana Jones" script. If the local library does not own *The End of Evolution,* probably it should purchase this work for the general reader.

ELLIS L. YOCHELSON

Louise H. Marshall; Horace W. Magoun. *Discoveries in the Human Brain: Neuroscience Prehistory, Brain Structure, and Function*. xii + 324 pp., illus., bibl., index. Totowa, N.J.: Humana Press, 1998. $59.50 (cloth).

This book examines the historical development of studies of the brain and behavior from the early work of Aristotle and Galen up to the late twentieth century. Modern neuroscience, a multidisciplinary endeavor, emerged only recently as a unified field (the Society for Neuroscience was founded in 1970). This book does not treat the disciplinary history of neuroscience per se but, rather, the history of attempts to understand the nervous system and its relationship to behavior from a constellation of disciplines all related to what we now call "neuroscience": anatomy, physiology, psychology, psychiatry, evolutionary theory, and anthropology.

Louise H. Marshall is a neuroscientist and director of the UCLA Brain Research Institute's Neuroscience History Archives. Her coauthor, the late neuroscientist Horace W. Magoun, founded the Department of Anatomy at UCLA's Medical School in 1953. Their book grew out of



Latin MS of late twelfth to early thirteenth century depicting eyes and nerves (reprinted from Louise H. Marshall and Horace W. Magoun, Discoveries in the Human Brain: Neuroscience Prehistory, Brain Structure, and Function, *plate 2).*

a series of poster presentations put together by Magoun for several national and international neuroscience meetings during the early 1980s. Magoun wrote a twenty-seven-page brochure after receiving much enthusiasm from neuroscientists at these meetings—both students and those more established in the field—and many wanted a publication.

Not surprisingly, given its early beginnings in poster presentations, the book is richly illustrated. The chapters are arranged only loosely chronologically; their sequence is directed more explicitly by investigative themes. The first chapter outlines three basic "postulates" that direct the organization of the rest of the book and act as conceptual threads: phylogeny (the evolutionary line of descent of living beings), the idea of a structural and functional hierarchy in the nervous system, and the notion that function determines structure. The last chapter, by way of discussion, moves into twentieth-century developments in the understanding of certain "integrative" systems in the brain and the recognition by neuroscientists of the need for multidisciplinary approaches that integrate anatomical, physiological, and behavioral perspectives.

The third postulate, the idea that "form follows function," receives the most emphasis in the book, and most chapters touch on the oscillating relationship between studies of form (anatomy) and studies of function (physiology). The book also illustrates certain historical trends: the anatomical studies of the ancient and Renaissance periods, the more physiological and clinical studies of the nineteenth century, and the instrument-centered approaches of early twentieth-century neurophysiology.

The book has certain strengths and weaknesses related to the authors' perspective as neuroscientists. As one might expect, elements of presentism arise, as the work of some investigators is described as "anticipating" that of later scientists, and other research—for example, J. L. W. Thudichum's work on brain chemistry—is deemed "surprisingly modern" (p. 158). However, the book gives wonderfully detailed, precise accounts of scientific developments related to brain and behavior. The authors demonstrate a critical mastery of both primary and secondary sources, with thorough citations, and the book comes with a comprehensive bibliography.

Discoveries in the Human Brain does not place neuroscientific developments within a wider cultural or social context, but the authors had no ambitions to do so. They even point to drawbacks of such historical approaches, arguing that they "create issues where none exist and . . . couch ideas in such convoluted language that the events and concepts become unfamiliar and difficult to fathom" (p. 279). This position reflects their intended audience: neuroscientists interested in the history of their field. While there is certainly room for professional historians of science to tackle the history of neuroscience, this book will be valuable for historians because literature in the history of neuroscience is sparse. However, it is likely to be of greater value to neuroscientists—in the authors' words, "those workers at the bench who are curious to learn how it all happened."

TARA H. ABRAHAM

Johannes Fabian. *Out of Our Minds: Reason and Madness in the Exploration of Central Africa.* xvi + 320 pp., illus., app., bibl., index. Berkeley: University of California Press, 2000. $50 (cloth); $19.95 (paper).

This book undertakes a voyage back to the colonial heritage of anthropology to investigate the connection between imperial colonialism and ethnographic research. It is a history of explorers' being "out of our minds" with alcohol, drugs, opiates, fatigue, fear, delusions, and anger in their search for knowledge. In short, it is a story about scientific "travel as tripping" (p. 3).

Nineteenth-century explorers of Africa often fashioned themselves as intrepid, heroic, and courageous seekers and promoters of rational knowledge in a wild and savage territory. It is this myth of science as a progressive conqueror of the unknown and the wild that Johannes Fabian destabilizes through his deconstructive literary analysis of scientists' travelogues. We learn that explorers were not self-composed heroic solitary individuals with the ability to control others in a hostile environment. On the contrary, explorers gained knowledge when they reached out and embraced the unfamiliar by stepping outside of their preconceived, established, and rationalist framework of exploration. This experience of the *ecstatic* (a key word in this book) proved fertile ground for scientific results.

A fascinating discussion of the role of various ecstatic experiences in the daily life of the explorer makes up most of the book. We learn about the organizational structure of scientific caravans, the character and personal desires of explorers, the role of auxiliaries and intermediaries, and the social implications of engaging unfamiliar tribal networks. Each explorer's daily struggle to survive such hardships as fever, ma-

laria, and unknown illnesses, often with the use of foreign medicines, serves as one example of ecstasy leading to new knowledge. Opiates and alcohol were in this respect helpful in easing the encounters between locals and the explorers, and sexual and erotic relationships between them allegedly secured intimate knowledge of local cultures. Likewise did the ecstatic experience of beating and even killing Africans in order to gain access to the unknown. Fabian's account of ecstatic ways to research ends with a long chapter about the interest among anthropologists in studying African tribes familiar with cannabis.

There are many valuable aspects of Fabian's critical study of early ethnographic research in Africa, including a rich description of the culture of scientific expeditions and plenty of evidence that the context of ecstatic discovery was different from the logic of explanation in scientific papers and books. Few historians now believe without qualification that imperial scientists were the once-constructed great heroes expanding rational knowledge; Fabian admits that in this respect he is "fighting a straw man" (p. 11). Another problem is that he hardly discusses those travelogues that typify the heroic narrative of modernity's march into central Africa, such as those written by David Livingston and Henry Stanley. Instead, he bases his argument on less studied diaries and material by explorers such as, for example, Jérôme Becker, Leo Frobenius, Paul Pogge, and Hermann von Wissmann. Therefore even well-rehearsed historians of colonial exploration will find something original to enjoy (or to bite on). Yet since the narrative of heroic exploration is mostly known from the writings of explorers such as Livingston and Stanley, it is likely that Fabian could have made his argument stronger if he had scrutinized their journeys to make his point. This is not to say that this book is not a valuable contribution to colonial history of anthropological research—worth both time and attention.

PEDER ANKER

John Woodward; Robert Jütte (Editors). *Coping with Sickness: Medicine, Law, and Human Rights—Historical Perspectives*. (History of Medicine, Health, and Disease, 3.) xii + 211 pp., bibl., index. Sheffield, England: European Association for History of Medicine and Health Publications, 2000. £24.95.

These essays, first presented at a conference, "Coping with Sickness," held in Italy in 1997, address ethical and regulatory medical issues within a historical context. Many of the essays, while addressing interesting topics, combine policy analysis and critical cultural theory. Critical cultural theory can be intellectually engaging at times but is generally irrelevant to public officials concerned with specific policy issues.

Coping with Sickness is the third and final volume derived from a series of conferences cosponsored by the European Science Foundation and the Euroconferences Activity of the European Union. The eight essays are organized chronologically and cover a range of disparate topics: medical practitioners and the Spanish Inquisition (José Pardo-Tomás and Alvar Martínez-Vidal), the history of autopsy legislation in German since 1800 (Cay-Rüdiger Prull), the history of "sadism" as a medical term in the nineteenth century (Angus McLaren), folk medicine in Holland in the late nineteenth century (Willem de Blécourt), abortion in Weimar Germany (Cornelie Usborne), drug testing in Africa in the early twentieth century (Helen Power), comparative policies toward STDs (Roger Davidson and Lutz D. H. Sauerteig), and the debate over brain death in Germany (Claudia Wiesemann). As might be expected in an anthology of this sort, the quality varies considerably. Nonetheless, the subjects addressed in this volume are engaging—much to the credit of the editors.

Two pieces in particular represent the range of these collected essays. In "Vacher the Ripper and the Construction of the Nineteenth-Century Sadist," Angus McLaren, one of the best historians writing on the history of sexuality today, explores the "discovery" of sadism in the late nineteenth century by focusing on the dramatic trial of Joseph Vacher, who was charged in 1895 for the brutal sexual murder of a woman in Champuis. He later confessed to the murder and the sexual violation of another seven females and four males. Vacher had a long history of mental illness; indeed, he had been institutionalized in July 1893 following a failed attempt at suicide that left a bullet lodged in his head. At the trial the criminologist Alexandre Lacassagne was brought in as an expert witness to testify that Vacher was not insane but an antisocial sadist, as revealed by his dabbling in anarchism, vagabondism, and homosexuality. As a consequence, Vacher was found guilty and given a death sentence.

McLaren finds in this trial an example of the social construction of a new medical concept, "sadism." The emergence of the concept of sadism, he argues, reflected a "gendered notion" of defining appropriate male and female behavior; physicians at the turn of the century believed that "civilized men were most threatened, not by ex-

cess passion, but by the enervation spawned by urban life" (p. 65). The concept of sadism was also employed by doctors to enhance their own authority and to alert the public to the dangers of a male manifesting "feminine traits" and to "beat back" homosexuality.

There is much interesting conjecture to McLaren's study, but what policy lessons should be drawn from this remains unclear. If every concept is actually a social construction, reflecting the social anxieties of the age, then is the fashionable concept of social construction itself socially constructed to enhance the authority of the medical historian?

More interesting methodologically is Claudia Wiesemann's absorbing essay on the historical debate in Germany over "brain death." Relying on Ulrich Beck's social theory of simple and reflexive modernization, she shows that, when confronted with complex scientific questions, the public has to decide between competing plausible scientific claims; as a result, political groups make use of scientific expertise and counterexpertise to push their favorite practical and legal solutions.

DONALD T. CRITCHLOW

Patrice Bailhache. *Une histoire de l'acoustique musicale*. 199 pp., illus., figs., bibl., index. Paris: CNRS Editions, 2001. Fr 150 (paper).

Suzannah Clark; Alexander Rehding (Editors). *Music Theory and Natural Order from the Renaissance to the Early Twentieth Century*. xii + 243 pp., illus., figs., bibl., index. Cambridge/New York: Cambridge University Press, 2001. $64.95 (cloth).

The relationship between music and science (or the sciences) occupies a small but nevertheless well-established niche in the history of science and is also becoming increasingly recognized as significant for the history of music theory, a much younger academic discipline. These books exemplify the challenges involved in furthering scholarly understanding of how music and science have been intertwined throughout Western history, especially in terms of their ambiguous relationship to "nature."

Despite obvious differences in structure, subject matter, and methodologies, as well as being targeted at rather different audiences, these books nevertheless agree that there was a fundamental shift in Western thinking about the nature of music at the turn of the seventeenth century—a period falling within the late Renaissance or the early scientific revolution, according to disciplinary preferences. Suzannah Clark and Alexander Rehding, for example, take the scientific revolution as the starting point for their collection of nine essays, identifying it as the point when music theory "ceased once and for all to be a central discipline" (p. 13), even while music itself was reduced to "quantifiable sound" (p. 12). Indeed, it could be argued that before Newton transformed the mathematics of physics, the paradigm was "nature is musical"; thereafter the formula became "music is natural."

This transformation was first systematically explored in H. F. Cohen's *Quantifying Music: Music at the First Stage of the Scientific Revolution* (D. Reidel, 1984), and the influence of this important study is also evident in Patrice Bailhache's brief history of "musical acoustics" from the Greeks to the twentieth century. The first half of this book, divided into chapters on ancient Greece (the Pythagoreans, Euclid and Aristoxenus), the Renaissance (Gioseffo Zarlino and Kepler), and especially the "classical age" (Descartes, Isaac Beeckman, Marin Mersenne, and Galileo) concentrates on the same problems and people that Cohen sees as important, albeit from the author's distinct perspective. Yet even though the remaining chapters on the Enlightenment (Euler and Jean le Rond d'Alembert), Helmholtz, and the twentieth century (no individuals singled out here) go beyond Cohen's temporal limits, I suspect that there is little here that will interest historians of science seeking alternatives to internalist, progressive narratives. Nevertheless, with its clear format and engaging style, Bailhache's account of advances in the analysis of consonance, traced through the successive emergence of mathematical physics, physiological acoustics, and experimental psychology, is likely to prove attractive to a lay audience, especially those with a grasp of mathematics.

An important historical issue that the book raises, but does not explicitly confront, is the contested and changing meanings of apparently simple terms such as "musical acoustics," "musical science," "scientific theory," and even "nature" itself—and this is to leave aside problems involved in translation. *Music Theory and the Natural Order,* by contrast, takes this mutability as given, although ironically what is meant by "music theory" itself is never discussed. This is unfortunate, since contributors were asked to reflect not only on the treatment of "nature" *in* music theory but also on the *nature of* music theory itself. Such a wide-ranging brief has clearly stimulated activity within this highly specialized field, and the book is certainly worth having in

the library, as at least some parts have obvious relevance to current themes in the history of science. The early modern period is represented by Daniel Chua's rather weak account of the "confused" ideas of Vincenzo Galilei (Galileo's father) on ancient and modern music and Linda Austern's eclectic exploration of the interface between music and natural philosophy in seventeenth-century England. The remaining essays—notably David E. Cohen's thoughtful account of musical "instinct" and musical cognition in Jean Philippe Rameau and Peter A. Hoyt's discussion of the savage and subconscious as sources of analytical authority—concentrate chiefly on eighteenth-century French and nineteenth-century German works, a clustering that intriguingly corresponds to Bailhache's trajectory of scientific progress from Galileo through to Helmholtz. It would be fascinating to discover whether Austern's claim that early modern inquiries into the nature and properties of music "were the equal province of the philosopher, the musician, the divine and the physician" (p. 30) equally obtained in the nineteenth century, or whether the divine, for example, ceased to have authority in this field (Helmholtz himself successfully combined the other three roles). Unfortunately, since no attention is paid to the occupational and social identities of the "music theorists" discussed in these chapters (Clark, for example, does not even mention that her protagonist Arthur von Oettingen was a physicist), the scattered hints that "music theory" might be generated and legitimated within the same institutional and professional structures as "scientific knowledge" fail to add up to any coherent hypothesis. Nevertheless, *Isis* readers who have already come to grips with works like Thomas Christensen's *Rameau and Musical Thought in the Enlightenment* (CUP, 1993) and David Cahan's edited volume *Hermann Von Helmholtz and the Foundations of Nineteenth-Century Science* (University of California Press, 1993) may begin to draw some interesting connections of their own.

PENELOPE GOUK

Stephen L. Dyson. *Ancient Marbles to American Shores: Classical Archaeology in the United States*. xiv + 323 pp., illus., bibl., index. Philadelphia: University of Pennsylvania Press, 1998. $35.

The last third of the twentieth century was a time of great change within the humanities, as new directions of study and intense interest in methodology challenged traditional approaches in even the most conservative fields and found practical expression in the growth of institutional structures intended to foster innovative and interdisciplinary approaches. One of the results of this academic self-consciousness was an increased interest in the history of scholarship. Stephen Dyson has attempted to provide a history of classical archaeology as it emerged in the United States, placing individuals and institutions in the context of American culture from the eighteenth century to the present. The aim is a good one, but the book is flawed in conception and execution.

Dyson follows a generally chronological organization, at the same time stressing several major themes: academic and para-academic programs and institutions, professional organizations, the practice of archaeological fieldwork, and museums. Each receives sharp criticism, for the most part based on accusations that the discipline is dominated by the "power brokers" (p. 174) of "the Establishment" (pp. 158–216), whose "elite private connections" (p. xi) enforce conservative practices and "stiffle [*sic*] intellectual innovation" (p. 86).

It is not clear for whom the book is intended. The professional audience is already familiar with the criticisms of its institutions and practices and has heard them voiced more convincingly. A subheading like "The Met Hot Pot and the Antiquities Trade" (p. 277; concerning the scandal of the acquisition of the Euphronios krater by the Metropolitan Museum of Art) suggests that a more general audience is intended, but if that is so, the nonspecialist reader will be hindered by an insufficient explanation of the scope and aims of the discipline and by the absence of a key to the abbreviations used in the bibliography.

Dyson cites some archival materials and has made extensive use of published sources such as diaries, reports, and obituaries. For the most part, however, his treatment seems to derive from secondary syntheses such as encyclopedias, institutional histories, and general accounts of historical periods. This reliance results in sometimes conflicting generalizations. The book lacks the kind of intellectual focus that characterizes, for example, Suzanne Marchand's *Down from Olympus: Archaeology and Philhellenism in Germany, 1750–1970* (Princeton University Press, 1996).

The exclusive focus on the "ruling elite" (p. 133) in the archaeological United States leaves many questions inadequately discussed. For example, the relationship between science and the humanities is a central interest for our

time, but the uneasy attempts by archaeologists to make their discipline "scientific" are only alluded to (pp. 97, 101), and the discussion of governmental funding for archaeology by the National Science Foundation and the National Endowment for the Humanities is cast in terms of the machinations of "the ruling elite in the humanities" (pp. 228–231).

Dyson's approval is reserved for the "New Archaeology" of the late 1950s and for academic programs that favor its anthropological approaches and promote new methods in fieldwork (pp. 247–254) instead of emphasizing excavation and the teaching of Greek and Latin (p. 284). Yet the limitations of survey archaeology are a topic of current discussion; nor does it seem unreasonable that scholars of the classical cultures should be able to read the texts.

The book is filled with small errors that cumulatively cast doubt on its reliability. For example: H. F. DeCou was murdered on 11 March 1911 on page 78, but on 10 March on page 79; "Eric Sjoqvist" and "Kurt Weitzman" (p. 245: for Sjöqvist and Weitzmann); Charles Waldstein "changed his name from Waldstein to Walton" (p. 56; for Walston) and is inaccurately quoted (p. 84).

A. A. DONOHUE

■ Antiquity

Serafina Cuomo. *Ancient Mathematics*. (Science of Antiquity.) xii + 290 pp., illus., figs., bibl., index. London/New York: Routledge, 2001. $80 (cloth); $27.95 (paper).

This book treats so-called Greek mathematics, developed in the Greek-speaking world between about 600 B.C. and 600 A.D. It consists of four parts: early Greek mathematics, Hellenistic mathematics, Graeco-Roman mathematics, and late ancient mathematics. Each part is divided into two chapters, "The Evidence" and "The Questions."

This separation of evidence and questions is significant. Serafina Cuomo has refused to follow the familiar method of weaving an apparently seamless history of Greek mathematics out of fragmentary and heterogeneous documents and conjectures about them. The chapters of questions, where she points to issues that remain open, are very suggestive. For example, most important documents about the early development of Greek mathematics derive from a single lost work, the *History of Geometry* by Eudemus. Cuomo dares to cast doubt on its authenticity. Though her reservations seem extreme, we should remember that no document is neutral: Eudemus's history is a compilation, which involved choices, and the fragments we have now are the result of selection, partly intentional, partly by chance.

Another merit of this book is that it considers a wide range of activity as mathematics. Practical mathematics—such as land surveying and accounting, with their sociopolitical importance—is emphasized.

Cuomo's chief claim is that the standard historiography that associates the development of Greek mathematics with Plato's philosophy is only the version promulgated by Proclus; other descriptions are also possible. In fact, Pappus, Iamblichus, and others had their own versions. This claim is reasonable and contributes to a better understanding of Greek mathematics and the authors of late antiquity.

In her citations, the author tries to let the text speak for itself, allowing as much as a full page to a passage or a proposition, and she refrains from using modern symbolism to explain mathematical content. Though this attitude is admirable, its cost is not negligible. Readers expecting to acquire a basic knowledge of Greek mathematics may find themselves at a loss when faced with highly technical propositions presented without elucidation.

What this compact book does not include should also be mentioned. In contrast to her enthusiasm for the social and political dimensions of ancient mathematics, the author seems somewhat indifferent to its technical and theoretical aspects. Archimedes and Apollonius command only 16 pages—less than 7 percent of the text—whereas T. L. Heath dedicated 150 pages of his 1,000-page history to them (the problem is not the proportion, but the absolute length of the treatment: it is extremely difficult for anyone to give a comprehensive account of these two vitally important mathematicians in 16 handy pages). Though documents showing the importance of land surveying are frequently quoted, little is said about the practice and the technical development of this art. The technical details of Ptolemy's works are practically dismissed—but should he not have an especially important role in alternative versions of the history of ancient mathematics because of his ingenious reconciliation of rigorous theory and the limitations imposed by reality in fields like astronomy and geography?

The scantiness of the technical ingredients (probably due to the limited length of the book) makes *Ancient Mathematics* more a history of discourses about mathematics than a history of

mathematics—though this is to some extent inevitable given the character of late ancient mathematics, as Cuomo correctly emphasizes. The plan of the series to which this book belongs may be too modest to accommodate the author's ambition. Another problem attributable to the publisher is that the notes appear at the end of each chapter, so checking the references is annoying. Short references could be put in parentheses, and footnotes would be more convenient for longer notes.

KEN SAITO

Zhentao Xu; David W. Pankenier; Yaotiao Jiang. *East Asian Archaeoastronomy: Historical Records of Astronomical Observations of China, Japan, and Korea*. (Earth Science Institute Book Series, 5.) x + 438 pp., illus., tables, apps., index. Amsterdam: Gordon & Breach, 2000. $115, £76.

Archaeoastronomy has grown from curious interest in alignments of ancient sites to disciplined attempts to research the social and cultural bases of astronomical activity. Scholarship has focused not only on ancient sites but also on the historical milieus in which purposes and methods of observation developed. While Zhentao Xu, David Pankenier, and Yaotiao Jiang highlight the term "archaeoastronomy" in the title of their book, they have provided researchers in the history of astronomy with a valuable resource of observational data in ancient China, Japan, and Korea.

Astronomical brocade from Eastern Han Dynasty (reprinted from Zhentao Xu, David W. Pankenier, and Yaotiao Jiang, East Asian Archaeoastronomy: Historical Records of Astronomical Observations of China, Japan, and Korea, *plate 7.)*

A number of catalogues have been published that contain observational records of comets, meteors, and other astronomical phenomena in ancient East Asia. Not limited to one type of phenomenon, this volume provides scholars with a comprehensive set of records and a general base for understanding why astronomical phenomena were observed. In addition to a catalogue of well-annotated observations, the authors have tried to provide background on and understanding of the methodologies used by ancient Asian observers in order to place their work in historical and developmental context. While the book perhaps lacks the strong emphasis on cultural adaptation and exchange necessary for understanding the significance of observation, the authors have provided a valuable database for scholars who may not be proficient in Asian languages but wish to probe the depths of astronomical development in these complex cultures.

The major part of the book contains sections devoted to observations of specific types of astronomical phenomena. Each section includes a brief description of the historical basis of observation, extensive references for those who wish more detailed discussion of principles, and a chronologically ordered set of English translations of observational records. For these alone, Western scholars interested in Asian astronomy will find this volume a very useful addition to their libraries.

Organizationally, a general introduction to observation in ancient East Asia is followed by a review of the more culturally specific use and meaning of oracle bone inscriptions in China. Subsequent sections are then devoted to observational records of eclipses, comets, novas, sunspots, auroras, and planetary movement. Introductions to each section include some attention to the methodological use and purpose of the observations. Records from well before the Christian era to the seventeenth century are included. Each translated record includes both text and specific reference to the manuscript or manuscripts from which the record was taken. Controversy with regard to authenticity or origin is duly noted.

The last third of the book contains an exten-

sive "appendix" of the same observational records written in the original language. This will be particularly valuable for scholars who have knowledge of Chinese characters and wish to check the accuracy of translations or seek more cultural subtlety in descriptions of observations. Scholars from East Asian countries will also find the comprehensive set of observations recorded in the original language invaluable as a data resource.

While many ancient observations from Japan and Korea are included, the volume shows the strong influence of Chinese astronomical development on these two cultures. The need for more exhaustive research into the historical and cultural development of observation and the subtle (and not so subtle) ways in which Chinese principles were adapted and changed for varying uses in Korea and Japan is not readily apparent in this volume. However, such interpretive work is far beyond the scope of the authors' purpose at this point.

The general discussions of historical and methodological bases are somewhat minimal, but the reader with more than a general knowledge of Asian astronomy will find enough in them to contextualize the records of observation that follow. For example, while the authors discuss the Chinese lunar calendar, readers with a strong interest in the development, adaptation, and mathematical base of this system will want to consult other sources. However, this volume will provide records of numerous observations related to calendar development, such records having been collected in a rigorous and disciplined fashion. This book is not an introduction to Asian archaeoastronomy. Readers who purchase it will no doubt want to consult the works cited in the numerous references for more exhaustive discussion of the subjects of each section.

Our knowledge of the complexity of the development of astronomy in Asian cultures seems to grow each year as more texts are translated and researchers cross linguistic and social barriers to share their work. This volume will provide an invaluable resource for scholars working primarily in English who wish to open the door to such understanding further.

STEVEN RENSHAW

Lynn E. Rose. *Sun, Moon, and Sothis: A Study of Calendars and Calendar Reforms in Ancient Egypt*. (Osiris Series, 2.) xxxvi + 339 pp., illus., apps., bibl., index. Deerfield Beach, Fla.: KRONOS Press, 1999. $38.

This book is an attempt to undermine the pillars on which Egyptian chronology has been built, in particular the view "that the Egyptians had monitored the heliacal risings of Sirius [Sothis] for millennia, and in such a way that we can date the various pharaohs and dynasties of even three or four thousand years ago by means of the 'Sothic dates' that they sometimes seem to provide" (p. 195). For the most part, the book is a reevaluation of key calendar-associated sources (the 25-year lunar cycle in Papyrus Carlsberg 9, the Censorinus text on the so-called Sothic period or cycle of 1,460 years, the Canopus Decree on the failed calendar reform of Ptolemy III Evergetes), but in the final chapters the author proposes a radical revision of the chronology of ancient Egypt. On the basis of a new analysis of the lunar dates in the El-Lahun papyri, he shifts the Twelfth Dynasty, and by implication the whole Middle Kingdom (traditionally dated in the early second millennium B.C.), by some fifteen centuries (i.e., a full Sothic period) to the period immediately preceding the conquest of Egypt by Alexander the Great in 332 B.C.

In doing so, Lynn Rose follows in the footsteps of Immanuel Velikovsky, whose unproven theory of a near-collision between Earth and Venus in the seventh century B.C. ("the Velikovsky Divide") he fully accepts. In his opinion, all documents referring to a year of 365 or 365¼ days or lunar months of 29½ days (such as the El-Lahun papyri dating from the Twelfth Dynasty) must therefore be younger than this supposed catastrophe. The evidence presented to support these and other assumptions (e.g., that the Egyptian year of 365 days was a Venus year) is unconvincing. Moreover, his overall approach is methodologically rather questionable. Convinced as he is of the superiority of astronomical evidence that must be considered decisive, Rose has little or no regard for historical, iconographic, and archaeological sources. And when he does refer to such material, it is only to dismiss it outright as suspect (p. 278: "Ramesside and Ptolemaic temples are very similar, but they have been placed a full millennium apart"), anachronistic (p. 273: "king-lists that mentioned the various monarchs of the Twelfth Dynasty can be from no earlier than the Macedonian period"), or even censored (the published results of radiocarbon dating; see pp. 278–281). He also fails to take into account the evolution of script over a period of fifteen centuries, when he proposes to shift the El-Lahun papyri, written in the so-called hieratic script, from the nineteenth to the fourth century B.C.

Despite all these flaws, some parts of *Sun,*

Moon, and Sothis deserve to be taken into consideration. This is especially true of the reassessment of the 25-year lunar cycle in the opening chapters, by far the best part of the book. Also interesting and useful are Rose's system to retrocalculate the dates of heliacal risings of Sirius and his discussion of the true length of the Sothic cycle. His findings in this respect should be compared with those of other scholars who have recently worked on the subject (in particular G. W. van Oosterhout in *Discussions in Egyptology,* 1992, *24:*72–111). Unfortunately, Rose himself ignores much of the recent work, not only on the heliacal risings of Sirius but also on the Egyptian calendar in general.

Finally, I have the impression that the author has a somewhat anachronistic idea of how ancient calendars and cycles functioned in practice. He believes in their rigid application and thinks that the ancients took a similar approach to these devices. In this respect, he should have remembered the words of one of the authors mentioned in his bibliography, E. J. Bickerman, who wrote, precisely with regard to the Egyptian calendar and Sothic dating: "A calendar is a tool which cannot be justified by either logic or astronomy" (*Chronology of the Ancient World,* rev. ed. [Thames & Hudson], 1980, p. 42).

GEORGES DECLERCQ

M. J. T. Lewis. *Surveying Instruments of Greece and Rome*. xx + 389 pp., illus., figs., tables, apps., bibl., index. Cambridge/New York: Cambridge University Press, 2001. $80.

The general neglect of ancient surveying by classical scholars can be demonstrated easily. The third edition of the *Oxford Classical Dictionary* (Oxford, 1996) has no article on surveying. The great *Real-Encyclopädie* has two short articles (in Vol. 5 and Suppl. 6) on the Greek *dioptra* but nothing at all on the Roman *libra. A History of Technology* (Oxford, 1956) has no section on surveying. Even the indefatigable Otto Neugebauer seldom mentions terrestrial surveying, and the best introduction to the subject is probably the chapter in Edmond Kiely's *Surveying Instruments: Their History and Classroom Use* (Teachers College, Columbia Univ., 1947), pages 18–44.

Until now. In *Surveying Instruments of Greece and Rome,* M. J. T. Lewis has gathered and set out a wealth of material, suggested important revisions to several accepted views, and set the study of ancient surveying instruments and techniques on a firm foundation.

The work is divided into three parts. In Part 1 Lewis draws on an impressive array of literary sources, plus archaeological evidence, later parallels, and his own trials with reconstructed versions of the *dioptra* and the *libra,* to trace the history, design, and capabilities of the instruments. He is particularly interested in the Greek *dioptra.* Here he brings to bear much new information (not least a previously unnoticed Arabic text by Al-Karaji, which derives, as Lewis shows, from a Greek original), and he emphasizes both the impressive pioneering efforts of the Greeks (where earlier scholars dealt primarily with Roman surveying) and the close connection in Greek surveying between theoretical science—mathematics and geometry—and technical achievement. On the Roman side, Lewis points out (following Kiely) that the *chorobates* would have been impossibly cumbersome in the field, and he argues convincingly that the *libra* was the instrument of choice for Roman surveyors. We do not know what this instrument looked like, but a close analysis of the word *"libra"* and of references to the instrument allows Lewis to suggest a plausible reconstruction. Relatively short but very interesting sections on the *groma* (used in centuriation and road planning) and on Vitruvius's hodometer complete Part 1.

Having established the nature of the instruments, Lewis deals in Part 2 with various practical applications. Fine chapters outline the attempts of the Greeks to determine the circumference of the earth and, related to that, the heights of mountains. In the area of civil engineering, Lewis discusses examples (he does not attempt to be exhaustive) of canals, aqueducts, tunnels, and roads. Here are some of the most impressive accomplishments of ancient surveyors: the very shallow gradients of some Roman aqueducts (one section of eight kilometers at 1 in 18,500 on the Nîmes aqueduct, for example); long tunnels, where occasionally we can identify a mistake in surveying; and the great straight stretches of Roman roads in Britain, where Lewis sets out a new theory on how they were surveyed and planned.

In Part 3 Lewis provides translations of four extended passages on the design and use of the *dioptra.* He also translates 106 shorter passages drawn from a remarkable range of authors, inscriptions, and papyri and dealing with the various instruments and their uses. The translations are not word for word except where they need to be, but they are accurate and admirably clear. Lewis is the first to provide translations into English of the major texts on the *dioptra* (a colleague translated the Arabic for him), and this

section will be a great help to scholars. My one regret is that he does not include the Greek texts on the *dioptra.* I understand the financial constraints; but few libraries have all of these texts, and including them would have added only some twenty-five pages.

Throughout this absorbing work, the level of scholarship is very high. Lewis's discussions of the evolution of the instruments and of ancient attempts to establish the circumference of the earth constitute an important contribution to intellectual history. Mistakes are few and insignificant. The author hopes that the volume will encourage further study, and it should. Quite apart from its importance for those who study ancient monuments and engineering projects, the book provides an excellent starting point for those investigating maps (currently a matter of debate), precision instruments and their manufacture (not discussed explicitly by Lewis), and the transmission of scientific ideas in the Greco-Roman world. Lewis writes engagingly, and his combination of first-rate scholarship and hands-on problem solving is irresistible.

GEORGE W. HOUSTON

■ Middle Ages & Renaissance

Olivier Boulnois. *Être et représentation: Une généalogie de la métaphysique moderne à l'époque de Duns Scot (XIIIe–XIVe siècle).* (Épiméthée.) 538 pp., figs., tables, bibl. Paris: Presses Universitaires de France, 1999. Fr 278.

Olivier Boulnois argues that a radical shift occurred in metaphysics at the end of the thirteenth century, a shift completed and systematized by Duns Scotus. The shift concerns being, the subject of metaphysics: from God (Aristotle, *Metaphysics* Λ; Averroes; Aquinas) to a universal exhibited by God and creatures alike (Aristotle, *Metaphysics* Γ; Avicenna; Duns Scotus). This shift went hand in hand with an account of signification as representation: signs, rather than being in some sense identical with the things they signify, merely represent these things. According to Boulnois—and this is where the thesis becomes original—the basic motivation for the shift in opinion on the nature of metaphysics lay in the insight of Roger Bacon that spoken and written signs signify things directly, without the mediation of concepts. This means that there is no place for an analogy of being, since "being" is a sign either of things that fall under one concept (univocity) or of things that together fall under many different concepts (equivocation). (The Thomist account of metaphysics requires that God and creatures are beings in an irreducibly different sense—hence the requirement for the analogy of being on this view.) "Being"—the object of thought—is identified by Scotus as *non nihil,* whatever is logically possible, and in this sense is univocal to the Aristotelian categories and to God. But identifying being in this way means that metaphysics does not restrict its study to the actually existent. It studies anything that is logically possible. (Hence Boulnois prefers "tinology" to "ontology" as the correct designation of general metaphysics.) But the logically possible is whatever can be represented: so the representable is in some sense the subject of metaphysics. Concepts are signs of the representable, and thought becomes a sort of semantics, a mental language of representational concepts whose objects are the representable, the logically possible. So ultimately the univocity of being can be traced too to the insights about representation: the status of being as the representable entails the univocity of being. Knowledge of the representable considered independently of actual existence is abstraction; knowledge that attends to the actual existence of the representable is intuition—though in both cases to know something is for a representation of it to inhere in the mind. The ultimately representable object is the divine essence, which somehow contains in itself all other representable objects—it contains "ideas" of everything other than the divine essence. As representable objects, however, these ideas depend for their existence on the divine essence: to this extent, God's mental contents are caused totally by himself. God does not require representations in order to cause; he causes the representable objects and thus causes his representations of them.

Clearly, if Boulnois is right, the sorts of innovation usually associated with Wolff can be traced not merely to Suarez but in fact to Scotus and his contemporaries. And the relation with Kant is obvious enough, though the medievals (all of whom were epistemological realists) would been unhappy with the Kantian twist—the distinction between the phenomenal and the noumenal.

The importance of the book in the history of science is to some extent tangential: in this case, more a matter of circumscribing areas of disciplinary competence and relevance—determining the domain of physics by showing what metaphysics pertains to. And there are some faults: a failure to engage with the latest Scotist scholarship on the question of the relation between the possible and the thinkable (representable) and, likewise, a failure to consider with

sufficient clarity the extent to which Scotus really agrees that spoken signs signify extramental reality directly. But overall *Être et représentation* is a significant achievement, essential reading for anyone interested in the history of metaphysics.

RICHARD CROSS

Marianne Pade (Editor). *Renaissance Readings of the* Corpus Aristotelicum. (Proceedings of the conference "The New Aristotle: Renaissance Readings of the *Corpus Aristotelicum,*" organized by the Forum for Renaissance Studies, University of Copenhagen, 23–25 April 1998, Copenhagen.) 261 pp., illus., index. Copenhagen: Museum Tusculanum Press, 2001.

Of the thirteen articles in this collection (two in Italian, the rest in English), the following are likely to be of most interest to historians of science.

In "Aristotle and Perspective in the Early Italian Renaissance" Marianne Marcussen argues that before pictorial perspective could be developed for painting, certain conceptual problems concerning infinity, motion, and the visiblity of a mathematical point (the vanishing point) had to be solved, and Aristotle's distinction between pure and applied mathematics had to be "softened" to allow motion and sensation to be introduced to mathematics. This was begun by Nicole Oresme, who combined pure mathematical proportions with physical qualities in his configuration theory to create visual models, and then by the perspectivist painters Fillipo Brunelleschi and Leon Battista Alberti, who similarly combined proportion theory with optics (medieval *perspectiva*). Unfortunately, Marcussen can give no evidence, beyond a few imprecise definitions of Alberti's, that painters were actually worried about infinity, motion, and the visibility of a point. Motion had long been an essential part of a mathematical science—astronomy—that, like music and *perspectiva,* was not an applied science but a middle or mixed science. Pictorial perspective is an applied science—the application of *perspectiva* to a simple kind of projection; a more complex kind, spherical projection, had been treated theoretically by Jordanus de Nemore in the mid-thirteenth century and had long before that been applied to astrolabes without any difficulties over infinity, motion, or the visibility of points.

Eckhardt Kessler argues in "Metaphysics or Empirical Science?" that the emancipation of natural philosophy from metaphysics, which characterized early modern science, began within Aristotelian thought in the early sixteenth century under the influence of the commentaries of Alexander of Aphrodisias and the Florentine neoplatonists. Implicit in Aristotle's works, Kessler notes, is the distinction between the "metaphysical" approach to natural philosophy of the *Physics,* which considers *ens mobile* from the first principles of matter, form, and potency, and the "naturalistic" or empirical approach of the *De generatione et corruptione* and the *Meteorology*, book 4, where the subject matter is *corpus potentia sensibile* and its principles, the four elements and their two pairs of contrary sensible qualities. Kessler then traces a new emphasis on the naturalistic approach through the works of Pietro Pomponazzi, Simon Portius, Girolamo Fracastoro, Girolamo Cardano, and Bernardino Telesio, suggesting along the way that the denial of substantial forms, especially as self-subsisting, seems to entail a general tendency to deny the immortality of the human soul. These naturalistic philosophers also reveal (though Kessler does not put it so pointedly) either a return to pre-Aristotelian cosmic principles (such as attraction and repulsion, like and unlike, warmth, and soul) or a new, naïve sort of quasi-Aristotelian metaphysics, where matter takes the place of substance and sensible accidents alone determine the nature of things. In this way they certainly foreshadow seventeenth-century materialist natural philosophers, though their philosophical shortcomings (as Kessler notes) were recognized by Francesco Patrizi and Tommaso Campanella, among others.

In "The Aristotelian Classification of Knowledge in the Early Sixteenth Century" Heikki Mikkeli sketches the rise in status of the mechanical arts in classifications of the sciences by Angelo Poliziano, Josse Clichtove, Juan Luis Vivès, and others. He plausibly attributes this to the influence of the humanists' emphasis on useful knowledge, and he notes the decline in the second half of the sixteenth century of the liberal arts as the basis of classification. Gianfrancesco Pico della Mirandola, nephew of the more famous Giovanni, is known in his own right for his religious writings, including a life of Girolamo Savonarola, and for his use of skeptical arguments against the philosophers. In "Giovan Francesco Pico e i presupposti della sua critica ad Aristotele," Cesare Vasoli presents Gianfrancesco's criticisms of Aristotle and peripatetic philosophy contained in the fourth book of his *Examen vanitatis doctrinae gentium et veritatis Christianae disciplinae.* Alexander of Aphrodisias infamously held that the soul was a material form; Olaf Pluta offers in "The Transformations

of Alexander of Aphrodisias' Interpretation of Aristotle's Theory of the Soul" an account of the medieval and Renaissance fortunes of Alexander's writings on the soul, arguing that Alexander influenced Pietro Pomponazzi through the works of Jean Buridan. Antonis Fyrigos, in "Joannes Cottunios di Verria e il neoaristotelismo padovano," gives a summary of Joannes Cottunios's interpretation of the rational soul from his *De triplici statu animae rationalis* (1628) and argues that Cottunios, a Greek who wrote in Latin, has been unjustly neglected and undervalued as an original interpreter of Aristotle.

Peter Wagner, in "Renaissance Readings of the *Corpus Aristotelicum*—Not among the Herbalists," argues to the negative conclusion that Renaissance herbalists generally did not rely much on the *corpus Aristotelicum,* with the notable exception of Andrea Cesalpino, who devised a more natural system of plant classification based mainly on the form and function of reproductive organs, an approach that was not taken up by other botanists until the next century. The philosophical dimension of Cesalpino's classification of plants is further considered by Kristian Jensen in "Description, Division, Definition—Caesalpinus and the Study of Plants as an Independent Discipline." Jensen shows how Cesalpino, drawing mainly on Aristotle's *De partibus animalium,* rejected classifications based on bipartite division by single *differentia* and sought instead to discover the defining characteristics of the species in the reproductive parts of the plant considered as instruments of the vegetative soul. In so doing, Jensen argues, Cesalpino contributed to the redefinition of the relation between medicine, the study of plants, and natural philosophy in general.

The other articles in this volume—by Gert Sørensen, David A. Lines, John Monfasani, Sten Ebbesen, and Bo Lindberg—concern topics of less interest to most readers of *Isis:* logic, ethics, politics, and humanist scholarship. The volume contains an index of names and passages in Aristotle's works, but no introduction.

W. R. LAIRD

Alison Cornish. *Reading Dante's Stars*. xii + 226 pp., figs., index. New Haven, Conn./London: Yale University Press, 2000. $25.

This is the first comprehensive book delineating, explaining, and exploring how and why Dante used astronomy and astronomical references in several of his works. For Dante, stars, in themselves "objective reality" or "observed phenomena," are "texts open to differing interpretations" (p. 142). They demonstrate the poet's fundamental conviction that science and faith are not distinct realms of experience and knowledge but are themselves the very proof of their interrelationship. After a brief introduction, the book divides into eight chapters, the first of which takes up Dante's interest in stars in his earlier works, the *Vita Nuova* and *Convivio,* and suggests how this passion follows him in the *Commedia.* Next is a chapter that shows how celestial bodies function to delineate the time of Dante's journey in the *Commedia,* even while the "time-references refer not just to the natural cycles of celestial objects, but to a particular, unrepeatable moment in linear time" (p. 27). From thence Alison Cornish focuses on particular cantos. Chapter 3, which advances highly original and imaginative readings of *Inferno* 20, 24, 26, clusters these cantos to show how, despite the fact that the stars are essentially absent from Dante's hell, the references to farmers introduce them even there. As Cornish puts it, "In the *Inferno,* it is the unassisted gaze of the pagan mind that seems to be the target of its few astronomical references, to each of which is attached the figure of a farmer" (p. 53). Farmers read the signs of nature, which gets its orders from celestial bodies.

Chapter 4 deals with *Purgatorio* 9, and Chapter 5 shows how time in *Paradiso* contrasts with its representation in *Purgatorio,* as a concrete notion of time ruled by celestial movement gives way to a metaphoric temporality. Chapter 6 examines *Paradiso* 13 and the heaven of the sun; and Chapter 7, on *Paradiso* 28, elaborates on the platonic link between astronomy and poetry and on Plato's scorn for both disciplines on the grounds that both are shadows or representations of something else and therefore neither express the truth. Chapter 8, on *Paradiso* 29, elaborates on the "penultimate, grand astronomical exordium of the poem that introduces Beatrice's discourse on the angels" (p. 119).

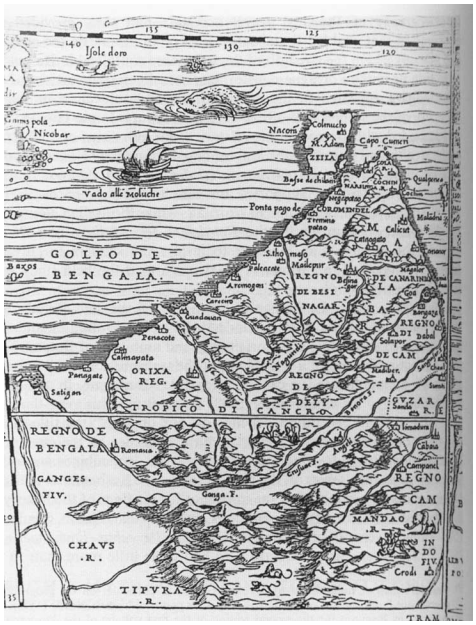
While *Reading Dante's Stars* is primarily directed at Dante scholars eager to understand the poet's profuse use of astronomical references, it nonetheless has much to offer those interested in medieval science and astronomy in general. Cornish's mastery of astronomy, medieval and ancient source texts, and complex mathematics draws attention to the encyclopedic scope of Dante's knowledge and to how he transformed this intellectual activity into mystical poetry. As such, this book provides a rich compendium of the complex machinery of the poem. For those interested more specifically in medieval science,

the book both explains medieval astronomy and demonstrates how this academic knowledge could be applied in a literary work with encyclopedic breadth.

BRENDA DEEN SCHILDGEN

Joan-Pau Rubiés. *Travel and Ethnology in the Renaissance: South India through European Eyes, 1250–1625*. (Past and Present Publications.) xxii + 443 pp., illus., tables, bibl., index. Cambridge/New York: Cambridge University Press, 2001. $74.95.

Travel and Ethnology in the Renaissance, based on a Cambridge dissertation, is a reaction to two related trends in recent writing about early modern European travel literature, both ultimately deriving from the "Orientalist" model presented in the work of Edward Said on the nineteenth and twentieth centuries. These are the trend that views travelers' accounts as more revelatory of European concerns than of the reality of the non-European societies they wrote about and the trend that analyzes these texts using the techniques of literary criticism and scholarship rather than those of historical investigation. Joan-Pau Rubiés argues, by contrast, that the texts were the product of a genuine, although sometimes distorted, cultural encounter between Europeans and others; the argument relies on a case study of European writing about south India, particularly the Hindu kingdom of Vijayanagara. The south Indian literature is less well known than that produced by early modern travelers to larger states such as China and the Ottoman Empire. Even in examining accounts of India itself, the Muslim Mughals of north India get more attention. Rubiés's task is complicated by the fact that there are so many mysteries in Vijayanagara history. The book will sometimes be heavy going for those unacquainted with south Indian history and culture.



Map of India perpared by Jacopo Gastaldi in 1554 (reprinted from Joan-Pau Rubiés, Travel and Ethnology in the Renaissance: South India through European Eyes, 1250–1625, *p. 45.)*

Rather than beginning with the Portuguese invasion of the Indian Ocean at the end of the fifteenth century, Rubiés starts with Marco Polo and discusses fifteenth-century Italian accounts. This emphasizes the continuity between the late Middle Ages, when Europeans arrived as travelers or merchants, and the early modern period, when Europeans were associated with an imperial project. The bulk of Rubiés's analysis is devoted to a group of texts produced by sixteenth-century Italian and Portuguese travelers. These travelers and missionaries in south India are less well known in the English historical literature than are their British and French equivalents, and Rubiés has unearthed some fascinating characters. Rubiés defends the accuracy, perceptiveness, and open-mindedness of the Western observers from charges of ethnocentrism. Since so much contemporary scholarly knowledge of early modern Vijayanagara is derived from these Western travelers' accounts, lauding them for their accuracy involves an obvious problem of circularity, which Rubiés attempts to evade by asserting the accounts' consistency with recently obtained archaeological data.

By contrast with the Latin Christian writers whose perceptive readings of Vijayanagara's culture Rubiés lauds, the Muslim writers of north India who wrote about Vijayanagara come off very poorly. Rubiés finds them, despite some insights, mostly unable to break from the paradigm of "holy war," a permanent structural conflict between Muslims and idolators, to perceive Vijayanagara as it was. In this case it is the Orientals who are the Orientalists.

The European writers employed two distinct sets of categories for the analysis of Vijayanagara, one historical, in which Vijayanagara was analyzed in terms of its conformation to an ideal of "civilization," and one religious, in which it was negatively evaluated as "idolatrous." Rubiés claims that these travelers' accounts had major

effects on the culture of Europe itself, in which the historical discourse expanded and partially incorporated the religious discourse. In a way reminiscent of traditional European intellectual historians such as Paul Hazard, Rubiés finds travel literature, with its interest in human diversity, to be a root of the Enlightenment. His case against the applicability of the Orientalist paradigm is convincing.

Although it is, unfortunately, priced beyond the reach of most scholars, this book will be of interest to students of the origins of ethnography and anthropology. Cambridge University Press and the author deserve praise for producing a book with proper bottom-of-the-page footnotes, a full bibliography, and a fine index.

WILLIAM E. BURNS

John Crawford Adams. *Shakespeare's Physic*. 192 pp., illus., bibl., index. London: Royal Society of Medicine Press, 2001. £10 (paper).

With *Shakespeare's Physic,* John Crawford Adams joins that group of physicians so fascinated by the medical aspects of Shakespeare that they cannot resist a foray into medical and literary history. Adams follows men like R. R. Simpson, whose *Shakespeare and Medicine* (Livingstone, 1959) was until recently the best book available on the subject. Like Simpson, Adams is not a historian, nor is he a literary critic, and like Simpson's book, *Shakespeare's Physic* has consequent strengths and deficiencies.

To be sure, Adams's book has a charming energy. He quotes widely and admiringly from Shakespeare's work and demonstrates considerable reading in medical history. His training in medicine, moreover, allows him to provide an interesting modern perspective on the illnesses that would have afflicted Shakespeare's contemporaries. Additionally, Adams, as neither a historian nor a literary critic, does not get bogged down in theory or minutiae, and so the book is accessible to nonspecialists.

Often, however, one wishes Adams had been a historian so that he might have been more scrupulous in his work, for the lack of historical rigor here seriously detracts from the book's usefulness. Aside from quotations from Shakespeare, and notwithstanding the brief bibliography that is included, Adams fails to cite any of his sources. We have no way of directly checking his facts, and there is no way to build on his work.

Moreover, if Adams had been more attentive to citing evidence for his claims, he might have more easily noticed when those claims were questionable. The bacterium that causes the plague, for example, is now properly called *Yersinia pestis,* not *Pasteurella pestis,* as Adams has it (p. 54; see Leeds Barroll, *Politics, Plague, and Shakespeare's Theatre* [Cornell, 1991]). More significantly, Adams's suggestion that madness was understood almost exclusively as a result of demonic possession and that it was treated only by exorcism is clearly incorrect (p. 126). Richard Napier's early modern practice frequently employed physical treatments for madness since some forms of madness, including melancholy, were thought to proceed from the same sorts of humoral imbalances that were held to produce other maladies (see Michael MacDonald, *Mystical Bedlam* [Cambridge, 1981]; cf. Timothy Bright's *Treatise of Melancholy* [London, 1586]). Moreover, Winfried Schleiner has discussed the therapeutic uses of illusion in the period and links that mode of treatment to several Shakespeare plays (see "Prospero as a Renaissance Therapist," *Literature and Medicine,* 1987, *6:*54–60). David Hoeniger has shown how Shakespeare demonstrates knowledge of the early modern use of music as treatment for mental illness (see *Medicine and Shakespeare in the English Renaissance* [Associated Univ. Presses, 1992]).

Also troubling is Adams's unscholarly tendency to assume that the medical aspects of Shakespeare's theater "must generally have been an accurate reflection of the world as it existed outside" (p. 7). Similarly, Adams repeatedly assumes that characters speaking on medical and spiritual matters must be voicing Shakespeare's own particular philosophy. Literary scholars now acknowledge that such claims are unreliable, if not naive. Malvolio in *Twelfth Night,* for example, firmly rejects the Pythagorean notion of metempsychosis or reincarnation; Feste, in the same play, insists that it is true; Graziano in *The Merchant of Venice* has thought it false but is tempted by Shylock's wolfish temperament to believe it. Which is the view of the general public? Which is Shakespeare's?

TODD PETTIGREW

■ Early Modern (Seventeenth and Eighteenth Centuries)

Peter Dear. *Revolutionizing the Sciences: European Knowledge and Its Ambitions, 1500–1700*. viii + 208 pp., illus., figs., bibl., index. Princeton, N.J.: Princeton University Press, 2001. $59.50 (cloth); $16.95 (paper).

Peter Dear's brief survey of the Scientific Rev-

olution joins a growing list of recently published books of the same sort, including my own. This proliferation is no bad thing. The subject is now so big that there is no one right way to summarize and characterize it. Dear's contribution, like each of the others I know about, makes one kind of sense of it, so that, taken together, they constitute a mosaic of useful understandings.

Dear starts with the ancient Greeks and works up to the early eighteenth century, covering along the way both physical and life sciences, with the emphasis on the former. This is not a primer but an advanced, though brief, conspectus. The prose is clear and the arguments cogent, but the approach is sophisticated and demanding and requires some previous background. This is not a book for the beginner but for the initiated, and even the specialist may be engaged by it. Dear assists the reader by providing a brief bibliographical essay, a list of the thinkers discussed in the text, and a glossary of difficult terms.

Dear concentrates on certain key thinkers and devotes little attention to others. Those on the A-list include Aristotle, Paracelsus, Bacon, Galileo, Descartes (who gets an entire chapter and then some), and Newton. Those who get much less coverage are Boyle and Pascal, for example, while Bruno, Campanella, Mersenne, Gassendi, and Wilkins are passed over with barely a mention.

Certain themes are also emphasized and others neglected. Dear makes much of the impact of humanism on science but has less to say about such key issues as science and Protestantism or science and the English Revolution. In general, his account is distanced from the larger religious, social, and political contexts of science. Instead, what he gives us are the philosophical, rhetorical, and educational contexts in which scientists were trained and formulated their arguments. The famous confrontation between Hobbes and some of the leading Fellows of the Royal Society, for example, is reduced to a discussion of their differences over the nature and validity of experiments and how to arrive at demonstrative truth. This is fine as far as it goes, but it begs the important question of how the ideological differences between Hobbes and his opponents in the Royal Society reflected a crisis of order and authority in society at large.

Dear's treatment has its strengths, of which I shall mention two that stand out for me. First, Dear shows how the Aristotelian framework in which medieval science was cast continued to operate both positively and negatively on the development of scientific argument throughout the sixteenth and seventeenth centuries. Second, Dear's extended treatment of the development of Cartesian natural philosophy by both Descartes and his followers is a model of exposition. My only caution here is that Dear gives short shrift to Descartes's *The Passions of the Soul,* published not, as he says, in 1647 but in November 1649. Recently scholars, led by John Cottingham, have put this work at the center of Descartes's philosophical enterprise and shown that his science had two goals, that is, to make us not only "masters and possessors of nature" but also masters of ourselves, which would give us emotional and psychological self-control. This a big theme, not just in Descartes but in early modern natural philosophy, and it deserves more attention in the textbooks.

Dear writes with a gravitas befitting his subject but ends on a distinctly anachronistic note. His final sentence reads: "The modern world is much like the world envisaged by Francis Bacon." But Bacon's science is sponsored by an autocratic state and is meant to be benevolently and systematically directed to the relief of human misery and want. As Bacon says, "the role of science is both to inquire [into nature] and to desire wisely" (*Novum Organum,* bk. 2, aphorism 49). This latter function—the wise desiring—is fundamental to the emergence of modern science but light years from the modern world I know and live in. One must look elsewhere than to Dear for this crucial side of the story.

JAMES R. JACOB

Margaret C. Jacob. *Scientific Culture and the Making of the Industrial West*. xiv + 274 pp., illus., bibl., index. New York/Oxford: Oxford University Press, 1997.

For those in the so-called G-7, G-8, or G-20, searching for the formula for economic takeoff, this is a book that deserves a reckoning. It explores the "role of culture," which hitherto has had "no place in traditional economic explanations" (p. 105) of the history of industrial achievement. It is in the cultural and epistemological transformation of the eighteenth century that Margaret Jacob finds the foundation of industrial revolution. Jacob thereby dismisses the myth of the accidental genius or the inspired semiliterate backyard tinkerer that has reinforced the notion that nothing can be done except by lives of singular initiative.

By comparing the British most particularly with the Dutch and the French, Jacob argues it was precisely the utilitarian and egalitarian sen-

timents found in British scientific and philosophical societies that mattered most. Her theme is the much-contested vision of the efficacy of natural knowledge and the assimilation of natural philosophy by the utilitarian world of the artisan and the entrepreneur. The advance of mechanical philosophy under Galileo and Descartes notwithstanding, it was during the English Revolution of the mid-seventeenth century that natural philosophy was explicitly attached to a new social reformation and an inchoate democratic vision.

Isaac Newton had nothing but derision for the vulgar "inferior horde of people" (p. 88). Yet his disciples launched into careers as purveyors of his science in public lectures heavily weighted to mechanical explanation. Often viewed with suspicion elsewhere in Europe, in England such lecturers flourished. Even the engineer James Watt's French contact J. C. Perier, who desperately sought as many British secrets as possible, had initially learned his science from the lecturer abbé Nollet. An increasing awareness of the practical consequences of the new science was essential to the economic achievements of the late Enlightenment. This ultimately is a major thrust of Jacob's argument, for many early industrialists "were smarter than historians have allowed them to be" (p. 206). Those who sought the new steam engines for their mines were perfectly well aware of the mechanical limitations and natural variables that might affect the success of their enterprise. Jacob is quite correct that "the Baconian vision lived on, eventually subsumed under the larger rubric of Newtonianism" (p. 33). At the end of the century, the Napoleonic technocrat Jean-Antoine Chaptal appears as representative of the case for applied science by promoting the Baconian unity of theory and practice.

The most innovative part of Jacob's story is the attention she pays to the role of science in the attitudes of Midlands entrepreneurs like James Watt and Matthew Boulton. Both insisted on a scientific education for their sons, and Boulton recommended as much for the sons of foreign visitors to the Soho foundry. Both Watt and Boulton were particularly adept in the experimental laboratory, although they were also highly suspicious of the growing public technical interest and scientific curiosity that they feared might undermine their own patent monopoly. This suspicion grew out of their own deeper unease at the burgeoning democratic sympathies. Boulton and James Watt, senior, were extremely critical of radicalism—so we need not, therefore, draw any simple equation between industrial innovation and political reform. The currents of late eighteenth-century Britain were far too complicated for that. Jacob, nonetheless, draws broad and interesting conclusions about the ways in which Continental cultural and political factors obstructed the spread of the very natural knowledge that gave the British such advantages. In her view, especially in this age of a "knowledge economy," it might now do us well to remember the potential effects of "the widest dissemination of scientific knowledge" and "the democratization of learning" (p. 207). In Jacob's version of the Industrial Revolution, the broadening and deepening of democracy followed the spread of natural learning beyond the uses of traditional elites.

LARRY STEWART

Vladimir Janković. *Reading the Skies: A Cultural History of English Weather, 1650–1820.* xiv + 272 pp., frontis., illus., tables, app., bibl., index. Chicago/London: University of Chicago Press, 2001. $55 (cloth); $20 (paper).

English people have long been renowned for their obsession with the weather: Francis Bacon chose to write about the wind for the first installment of his natural history. Place is central to Vladimir Janković's analysis, so it is highly appropriate that he should focus on England to study the prehistory of quantitative meteorology. Janković's major innovation is to argue that local interests in recording strange weather conditions later became converted into the global concerns of nineteenth-century scientists. Before then, he maintains, reading the skies was not a science, because it meant engaging with the human rather than the physical environment. Janković demonstrates convincingly that the activities of provincial naturalists, and their concern with investigating abnormal events to strengthen regional identities, played vital roles in altering the observation and interpretation of weather conditions.

Meteorology is a tricky discipline to discuss historically because the word's significance has changed dramatically. In Janković's period, the seventeenth and eighteenth centuries, it embraced diverse celestial phenomena—meteors, comets, and the northern lights as well as rain, snow, and so forth. Rather strangely, considering his historical sensitivity, Janković uses the word "meteorology" throughout, a confusing decision. Well organized into seven distinct chapters, *Reading the Skies* opens by examining how traditional beliefs fashioned weather reports and includes a fine investigation of the political and

religious implications of extraordinary phenomena. The central section explores the importance of regional interests in recording weather characteristics for local natural histories. Clergymen were particularly important in maintaining research traditions based on provincial pride, and the career of an erudite Cornish naturalist, Reverend William Borlase, illustrates the relationships between national and local concerns. The final two chapters concentrate on changes toward the end of the period, discussing how agricultural requirements and the demands of metropolitan chemical reformers resulted in shifts away from localized studies based on classical principles. In the early nineteenth century specialized experts and instrument makers were converting the whole world and its atmosphere into their laboratory. They sought to measure ordinary weather accurately so that they could build causal, predictive models.

As in many good books, a third is taken up with notes and bibliographies. This type of research involves retrieving fleeting references from a wide range of texts, and the succinct, scholarly prose relies on an impressive breadth of reading. The intriguing illustrations are supplemented by text boxes—framed lists of articles, peoples, and episodes that would normally be relegated to an appendix but that here provide valuable visual evidence to support an argument. Nevertheless, since *Reading the Skies* claims to be "a cultural history," it is disconcerting to find that artistic and literary sources have been deliberately excluded. This is very much a gentlemanly account. Little attention is paid to the farmers, laundresses, and other workers for whom weather vagaries were not just diary items but held a very real and practical significance. Women are also absent, even though many men held witches responsible for the unpredictable and freakish behavior of unruly female nature.

This is a learned study that engages with historiographical issues yet also provides a wealth of detailed primary information. It empathetically reconstructs the preoccupations of English provincial gentlemen who then, as now, were convinced that England's weather was deteriorating.

PATRICIA FARA

Edward J. Cashin. *William Bartram and the American Revolution on the Southern Frontier*. xvi + 319 pp., frontis., illus., bibl., index. Columbia: University of South Carolina Press, 2000. $39.95.

This book is aptly titled. William Bartram's political setting, not his scientific accomplishments, is the focus of Edward Cashin's work. In that sense, this is a companion piece to his earlier volume, *The King's Ranger: Thomas Brown and the American Revolution on the Southern Frontier* (Georgia, 1989). Cashin mined Bartram's *Travels* as a basis for commentary on each person, place, and event mentioned, putting them in a political perspective rather than focusing on the botanical, geographical, or zoological aspects of Bartram's work. These digressions may span decades on either side of Bartram's journeys. Sometimes this material is related only tangentially to Bartram himself. Many readers will be frustrated by Cashin's speculative habits, as when he suggests that "Bartram must have" known or done something or that "it would have been important for Bartram to know" something without providing sufficient basis for such claims. Cashin has written a book about eighteenth-century Georgia and South Carolina; *Isis* readers seeking a monograph on Bartram as a scientist will have to await another author.

JULIE R. NEWELL

June Z. Fullmer. *Young Humphry Davy: The Making of an Experimental Chemist*. (Memoirs of the American Philosophical Society, 237.) xviii + 385 pp., frontis., illus., bibl., index. Philadelphia: American Philosophical Society, 2000.

Humphry Davy remains an ambiguous figure in the history of science. He is celebrated as one of the father figures of electrochemistry. His invention of the miner's safety lamp is held up as an example of science's practical benefits. His rags-to-riches story provides a fairy-tale account of science's self-improving potential. On the other hand, his metaphysical speculations, his links with Romanticism, and his forays into self-experimentation and laughing-gas-induced hallucinations sit uneasily with the image of a sober-minded man of science. His troubled relationship with Michael Faraday, that other paragon of scientific self-improvement, has brought accusations of jealousy and petty-mindedness. During his own lifetime, Davy was condemned as well as celebrated for his rise to eminence. There are many reasons therefore why an examination of Davy's early career might tell us a great deal about the culture of English natural philosophy at the end of the Enlightenment. In this book the late June Z. Fullmer sets out to do just that. Fullmer devoted much of her life as a historian of science to Humphry Davy, producing a number of important papers on his career

and its context. This biography is a fitting culmination to her research.

Fullmer sets out to examine Davy's life and career from his childhood in Penzance in Cornwall to his arrival in London in 1801 to take up his post as assistant lecturer (and soon professor) of chemistry at the newly founded Royal Institution. She provides a detailed and nuanced account of Davy's Cornish upbringing and in so doing disposes once and for all of the myth of Davy's poverty-stricken early life. As Fullmer convincingly demonstrates, by provincial English standards the Davys were far from paupers. On the contrary, they were respectable members of local middle-class society. Crucially, Fullmer shows how networks of family, friendship, and obligation served to link Davy's provincial Cornish social circles to centers of cultural and political power. In so doing she provides a compelling account of Humphry Davy's translation from obscure Penzance apothecary's apprentice to Thomas Beddoes's chemical assistant at Bristol's Pneumatic Institute. Her account of Davy's social circle as the radical doctor Beddoes's protégé in Bristol is equally convincing as she shows how Davy's chemical reputation spread through similarly informal networks of communication. It is in tracing these threads of personal interaction and the role they played in forging Davy's career that Fullmer is at her best.

Fullmer provides careful dissections of Davy's chemical experiments as well, however. She places his early efforts in the context of William Nicholson's *Dictionary of Chemistry* (1795) and its outline of the state of chemical knowledge at the end of the eighteenth century from an English perspective. She shows how Davy's chemical researches at Beddoes's Pneumatic Institute—an institution devoted to curing disease through the administration of different kinds of air—both fitted into and developed out of Beddoes's own ambitious program for pneumatic chemistry. Fullmer demonstrates how Davy's experimental practice built on and responded to new discoveries, claims, and counterclaims in the context of the "chemical revolution." She is particularly good at recovering the earnestness with which Davy and his fellow-experimenters approached their nitrous oxide experiments. Exercises in auto-experimentation like these were central to the way in which many late Enlightenment natural philosophers fashioned themselves, turning their own bodies into scientific instruments. She concludes this volume (originally intended as the first part of a more comprehensive biography) with Davy's arrival in London, eager to make use of the Royal Institution's facilities to make a name for himself and to continue the electrochemical researches he had already started at Bristol.

This is an eminently readable biography, though it suffers from some minor irritations ("Brunoian" for "Brunonian" throughout I found particularly grating), presumably due to the author's sad death before the proofreading stage. It certainly reminds us how close attention to the personal can illuminate not only an individual natural philosopher's career but the broader culture within which he operated.

IWAN RHYS MORUS

Anthony F. C. Wallace. *Jefferson and the Indians: The Tragic Fate of the First Americans.* xvi + 394 pp., frontis., figs., illus., apps., index. Cambridge, Mass.: Harvard University Press, 1999. $29.95, £18.50.

To the recent boom in literature on the character of Thomas Jefferson we may now add Anthony Wallace's fine volume, which undertakes a painstaking analysis of Jefferson's abiding, multifaceted fascination with Native Americans to answer important questions about Jefferson's personality and the origins of America's "love-hate" relationship with Native peoples. Wallace contends that Jefferson's embodiment of some of the major dilemmas in American culture appeared most conspicuously in his relations with Indians (p. viii). A pioneer of interdisciplinary scholarship whose publishing career has spanned six decades, Wallace ranges far and wide in his attempt to pin down the sources and ramifications of Jefferson's interactions with Native Americans as scholar, land speculator, mourner, and president. The result is a richly detailed and provocative contribution to historical understanding of this formative era in American Indian policymaking.

Wallace is interested in explaining the mental gymnastics that enabled Jefferson to be at once the learned admirer of Native character, language, and artifacts and also the architect of the removal policy: that ostensibly "final solution" (p. 338) to the U.S. "Indian problem." Those interested in Jefferson's career as an early American ethnologist and practitioner of scientific inquiry into Native American culture will find much to contemplate in this book. Wallace provides, in Chapters 3–5, a compelling study of the ways in which Jefferson's "mordant fascination with the image of the Indian as a conquered and dying race" (p. 79) affected his scholarship. Tying together the various strands of Jefferson's interest in Indians, Wallace docu-

ments a series of misrepresentations, falsifications of evidence, and silences in a variety of Jefferson's publications and private writings on Native peoples that all served to justify his own interests in land speculation, national territorial expansion, and, ultimately, the entire program of imposing "civilization" on Indian "savages" that received pivotal emphasis during his presidency.

The remainder of the book traces the process by which attitudes originally formed during Jefferson's early academic inquiry into Native history were reinforced by his experiences in national office after 1790 and eventually translated into national policy. Wallace breaks down the essence of Jefferson's Indian policy into seven key elements (p. 225). Essentially, these involved Jefferson's reliance on the legal authority and administrative mechanisms of the Indian Trade and Intercourse Acts, as well as the presidential power to enter into treaties, to secure Indian land for the future expansion of the white American population while simultaneously seeking to assimilate Native peoples into that population as "civilized" farmers.

If Wallace is occasionally prone to indulge in educated guesses about facts, people, or events that Jefferson "must have been" familiar with, if some of his secondary sources are not the most up to date, and if his concluding discussion of Jefferson's legacy is disappointingly brief, he has nevertheless produced a monograph that deserves wide readership and discussion. The consequences of Jefferson's policies, as Wallace explains, are visible today in the bitter struggle Native Americans are currently waging to restore sovereign identities and reclaim lost lands. Yet, as we learn from Wallace, Jefferson has also cast a long shadow in an intellectual sense, insofar as contemporary academic inquiry into American Indian history and culture all too often reflects the values and interests of the dominant culture and fails to seek input or criticism from its Native "subjects." In seeking improvement in relations between Americans and Native Americans, so long distorted by the paradoxically romantic and racist perception of Indian culture bequeathed by Jefferson and others, we might benefit from shifting our agenda to finding ways to study with Native peoples, rather than remaining content with simply more studies of them.

JON PARMENTER

Jane Sharp. *The Midwives Book; or, The Whole Art of Midwifery Discovered.* Edited by **Elaine Hobby.** (Women Writers in English, 1350–1850.) xliv + 323 pp., illus. Oxford: Oxford University Press, 1999. $49.95.

The Midwives Book; or, The Whole Art of Midwifery Discovered, published in London in 1671 by the midwife Jane Sharp and now edited and annotated by Elaine Hobby, is a valuable contribution to an important topic in women's history that makes a relatively obscure female author accessible to a wider audience. Although Sharp's manual has been generally available as a primary source through a reprint edition published by Garland in 1985, Hobby's notes and glossary of medical terms will assist the lay reader as well as those with a special interest in early modern midwifery in coming to terms with information Sharp thought would be of value to midwives of the period.

Recent research has cast new light not only on who London midwives were but on how they were trained and licensed. As Hobby rightly points out, it is also important to understand the type of manuals available to them, even though these manuals were of limited use for the actual practice of child delivery. Since the medical practices of the early modern period embraced the humoral theory (briefly explained by Hobby), *The Midwives Book* contains much advice that modern medical practice would decry. However, it has been demonstrated elsewhere that experienced, licensed midwives, trained in a "hands-on" system, did not rely on the printed word for their knowledge. Just as well, since even such an experienced practitioner as Sharp has borrowed from earlier male medical authors, trained in the Galenic tradition, in addition to publications that may have been authored by other London midwives.

Aside from the short introduction, this book is Sharp's original six-part work, with minor emendations as well as brief definitions and explanations. It begins with a description of female and male "generative parts" and moves on to conception, sterility, the conduct of labor, "miscarrying," illnesses and diseases related to pregnancy, postpartum care, wet nurses, and, finally, a discussion of the normal newborn as well as common childhood diseases.

The main strength and contribution of this new edition of *The Midwives Book* is that it makes readily available a carefully edited and annotated primary source by a seventeenth-century woman who is addressing her peers. Although the subject matter was generally acknowledged as relating to "women's work," female authors rarely entered the domain of the male medical "expert." Sharp's was a voice nor-

mally excluded from the public forum provided by publication. This edition will be welcomed by those working in women's history, especially the history of medicine and midwifery, as well as those with a general interest in early modern English texts.

DOREEN EVENDEN

Francesco Paolo de Ceglia. *Introduzione alla fisiologia di Georg Ernst Stahl*. 236 pp., bibls., index. Lecce, Italy: Pensa Multimedia, 2000. L 28,000, €14.46 (paper).

Georg Ernst Stahl (1659–1734) is one of the most outstanding physicians and naturalists of the so-called *Frühaufklärung* in Germany. His scientific, philosophical, and ideological influence was deep and widespread in almost all the German states. As a professor at the University of Halle, the First Royal Physician in Berlin, and president of the Higher Medical Board of Prussia, he created a Stahlian school of medicine and chemistry, shaping some branches of medical thought, as well as of pure and applied chemistry. In the history of chemistry his name is generally associated with the theory of phlogiston, and in the history of medicine he is described as a vitalist. Stahl proposed a different idea of chemistry that represented a novelty and influenced the chemical theory of the Enlightenment era. Stahl's chemical conceptions were parts of a general scientific program that had a strong socio-ideological dimension. Characterized by harsh criticism of the iatromechanics, his work represents a main chapter in the history of modern chemical and medical science.

In the 1930s Hélène Metzger published pioneering contributions to the history of Stahlian chemistry, which have been the main source of information for many generations of historians. In 1982 Karl Hufbauer demonstrated the crucial role played by Stahl and his followers in creating a German national community of chemists. In the historiography of early modern science one can find many references to Stahl, but his work has not been studied much, and we do not have an up-to-date biography or an exhaustive reconstruction of his ideas and multiform activities. Stahl is not a fascinating topic for historical research: his literary production is enormous, his Latin language is not easy to grasp, and his scientific interests span from many branches of medicine to pharmacy and applied chemistry. However, in Germany and Italy, one notes a recent revival of the historians' interest in Stahl, and real contributions are being published. Francesco Paolo de Ceglia's new book on Stahlian physiology, for example, is a good contribution that clarifies some main aspects of Stahl's physiological conceptions.

De Ceglia's book does not aim at a complete reconstruction of the Stahlian physiological ideas. As is clear from its title, this volume presents an introduction to Stahl's physiological reflections. In the preface (p. 7) the author states that his book contains the first, very provisional results of a more general research project on Stahl. De Ceglia focuses his attention on Stahl's *Theoria medica vera* (1708), a huge textbook composed of four introductory treatises proposing some philosophical and methodological perspectives, a physiological section, and a three-part pathological section. De Ceglia's book is divided into three chapters. In the first de Ceglia illustrates the physiological elements of Stahl's *Theoria*—that is, the crucial differences between the mixed body and the living one, and emphasizes Stahl's definition of the living body as corruptible. For Stahl, life was the conservation, through movement, of a very corruptible body. Here, some important aspects of Stahl's physiological conceptions of blood, bodily heat, respiration, nutrition, and generation are illuminated. In the second chapter de Ceglia considers the main theoretical tenets of Stahl's epistemology and physiology. The third chapter focuses on the foundation of Stahlian physiology, that is, on the soul. Stahl's animism was much debated at the time; Leibniz, for example, strongly criticized his idea of the relationship between soul and body, but this animism was an important step toward a new definition of the living body.

Sometimes de Ceglia only touches on some main conceptual points, such as the influence of Lutheran pietism on the Stahlian idea of soul. Still, as an introduction this book attains its goal.

FERDINANDO ABBRI

Barbara Howard Traister. *Notorious Astrological Physician of London: Works and Days of Simon Forman*. xviii + 250 pp., tables, app., bibl., index. Chicago/London: University of Chicago Press, 2001. $30, £19 (cloth).

Simon Forman, as Barbara Howard Traister puts it, "turned himself into text": an obsessive writer, he left a cache of manuscripts, some of which—like the earliest surviving chronological case records—are of great historical value. Some of Forman's manuscripts are autobiographical, and it is for the more intimate details of his life that Forman has been known in recent years. He is "notorious" today largely for his sex life, being the subject of A. L. Rowse's well-known study,

Simon Forman: Sex and Society in Shakespeare's Age (London: Weidenfeld & Nicholson, 1974). But Traister looks at Forman's manuscripts in their entirety, and the bulk of her book considers the intellectual and professional aspects of his medical and astrological practice. This provides a welcome counterweight to Rowse's account of Forman as well as some corrections to the fleeting and often misleading impressions given in other secondary literature.

In so far as Forman's professional life is known, it is as a charlatan who preyed on the gullible to acquire fame, money, and sexual favors. From Traister's account there would seem to be a kernel of truth in this but a great deal of distortion too. Though by no means averse to taking payment "in kind" from the women he treated, Forman was a widely recognized authority on the diseases of women and his case notes show that he took his practice seriously. The same is true more generally of Forman's astrological practice (medical and nonmedical), which was founded on a deep belief in the accuracy of astrological predictions. Indeed, Forman claimed that only through astrology could a physician successfully diagnose and treat disease.

Forman was, however, a controversial figure in his own age, being "notorious" in the sense that he repeatedly fell foul of the London College of Physicians, which attempted to enforce its monopoly of medical practice in the English capital and its environs. Forman was brought before the college on several occasions for practicing without a license and received a number of fines and jail sentences for his repeated violations of the law. He was continually hounded and ridiculed by the college, partly because he was so successful. People—especially women—from all sections of English society came to consult Forman as a physician and astrologer, although it was largely for the former that he was venerated. Although he was mocked by the college for his ignorance of classical learning, Forman's medical knowledge was considerable, if not entirely systematic, though he lacked the necessary "polish" to present his knowledge in a manner acceptable to such a conservative body. Nevertheless, Forman's medical reputation was sufficiently impressive for him to be granted a license to practice by Cambridge University, after what was most likely a serious examination.

Forman's case notes show that he worked within a largely Galenic framework, and most of his treatments aimed at restoring humoral equilibrium. But, like many other practitioners of the time, he embellished his practice with chemical drug therapies; and there is evidence that he incorporated Paracelsian ideas. His case notes are so valuable because they show Forman's knowledge and practice evolving steadily over time: a development that is evident, for example, in his writings on plague. His first essay on plague was composed during the London epidemic of 1593, his second during the outbreak of 1607. The former treated the disease astrologically and theologically, seeing the epidemic as a form of divine punishment for the sins of mankind. The second dealt with the physical nature of the disease and its immediate causes. The two works should not be seen as set in opposition to one another, for they dealt with different features of a complex web of causation, with the increasing concern related to natural causes typifying much of the writing on plague at this time. Growing familiarity with plague—which typically broke out in major cities every ten years or so—permitted closer observation of the disease as a natural phenomenon. Forman was well placed to do this because he remained in London during both outbreaks, a decision that did much to enhance his professional reputation and expand his clientele.

The importance of Forman's manuscript collection ensures that this book will reach a wide readership, and few will be disappointed. The author writes clearly, with sophistication and insight, and avoids the anachronistic sensationalism of earlier accounts. As might be expected from the title, the book is especially enlightening about medical astrology, the dominant aspect of Forman's professional life. It is interesting to read this account alongside Michael MacDonald's study of Richard Napier—*Mystical Bedlam: Madness, Anxiety, and Healing in Seventeenth Century England* (Cambridge: Cambridge University Press, 1981)—for there are many areas of commonality as well as significant differences. All in all, this is a truly fascinating book. Historians of medicine and astrology will find much in it that is relevant to their own areas of specialization.

MARK HARRISON

■ Modern (Nineteenth Century to 1950)

Alan Rauch. *Useful Knowledge: The Victorians, Morality, and the March of Intellect.* ix + 292 pp., illus., bibl., index. Durham, N.C./ London: Duke University Press, 2001. $59.95 (cloth); $19.95 (paper).

Much historical investigation has been conducted into the Victorians' fear of moral decline

at the end of the nineteenth century. In part, concerns about the future of human morality and ethics were intimately connected with the rise of materialist science that appeared to be permeating every facet of human life and civilization. Uniquely, Alan Rauch's work moves this investigation back in time to examine the fear of moral decline in the early years of the Victorian era. Rauch posits that in this period there was, on the one hand, a cultural recognition of the importance of the growth of "knowledge" production, often of the scientific kind. Such knowledge was considered to be useful in making technological advances that would improve human life. Yet, on the other hand, there was also concern that too much emphasis on the "getting and spending" of knowledge, especially scientific knowledge, would lead to materialism, atheism, and, thus, a loss of moral and ethical standards.

Arguing that novels in this period reflected and responded to the larger cultural movements of the time, Rauch takes up a discussion of five novels: Jane Loudon's *The Mummy* (1827), Mary Shelley's *Frankenstein* (1818), Charlotte Bronte's *The Professor* (written before 1847, published posthumously in 1857), Charles Kingsley's *Alton Locke* (1850), and George Eliot's *The Mill on the Floss* (1860). Each of these novels, he argues, attempts to balance the need of British society to recognize the usefulness of scientific knowledge with the desire to retain traditional morals and values: "The challenge for the novelist was to create societies that responded to these inevitable developments without succumbing to a cynical dismissal of moral responsibility" (p. 16). Before beginning his discussion of the novels, each of which gets a chapter, Rauch includes a useful introduction to the process of the dissemination of knowledge in this period that attends to the rise of encyclopedias, the Society for the Diffusion of Useful Knowledge, and popular works for children. In each instance he shows how knowledge was paired with moral responsibility in this wider literary milieu. Rauch then moves on to show how novelists were clearly as fascinated as other writers by scientific and technological advances. They were also as worried as other groups in society that such progress could cause humans to lose their sympathy and concern for others, their spirituality, their ability to cooperate with members of other classes, their ability to grow and change for the better as physical, moral, and religious beings, and their respect for the natural world. The novelists Rauch discusses seem to argue that a balance may be struck between scientific knowledge and morality, maintaining the advantages of both.

While the last chapter, on *The Mill on the Floss,* takes the reader beyond Rauch's time frame of the early Victorian period, his aim here is to show how the depiction of the relationship between science and morality in the novel must necessarily change after Darwin. Rauch argues that, unlike the earlier novelists, Eliot "constructed a world where scientific knowledge is, prima facie, the appropriate measure of the material world, and moral knowledge must bend to accommodate it" (p. 191). Still, despite her acceptance of the laws of nature, Eliot, according to Rauch, still looked for a way to maintain human free will and moral duty. The introduction of this post-Darwinian writer, in contrast to the earlier writers, raises the question of whether there were other novelists attempting to reconcile science and morality in this way in the later period and, if so, how they went about it.

Rauch's main argument is convincing and contributes to the work to date on the relationship between literature and science and, more generally, to the recognition of the importance of science as a powerful cultural force in nineteenth-century society. The book would certainly have benefited, however, from a much stronger gender analysis. Of the five novels treated, four are by women. The Victorian association of women with the moral, spiritual, and religious world, together with the female authorship of the majority of novels under discussion, leaves the reader to ponder the relationship between Rauch's main thesis and the gender of his chosen novelists. This is particularly relevant as Rauch argues that only Kingsley's novel fails in its endeavor to "link science with tradition" and attributes that failure to his insistence on "invoking religion" rather than a moral tradition that is not necessarily connected with the Church (p. 189). Are women novelists alone responsible for successful explorations of the relationship between morality and scientific knowledge? Do the men who explore this relationship always fail? Considering his choice of novelists, this is an issue Rauch ought to have explored.

SUZANNE LE-MAY SHEFFIELD

Christopher Herbert. *Victorian Relativity: Radical Thought and Scientific Discovery*. xvi + 302 pp., bibl., index. Chicago/London: University of Chicago Press, 2001. $43, £27.50 (cloth); $16, £10.50 (paper).

Christopher Herbert, provoked by the Alan Sokal affair and by bullying critiques of "relativ-

ism," has written this study to demonstrate the prominence of relativistic thought in the sciences of the last two centuries. Although he draws back from any claim that relativity and its opposite, philosophical realism, lead of necessity to particular political positions, he associates the former with liberal tolerance and the latter with mandatory worship in a repressive "church of 'absolute truth'" (p. 19). Nazi physicists such as Philipp Lenard, he argues, "knew better than to regard relativity as a 'purely scientific matter,' seeing it instead as a mode of awareness implacably hostile to their own" (p. 12), and for him it is no less consequential.

Herbert runs the risk, he allows, of incurring Sokal's laughter. "For the benefit of any reader who will be reassured by the admission, I will concede the methodological dangers of proceeding as I do in this book, constructing intellectual history out of a congeries of remote-seeming texts on the strength of what may seem like freely interpretive reading, unbuttressed by claims of demonstrable direct 'influence'" (p. 33). His concession, however, is off the mark. Does anybody now regard "influence" as the only legitimate category of explanation in intellectual history? A serious argument for a common discourse, rooted perhaps in a shared reaction to contemporary events or circumstances, can provide sufficient justification for a wide-ranging cultural account. But his study is held together by little more than a word, "relativity." Did it really, as he implies, have the same meaning in the writings of Herbert Spencer as in those of Albert Einstein?

Although it seems unpromising, the possibility cannot be excluded in principle. To make it plausible would require a searching investigation of their scientific aims, their philosophical positions, and perhaps also their political purposes. Einstein was a German, a mathematical physicist, whose political views tended to pacifism and socialism. Spencer, who worked at the intersection of biology and social science, sought a unifying principle in a progressive cosmic tendency to advance from homogeneity to heterogeneity and by the 1880s had grown bitterly resentful about the growth of the social state in Britain. It seems at first comical to construe them as allied in advocacy of the doctrine of Protagoras, as Herbert labels it, that man is the measure of all things. Herbert simply ignores considerations of this kind, and there is nothing deeper in the book to alter one's initial reaction. Instead, he reaches more widely, enlisting on the same relativistic team an improbable alliance of Jeremy Bentham, John Stuart Mill, Charles Darwin, marginalist economics, Ernst Mach, Karl Pearson, quantum theory, Saussurean linguistics, and, in our own time, Richard Rorty and Jacques Derrida. Few opponents of this Victorian relativity are identified, for almost everyone has said something that can be associated with one or another meaning of "relativity." In asides and footnotes, Herbert reaches back to the Enlightenment philosophes, and even to Hobbes, as relativistic progenitors. Well may Sokal laugh.

Herbert is not a historian, but a literary theorist. He has this in common with the intolerant scientific realists he chastises: that he has composed a work of history—to which he indeed applies the name "intellectual history"—without bothering to canvass the historical scholarship. Would a great university press publish a book on the rise of the novel by an author who had never looked to literary scholarship, or on landscape painting by one who disdained to read art history? Herbert seems a perfectly decent fellow, but his book suggests that the arrogance of theory is fully a match for the arrogance of science.

THEODORE M. PORTER

Francesca Maria Crasta. *La filosofia della natura di Emanuel Swedenborg*. 336 pp., illus., bibl., index. Milan: FrancoAngeli, 1999. L 42,000 (paper).

Emanuel Swedenborg (1688–1772) has been a literary celebrity for more than two centuries because of his vivid depictions of heaven and hell. But to a considerable extent this renown has also excluded him from the history of science, to which he actually belongs. He was active as an exegete and a visionary only during the last twenty-five years of his long life, and before he got a divine call to found a new Christian church he had published a great number of scientific works.

Immanuel Kant's negative polemics in *Träume eines Geistersehers* (1766) offers an explanation as to why historians of science have only rarely taken an interest in Swedenborg, but it is not the only one. Swedenborg does not seem to have exerted any particular impact on contemporary science, and many of his writings—for example, the voluminous manuscripts on the human brain—remained unpublished for more than a century. He was employed not as a university professor but as a civil servant in the College of Mines, and therefore he had no actual disciples. Consequently his contributions to science have mostly been studied by literary historians as precursors to his visionary works.

Francesca Maria Crasta, an Italian historian of

science, has reacted against the prevailing views that depict Swedenborg as a visionary fool. As she declares in the preface to this new book on Swedenborg's philosophy of nature, she set out to study him as a scientist in the context of his time, an approach that perhaps makes him less exciting but certainly better defined historically. Within self-imposed limits she has carried out her task quite well. She has carefully analyzed the scientific works published through 1734, including the magnum opus *Principia rerum naturalium* published in that year. This means that she has discussed Swedenborg's studies of inorganic nature in toto. On the other hand, she has abstained from addressing his extensive studies of the human body during the following decade, an effort that resulted in two large works in print and a great number of posthumously published manuscripts.

The title of Crasta's book may therefore arouse unmet expectations, but what it does provide is valuable enough. The author starts with a survey of Swedenborg's works in natural philosophy from 1716 to 1734, in which she proves herself to be well versed both in the original sources and in the secondary literature. In the next four chapters she then discusses, in sequence, how Swedenborg discerned the origin of the universe in the mathematical point and its further development up to the birth of the planetary system through a series of finite particles. She emphasizes that Swedenborg got his decisive inspiration from Descartes, an author "constantly present although very seldom directly quoted in the Swedenborgian *Principia*" (p. 236). No doubt this is a correct observation; the notorious scarcity of references in Swedenborg's writings makes life complicated for those who are trying to describe his intellectual background in detail.

It is not surprising, then, that Crasta has not been able to detect any previously unknown sources. However, she has presented fresh and interesting aspects of the intellectual environment in which Swedenborg's philosophy of nature took shape. One example is her comparisons between Swedenborg's, Leibniz's, and Vico's ways of using the concepts of "mathematical" and "metaphysical" point, which show that Leibniz and Vico distinguish clearly between them while Swedenborg lets them merge. In that context she has also noticed that in the later parts of the *Principia* Swedenborg starts to make use of eggs and similar biological metaphors (p. 160). This is an important observation that lends support to her thesis that mechanistic Cartesianism is balanced in Swedenborg's philosophy of nature by the influence of Leibniz. Quite reasonably, Crasta also maintains that Swedenborg's presentation of magnetism comes much closer to Leibniz's optimism than to the Neoplatonic idea of decay he had met in Thomas Burnet (p. 218).

Another topic of particular interest is the question of a potential Swedenborgian influence on Kant's *Allgemeine Naturgeschichte und Theorie des Himmels* (1755), although not as transmitted by Buffon, as has sometimes been assumed, but by Thomas Wright's *An Original Theory or New Hypothesis of the Universe* (1750). Crasta has been unable to reach a firm conclusion, but she has proved that this is an issue well worth raising. Despite some inaccuracies with regard to chronological and bibliographical data, this learned book is a substantial contribution to our understanding of Emanuel Swedenborg as a scientist in his time.

INGE JONSSON

Nicola Luckhurst. *Science and Structure in Proust's* A la recherche du temps perdu. (Oxford Modern Languages and Literature Monographs.) x + 262 pp., bibl., index. New York: Clarendon Press/Oxford University Press, 2000. $74.

This is a welcome book, for it challenges intelligently the conventional view that Proust's novel is strictly a narrative of metaphor and memory. Nicola Luckhurst's project here is to demonstrate that, with its profoundly legislative impulses and knowledge-seeking, theory-laden tone, *A la recherche du temps perdu* is as much about hypothesis, experiment, and scientific law making as about nostalgia and reminiscence.

The entry point for Luckhurst's discussion is a perceptive chapter on Proust's maxims, those abundant *sententiae* that on the one hand link him to the moralistic tradition of La Bruyère and La Rochefoucauld and on the other demonstrate his need to generate laws that explain behavior and experience. She offers chapters on the novelist's use of hypothesis, modeling, and retroduction; the last notion is especially revelatory in a Proustian context. Borrowing from Aristotle, the philosopher Charles Sanders Pierce coined the term "retroduction" to describe a type of inference that contains within itself the nascent theory, the hypothesis, and the data being examined. Later Norwood Hanson spoke of the simultaneous "dawning of an aspect and a dawning of an explanation [that] both suggest what to look for next" (p. 71). Applied to *A la recherche,* retroduction aptly describes the main structure of cognition in Proust: intuitions about a singulative event and hypotheses about that event's

relationship to a broader law or theory are always co-present, with Proust's narrator representing both the datum being examined and the voice speculating on the resulting science.

Chapters 5, 6, and 7 focus in large part on the narrative's fascination with male and female homosexuality and assess the models and theories applied to each. The botanical analogy for male homosexuality (the Charlus-Jupien episode) has of course been widely discussed. But with the help of Antoine Compagnon's notes to *Sodome et Gomorrhe* (Pléiade edition), Luckhurst provides a convincing account of the impact made on Proust by the reading of Darwin, Maurice Maeterlinck's *L'intelligence des fleurs,* various essays by Jules Michelet, the zoologist/embryologist Elie Metchnikoff's *Etudes sur la nature humaine: Essai de philosophie optimiste,* and (perhaps) Remy de Gourmont's *Physique de l'amour.* However, as she notes, lesbianism in Proust's novel is an epistemological dead end. No model can account for it, and Proust's narrative, in *Albertine disparue,* becomes bogged down and "embalmed." Here, Luckhurst's own narrative loses momentum (Ch. 7) because of the space it devotes to this nonsignificance.

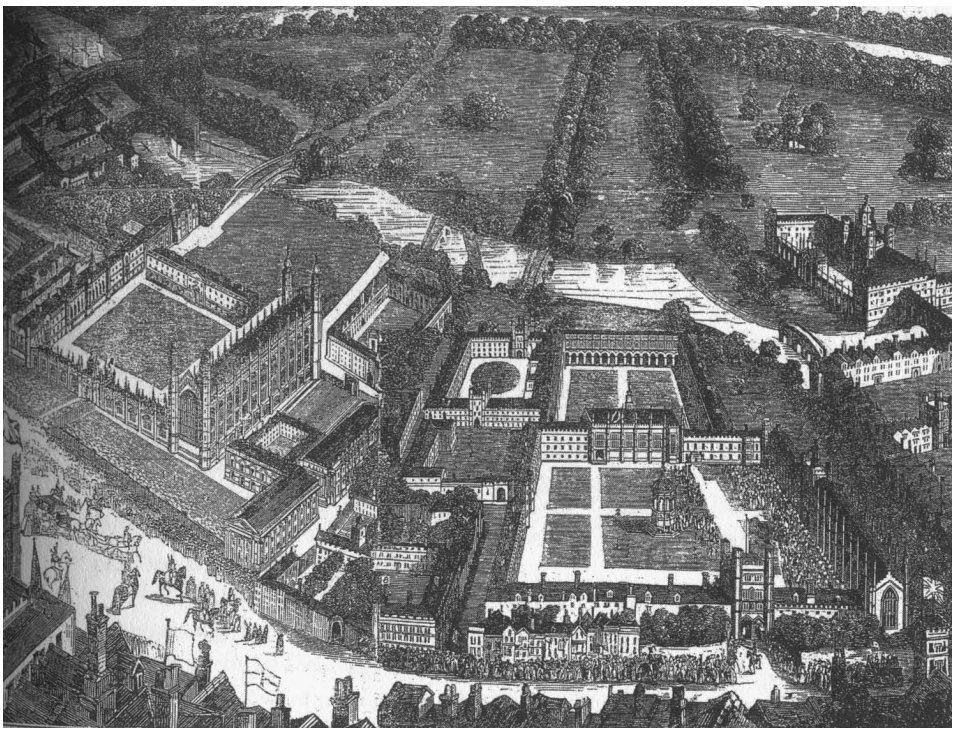
But the central, and persuasive, argument of this study concerns the converging attempts within the scientific and literary communities of the early twentieth century to narrate and conceptualize creativity. The tendency in literature toward what David S. Luft has called "essayism"—so clear cut in Proust—affected a generation of writers convinced that theory and story must converge, that "the tasks of philosophy, literature and cultural criticism" had come together (p. 238 n 21). In parallel, within the scientific community, intuition, analogy, and a kind of aesthetic sense were becoming acknowledged methodological tools, especially in the work of the French mathematician Henri Poincaré, whose speculations on the role of the unconscious in cognition closely parallel those of Proust (Ch. 8). Luckhurst's study demonstrates above all that underlying Proust's adamant distinctions between the particular and the general, between the intuitive/instinctive and the intellectual, is a deep esteem for science and an attraction to its laws. Metaphor itself, the Proustian cognitive device par excellence, must in his own words be exact enough to create a relationship "analogous in the artistic world to the unique relationship of the law of causality in the scientific world." It is the empowerment offered Proust by scientific truth that this study brings so nicely into focus.

MICHAEL R. FINN

James A. Secord. *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of* Vestiges of the Natural History of Creation. xx + 624 pp., illus., bibl., index. Chicago/London: University of Chicago Press, 2000. $35, £22.50.

This is a steamer trunk of a book! Its chapters, like so many tightly stuffed drawers with their numerous partitions, are full of all the apparel needed for a five-hundred-plus-page voyage across the thirty years of Victorian history that surround the *Vestiges of the Natural History of Creation* and its anonymous author, Robert Chambers. We find storage places for observations on the new steam presses, on the reading public—both high- and lowbrow—on phrenology, on Scottish science and the Free Church movement, on science in the great urban centers of England, on Von Baerian development, on a multitude of reviews, letters, personal journals, and salon gossip, on the contrasting styles of classical gentlemanly and the nouveau commercial science of industrial England, on the career of Chambers and many of his associates, and much more. A list of such static cubbyholes, however, fails to do justice to the unconventional and careful packing by the book's author, James Secord. More revealing is the hanging compartment of this steamer trunk, which meticulously folds the larger vestments against the pullout drawers. Here one finds the central themes that should concern all historians of science. These pertain to the varied recipients as well as the producers of science, publishers and illustrators as well as the technologies of elite and mass production, the nature of professional and amateur science at midcentury, and the clash between various ideologies, theologies, and politics as they bear on all of the above.

With this extraordinary marshaling of historical material, backed by years of intensive sleuthing and broad reading, Secord dares to provide a near-total history of and revision of a traditional minor affair in the history of science. He states one of his important goals at the outset in the form of a challenge: "What once made sense as the 'Darwinian Revolution' must be cast as an episode in the industrialization of communication and transformation of reading audiences" (p. 4). Although the book is not focused on this challenge till the end of the last chapter and epilogue, Secord's endgame becomes clear when he revisits the question about the contrasting style, production, and reception of the *Vestiges* and the *Origin of Species.* He finishes with the provocative conclusion that "the *Origin* was im-



Bird's-eye view of Cambridge, 1843 (reprinted from James A. Secord, Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of *Vestiges of the Natural History of Creation,* p. 225).

portant in resolving a crisis, not in creating one" (p. 514). By this Secord means that the *Origin* forged a new alignment of professional and entrepreneurial biologists and the reading public around a new, often misunderstood developmental hypothesis; so "Darwinism" becomes "the science of the future" (p. 514). It strikes me that the author's conclusions belie his initial claim. Without question Chambers and the *Vestiges* must now rank as major indicators and an episode of great importance in early Victorian culture. To me, however, Darwin and the *Origin* in its past, present, and future contexts—*pace* modern contextualists—still make sense as a broader, contentious episode known as "the Darwinian Revolution."

I profited particularly from Secord's discussions about the revisions and the target audiences of various editions of the *Vestiges,* his evidence that many professional scientists grew confident of Chambers's alleged authorship, and his demonstration that the evangelical Free Church in Scotland enabled a shift by the 1840s of the historical sciences from Edinburgh to the English urban centers. "From the Chamberses' perspective, the 'Athens of the North' had degenerated into a provincial backwater ruled by Calvinist fanatics" (p. 494). My understanding of the history of science has been permanently altered by Secord's extended attention to communications and the reading public. I would have liked more reflection on the contemporaneous scientific and cultural scene in Germany and a deeper discussion of Herbert Spencer, but you cannot cram everything into a steamer trunk. This book is exhaustively illustrated, broadly researched, and well written. It contains a fine bibliography and index. I strongly recommend *Victorian Sensation* to anyone concerned about the twin processes of representation and communication in the history of science and for all interested in Victorian cultural history.

FREDERICK B. CHURCHILL

Sharon McGrayne. *Prometheans in the Lab: Chemistry and the Making of the Modern World.*

244 pp., illus., bibl., index. New York: McGraw Hill Publishing, 2001. $24.95 (cloth).

A journalist and science writer, Sharon McGrayne seeks here to demonstrate, by the use of nine carefully chosen biographical episodes, the historical importance of the chemical sciences in the development of modern technology. She follows a long tradition of such writing, most of it old and much of it overly popularized or obviously promotional—such as Bernard Jaffe's *Crucibles* (Simon & Schuster, 1930), Cressy Morrison's *Man in a Chemical World* (Scribner's Sons, 1937), or Williams Haynes's *This Chemical Age* (Knopf, 1942).

The early going was not promising. Only a few pages into the book, I found too many slips for comfort: atmospheric air in a list of chemical compounds, Lavoisier described as a "tax collector," and Benjamin Thompson (the "Count of Rumford") and Paul Ehrlich's Salvarsan misspelled (pp. 2, 3, 28). However, the book steadily grew on me. For each of her case studies, McGrayne used the latest and best secondary literature, including some untranslated French and German sources and at least one that was still in press. She profited from the assistance of leading specialist historians, and she also fruitfully mined manuscript collections for certain of her chapters.

The episodes are Nicolas Leblanc and artificial soda; William Henry Perkin and synthetic dyes; Norbert Rillieux and sugar refining; Edward Frankland and clean water; Fritz Haber, nitrogen fixation, and gas warfare; Thomas Midgley, leaded gasoline, and CFC refrigerants; Wallace Carothers and nylon; Paul Müller and DDT; and Clair Patterson and the fight against lead in the environment. The first five (fairly short) chapters cover material that is generally well-treated elsewhere. However, it will be useful to have these briefer and more popular treatments available, and I can particularly recommend the Rillieux and Haber chapters for their skillful blending of biographical and science-historical matter.

The last four chapters constitute 60 percent of the book and are the best of the lot. McGrayne's recounting of Midgley's and his bosses' callous suppression of the obvious effects of lead poisoning is chilling; the episode forms a stark contrast with Patterson's extraordinary services in uncovering the dangers of and fighting to limit and ultimately ban leaded gasoline. The chapter on Carothers is the longest of the nine, and the best. Based on broad and deep research, the piece provides a telling psychological portrait as well as a realistic recounting of his research.

The greatest thematic novelty of this book is McGrayne's conscientious endeavor to describe, in evenhanded fashion, both the benefits that chemists have conferred on the world and the social and environmental problems that they have inadvertently caused. In the propaganda wars between industrial and environmental chemistry this is indeed unusual, if not unique, and in general McGrayne is successful in showing both sides—though it is obvious that she has real sympathy with the chemists. Haber gave the world inexpensive nitrogen fertilizer, hence abundant food and eutrophied lakes, as well as the first effective chemical warfare agent. Midgley's R-12 replaced earlier highly dangerous refrigerants and saved millions of lives by making possible the spread of inexpensive refrigeration, then was implicated in ozone depletion and the greenhouse effect. Müller's DDT became an agricultural panacea, then an environmental disaster, in the developed world; but many countries still argue passionately for its continued use against the scourge of malaria.

McGrayne skillfully draws out these contrasts and ironies and resists generalized or polarized conclusions to which others have been drawn. Her portraits of warts-and-all personalities and private lives are engaging. The story communicated in her book is both important and complicated—if it were simple, it would be less interesting. In sum, despite the popular tone, *Prometheans in the Lab* is well done and worth reading—certainly for students, but even for academic historians.

ALAN J. ROCKE

James G. Cassidy. *Ferdinand V. Hayden: Entrepreneur of Science*. xxviii + 389 pp., illus., figs., bibl., index. Lincoln/London: University of Nebraska Press, 2000. $55, £37 (cloth).

According to the *Random House Dictionary,* an entrepreneur is "a person who organizes and manages any enterprise, esp. a business, usually with considerable initiative and risk." The subtitle of this book could not have been more aptly chosen. Throughout his career, most famously when he led the United States Geological and Geographical Survey of the Territories, Hayden made a business of exploring the trans-Mississippi West. Though abounding in opportunity, this was an uncertain, taxing line of work, demanding tireless initiative, endless salesmanship, and a constant hardiness in the face of harsh and dangerous conditions. Starting in near des-

titution, but driven by extraordinary ambition, Hayden rose to the challenge. Year in and year out he obtained for his survey the congressional appropiations he needed to sustain and expand his operations. Nothing was left to chance. Hayden actively cultivated and engaged the interests, not only of his political patrons, but of the scientific community as well, making gifts of reports, maps, and natural history specimens and providing through his survey employment opportunities and publication outlets for many fellow scientists, including Edward Drinker Cope and Leo Lesquereux. Nor did Hayden neglect army officers, who could help with supplies and transportation, or businessmen and Western boosters, who could assist him in various ways. Dependent upon government funds, he did not overlook opportunities to bring his name and the utility of his enterprise to the notice of the public at large. He was conspicuous in promoting Yellowstone as a national park and provided a "spectacular showcase" (p. 234) of his survey at the 1876 Centennial Exhibition in Philadelphia. One should not be too critical of Hayden for all this self-promotion, since continued funding required broad, ongoing justification; and three other major western surveys led by Clarence King, George Morton Wheeler, and John Wesley Powell were in the field at the same time, competing for appropriations. Nevertheless, the story of Hayden's rise from total obscurity to great national and even international prominence through hard-driving, sometimes shady measures has about it the air of a morality play, with a message for those preoccupied with "saving their lives." The creation of the United States Geological Survey brought an abrupt end to the Survey of the Territories, and the directorship of the new organization went—thanks to the successful maneuverings of Hayden's critics—not to him, but to Clarence King. Yet the legacy of the Hayden survey lived on, not only through the substantive achievement of its discoveries and reports, but also—and this is what Cassidy emphasizes—through the foundations it erected for the federal patronage of civilian-led science.

In developing his portrait of Hayden as an entrepreneur Cassidy has much to say about the general characteristics of patronage, reputation, and other institutional elements in the practice of science. Indeed, the book is tightly focused on social process, on the varied tactics Hayden employed to promote and further his career. Beyond hints that Hayden was not a particularly distinguished scientist, Cassidy offers no assessment of what Hayden or his survey contributed to the progress of scientific knowledge. The narrowness of focus is perhaps justified as a way of complementing, without duplicating, the older literature, including William H. Goetzmann's balanced appraisal of Hayden's survey in *Exploration and Empire* and Mike Foster's full-scale Hayden biography, *Strange Genius* (Roberts Rinehart, 1994). Yet, in dwelling almost exclusively on Hayden's politicizing, Cassidy leaves a cynical impression: that the scientist—in organizing the study of western geology, unearthing troves of rock and fossil specimens, and publishing volumes of factual findings—was thinking only of what he could do to advance his own worldly status.

Surely, to some degree, the ideal of knowledge for its own sake underpinned Hayden's work, but it must be admitted that viewing the myriad activities of this self-made man through the lens of entrepreneurship is quite fruitful and illuminating. The author draws on a wide range of published and unpublished materials, develops his thesis forcefully and cogently, and writes deftly. The portrait of Hayden presented is unidimensional, but for understanding better the social-political dynamics of publically supported science, it gives us much to ponder.

ROBERT H. SILLIMAN

Lila Marz Harper. *Solitary Travelers: Nineteenth-Century Women's Travel Narratives and the Scientific Vocation*. 277 pp., illus., bibl., index. Madison/Teaneck, N.J.: Fairleigh Dickinson University Press; London: Associated University Presses, 2001. $45.

Solitary Travelers takes its place alongside other revisionary works that assess the contribution of women writers to nineteenth-century fields of study and disciplines of learning identified as male and associated with science. Lila Harper foregrounds the role of travel narratives in her analysis, arguing that they facilitated access to a scientific vocation for women writers and, indeed, that some women gravitated to travel writing "in a common quest for the professional recognition which seemed to be promised within a territory marked 'natural philosophy,' 'natural history,' and, finally, 'science'" (p. 29). In an interesting and well-written introduction to her subject, Harper demonstrates her sensitivity to basic historical issues of what constituted "science" in nineteenth-century England and makes appropriate distinctions not just between the amateur and the professional but also between science as practice, hobby, vocation, and mode of observation.

Harper is interested specifically in natural his-

tory travel narratives, her introduction tells us, and she chooses for her subjects Mary Wollstonecraft, Harriet Martineau, Isabella Bird Bishop, and Mary Kingsley. In separate chapters studying each of these key figures, she uses biographical data to undergird her analyses of the rhetorical strategies that each woman invoked in her travel writing. Her thorough examination of Wollstonecraft's *Short Residence in Sweden* (1796) adds much-needed dimension to a writer known mainly for her *Vindication of the Rights of Man* (1790) and *Vindication of the Rights of Woman* (1792). Locating within Wollstonecraft's text evidence of her interest in the Scandanavian landscape and wilderness, Harper stresses her close observation of the natural world and her apparent commitment to natural history. The subsequent attention in this chapter to Wollstonecraft's relationship with Gilbert Imlay, however necessary it may be to understanding her motivations for travel and need for authority, short-circuits the analysis of Wollstonecraft's contributions to natural history at the turn of the century.

Harper's chapter on Harriet Martineau examines her "ability to shape travel writing into an investigative tool for scientific observation while substantiating her influence on others and her intellectual stature" (p. 83) and to this end is geared also to basic issues of rhetorical strategy and authoritative voice, particularly as they surface in *Society in America* (1837), *Retrospect of Western Travel* (1838), and *Eastern Life, Present and Past* (1848). Martineau is arguably the most written about of the women studied in *Solitary Travelers,* and consequently much of the material and perspective that Harper presents will be familiar to her readers.

The chapter on Isabella Bird Bishop takes note of a crucial shift in the audience of this famous traveler, who began by writing for ladies' magazines but eventually addressed an audience of geographers, explorers, and members of colonial government. Harper sees Isabella Bird Bishop and Mary Kingsley, her final subject, as driven by the ambition to gain access into "the scientific community" (p. 174), but her analysis is nevertheless far more focused on the content of their respective travel books than on any more tangible activity linked to professionalized science. Harper finds in Kingsley's prose, as in that of her other subjects, evidence of her manipulation of "a feminine discourse" and a corollary concern with "masculine scientific discourse" (p. 177); more provocative to my mind is her claim that Kingsley adopted the role of "martyr . . . both for scientific endeavors and for the future of developing colonial Africa" (p. 177).

This book's chief virtue is its readability. With each of her subjects, Harper provides a comprehensive and yet relatively succinct overview of the ways they used travel to undertake work construed, however loosely, as "science." Although she claims that Kingsley's travel writing represents the culmination of a tradition of "women's naturalist travel writing," I found myself somewhat suspicious of the term and yearning for more nuanced investigations of generalized categories such as "masculine scientific discourse." Although each of the women Harper studies undertook travel writing and incorporated descriptions of the natural environments she encountered into her accounts, does it necessarily follow that they were working within, even helping to construct, a tradition of "naturalist travel writing"? Kingsley would probably have been the only one of these four to identify herself as a naturalist. The introduction to *Solitary Travelers* seems to promise a study of women using travel to work in natural history, but the book itself looks far more broadly at the rhetoric of authority and the appeal of "science" (and solitude) to these women.

MARIA H. FRAWLEY

Beatrice Häsler; Thomas W. Baumann. *Henri Pittier, 1857–1950: Leben und Werk eines Schweizer Naturforschers in den Neotropen.* 455 pp., illus., bibl., app., index. Basel: Friedrich Reinhardt Verlag, 2000.

Henri Pittier, a nineteenth-century savant of Swiss origin who spent his professional life in the tropics of the New World, is the object of this traditional life-and-work. Pittier is a significant figure. Born and educated in Switzerland, he participated in the establishment of a national meteorological service and a physical-geographical institute in Costa Rica (1887–1904). He then traveled in Central America, Colombia, and Venezuela as the paid agent of the U.S. Department of Agriculture (1905–1919) before returning to Venezuela, where he variously directed the Commercial Museum in Caracas, the National Meteorological Observatory, the Botanical Service of the Ministry of Agriculture, and the Department of Forest Research.

Beatrice Häsler (a specialist in pharmacy and graphic design) and Thomas Baumann (a professor of plant physiology at the University of Zurich) focus sympathetically on Pittier's personal life and character. They include a useful selection of documents and an exhaustive review

of Pittier's publications. Documents are cited with accuracy and great care in their original language (French, German, English, or Spanish), an erudite feature that readers might find too weighty for the material in question. Häsler and Baumann discuss the problems Pittier faced in Venezuela regarding his and his collaborators' language of publication (English vs. Spanish; pp. 266–268). Instead of commenting on the cultural and political dimensions of this issue, they present Pittier as a moral example: "On the other hand, it is a real shame that people from the Americas, a continent which is almost bilingual, then as now cannot understand each other because, due either to indolence or ignorance, they do not want to learn the 'other' language. Henri Pittier spoke and wrote in five different languages" (p. 268). Sometimes the documents speak for themselves; elsewhere the authors provide their interpretations. They are cautious about offering judgments. They rank Pittier's correlation of rainfall and earthquakes from the late 1880s as pioneering work, but "we leave it to the experts to decide whether in this area Pittier was ahead of his time" (p. 110). The chapter devoted to Pittier's work for the U.S. Department of Agriculture is a starting point for further analysis. The appendix presents concise identifications of personalities (111 names; pp. 405–414), the names of the taxa described by Pittier (pp. 415–431) and those named to honor him (pp. 432–435), and a bibliography of Pittier's publications (pp. 436–447). The index is too broad to be useful: "Caracas," for example, is followed by eighteen undifferentiated page references. The book is profusely illustrated with images pertaining to Pittier's life and influence.

Pittier is the subject of two books by the Venezuelan historian Yolanda Texera (*La exploración botánica en Venezuela, 1754–1950* [Fondo Editorial Acta Científica Venezolana, 1991], and *La modernización dificil: Henri Pittier en Venezuela, 1920–1950* [Fundación Polar, 1998]) and a doctoral dissertation by León Yacher ("Henri François Pittier: Geographer, Natural Scientist, and the Development of Geography in Costa Rica" [Syracuse Univ., 1998]). Both Texera and Yacher focus on Pittier's connections with scientific institutions and disciplines, Texera concentrating on his role in promoting the science of botany in Venezuela and his part in "modernizing" the country. Häsler and Baumann's focus, though more narrow, nevertheless invites historians of science to consider general questions: the history of fieldwork, field naturalists and taxonomy, the scientific side of geographical boundary surveying, "useful" science, imperialism, museum history, meteorology, and scientific migration. The attention devoted to producing this book compensates for its historical naïveté.

IRINA PODGORNY

Pat Shipman. *The Man Who Found the Missing Link: Eugène Dubois and His Lifelong Quest to Prove Darwin Right*. [xii] + 514 pp., frontis., illus., figs., bibl., index. New York/London: Simon & Schuster, 2001. $28, Can $41.50.

In 1892, near Trinil on the island of Java, laborers under the direction of the expatriate Dutch physician-anatomist Eugène Dubois uncovered fossil bones that, Dubois believed, belonged to a single member of a hitherto-undiscovered species. Dubois named the species *Pithecanthropus erectus* ("ape-man that walks erect"), a reflection of his steadfast belief in its transitional role in human evolution. The fossil, popularly known as "Java Man," is now classified as *Homo erectus*—a species not fully human but far closer to us than Dubois envisioned.

Dubois and *Pithecanthropus* have been covered before: briefly in Carl Swisher, Garniss Curtis, and Roger Lewin's *Java Man* (Scribner, 2000) and at length in Bert Theunissen's *Eugene Dubois and the Ape-Man from Java* (Kluwer, 1989). Pat Shipman's interests are more strictly biographical: Dubois as a family man, as a European in Southeast Asia, and as a scientist struggling to establish a career and a reputation. *The Man Who Found the Missing Link* weaves together the multiple threads of a complex life: extensive travels, eclectic scientific interests, tempestuous relations with colleagues, and a slowly disintegrating marriage. It vividly conveys Dubois's near-obsession with the *Pithecanthropus erectus* fossils and his tenacity in defending their significance. Shipman tells the story almost exclusively from Dubois's point of view and almost exclusively in the present tense, sacrificing authorial distance for immediacy. The book often reads, therefore, as if it were Dubois's own memoir, "as told to" Shipman.

Lavishly produced by a major trade publisher, Shipman's book is clearly aimed at readers who have enjoyed the work of Dava Sobel, Bella Bathurst, and Simon Winchester. Taken on those terms, it succeeds admirably. Like Sobel's *Longitude* (Penguin, 1995) and Winchester's *The Map That Changed the World* (Harper Collins, 2001), it is a vigorous tale of an embattled scientific maverick who triumphs despite repeated setbacks. Like them, it succeeds both as literature and as a character study.

Historians interested in the book for professional reasons will find it less satisfying. It is full of recounted conversations and descriptions of unvoiced thoughts and feelings—these elements are the heart of the book's literary style and essential to its characterization of Dubois. The sparse and sketchy endnotes make it impossible, however, to determine the sources of this material or to draw clear lines between contemporary accounts, after-the-fact recollections, the author's inferences, and outright speculations. Shipman writes, in one alarming example, that "Dubois is something else, something extraordinary, the coolies conclude. He is a chosen man. Clearly he has been given a special role or *dharma* through a divine revelation: that is the only possible explanation" (p. 138). With no endnote to explain the source of this insight into the coolies' thoughts, readers are left to wonder if it is Dubois's own imperialist fantasy, reported as if it were fact.

Shipman also does little to relate Dubois to the scientific world of his day. Coverage of the culture of colonial science, Dutch scientific institutions, and turn-of-the-century debates over human evolution is scant. Scientists like Henry Fairfield Osborn, Ales Hrdlicka, and Ralph Von Koenigswald are introduced with only vague indications of their interests, status, or intellectual commitments. Hominid fossils other than *Pithecanthropus erectus* receive little more attention; even the Neanderthals, on whom Shipman has coauthored an authoritative book, are accorded only a handful of pages. All of this makes it difficult to assess whether Dubois's experiences were typical of the times, places, and fields in which he worked. It also undercuts Shipman's claim that Dubois, by his dogged insistence on the "missing link" status of *Pithecanthropus erectus,* forced his fellow scientists to confront the reality of human evolution.

A. BOWDOIN VAN RIPER

Benoit Marpeau. *Gustave Le Bon: Parcours d'un intellectuel 1841–1931*. 374 pp., fig., bibls., indexes. Paris: CNRS Editions, 2000. Fr 27.48 (paper).

This impressive and closely researched intellectual biography transcends the usual categories in which Le Bon has often been placed: a precursor of fascism and the originator of prescriptions for Hitler, Mussolini, Lenin, or Stalin in their manipulation of the masses. Benoit Marpeau's concern is to present a valid historiographic account that involves both a closer and a wider view of Le Bon's prolific work. In recounting his intellectual journey ("parcours d'un intellectuel"), the author devotes no space to discussing Le Bon's personality. However, Le Bon's dominant characteristics become clear: he was elitist, conservative, and anti-Semitic; he valued the individual over the collective. Marpeau shows his sense of superiority to have been fragile, for his medical qualifications, at the start of his career, were minimal. He was a self-styled "docteur." His need to excel fueled his ambition to write on a variety of areas—scientific, psychological, sociological, educational—and on contemporary affairs.

One of the major features of this biography is the importance that Marpeau attaches to the system of relationships in which Le Bon was involved. These centered on two regular social/intellectual reunions in which he was a key figure and organizer. Through these social events he was able to make useful connections that would advance both his social and intellectual status. Nevertheless, Le Bon remained outside the university domain; nor was his scientific work formally recognized by the Academy of Sciences; and he was overlooked for the Nobel Prize for science in 1903. If Le Bon was in some sense "marginal"—and Marpeau questions this description—he nevertheless exerted influence when he founded and became the director of the important Collection of the "Bibliothèque de Philosophie" published by Flammarion. Marpeau shows how this editorial post enhanced his standing and helped to disseminate his views, for Le Bon had the power to decide what could be published.

The author also shows how Le Bon was a man of his times, for he reflected the views and interests held both by intellectuals and by a wider public. The idea of degeneration and the decadence of France was a particular concern. The scientific theory of recapitulation informed the hierarchy of races and defined both children and women, together with "primitive" races, as inferior. Le Bon endorsed these ideas, which were supported by craniometric studies.

In his analysis of the most well-known work, *The Psychology of Crowds* (1895), Marpeau agrees in general with the thesis of Robert Nye (*The Origins of Crowd Psychology* [Sage, 1975]), to which he makes some additions and qualifications. Marpeau does not see Le Bon as the originator of the methods of crowd manipulation, nor of the "offensive system" that the French military leaders later adopted.

This biography is truly an intellectual one, constructed on historicist principles: hence the placing of Le Bon's work in its appropriate sci-

entific and relational contexts. At various points Marpeau justifies a method that entails an examination in great detail of Le Bon's correspondence and the composition of his entourage at various stages of his life. However, this strategy is somewhat overstretched and produces accounts of undue length.

This is not a book for readers interested in Le Bon's work or character. This is, rather, a biography for historians particularly interested in Marpeau's method and in historiographic issues. As such, it can serve as an example of commitment and thoroughness; it also offers an opportunity for the critical mind to assess the value of the various contributions that might explain an intellectual journey.

DIANA P. FABER

Tracey L. Adams. *A Dentist and a Gentleman: Gender and the Rise of Dentistry in Ontario*. ix + 236 pp., illus., refs., index. Toronto: University of Toronto Press, 2000. $45 (cloth).

In *A Dentist and a Gentleman* the sociologist Tracey Adams retells a familiar professionalization story, this time about elite dental practitioners in nineteenth-century Ontario who launched a status-enhancement project to reshape their self- and public image into "professional gentlemen" and establish monopoly control over dental practice. Dentists secured legislation in 1868 giving them authority to set entrance requirements, test and license practitioners, and establish a college. In subsequent decades they campaigned against those they called "quacks" who practiced without a license, advertised, charged low fees, maintained dirty offices, misled patients about pain, or brought the gentlemanly status of dentistry into disrepute.

Despite early resistance, the project had mostly succeeded by 1918, as dentists skillfully linked their expertise to emerging public health concerns and a professional image subtly reoriented toward an ideal of public service. By 1900 dental rhetoric was warning Canadians of an impending crisis of tooth decay and dental disease that would bring physical deterioration and mass feeblemindedness in its wake. Racial degeneracy, dentists warned, began in the mouth; the advance of civilization produced too much soft food and sugar, too little chewing, and too many high-strung and overindulgent mothers. Only dentists, as "sentinels at the portal of the alimentary tract" (p. 93), could stand against this threat to Anglo-Saxon civilization. Rhetoric like this, combined with doses of guilt heaped on mothers, teachers, school boards, and public health agencies, resulted by 1915 in regular dental inspections of schoolchildren, free dental clinics, and the creation of the army dental corps.

Dentistry brought unique twists to this familiar professionalization story. Prior to 1860 dentistry was an especially low-status craft, associated with uneducated, itinerant tooth-drawers or craftsmen like blacksmiths and gunsmiths (since dentures were manually fashioned from soft metals, like gold). Professionalizing dentists therefore felt a special imperative to recast themselves as "gentlemen"; this image also reassured the upper-middle-class women who were dentists' main clientele. Unexpectedly, as Adams shows in an intriguing analysis, dentists benefited from an invaluable social alliance with physicians, who offered no opposition of the sort they raised against other rival groups of healers to dentists' professional aspirations.

The study deals extensively with how gender shapes professions. Dentistry's professionalization, Adams insists, involved the adoption of a new ideal of personal masculinity, as seen in admonitions to dentists to establish firm, courteous, expert authority over their (mostly female) clients and in dentists' efforts to shape the imagined persona of the ideal dental assistant as uniquely female and wifelike. This masculine crafting of the profession notwithstanding, Ontario dentists evinced less overt opposition to women aspirants than did doctors or lawyers. Nevertheless, the proportion of female practitioners was much lower in dentistry than in medicine or law and remains substantially lower today. Adams brings considerable insight to her discussion of these facts and their causes, and she presents useful comparisons among the professions.

This study has a few limitations. Perhaps because the author's sources are mainly professional journals, her account mostly mirrors the outlook of the professionalizing elite and her prose occasionally adopts their moralizing and improving tone. Regrettably, the book deals scarcely at all with what dentists actually did. One learns little about how (and whether) changing scientific and medical ideas affected dentists' work, how dentists utilized anaesthesia and coped with the problem of pain, and what controversies over diagnostics and treatment animated the profession or about dentists' training and income. This is a history of dentistry's professionalization, not of dental medicine per se. But *Isis* readers impressed by this solid and modest study will hope that a history of dental practice will be Tracey Adams's next project.

R. STEVEN TURNER

Mark S. Micale; Paul Lerner (Editors). *Traumatic Pasts: History, Psychiatry, and Trauma in the Modern Age, 1870–1930*. xiv + 316 pp., bibl., index. Cambridge/New York: Cambridge University Press, 2001.

In 1980 the American Psychiatric Association gave official recognition to Post-Traumatic Stress Disorder (PTSD) in its *Diagnostic and Statistical Manual of Mental Disorders,* a massive volume devoted to the classification of all nervous and mental disorders. The creation of this new category owed much to the psychological suffering experienced by Vietnam War veterans. Since that time the category of trauma has broadened dramatically and now includes physical and sexual abuse in childhood, rape trauma, and battered spouse syndrome, to cite only some of the more obvious examples.

The identification of new diagnostic categories (or the elimination of older ones) invariably impacts the writing of history. When particular diagnoses (e.g., neurasthenia) disappear from medical nosology, scholars then attempt to explain the appearance and disappearance of such diagnostic categories in terms of their social and cultural roots. New diagnoses, by contrast, often lead to efforts to rewrite history by imposing these categories on the past. At present there are clear indications that the medical category of trauma is becoming increasingly important as an explanatory tool for scholars.

Traumatic Pasts brings together a variety of essays that focus on the concept of trauma in different national settings and from a variety of perspectives. Fortunately, the authors are reluctant to accept uncritically the contemporary medical definition of trauma; they recognize that the diversity of responses to past traumatic events renders it virtually impossible to apply a contemporary category to different situations and different time periods. The concept of trauma, as Paul Lerner and Mark Micale note in their perceptive introduction, cannot be defined "by external, objective criteria." Indeed, trauma is not an event per se, "but rather the *experiencing* or *remembering* of an event" (p. 20). To define this category in such a way is not to trivialize its significance, but to acknowledge "the central subjectivity of perceiving and remembering in the psychology *and history* of trauma" (p. 21). The goal of clinical medicine, the editors note, is to move from individual cases to generalized categories. Historians, on the other hand, tend to be skeptical about an all-embracing and universal concept of psychological trauma and prefer instead to demonstrate its cultural and social contingency by analyzing specific case studies.

The essays in this volume aptly illustrate the differences between medicine and psychiatry on the one hand and history on the other. In the first section, essays by Ralph Harrington and Eric Caplan detail the role of railway accidents in late nineteenth-century Britain and America in creating post-traumatic symptom formation. The financial and legal stakes played a major role in the contemporary debate, for railroads had a vested interest in discrediting claims that many somatic symptoms resulting from accidents were traceable to the traumatic experience. The second section includes essays by Wolfgang Schäffner and Greg A. Eghigian that focus on German-speaking Central Europe. The former describes the inclusion of "traumatic neuroses" within the framework of Germany's compulsory social welfare system and how this issue became part of the debate over the allegedly negative consequences of public welfare; the latter relates the debate over trauma to the triumph of statistical probability in the nineteenth century and emphasizes the role of power. The third section includes essays by Mark Micale (French trauma theory in the late nineteenth century), Paul Lerner (the career of Hermann Oppenheim), and Lisa Cardyn (the construction of female sexual trauma in American medicine). The concluding section covers shock, trauma, and psychiatry during World War I and includes contributions by Peter Leese (Britain), Bruna Bianchi (Italy), Marc Roudebush (France), and Carolina Cox (United States).

The variety of essays in this volume makes it difficult to provide an adequate critique in a brief review. Those interested in the historical emergence of the concept of trauma will find *Traumatic Pasts* an important and stimulating contribution. Those who believe that trauma is a universal diagnostic category will be less persuaded. Intellectual and professional differences, however pronounced, may yet serve the useful purpose of stimulating both research and thinking about a subject that arouses passions, if not enlightenment.

GERALD N. GROB

John S. Haller, Jr. *The People's Doctors: Samuel Thomson and the American Botanical Movement, 1790–1860*. xvi + 378 pp., illus., tables, apps., bibl., index. Carbondale/Edwardsville: Southern Illinois University Press, 2000. $49.95.

Samuel Thomson (1769–1843), a New Hampshire farmer, devised a form of medical treat-

ment that became popular in the United States for about three decades to the middle of the nineteenth century. Thomson relied on steaming and botanical substances—mainly cayenne pepper and lobelia—to increase the body's temperature and restore health. He practiced on others, acquired a patent for his medicine, sold a "right" to others wishing to practice his methods, and formed the "Friendly Botanic Society." In 1822 the first of many editions of his *New Guide to Health; or, Botanic Family Physician* appeared, along with *A Narrative of the Life and Medical Discoveries of Samuel Thomson.* His practices were lucrative: even to buy the *New Guide to Health* at $2, a follower must already have purchased a right for $20. Going well beyond self-help medicine, his publications also demanded reform in education, politics, religion, and attitudes to class and gender, among other spheres. Thomson's followers then produced their own books and journals, spreading Thomsonianism among reform-minded Americans from North to South and into the Midwest.

The People's Doctors, by John Haller, assumes that readers know this story and its context. The book opens not with Thomsonianism (or Thomsonism, as Haller suggests was preferred by at least one practitioner) but with phrenology. A quotation from Daniel Drake, a prominent physician, seems then to herald a sophisticated study of this sect as an antiestablishment group of reformers. As Haller points out, such a study would be a prolegomenon to the later history explored in his three other books on American botanic medical practices. Instead, *The People's Doctors* primarily documents the development of Thomson's sect in three parts: "The Man and His Medicines," "The Middle Years," and "The Diaspora." The central chronology is framed by two four-page sections that introduce and explain Thomson's role in the history of American medicine. Ten appendixes present organizational records of Thomsonianism (such as Thomson's patent and three separate constitutions of Thomsonian societies).

Steeped in Thomsonian journals, this study demonstrates their richness for historical investigation. Thomson appears here in a gentler historical light than that of untutored lay healer; indeed, adopting modern terms, Haller portrays Samuel Thomson as a pioneer in the concept of a national prepaid health-care plan (pp. 150, 250). These journals also reveal the extent to which Thomsonianism was not monolithic, despite Samuel Thomson's bids to control both his medicines and his followers: by the late 1830s, "new light Thomsonians" emerged to pursue improvements on his original botanic medicines.

Reliance on Thomsonian journals and Thomson's autobiography simultaneously leads to the adoption of the views of those studied. From the outset, for instance, the study refers to "Dr." Thomson; it is some time before the uninformed reader learns that Thomson was not a doctor in the usual sense. The eventual split among followers is attributed in part to Thomson's having become "an angry old man" (p. 163), drawn "deeper and deeper into petty quarrels," who "seemed too opinionated" (p. 250)—despite evidence that, from an early age, Thomson had an unbridled urge for control exercised through lawsuits, defamation of character, and ostracism. More important, this reliance raises questions about context. The sect's publishing, organizing, and educational activities are not connected to the activities of mainstream medicine in the same period. Haller situates the sect historically only in his last section, "Reassessment," but without citations; in fact, other studies on Thomsonianism are relegated mainly to the acknowledgments, with many excluded from the bibliography.

A rich and potentially rewarding subject for social historians, Thomsonianism reflected intricately woven assumptions about reform issues in a turbulent period in American history. Haller has contributed an internal, organizational perspective on the sect that may aid future researchers. Yet while his observation that the "history of Thomsonism is illustrative of the divided soul of American sectarian medicine" (pp. 250–251) may be true, we need a broad-based, balanced history to be able to evaluate it.

JENNIFER J. CONNOR

David A. Mindell. *War, Technology, and Experience Aboard the USS* Monitor. xii + 187 pp., frontis., illus., bibl., index. Baltimore/London: Johns Hopkins University Press, 2000. $35 (cloth); $14.95 (paper).

This is a fun, chatty little book that also manages to say, comprehensibly, much that is profound about technology. This trifecta is an embarrassment to those of us phlegmatic of mind and turgid of prose, but more about that presently. First, the book.

For any kid who ever doodled his way through junior high school American history—and therefore for most of our countrymen—the "victory" of the Union ironclad *Monitor* over the CSS *Virginia* (née USS *Merrimack*) preserved the Union blockade, assured Northern victory in

the Civil War, and signaled both a technological revolution in naval warfare and the advent of American technological preeminence. As David Mindell shows, however, none of the above. Moreover, these historical myths resulted not from fond recollections of a remote, more heroic time and place but, rather, from the carefully orchestrated real-time machinations of interested historical actors: designers (most notably John Ericsson) and allied contractors and politicians (often one and the same), navy bureaucrats and officers, cabinet-level officials, newspaper propagandists, and earnest intellectuals, including Nathaniel Hawthorne and Herman Melville.

The *Monitor* barely made it to her famous pas de deux with the *Virginia* in Hampton Roads, and while she achieved tactical stalemate, *Virginia,* by her continued existence, strategically checkmated the *Monitor,* the Union Navy, and the Army of the Potomac in the Peninsula Campaign. Even after the *Virginia* was scuttled by the retreating Confederates, *Monitor* continued to swelter in futility, anchored for most of the summer of 1862 up the James River, just short of Richmond. But by then she was a sacred icon: in the telling phrase of the major source for this book, the ship's paymaster, Lt. William Keeler, she was "an iron ship in a glass case," too valuable to Union morale to risk in combat.

The six months of idle futility did give Keeler and his compatriots more than ample time for surprisingly deep ruminations on the nature of war and technology: on what classic military virtues such as "heroism" or "courage" or "honor" could possibly mean for those invulnerable behind iron armor; on whether "casualty-free war" would be morally acceptable either to the now "machine operators" of the *Monitor* or to her victims; on the irony of life in a ship that was really a submersible, dependent on sealed hatches, often inadequate and unreliable ventilation blowers, and bilge pumps for life support; on the disjuncture between the "public" *Monitor* and life on the James; presciently, on what death might be like in an iron coffin. Nathaniel Hawthorne, who visited the ship soon after the engagement in Hampton Roads, made perhaps the most chilling observations for those of us fed a steady diet of "smart bombs," cruise missiles, UACVs (Unmanned Aerial Combat Vehicles, in Pentagonspeak), and the happy prospect of war by remote control: "human strife is to be transferred from the heart and personality of man into cunning contrivances of machinery, which by-and-by will fight our wars with only the clank and smash of iron, strewing the field with broken engines, but damaging nobody's little finger except by accident" (quoted on p. 84). Not then; probably not yet.

Mindell probes and explores these weighty issues with a grace and aplomb that virtually indict more ponderous efforts. Yet therein lies a sublime reflexive paradox: Could this argument, so clearly expressed, have been thought, much less understood, without the twenty-five years of scholarship comprised by EPOR (the Empirical Programme of Relativism), SCOT (the Social Construction of Technology), or actor network theory? Happily, none of the jargon of any of these schools intrudes in this story. But the narrative, so clearly written, and the side comments within the narrative, so cleverly phrased, evoke a whole analytical structure, laboriously, if often murkily, assembled by tedious writers and tendentious theorists. Like the *Monitor* herself in her glass box, the history told here reflects as much as it reveals. Elegant and understandable historical narrative and theoretically animated (albeit often abstruse) inquiry are not, it would seem, mutually exclusive antagonists, but the warp and weft of collective intellectual endeavor. If some see farther or more clearly than others, it may be because, sometimes, they stand on the shoulders of industrious moles.

ED CONSTANT

Malachi Haim Hacohen. *Karl Popper: The Formative Years, 1902–45: Politics and Philosophy in Interwar Vienna.* xiv + 610 pp., bibl., index. Cambridge/New York: Cambridge University Press, 2000. $54.95.

What could be the motives for producing a Popperian half-life such as the present volume? This work, which takes Karl Popper right up to his debut on the world stage with the assumption of his position at the London School of Economics, displays no inclination to follow up with the complementary second half of Popper's life sometime in the future. Indeed, the author admits that the omitted subsequent "public Popper" was frequently an embarrassment. Here is truncation with a purpose: this book is written to recommend the work of the early Popper to the "academic left" and to "historiciz[e] the postmodern predicament [as] an antidote to current false consciousness" (p. 262). Since this is a work in social/political theory masquerading as biography, I shall respond in kind.

There have recently been a spate of attempts to revisit the major figures of the philosophy of science in the twentieth century, primarily for the purpose of explaining to ourselves what it was that provoked such an efflorescence of ingenuity,

only to result in the subsequent letdown we now confront or enjoy: Cartwright *et al.* on Neurath, Fuller on Kuhn, and Kadvany on Lakatos. Malachi Hacohen admits that this constitutes his motive as well: he is impressed with the early Popper's political leftism and anti-foundationalism, regarding the later Cold Warrior as a sad retrogression. It seems to me, however, that he has missed the major lesson of all these retrospectives. Crudely, what emerges from these exercises, as a group, is the thesis that the looming significance in general intellectual discourse of philosophers of science in the mid-twentieth century (as opposed to their current irrelevance) was due to the fact that they were doing social theory all along under the guise of describing Science. Furthermore, their question of the true nature of legitimate Science was seen as a crucial preliminary to understanding which politico-economic system would come to dominate in the Great Depression, World War II, and the Cold War.

From this perspective, I strongly doubt that Popper's social significance ever derived from his durable "solution" of any pressing *philosophical* problems: Did he really proffer a usable "demarcation criterion" for science, or rectify the problem of induction, or adequately describe probabilities as propensities, or banish "subjectivism" from physics, or even really demonstrate that Marxism was untestable? (This is not the way we now view the fall of the Wall.) No, during the Cold War it turned out that the very best apologists for Western society were leftists and anti-foundationalists; and one can observe this in the history of postwar social sciences like economics (*vide* the Cowles Commission) and psychology, as well as the philosophy of science. Kuhn *et al.* then took the next logical step in the sequence: something like "critical rationalism" was widely deemed a thoroughly implausible account of *social* organization in an anti-foundationalist context (certainly Popper himself never presided over an "Open Society" of scholarship, as Hacohen ruefully admits). Social order had to be reconceptualized and reimposed, be it through "normal science," "progressive research programs," or whatever.

Hence, banishing the half-life of the Cold Warrior from the biographical account is to parade a pointless Popper in a plotless Punch-and-Judy show. Hacohen rightly cautions the reader about the unreliability of Popper's autobiography *Unended Quest,* but he should have taken more to heart Popper's assertions therein that although he knew almost no social theory, that didn't prevent him from seeking to dictate good "scientific method" to the social sciences (p. 121); and furthermore, that his vaunted method of "situational analysis" was little more than a repackaging of neoclassical economics (itself a physics imitation) as a general methodology for the social sciences (p. 117). Thus when Hacohen proposes "situational logic" as a template for his historiography, is he sufficiently aware that he is merely participating in the general movement to extend neoclassical economics as a Theory of Everything for our contemporary globalized situation? If the purpose really was to demonstrate that Popper was situatedly rational, there would be no pressing need to write a biography; one could just as well fit his data to a generic maximization model. History should make us rather more self-conscious about our scholarly and political options, not less.

PHILIP MIROWSKI

Ellis L. Yochelson. *Smithsonian Institution Secretary, Charles Doolittle Walcott*. 589 pp., illus., notes, bibl. Kent, Ohio: Kent State University Press, 2001. $55 (cloth).

This volume continues the biography that Ellis Yochelson began with *Charles Doolittle Walcott: Paleontologist* (Kent State, 1998), which Ronald Rainger reviewed for *Isis* (1999, *90:* 843). The present volume covers Walcott's years as Secretary of the Smithsonian Institution, from 6 May 1907 until his death on 9 February 1927. The precise dates here are simply a sign that, *mutatis mutandi,* most of Rainger's criticisms regarding the first volume also apply to the second. Yochelson continues with his rigidly chronological presentation, including virtually every recorded event from Walcott's daily life during these nearly twenty years, with almost no selectivity. This is a particularly frustrating method of presentation for several reasons. Not only is almost everything of apparently equal value—administrative and scientific work and controversies, Christmas and Thanksgiving holidays, and the multiple trips to the dentist to remove teeth, one by one, in the latter chapters—but the interwoven presentation makes it very hard to follow the threads of the different stories that are of real interest to a historian. Events that unfolded over years here are revealed over the course of chapters, often one line at a time. While reading the book I often felt that I had been hijacked on the New York City subway system and that the train was hurtling from one station to another, along every line, with no apparent reason and hardly any indication of where I was headed. Surely not even in his own living

of his life could Walcott have experienced it as so without organization.

This book is not without themes, however, and several finally become very apparent. First, Yochelson evidently believes that Walcott was a near-perfect scientist and administrator. Second, Yochelson obviously does not approve of a number of things about the more recent history of the Smithsonian and Congress's way of dealing with the institution. Third, Yochelson very much disagrees with Stephen Jay Gould's treatment of Walcott in *Wonderful Life: The Burgess Shale and the Nature of History* (Norton, 1989). Treating these in reverse order: Gould does not need whatever poor defense I might be able to make. Yochelson's presentation might have more credibility if he did not seem so pointedly sarcastic at times. And Walcott does not have to have been perfect to have been highly influential. Walcott and historians alike will be much better served when some historians sit down to study him as a human being, replete with abilities and foibles, a man of his time, a scientist, businessman, and administrator, with a hand in much of the development of the late nineteenth- and early twentieth-century American scientific establishment. This treatment will of necessity be different than Yochelson's "day-planner" approach. Finally, Yochelson could profitably have taken a lesson from Walcott, who evidently devoted much more time to proofreading than Yochelson did. This book is replete with typographical errors, at least some of which are also obvious errors of fact. This should make readers hesitant to rely on Yochelson alone as an authority.

Yochelson has performed a useful task in drawing attention to Walcott, but I hope this work spurs historians to give him sustained, serious, and discriminating attention. His well-documented career gives every indication of providing rich rewards to such study.

JAMES G. CASSIDY

Erhard Scholz (Editor). *Hermann Weyl's* Raum-Zeit-Materie *and a General Introduction to His Scientific Work*. (DMV Seminar, 30.) viii + 403 pp., bibl., index. Basel/Boston: Birkhäuser Verlag, 2001. $45 (cloth).

In the range of his intellectual interests and the profundity of his mathematical thought Hermann Weyl (1879–1955) towered above his contemporaries, many of whom viewed him with awe. This volume, the most ambitious study to date of Weyl's singular contributions to mathematics, physics, and philosophy, looks at the man and his work from a variety of perspectives, though its gaze remains fairly steadily fixed on Weyl the geometer and space-time theorist. Structurally, the book falls into two parts, described in the general introduction by the editor: Part 1 contains four essays (two in English, the other two in German) on particular aspects of Weyl's work, highlighting ideas he developed in various editions of his classic *Raum-Zeit-Materie*. Part 2 presents a lengthy study (in English) by Robert Coleman and Herbert Korté covering nearly the whole gamut of Weyl's mathematical research, an impressive feat. Both in the introduction as well as in footnotes to the articles Erhard Scholz's editorial voice chimes in discreetly, helping tie all five studies together.

Coleman and Korté begin chronologically with Weyl's early work in analysis and the modern theory of Riemann surfaces before turning to differential geometry, unified field theory, and the space problem, a topic they use as a springboard for a discussion of their own recent work on the foundations of space-time. They then take up Weyl's shift to group representation theory and its applications to quantum mechanics, ending with his much earlier research on the structure of the continuum. All of these topics are well handled, but the authors' own agendas coupled with their penchant for overlooking chronology in order to package Weyl's work into neat little bundles leave one feeling rather stranded and far removed from the sources of Weyl's inspiration. Moreover, the narrative style makes this part of the volume read like a technical appendix, albeit a most informative one. Readers who tackle Scholz's far more contextualized essay will be amply rewarded by comparing his views with the opinions set forth by Coleman and Korté in Part 2.

Scholz gives a masterful account of Weyl's intellectual journeys from 1917 to 1925 in a study that serves as a fulcrum for the entire volume. Drawing on a number of recently published studies, including his own, on the interplay between mathematics and physics inspired by Einstein's theory of general relativity, Scholz describes how Weyl responded to this challenge by developing a truly infinitesimal space-time geometry that generalized classical Riemannian geometry. Although unconvinced by Einstein's critique of his unified field theory, Weyl shifted his focus from this realm to the classical space problem, analyzed earlier with more primitive techniques by Hermann Helmholtz and Sophus Lie. In this connection, it should be mentioned that Thomas Hawkins has given a probing analysis of Weyl's related work on the representation of Lie groups in his tour-de-force work, *Emer-*

gence of the Theory of Lie Groups (Springer Verlag, 1999). Scholz argues that Weyl's struggle to tame his modernized version of the space problem stemmed from a deep-seated belief in his geometrical ideas, which in turn were nourished by philosophical musings. By demonstrating the closely related conceptual links that motivated Weyl's research in infinitesimal geometry, space-time physics, and the foundations of mathematics, Scholz nicely illuminates the underlying fabric of epistemological concerns that occupied Weyl's attention during this fertile period.

The three remaining essays in Part 1 focus on other aspects of Weyl's work in mathematical physics and cosmology. Skuli Sigurdsson's "Journeys in Spacetime" offers a broad interpretation of Weyl's career, one that emphasizes Weyl's sensitivity to cultural tensions as reflected in his philosophical roots, which combined phenomenology with facets of German idealism. Shaken by the annihilation of cultural values in Nazi Germany, Weyl became deeply aware of the gulf that separated his earlier life in Göttingen and Zurich from the one he took up at Princeton's Institute for Advanced Study in 1933. He tried to adapt, but felt out of place in an Anglo-American scientific culture openly hostile toward metaphysics and speculative philosophy. Sigurdsson stresses these tensions, contrasting the introspective, creative individual against the backdrop of the collective in the age of the machine, but without spelling out which collective(s) were most important for him. Wolfgang Pauli thought he knew and, like Einstein before him, he had no compunction about bluntly telling Weyl he was a mathematician, not a physicist.

Pauli's opinions notwithstanding, Weyl did far more than just dabble around the mathematical edges of the new physics. If Coleman and Korté perhaps press their case for his visionary accomplishments too far, Norbert Straumann's essay "Ursprünge der Eichtheorien" suggests why Weyl's reputation among physicists has risen steadily ever since the advent of Yang-Mills theory in the 1950s. In the course of describing Weyl's adaptation of his gauge transformation formalism to Dirac's electron theory, Straumann sheds considerable light on Pauli's role as self-appointed watchman guarding the disciplinary boundary that separated theoretical physics from physical mathematics (jargon introduced by Pauli's teacher, Arnold Sommerfeld). He further suggests that disciplinary jealousy was a major reason why Pauli dismissed Weyl's two-component formalism for spinors out of hand.

In the realm of cosmology, on the other hand, Weyl's work has long since passed into the dustbins of history, as Hubert Goenner remarks in recounting a fascinating chapter in the infancy of space-time physics. While doing so, Goenner shows how initially Weyl almost slavishly adopted what Einstein called Mach's principle, which asserts that the metric structure of space-time is solely determined by the distribution of matter in the universe. This notion was quickly challenged by Willem De Sitter, who showed that Einstein's matter-free field equations admitted a global solution with non-zero constant curvature. Both Einstein and Weyl tried to argue that invisible masses must be present just over the "spatial horizon" of De Sitter's world in order to account for its curvature. Goenner meticulously analyzes the physical and mathematical issues at stake in this debate, stressing how Weyl gradually moved away from a strong physical interpretation to one in which mathematics models rather than physics models simply reveal natural phenomena. He argues further that Weyl's cosmological principle arose as the final expression of his search for a deeper physical meaning.

Given the quality of these essays, it is regrettable that this book contains so little about Weyl's professional career, a weakness the editor could have redressed at least partially in his general introduction. This omission is all the more unfortunate given the dearth of readily accessible information about Weyl's life available elsewhere. For however mundane his outward existence may have been, the reader cannot be expected to appreciate the interplay between the world Weyl knew and his creative responses to it without fairly detailed knowledge of his biography. Shorn from these contexts, it becomes difficult to form a flesh-and-blood image of Weyl beyond the cliché-ridden stereotype that sees him as a "heroic thinker in the grand German tradition." While none of the authors falls into this trap, the collective impression they leave suggests a most enigmatic figure. Either Weyl the man tends to get lost in the shadows of his collected scientific output or he appears as a mystic loner, an outcast who abhorred the machine age in which he lived. Closer attention to the people in his life would no doubt produce a very different picture of the man and his interests. This major lacuna notwithstanding, the present volume will surely remain an indispensable resource for any future investigations of Weyl's staggering intellectual achievements.

DAVID E. ROWE

Alain Herreman. *La topologie et ses signes: Éléments pour une histoire sémiotique des mathématiques*. 348 pp., figs., tables, index. Paris/Montreal: L'Harmattan, 2000.

Topology uses simple geometric and algebraic ideas, but its huge success and vast ramifications make it a tough nut for historians of twentieth-century mathematics. Two books have addressed it well: Dieudonné (1989) chronicles about one thousand key definitions and theorems, and essays in James (1999) focus on forty central themes. Both assume considerable mathematics, but neither offers a historical synthesis of the simplest core ideas. Now, Alain Herreman uses semiotics to watch these leading ideas develop through the founding works of Henri Poincaré, Oswald Veblen, James Alexander, and Solomon Lefschetz. Herreman states outright (p. 24) that semiotics will not exhaust these meanings, but he makes it a revealing tool.

The method is especially suited to Poincaré, who will define one technical term repeatedly in a single work, each time differently, as if it was the first, and perhaps no definition will match any use of the term in proofs. For Poincaré no term gets meaning from a definition. Each functions in relation to the others—that is, specifically in relation to other terms in Poincaré's work. It is no use invoking standards of rigor current in Poincaré's time and place or then-current definitions. Poincaré was well known at the time for using neither: Poincaré's meanings must be derived from his writing, as Herreman does.

Herreman bases his semiotics on Hjelmslev yet refutes Hjelmslev's concern that mathematics may be "monoplanar," with no content beyond the signs themselves (p. 37f). The book depicts four levels of content at work in these authors: algebraic, geometric, arithmetic, and set theoretic. Herreman says a sign has algebraic content if its use depends on its written expression, the way polynomials are formal expressions added and multiplied by formal rules. Early topologists—here especially Alexander—sought purely combinatorial methods. Thus a "cube" is a set of six "faces," twelve "edges," and eight "vertices," each taken as primitive and described only by a short table showing which ones meet which. Combinatorics typifies arithmetic content for Herreman. Yet a cube is also an infinite set of points. Herreman speaks of geometric content when a sign indicates both a set of parts and a set of points. Today we might use different notations for the set of points and the set of parts—Poincaré *et al.* did not. Herreman will not reconstruct their works or restate them in other words; rather, he uses these levels of content to organize extensive quotations and analyze the relations in each text as they move toward deeper union of the algebraic and geometric.

The chapter on Lefschetz makes a great finale. Lefschetz is arguably Poincaré's closest and greatest student, though the two never met. Like Poincaré's, his work is at once compelling and baffling, decisive for the future of mathematics yet brutally difficult to absorb. Semiotics serves well in presenting this mathematician who "never stated a false theorem or gave a correct proof," as his friends joked.

The book does not go far into theorems. Yet it requires some background. A beginner might enjoy it with Alexandroff (1961), a gem itself, written with unusually strong historic sense. Specialists will enjoy reading it with the original works for its fresh viewpoints and novel connections. It is a fine way to analyze the works, to see how they create their own meanings.

COLIN MCLARTY

Javier de Lorenzo. *La matematica: De sus fundamentos y crisis*. (Colleccion Ventana Abierta.) 190 pp., bibl. Madrid: Editorial Tecnos, 1998.

In this essay Javier de Lorenzo reconstructs the so-called crisis of the foundations of mathematics, a crucial scientific debate of the early twentieth century whose larger significance is still in need of much research. This is not an introductory text, as some background knowledge of the positions of the main actors is taken for granted. Rather, we are offered a historical interpretation of the emergence of this debate that connects it with more general changes in contemporary mathematical practice.

The author presents his interpretation in opposition to what he identifies as the "canonical-orthodox" historiographical approach to the debate. According to this approach, around 1900 the foundations of mathematics were shattered by the discovery of unforeseen antinomies and paradoxes, anomalies that seemed to reveal major logical problems within certain basic assumptions. The "foundational programs" were designed to offer mathematical practice new and solid grounds.

By contrast, de Lorenzo turns his attention to the actual practice of the mathematicians who were involved in the turn-of-the-century debate. He describes the foundational crisis as originating through a series of conceptual and technical changes that were reshaping the field in the late

nineteenth century. In general terms, these changes were related to the gradual shift from a practice still linked to geometrical intuition (*hacer figural*) to one based on purely structural considerations of the most abstract kind (*hacer global*). To this end, rather than referring to the usual case of non-Euclidean geometries, de Lorenzo sketches the process of generalization and formalization in fields such as projective geometry and the theory of functions. He focuses on the changing meaning of the notion of "demonstration" and on its new epistemological and ontological implications. Similarly, the notion of "axiomatic method" is scrutinized, and the controversy between Frege and Hilbert is employed to clarify the passage from the traditional to the modern conception.

Against this background of changing practices and shifting concepts, the "discovery" of antinomies loses much of its catastrophic flavor. The set paradoxes and the teratology of curves should be seen not as revealing the logical shortcomings of a preexisting and monolithic edifice of mathematics but, rather, as signs of the early development of a different form of mathematical practice. In this sense, paradoxes functioned as fruitful conceptual spaces in which mathematicians explored, negotiated, and legitimated new demonstrative methods, new definitions of basic notions, and the new meaning of existential proofs.

From this analysis, a view of mathematics as simply the product of a certain kind of human praxis emerges. The "foundational" enterprise cannot refer to any transcendental principle but, rather, should be viewed as a critical activity of conceptual clarification. In this sense, the book contributes to the study of logical and mathematical knowledge by focusing on the practices and the purposes of working mathematicians, in contrast to the rational reconstructions provided by much philosophy of mathematics. However, its significance would have been increased had de Lorenzo related his argument more explicitly to the body of recent historiography that has been dealing with these processes of conceptual and technical change and with their wider cultural meaning.

MASSIMO MAZZOTTI

Vaclav Smil. *Enriching the Earth: Fritz Haber, Carl Bosch, and the Transformation of World Food Production*. xx + 338 pp., illus., figs., tables, apps., indexes. Cambridge, Mass./London: MIT Press, 2001. $34.95.

Was the Haber-Bosch ammonia synthesis the twentieth century's greatest invention, surpassing all others—the airplane, plastics, antibiotic drugs, and the computer? This is Vaclav Smil's claim in *Enriching the Earth,* a historical-biographical-geographical account of how the ammonia synthesis transformed world food production.

Smil begins with a discussion of those responsible for discovering elemental nitrogen and fixed nitrogen's role in agricultural processes. He includes Justus von Liebig, Jean-Baptiste Boussingault, Daniel Rutherford, Joseph Priestley, and Antoine Lavoisier. Two tragic Nobel Prize–winning German chemists, Fritz Haber (1868–1834) and Carl Bosch (1874–1940), are Smil's main characters, however, and their invention and development of the high-pressure ammonia synthesis make up the substance of his story.

Sir William Crookes's 1898 address to the British Association for the Advancement of Science warned the scientific community that the world faced a serious food shortage unless scientists could find a way to fix the atmosphere's nitrogen for fertilizer production. Low-quality impure sodium nitrate mined from beds in Chile since 1820 was the only commercially available source of fixed nitrogen for the preparation of compounds such as nitric acid. Without a better source, Thomas Malthus's century-old prediction that population would outrun food production threatened humanity's survival.

Several scientists achieved moderate degrees of success with nitrogen fixation processes, but only the patient and persevering Haber invented a process that effectively ended the search for an alternative to Chilean nitrate. The first of these new processes was the cyanamide process that the Germans Adolf Frank and Nikoden Caro developed in 1902. The next year Kristian Birkeland and Samuel Eyde in Norway invented the Birkeland-Eyde electric arc process. Both processes were technologically successful but had shortcomings that made the Haber-Bosch ammonia synthesis even more significant.

As Smil points out, Haber was not the first scientist to attempt to synthesize ammonia from its elements. Unintended or chance events led to the failure or abandonment of such efforts by Wilhelm Ostwald, Henri Le Chatelier, and Walther Nernst. Ostwald withdrew his 1900 patent application after Bosch convinced him that he had not really synthesized ammonia from its elements. Le Chatelier abandoned his year-long catalytic study in 1901 when his small high-pressure apparatus exploded in his laboratory. Nernst thought the yields that he and Haber ob-

tained independently in 1904–1907 were too low and the synthesis unfeasible.

Crookes's warning had been the impetus for Haber's ammonia synthesis, but the outbreak of World War I in August 1914 and Germany's shortage of explosives to fight a long war accelerated BASF's industrialization of the synthesis. In 1910–1911 Bosch introduced the double-tubed hydrogen-resistant converters that withstood the high temperature and high pressure the synthesis required. Alwin Mittasch tested 2,500 substances in 6,500 experiments between 1909 and 1912 and found that a catalyst of iron activated with aluminum oxide or other metallic oxides worked best. In September 1913 BASF's industrial-scale plant in Oppau began production of ammonia; the industrial-scale platinum-catalyzed conversion of ammonia to nitric acid that the German military needed for explosives followed in May 1915; and the larger Leuna ammonia plant went onstream in April 1917.

In the remaining chapters Smil traces the lives of Haber and Bosch, the modifications of the Haber-Bosch process that others introduced, and the impact of the Haber-Bosch process on civilization. Haber's life included a series of unfortunate events, such as his commitment to gas warfare resulting in his wife's suicide, his controversial 1919 Nobel Prize in chemistry, and his exile from Germany and death in Switzerland. Although a 1931 Nobel recipient, Bosch experienced depression and disillusion following Hitler's election in 1933. He died in April 1940, when the German military appeared invincible, and so never saw Germany's demise.

Smil mentions another hydrogenation process, the coal-to-oil synthesis that Friedrich Bergius, a student of Haber and Nernst, invented in 1912–1913, shortly after Haber's ammonia synthesis. With the support of the German military, Bergius tried unsuccessfully during World War I to develop the much more difficult coal-to-oil hydrogenation process and thereby provide Germany with a domestic source of oil. By the World War II years, IG Farben had industrialized Bergius's process, and just as synthetic ammonia prolonged World War I, synthetic oil prolonged Germany's participation in World War II.

A little-known episode missing from Smil's history of nitrogen fixation deserves a look. Beginning in 1939, Farrington Daniels at the University of Wisconsin developed a thermal process for nitrogen fixation, using a regenerative pebble-bed furnace. He envisioned the high-temperature combining of atmospheric oxygen and nitrogen as a cheaper, more practical alternative to the high-pressure Haber ammonia synthesis, and by 1942 his furnace synthesized small amounts of nitrogen dioxide gas for the production of fertilizers and explosives. After the war the United States Army and the Food Machinery Company constructed a forty-ton-per-day demonstration plant in Kansas, but because of unfavorable economics the plant ceased operation in September 1954. Ironically, a new breakthrough in an older technology, a cheaper way of producing hydrogen gas in the Haber synthesis, doomed Daniels's high-temperature process.

In his opening pages Smil makes the claim that the synthetic ammonia process was the greatest invention of the twentieth century. Has he made his case? I think most readers of *Enriching the Earth* will agree.

ANTHONY N. STRANGES

Ulrike Fell. *Disziplin, Profession und Nation: Die Ideologie der Chemie in Frankreich vom Zweiten Kaiserreich bis in die Zwischenkriegszeit*. (Deutsch-Französiche Kulturbibliothek, 14.) 384 pp., figs., tables, bibl., index. Leipzig: Leipziger Universitätsverlag, 2000. DM 78.

Is there an ideology of chemistry? Given recent developments in history of science, we would almost certainly answer "yes" in some form to this question; and in this detailed institutional study Ulrike Fell uses the example of chemistry in France to define more precisely what the "ideology" of a science would be. Fell outlines the collective system of common beliefs and cultural goals of chemists using three interrelated concepts: "discipline" (defining borders of academic territory), "profession" (separating experts from lay people by specialized training and certification), and "nation" (regulating disciplines and professions by linguistic, legal, and political barriers).

Fell's multilayered analysis is divided among three major episodes. The first section of the book treats the institutional and educational patterns in French chemistry to 1914, focusing on the formation and professionalization of the Société de Chimique de Paris with a prosopographical study of its membership by subdiscipline and occupation. She also notes that the loss of the Alsace-Lorraine region in 1871 reinforced among French chemists their sense of inferiority to the Germans (who even before 1871 were perceived as "ahead" in chemistry), leading to a desire to emulate the chemical educational system found in Germany. In the second part Fell closely analyzes the effect of World War I on the identity of French chemists. Prior to 1914 French

chemists, as indicated by the study of the membership of the Société de Chimique and the content and origin of the publications in its *Bulletin,* were predominantly academic and oriented toward pure research, rather than industrial chemists interested in practical results. The outbreak of war spurred the growth of a native French chemical industry, especially in dyestuff production, along with the apparatus necessary for research in applied chemistry and the production of chemicals on an industrial scale. This increasing importance of industrial chemistry resulted in the formation of the Société de Chimie Industrielle. In the third section Fell shows how chemists in France attempted to show the unity of their discipline amid increasing fragmentation. Both the Centenaire de Marcelin Berthelot in 1927 and the creation of the Maison de la Chimie in 1934 were attempts to show the unity of chemists.

Fell has a thorough knowledge of the large secondary literature on French science and has based much of her argument on archival documents. Her book, in which she reworks and rethinks the long-standing issue of the "decline" of French science and Parisian centralization, is a welcome addition to our understanding of science (especially chemistry) in the French state and the dynamics of professional identity.

PETER J. RAMBERG

Gwyneth Hoyle. *Flowers in the Snow: The Life of Isobel Wylie Hutchison*. 271 pp., illus., apps., bibl., index. Lincoln/London: University of Nebraska Press, 2001. $29.95.

Born in 1889 at Carlowrie, the turreted castle her grandfather had built near Edinburgh, Isobel Wylie Hutchison grew up as one of five children in a prosperous Scottish household. Little in her idyllic, sheltered childhood suggested that the modest and reserved girl would one day escape the bonds of her conventional existence and venture alone to the Far North. Nevertheless, between 1927 and 1936, aided by a small inheritance from her father, Hutchison traveled extensively in the Arctic regions of Greenland, Canada, and Alaska as well as in the Aleutian and Pribilof Islands. Back home in Scotland, she wrote four books on her trips, gave illustrated lectures and BBC radio talks, and penned articles for *National Geographic,* the *Scottish Geographical Magazine, Nature,* the *Polar Record, Blackwood's,* the *Times* of London, and numerous other publications. Precluded from further excursions by World War II, Hutchison spent her later years at Carlowrie, where she died in 1982.

In *North to the Rime-Ringed Sun* (Blackie, 1934)—the title comes from a line in Rudyard Kipling's "The Long Trail"—Hutchison told of finding a yellow poppy bursting through the snow-covered Arctic tundra during her 1933–1934 visit to Alaska and Canada. Taking her book title from this vignette, Gwyneth Hoyle shows how Hutchison collected plants for individuals and institutions to underwrite her northern travel. For example, the Royal Botanic Gardens at Kew provided drying paper, presses, field notebooks, and other supplies for the Alaska-Canada trip, promising to pay a small sum for the specimens she brought back. Afterward, Kew scientists helped Hutchison prepare the list of specimens she included in *North to the Rime-Ringed Sun,* an extract of which appears in Hoyle's biography. Citing such contributions, along with her writings and "journeyings worthy of romantic saga" (p. 2), the University of St. Andrews awarded Hutchison an honorary doctorate of laws in 1949.

Thus both from the title and from the dust jacket, which is illustrated with a large Arctic poppy, some readers may expect *Flowers in the Snow* to focus primarily on Hutchison's botanical activities. Indeed, calling her an "orderly and meticulous collector" (p. 198), Hoyle clearly places the largely self-taught Hutchison in perspective among the many dedicated amateurs who have added to the world's herbaria.

Yet it is Hoyle's portrayal of Hutchison as traveler and author that is the main strength of the book. A research associate at the Frost Centre for Canadian Studies and Native Studies at Trent University, Peterborough, Ontario, an avid canoeist, and the coauthor (with Bruce W. Hodgins) of *Canoeing North into the Unknown: A Record of River Travel: 1874 to 1974* (Natural Heritage/Natural History, 1994), Hoyle combines firsthand knowledge of the territory with extensive research in Scottish, English, and North American archives to depict Hutchison within the context of Arctic travel during the 1920s and 1930s. She also compares and contrasts Hutchison with other noted women travelers; especially valuable is her essay "The Literature of Travel and Adventure," which is included as an appendix. Five maps and a section of photographs featuring a snapshot of Hutchison in her "Greenland costume" help complete the picture. In sum, this readable and well-researched biography emphasizes Isobel Hutchison's journeys and publications. Those who wish detailed information about the plants she gathered will want to turn next to Hutchison's own works.

MAXINE BENSON

Gary E. Weir. *An Ocean in Common: American Naval Officers, Scientists, and the Ocean Environment*. (Military History Series, 72.) xx + 404 pp., illus., bibl., index. College Station: Texas A&M University Press, 2001. $44.95.

The beginnings of oceanography are protean. Did a new science spring from the great *Challenger* Expedition of 1872–1876, or from the definition coined by its chemical analyst William Dittmar in the 1880s, or from the need to ameliorate fisheries problems in Europe and North America beginning in the mid-nineteenth century, or from the needs of Scandinavian peoples for better weather forecasts at the beginning of the twentieth century? One conclusion is certain. By the end of World War II, a science recognizable to its practitioners and its patrons had come into being and—especially in the United States—was attracting not only unusually large amounts of funding but also nearly unprecedented public attention.

How this came about in a specific setting, the increasing need after World War I for the U.S. Navy to understand the properties of the ocean for effective antisubmarine warfare, is the subject of Gary Weir's intensively documented study of the changing relations between civilian scientists and the U.S. military between 1914 and the 1960s. It emphasizes the importance of individuals Weir calls cultural "translators," capable of "cross-cultural communication" in carrying scientific ideas to a tradition-bound Navy and of enabling civilian marine scientists to understand the viewpoints of naval officers fighting wars or preparing to fight them.

This three-part study deals first with early attempts to bring science into the U.S. Navy, including the fruitless attempt, in the Interagency Conference on Oceanography convened in July 1924, to bring together naval and civilian groups, and the eventual establishment of a Naval Research Laboratory. Between the wars ad hoc arrangements between scientists and the Navy were increasingly common, but never easy. The second war, however, brought the need for detailed knowledge, especially of the ocean's acoustic environment, and a simple tool contributing to the solution, the bathythermograph, useful to both submarine hunters and submariners. In a way the war never ended, for after 1945 the Cold War and nuclear armaments kept the need for oceanographic information high, resulted in the development of a liberal system of patronage through the U.S.N. Hydrographic Office and the newly founded Office of Naval Research, and thus created a community of ocean scientists in the United States. In a chapter titled "Back to the Sea with a Flourish," Weir gives an important account of the postwar expansion of oceanography through worldwide expeditions—and their close link to antisubmarine warfare—and in another, "Listening, 1946–61," he offers a valuable analysis of how the military's sound surveillance system developed and was deployed.

This very important work on how civilian science and military needs intersected and eventually interacted deserves close study, but it is not flawless. In focusing with such keen vision on developments in the United States, Weir cannot pay sufficient attention to non-American developments in ocean science and its patronage (a deficiency of which he is aware). But his concentration on "translators" (of whom T. Wayland Vaughan between the wars, Columbus Iselin during World War II, and Roger Revelle during the second war and after are important examples), on the technology important to oceanography and naval warfare, and on postwar developments in ocean science offers new resources and new ideas for advancing our knowledge of how this scientifically, militarily, and economically important big science came to flourish in the United States. Traditionally, historians of science have paid little attention to the history of oceanography. Weir's book helps to show us why they should.

ERIC L. MILLS

William B. Meyer. *Americans and Their Weather*. x + 278 pp., illus., index. Oxford/New York: Oxford University Press, 2000. $35.

How, since colonial times, have Americans coped with droughts, blizzards, floods, hurricanes, and other weather-related hazards? How have they exploited abundant rains, fertile soils, healthful airs, and other weather-related resources? How have the definitions of hazards and resources changed? Since social change is more dramatic and occurs much faster than changes in weather or climate, William Meyer argues that the former is the most important variable transforming the weather-society relationship.

Meyer's thesis echoes those of the distinguished geographers Isaiah Bowman—"The physical world changes constantly in its *meaning* to man"—and Preston James—"No climate . . . should be described as inherently favorable or unfavorable except in terms of specific human cultures" (p. 172). Meyer, however, goes considerably further by claiming that weather and cli-

mate are part of the "neutral stuff" of nature and have played no role in shaping American life and activities (p. 7). He supports this with a counterfactual argument: "In the end, the surprising differences in the American climate from what had been expected had little impact on the ways in which the colonies developed" (p. 22). How can he be sure?

Meyer considers weather a neutral physical phenomenon that—depending on social organization—constitutes either a resource or a hazard for human purposes and pursuits (p. iv). He argues that changes in the ways weather influences society do not necessarily mean that the weather itself has changed. The automobile transformed snow from a winter transportation resource into "a pure hindrance"; and air-conditioning and the growth of the sun-belt economy are much more significant than a few tenths of a degree increase in average temperatures (p. 6). Still, on timescales of decades to centuries, the elements remain as formidable constraining factors (if not so much variables) in social relations. There are places that have remained uninhabited for four centuries (for good reason); French settlers' houses in Missouri have large shaded porches (for good reason); and, although low-lying Charleston and New Orleans have certainly changed dramatically over time, citizens still have good reason to fear the wrath of hurricanes.

The book is organized into five major sections by time period: founding myths, antebellum, postbellum (to 1918), modernizing (to 1945), and since 1945. In each section Meyer reviews sectional differences and discusses issues related to weather and climate modification. Subheadings include recurring discussions on migration, transportation, industry, domestic life, and health. This gives the book a clear (if repetitive) structure and allows Meyer to generate an inventory of weather-related topics of interest to social historians.

Meyer's historical stage is largely free of actors and his research is not archivally based. Oblique mention often substitutes for detailed analysis. For example, he is critical of Thomas Jefferson for "retailing an error" common at the time that one can tell the character of the people by the latitude, but he does not bother to trace this idea to its ancient roots and has little else to say about Jefferson's considerable weather-related interests (p. 23). Meyer has no interest in Native American societies (which appear twice in the index as "Indians, American"). Furthermore, there is no analysis and almost no mention of how Americans changed their relationship to the weather during wartime. Meyer is in error when he implies that Americans used Norwegian methods in the D-Day forecasts in World War II (p. 152). Irving Krick and his American associates at Widewing were firmly committed to analog methods, while Sverre Petterssen at Dunstable (a Norwegian on loan to the British) was the only Bergen-school meteorologist on the forecast team.

Americans and their weather is a vast topic, beyond the scope of any one book. Meyer's breezy survey of weather-society relationships and his adaptation of a classic thesis in geography will be of interest to the general public and may suggest to historians new topics for research.

JAMES RODGER FLEMING

Jan Witkowski. *Illuminating Life: Selected Papers from Cold Spring Harbor (1903–1969).* Foreword by **James D. Watson.** xvi + 383 pp., illus., figs., tables, apps., indexes. Cold Spring Harbor, N.Y.: Cold Spring Harbor Laboratory Press, 2000. $25.

Historians of science who are asked to write histories of a scientific institution are frequently given specific instructions to include the important contributions of the institution to current scientific ideas and to look to the future, not to the past. While Herbert Butterfield and his legion of followers may blanch at such a suggestion, many historians of science have become accustomed to the implicit positivistic orientation of these requests from their scientific peers and have attempted to mollify them by approaching institutional history in unusual ways. Thus, instead of writing a historical narrative of Cold Spring Harbor, Jan Witkowski has opted to present seminal papers, arranged in chronological order, with the expectation that "anyone reading only them will learn the story of these years [1903–1969] at Cold Spring Harbor" (p. ix).

The papers Witkowski selects (there are actually twenty-three papers, not twenty as he suggests in the preface) are certainly important and represent some of this country's most eminent biologists. Among them are works by C. B. Davenport, G. H. Shull, Milislav Demerec, Oscar Riddle, Barbara McClintock, Alfred Hershey and Martha Chase, and John Cairns, certainly an impressive list of scientists. The topics, from human genetics to mechanisms of chromosomal inheritance, are also important and of great interest. Witkowski intended the papers, along with the short biographical introductions to the authors he provides, to achieve four goals. First, they describe the scope of the research at Cold

Spring Harbor. Second, together with the introductory material, they set the research in a historical context. Third, the biographical introductions provide historical information about the scientists. But the fourth goal, to present a history of research at Cold Spring Harbor, is less fully achieved.

Certainly, *Illuminating Life* gives a sketch of life and work at Cold Spring Harbor, but historians reading the historical material and the selected papers may wonder why there is not more information about the various changes in research imperatives and more institutional history. The laboratory had a curious relationship with the Eugenics Record Office, but little is written here about the ERO and its leaders, Charles Davenport and H. H. Laughlin. In addition, the papers suggest a fundamental change in research directions during the twentieth century, from physiological work investigating hormonal control in animals to studies in molecular biology and chromosome behavior. Was this shift a deliberate choice, or did it simply mirror larger changes in American biology? Finally, the book lacks any real discussion of the complex relationships and the dynamic nature of the different institutional configurations behind the history of Cold Spring Harbor. The Brooklyn Institute of Arts and Sciences, the Station for Experimental Evolution supported by the Carnegie Institution, the Eugenics Record Office, the Long Island Biological Association Biological Laboratory, and the Cold Spring Harbor Laboratory of Quantitative Biology are all mentioned—but these various institutional forms require more informative treatment than is provided in *Illuminating Life*. However, this expanded story may be in preparation, to be presented in a later publication.

Historians of American biology may find that this book provides them with valuable information concerning one of this country's premier research centers. Like histories of other laboratories, it provides a glimpse at the institutional development of the life sciences in the United States.

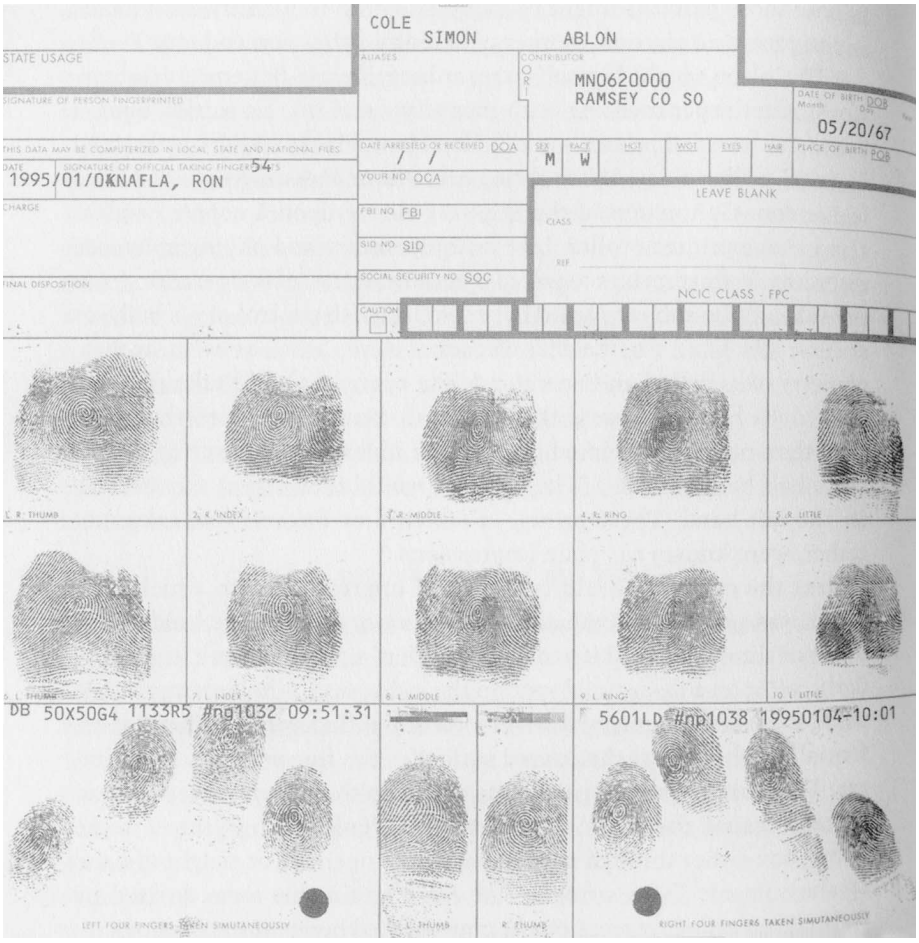
KEITH R. BENSON

Rita Caccamo. *Back to Middletown: Three Generations of Sociological Reflections*. xxvi + 149 pp., bibl., index. Originally published in 1992 in Italian. Stanford, Calif.: Stanford University Press, 2000. $45.

Having lived in Rita Caccamo's Rome and other Italian cities for long periods, I was intrigued by Arthur J. Vidich's foreword, which notes the sociologist Caccamo's Roman background and hence her ability to see Middletown as an anthropologist might, from "the perspective of an 'other'"—a position, he explains, very different from that of Robert S. and Helen M. Lynd, who made Muncie, Indiana, famous in their 1929 and 1937 studies. There are hints of that perspective in these pages. In the preface to this edition, Caccamo remarks on the sense of alienation she felt while living in Muncie as she worked in the archives; and in a very brief concluding chapter, "The Nineties in Middletown," she suggests that Muncie suffers, like the rest of the country, from "the lack of cumulative (historical) experience" (p. 121) of the sort that Rome (by implication) has plenty of. But that's about it for anthropology and the "other," at least on the surface. "We European scholars," she writes, "need to look at these phenomena without our European points of reference" (p. 118).

What stands out in this assessment of *Middletown* (1929), *Middletown in Transition* (1937), and Theodore H. Caplow's late-1970s investigation is Caccamo's deep affinity with the Lynds' radical, pessimistic argument: that Middletown's community life was rapidly being eroded by limited economic mobility and, especially, by a consumer culture that increasingly valued money and the things it would buy above all else—and that Munsonians, in the grip of a power elite and overwhelmed by mass culture, were helpless to do anything about it. Although Caccamo sees weaknesses in the Lynds' schema—a simplistic two-class model, a lack of attention to immigrants and outsiders, the failure to appreciate the adventurousness that comes with modernity, little recognition of the potential of Muncie's residents to challenge and resist power—none of these arguments is pursued with much conviction. Instead, she presents the Lynds as part of a larger group of interwar sociologists who understood the American experience very well. By the same token, she takes Caplow's "Middletown III" to task for its unwarranted optimism about family life, religion, class, and other matters. In a final assessment, she underscores Muncie's obsessions with privacy and marriage and disregard for single people. Caccamo believes that late twentieth-century Muncie manifests the same maladies that the Lynds uncovered some seventy years ago.

Those familiar with the literature on Middletown will not, I suspect, find much that is new here, except perhaps for an extensive discussion of the Lynds' controversial dependence on, and use of, the work of Lynn Perrigo for their chapter "The X Family" (the Ball brothers of Ball jar



Representative fingerprints of the author (Simon A. Cole) of Suspect Identities: A History of Fingerprinting and Criminal Identification, *p. 76.)*

fame) in the 1937 volume. What they will find is part biography, part history of sociology, part polemic: an effort to understand the Lynds as "committed intellectuals" (p. 118) and to appreciate what it was they found so repugnant about life in a city they had decided was typical of the American experience. That Caccamo succeeds so well has much to do with what some may see as an uncritical acceptance of Marxist theory and Frankfurt school arguments about the mass media that underpinned or reinforced the Lynds' work, but much to do also with the outsider Caccamo's Rome, whose coffee bars, neighborhoods, and vibrant streets bear witness to what Muncie once was, and could have been.

WILLIAM GRAEBNER

Simon A. Cole. *Suspect Identities: A History of Fingerprinting and Criminal Identification*. [xii] + 369 pp., illus., tables, index. Cambridge, Mass./London: Harvard University Press, 2001. $35.

We live in a wondrous age. Cyberspace, cloning, AI, cosmetic surgery, sex reassignment, organ transplants, and so on are chipping at our notion of the physical body as a stable entity that defines us from the cradle to the grave. But before we start to think of ourselves as ethereal entities for whom body parts are merely resources, Simon Cole presents us with an intriguing history of how we came to equate ourselves with our bodies. *Suspect Identities* offers clear style, innovative materials, solid research, and instruc-

tive analysis. The result is a rich but accessible book that should become an important text for everybody interested in science and society and in the histories of biological thinking and criminal identification.

In the first part of his book Cole follows the late-nineteenth-century search for reliable physical indicators of social tendencies, potential criminality in particular. This hope, we know, did not materialize. Criminal potentiality could not be read directly from the body. What could be read from the body, however, was the identity of its owner. That may have disappointed the anthropologists and the criminologists, who believed that behavior has biological roots, but it certainly pleased the bureaucrats. Thus, by the end of the nineteenth century, two technologies, anthropometry and fingerprinting, were already used by the growing arms of the state to identify criminals and link them to their history, stored within the burgeoning paper memory of the state.

Anthropometry and fingerprinting were developed in different contexts. Anthropometry emerged in the cities of Europe. Fingerprinting developed on the imperial frontiers. Anthropometry, with its careful protocol and exact quantitative measurements of the human bones, enjoyed more scientific credibility. Nevertheless, it was the visual technique of fingerprinting that won the day. The choice, Cole argues, was more than a mere technical decision in favor of a better technique. It was a preference for industrial-style speed and economy over scientific accuracy and precision. Mechanical in nature, fingerprinting transferred bodily inscriptions of the undifferentiated colonial masses into colonial records with relative ease and effectiveness, and it did not take it long to migrate from the imperial frontiers to the rapidly growing and increasingly anonymous urban centers of Europe and America.

In the middle section of the book Cole follows the twentieth-century brilliant career of fingerprinting, mainly in America. Starting as a record-keeping technology, a way of identifying criminals and linking them to their past, fingerprinting soon developed into a formidable forensic technique that linked suspects to the scenes of their crimes. What intrigues Cole the most is the wholesale acceptance of fingerprint evidence, even though it always lacked an easily articulated scientific foundation. Fingerprinting, Cole points out, was never subjected to the careful scrutiny that is supposed to be inflicted on scientific and legal facts, and fingerprint experts were allowed remarkable leeway by the courts, testifying with almost complete immunity from attacks. Cole's search for an explanation of this peculiar arrangement produces some fascinating insights concerning the mutually constitutive relations between science and the state and the ways in which certain technologies gain credibility and become as common as flying in the air.

In the final part of the book Cole brings his history to bear on our present genetic age. Diagnosing criminals and diagnosing criminality were never clearly distinct. Thus, as Cole points out, every new criminal identification technique—photography, anthropometry, fingerprinting—has stimulated biological theories of criminality. The same happens now with DNA typing, the latest in this long line of technologies. A large number of people expect molecular biology, the science behind DNA typing, to allow us to explain human nature. The history of criminal identification, Cole argues, should teach us to heed the difference between a biological marker and a code and make us skeptical about the likelihood that biological information, genetic or otherwise, will ever provide a full explanation of social behavior.

TAL GOLAN

Lyn Schumaker. *Africanizing Anthropology: Fieldwork, Networks, and the Making of Cultural Knowledge in Central Africa.* xii + 377 pp., frontis., illus., bibls., index. Durham, N.C./ London: Duke University Press, 2001. $59.95 (cloth); $19.95 (paper).

The Rhodes-Livingstone Institute (RLI), founded in Northern Rhodesia (now Zambia) in 1937, was the first social science research institute in Africa. This book is a history of the RLI from its earliest beginnings with emphasis on the years up to 1960. The author, who identifies herself as a historian, supplemented her archival research with periods of fieldwork mainly devoted to oral history but including shorter spells of anthropological participant observation in association with African assistants employed by the institute. She is therefore well equipped to comment on the activities of the RLI, which consisted principally of field research in social anthropology and sociology.

She organizes her data in terms of "field generations," cohorts of researchers who, although they worked alone or with one assistant at their field sites, nevertheless developed a collective identity through visits to each others' sites and through participation in seminars and conferences arranged by the successive RLI directors. Many published histories of research institutions

focus on theoretical assumptions, but Lyn Schumaker instead looks more at the methods and practices of field research. Indeed, she tends to treat the divisions between theory and practice, and between theoretical and applied research, as unproblematic, while successfully avoiding any naïve positivism. She demonstrates her expertise as a historian by providing abundant details of the diversity of conflicting interests that influenced the choice of topics for investigation and of the modes of collecting data in the field. With over seven hundred footnotes, fifty interviews, and more than three hundred bibliographic items, the book serves anthropologists well.

The book's title indicates its main theme. The institute started in a colonial environment in which white anthropologists who had trained in Britain, the United States, and South Africa carried out anthropological research, mainly in rural areas, using African assistants as interpreters and language teachers. Over the years more of the inquiries were made in towns than in villages, with teams of African assistants working more and more autonomously. During the same time, the colonial regime was replaced after bitter struggle by indigenous political independence. The first Zambian director was appointed in 1973, by which time the institute's research had shifted from anthropology and sociology to psychology.

Schumaker uses the concept of "work culture" to explore the significance of the pattern of daily activity of researchers and assistants, thus exposing data usually neglected in anthropological monographs. She draws attention to the vigorous collective interaction that distinguished the RLI from many other research organizations: the RLI, for example, depended significantly for its success on its symbiotic link with Manchester University, where Max Gluckman, its director from 1941 to 1947, became Foundation Professor of Anthropology. Schumaker is, however, careful to point out that her book is not a history of the Manchester department, whose activities extended far beyond the RLI.

The author sometimes ranges widely to relate aspects of the RLI story to topics in the literature on the history of science. For instance, she links the problems faced by Elizabeth Colson, the only female RLI director, to wider discussions of the impact of gender on professional careers. Likewise, she discusses the frequently made claim that anthropology was the "handmaid of colonialism." Her book supplies more than ample ammunition to refute this claim and gives us an impressive and well-documented account of how a social science research institute operated in a colonial society undergoing radical transformation.

J. A. BARNES

David Patrick Keys; John F. Galliher. *Confronting the Drug Control Establishment: Alfred Lindesmith as a Public Intellectual*. x + 235 pp., illus., figs., apps., bibls., indexes. Albany: State University of New York Press, 2000. $57.50 (cloth); $18.95 (paper).

The appearance of a critical biography of the sociologist Alfred Lindesmith is timely, as initiatives to reform American drug laws gain adherents and visibility. Lindesmith is best known as a longtime critic of American drug policy and one of the first scholars to study heroin addiction as a social phenomenon. The object of a sustained campaign of harassment by Harry Anslinger and the Bureau of Narcotics over several decades, Lindesmith has assumed heroic stature in drug reform circles. Within the discipline of sociology, he exerted more influence than might be expected of a scholar who completed only one piece of fieldwork in his career. By deeming a despised population group worthy of sympathetic study and by insisting on absolute rigor in qualitative methods, he helped set the stage for the emergence of deviance studies and symbolic interactionism in the 1940s.

Lindesmith earned his doctorate at the University of Chicago in 1937 under the tutelage of Herbert Blumer. Based on interviews with some 70 heroin addicts, his dissertation argued that addiction occurred only when the addict recognized that the distresses of the withdrawal syndrome were caused by the absence of the accustomed drug and that a new dose would quickly relieve the pain, nausea, and shakes of withdrawal. He stressed the role of experienced addicts in cuing the novice to the meanings of the withdrawal experience. For Lindesmith, addiction was both an alteration in the body's physiological steady state and a social identity. This position was the foundation of his lifelong criticism of the criminalization of addiction and of the psychiatric view that addiction resulted from underlying personality defects.

Keys and Galliher have provided an uneven and problematic biography. They deeply appreciate Lindesmith (Galliher was his student), portraying him as a highly principled scholar who unflaggingly criticized federal drug policy, which he saw as cruel and misguided. They chart his opposition to the repeated stiffening of drug law penalties during the Cold War, when the Bureau of Narcotics connected the drug trade to

Chinese Communist attempts to enslave the American people. They also describe his influence on succeeding generations of sociologists through his coauthorship, with Anselm Strauss, of the multiedition textbook *Social Psychology.* At the same time, they note the limitations of his research and acknowledge Lindesmith's indebtedness to his fellow graduate student Bingham Dai, who was working on a similar project.

The account of Lindesmith as a "public intellectual" is more successful than the account of him as a theorist of addiction. Keys and Galliher expend considerable energy arguing that Lindesmith's ideas have been foundational for the addiction treatment industry that has emerged over recent decades and defending him against a few critics who have taken issue with specific aspects of his theory. However, their understanding of addiction, from either a theoretical or a clinical perspective, is weak, and their citations from the addiction literature, even the sociological literature on addiction, are narrow. By overreaching to praise, they undermine their own purpose.

As a scholar, Lindesmith can better be appreciated as helping to give rise to a tradition of drug studies in sociology that continued in the work of Howard Becker and, a generation later, in the profusion of ethnographic studies of drug use from the late 1960s to the present day. Lindesmith insisted that addicts' own interpretation of their experience was a critical input into any meaningful theory of addiction. In doing so, he helped create a disciplinary base for challenging a deeply entrenched orthodoxy and for arguing that addicts have a right to humane, nonpunitive health care.

The books suffers from sloppy production; citation errors are so pervasive that one should check any reference against another source to be sure of accuracy, and the index fails to note numerous text passages relating to specific entries.

CAROLINE JEAN ACKER

Paul Julian Weindling. *Epidemics and Genocide in Eastern Europe, 1890–1945.* xxii + 463 pp., illus., figs., tables, apps., bibl., index. New York: Oxford University Press, 2000. $95.

Arguably no historian of medicine has done more to draw the connections between the history of health in modern Germany and the destructive eugenic policies of National Socialism than Paul Weindling. And while a certain teleological strain tends to creep into many histories of German medicine, Weindling succeeds in this volume—where the historical ties between Nazism and German biological research and medicine appear most glaringly obvious—in asking at every important juncture whether there is something peculiarly "German" or protofascist in the developments he chronicles. At every turn, he discusses contemporaneous alternatives to modern German scientific practices and dogmas, giving particular attention to international standards at the time.

The result is that Weindling achieves something that until now has been done only piecemeal or drawn out in the terms of critical theory and cultural studies: he connects the dots that link the science of parasitology, technologies of disinfection, the public discourse of anti-Semitism, and the attempted destruction of Nazism's "racial" enemies. He does this by tracing the genealogy of the epidemic control measures, human experiments, and death camp crematoria established during World War II to their sources in sanitary measures and medical research, disinfection procedures, medical entomology and notions of the parasite, poison gas technology, and innovations in cremation dating back to the late nineteenth century.

Already before World War I, Weindling shows, expert and lay observers of epidemics had come to demonize lice as carriers of typhus, a disease that was widely associated with migrants, peddlers, Jews, and gypsies (indeed, it was commonly referred to as the "Jewish disease"). Combating the purported vectors of typhus took on an imperialist form, as German policy makers, bacteriologists, and medical hygienists in the nineteenth century pointed to Asia and eastern Europe as the source of a blight they believed threatened the civilized West. Modern German geo-medicine thus took on the guise of a nationalistic parasitology that sought to radically sanitize and hygienically police the boundaries between Germans and others.

Innovations in disinfectant sprays and gases, fumigation techniques, and hygienic measures (including cremation) designed to squelch the spread of disease that sprang up in the late nineteenth century became common practice during World War I. Mass delousing routines were established around 1915, and the development of poison gas, along with the popular acceptance of the notion of a need for *Lebensraum* (living space), meant that pest extermination became caught up in the project of chemical and gas warfare. After the war, a flood of refugees from the East and images of disease in Civil War Russia only reinforced calls for containment, while poison gas began to be marketed globally and the goal of *Lebensraum* lent a timeliness to lobbying

groups attempting to promote the widespread use of cremation in the 1920s.

Thus, as Weindling contends, a new "death control" impulse, a counterpart to the birth control and sexual reform movements of the same period, existed in Germany on the eve of Hitler's rise to power. Once the regime waged war against eastern Europe and the Soviet Union from 1939, then, it is hardly surprising that not only the military and SS but also hygienists, pest exterminators, and medical researchers played prominent roles in the Nazi racial reordering of Europe. As the Nazified concept of the parasite began playing an ever greater role in the Nazi-occupied East, death itself came to be seen as a form of disinfection.

We can only hope that Oxford University Press will eventually issue an affordable paperback of this important (and now even more timely) book. Weindling's disturbing portrait of medical, scientific, and technical complicity in the worst offenses of National Socialism confirms much of what historians of the Third Reich and Soviet communism have been showing us for well over a decade. Assessing expert behavior on an imagined spectrum between collaboration and resistance does little to help us understand the complex interpenetration of scientific and political enterprises in the twentieth century. As Weindling shows, self-serving opportunism, utopian faith and reformist ambition, militant nationalism and racism, and scientific and institutional parochialism all played a role in radicalizing scientific and medical thinking and practice in a most radical age of ideology.

GREG EGHIGIAN

Joy Elizabeth Hayes. *Radio Nation: Communication, Popular Culture, and Nationalism in Mexico, 1920–1945*. xx + 155 pp., illus., figs., bibl., index. Tucson: University of Arizona Press, 2000. $35.

Radio Nation is a methodologically sophisticated book on the mutual relationships among radio broadcasting, popular culture, and nationalism in Mexico at the local, regional, national, and global levels, covering the period from 1920 to the end of World War II. An epilogue continues the story through the radio-based transition to television in the postwar era. The main social groups examined include the Mexican government, the U.S. Office of the Coordinator of Inter-American Affairs (CIAA), the Raul Azcárraga radio conglomerate, and listeners.

Joy Hayes carries out her ambitious project by developing a multilayered methodology. She brings together the concepts of antimodernism and hegemony from her mentor, T. J. Jackson Lears; recent literature on Mexican nationalism and nation-building as a negotiated process; cultural studies work on the aural characteristics of the radio medium (from Roland Barthes and W. J. Ong); and the formation of culture and nation as a communications process. Hayes argues that "both radio and nation are social practices that interract to actively resist the concept of modernity" (p. 13). Thoroughly modern as far as their both being forward looking, the radio and the nation (which helped to create each other) are nevertheless antimodern institutions in overall effect, because they recreate premodern traditions of music, storytelling, and paternalism that can be developed into other forms of mass communication. In this regard, government station XEQ promoted a "musical nationalism," but the "market nationalism" of Azcárraga's powerful, 200-kilowatt, commercial station XEW eventually triumphed. Even though Azcárraga depended upon radio manufacturers and networks in the United States, his market nationalism was structured by the Mexican state.

An intriguing diagram of power relations in Mexico during World War II shows triangular negotiations between the Azcárraga Group, U.S. media corporations and the CIAA, and the Mexican state—producing and influencing culture markets, U.S. politics, and national culture. Unfortunately from my point of view, the diagram shows radio content and radio audiences as groups outside these negotiations, even though the author pays attention to listening contexts, audience reactions, and the creation of new forms of "traditional" Mexican music for the aural needs of the radio.

The book discusses other issues of interest to historians of science and technology, including government regulation (requiring stations to play a certain percentage of Mexican music and banning nongovernmental political discourse), the agency of users (preferring Mexican and Latin American music, as shown in unsealing radios provided by the government to local communities to tune them to stations other than XEQ), and the appropriation of a technology for use in another society. Very little is said about physical aspects of radio, except for the "presence" of the radio voice, problems with shortwave transmission, which hampered U.S. efforts to beam its message of Pan Americanism and consumer culture to Mexico during World War II, the high power of XEW, and Azcárraga's building a radio

network by means of bicycle messengers because of the lack of telephone lines.

Although *Radio Nation* is convincing, its brevity left me wanting to hear more about programming in the commercial and government stations, changes in regional culture and in Mexico City that might have influenced radio listening habits and vice versa, and more examples of antimodernist tendencies (an argument that sometimes borders on technological determinism). Theory and context tend to crowd out discussions of radio programs, producers, and listeners. That said, *Radio Nation* is a stimulating book that significantly contributes to our understanding of the complex relationships between communications technology and cultures other than those in the United States and Europe.

RONALD R. KLINE

Edmund Russell. *War and Nature: Fighting Humans and Insects with Chemicals from World War I to "Silent Spring."* xx + 315 pp., illus., index. Cambridge/New York: Cambridge University Press, 2001. $49.95 (cloth); $19.95 (paper).

War and Nature is an important, cogent, and timely book about the double-edged nature of technology. Edmund Russell, through meticulous research, establishes a key nexus between the increased use of chemicals in war and peace during several key decades of the twentieth century and the generalized backlash against technology and its unintended consequences that occurred beginning in the mid-1960s. He clearly places pesticides, rodenticides, herbicides, and chemical warfare agents alongside atomic energy, electronics, massive water harnessing and diversion projects, and other prime examples of America's romance with progress. Thus, he joins his work with a significant body of research examining the United States's headlong plunge to embrace technology, along with sometimes belated efforts to grapple with its effects.

The idea of progress has always been a distinctive factor in American identity. However, the fascination with progress was perhaps never so intense, so unreserved, and so naive as in the fifty years from 1914 to the early 1960s. During these years, as Russell points out, the pace of change was accelerated by two world wars that spawned the growth of powerful government agencies as well as commercial companies whose new products benefited richly from government-funded research. Wartime imperatives hastened development of "miracle chemicals" that preserved the health of American soldiers, as when DDT was widely employed in the Pacific theater of war to kill malaria-bearing mosquitoes. The result was what President Dwight D. Eisenhower, as he left office, termed the "military-industrial complex."

Russell uses well-chosen examples from the archives of chemical manufacturers to illustrate how defense contractors supplying electronics, atomic expertise, and other forms of technological expertise useful in warfare softened their public images by touting beneficent peacetime applications of their products. "Better Living Through Chemistry" along with "Progress Is Our Most Important Product" and other slogans familiar to citizens of the 1950s appealed not just to America's love of convenience and gadgetry. They also appealed to a fundamental U.S. belief that "modern science" would solve age-old problems, erase perennial blights, and free people to live on a higher plane than any previously experienced by humankind.

However, because defoliants, flame-throwers, poison gases, and other chemical weapons that aided the war effort almost always conjured up negative images in the public mind, manufacturers used analogies of "victory" in war to victory over man's environment to retain a reputable and lucrative place in peacetime society.

Perhaps most striking are the parallels between chemicals and atomic energy during the 1940s, 1950s, and early 1960s in the United States. Both chemicals and atomic energy proved their awesome power in hastening victory in World War II, and both needed softer images after the war ended. The promises of disease-free fields and enormously increased crop yields appealed to the American public, just as did the hope of electricity "too cheap to meter" and nuclear medicine that might cure cancers.

When chemicals began to poison birds, fish, and other species recognized as valuable and desirable to man, when fields lost their productivity, and when illnesses arose in certain occupations that could be linked to chemical exposure, the downsides of progress became evident. America recoiled and reevaluated. Russell weaves congressional testimony together with advertisements, popular cartoons, and other ephemera to bring to life this uniquely American story. He compels the reader to ask striking questions about a nation maturing and itself asking complex questions about one of its most cherished ideals.

The final irony of Russell's book may lie in the fact that it was written before the World Trade Center attack on 11 September 2001 but published just afterward, in the midst of Amer-

ica's anthrax episodes in the fall of the same year. The world focus on chemical and biological warfare should now provide a whole set of additional readers for this valuable book.

MICHELE S. GERBER

Raùl Rojas; Ulf Hashagen (Editors). *The First Computers: History and Architectures*. (History of Computing.) xiv + 457 pp., illus., figs., tables, app., index. Cambridge, Mass./London: MIT Press, 2000. $39.95.

How much should we know about the underlying structure of a technological artifact in order to understand its history? Quite a bit, according to the authors of *The First Computers: History and Architectures.* This book, a collection of papers presented at the International Conference on the History of Computing in 1998, is aimed at computer scientists and programmers as well as historians of science and technology. The term "architecture" is used in computing to refer to the structure and capabilities of a computer; it includes both logical design (how data are represented, what types of instructions can be performed) and physical design (the use of particular components such as vacuum tubes or electromagnetic relays). Architecture is an abstraction that helps the authors make comparisons between computers from different countries and eras.

The First Computers brings together a wealth of information on computers from the 1940s and 1950s, which the editors present in five main sections. Part 1, "History, Reconstructions, Architectures," addresses general themes in the history of computing. Michael Mahoney's chapter, "The Structures of Computation," examines the relationship between computer science and "pure" mathematics. Even in its most theoretical aspects, Mahoney notes, computer science has always focused pragmatically on feasible implementations of mathematical ideas, thus "blurring commonly made distinctions among science, engineering, and craft practice" (p. 20). Robert W. Seidel examines the rationale for reconstructing historic computers, an activity that has engaged many of the book's contributors. Seidel notes that "as the artifacts of modern computing become invisible, older, larger computers supply a symbol of computing to practitioners, the public, and patrons which is not only visible, but comprehensible" (p. 35).

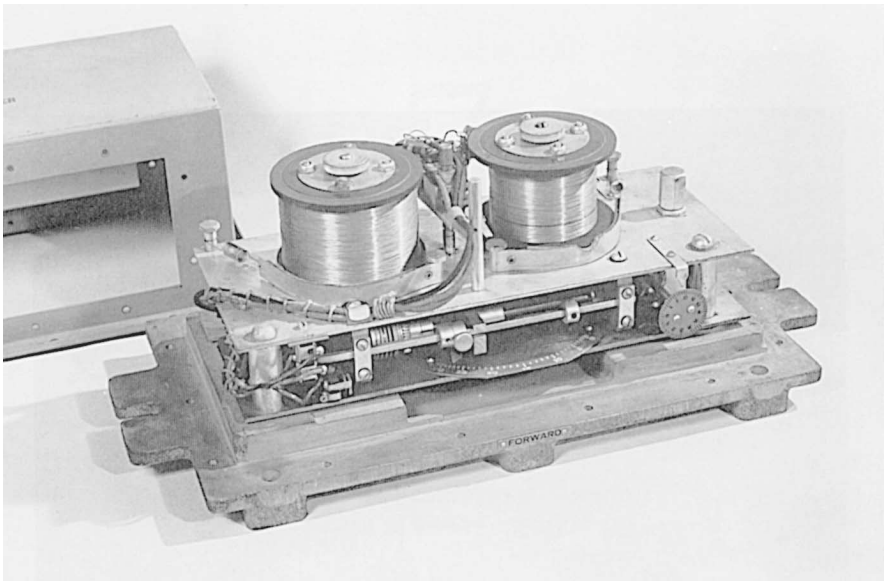
The four remaining parts cover computing developments in America, Germany, Britain, and Japan. The American computers include the Atanasoff-Berry Computer, Aiken Mark I, ENIAC, and the Institute for Advanced Study computer. The section on Germany contains several chapters on Konrad Zuse as well as on such developments as the Göttingen machines, which may not be that well known. Britain is represented by the Colossus, Manchester Mark I, Atlas, and EDSAC. Perhaps least familiar to historians are the Japanese computers: Seiichi Okoma surveys nine early Japanese computers, and Eiiti Wada describes the PC-1, which he helped build. Many of the chapters, especially those written by engineers, begin with a brief history of the machine followed by a detailed technical description of its design and operation. The contributions by historians tend to describe the computer's features in more general terms and discuss the intellectual, organizational, and economic context in which the machine was built.

Because so many of the authors are technically trained, they are able to offer in-depth information on the theoretical underpinnings and design of early computers. But often the discussion is difficult to follow, even for historians working in this area. Tony Sales's chapter on the Colossus, a British code-breaking computer, exemplifies the strengths and weaknesses of accounts written by technical specialists. Sales begins with a clear and engaging narrative of the origins of the Colossus, its method for decoding messages, and its significance to the Allied effort in the Second World War. He then veers into a highly technical discussion that will likely be opaque to readers unfamiliar with terms such as "thyratron rings." The final section is a personal story of Sales's successful effort to have the machine rebuilt. Like the book as a whole, Sales's chapter can be read for a quick introduction to a milestone in the history of computing or for insight into the meanings embedded in technological artifacts—but readers should be prepared to decipher (or skip over) some formidable technical jargon along the way.

JANET ABBATE

Semi Joseph Begun. *Magnetic Recording: The Ups and Downs of a Pioneer: The Memoirs of Semi Joseph Begun*. Edited by **Mark Clark.** viii + 159 pp., illus., apps. New York: Audio Engineering Society, 2000. (Paper.)

Semi Joseph Begun, who died in 1995, is credited with the development of the first commercial tape recorder to be successfully marketed for home use in the United States. His postwar version of the "Soundmirror," a reel-to-reel tape recorder using paper tape coated with ferromag-



Aircraft magnetic recorder (forerunner of black box) (reprinted from Semi Joseph Begun, Magnetic Recording: The Ups and Downs of a Pioneer, *fig. 4.)*

netic powder, was introduced by the Brush Development Company in 1946. Begun's autobiography, edited by Mark Clark after Begun's death, documents the inventor's lifelong obsession with magnetic recording, ranging from his early efforts to develop a magnetic dictation machine to his flight from Adolph Hitler's Germany in 1935, his arrival in the United States, and his appointment as an engineer at Brush in 1938, where he eventually rose to the rank of vice president and chief engineer.

Begun's dictation machine, the "Dailygraph" (ca. 1930), was a wire recorder. The wire was housed in a cartridge that could easily be loaded and unloaded by the average office worker. In 1933 Begun engineered the "Steeltone Tape Machine" for the Lorenz Company. Though the steel tape was virtually impossible to edit and cumbersome (a fifty-minute reel weighed forty pounds), it found immediate use among English and German broadcasters for the transcription of radio programs.

Begun's career in the United States illustrates the obstacles encountered by this new medium in a sound recording industry already dominated by highly capitalized corporate interests, including the record industry, moving pictures, and live radio broadcasting. Only the special demands of the armed forces for new sound recording technology during World War II enabled Begun to bring magnetic recording to a stage of development that would make it commercially viable in the postwar years.

For the U.S. Air Force, Begun designed a recorder for cockpit conversations, including reconnaissance observations; years later, this technology was re-engineered to create the black box flight recorder found on every commercial airliner. For the Navy, Begun developed a magnetic player designed to simulate the sounds of an invading fleet. This was used in the invasion of Sicily and at Anzio to divert enemy troops away from the actual invasion site. Variable-speed magnetic recorders were also used to code military messages that were recorded at one speed and transmitted at another.

Postwar applications of these devices can be found in recorders for conversations between air traffic controllers and pilots, recorders for the phone company to play back weather reports updated every half-hour, and a "Mail-a-Voice Recorder" that recorded voice-letters on a magnetically coated piece of paper that could be sent through the mails.

A cheaper version of the 1946 "Soundmirror" was marketed in 1950, exchanging the earlier model's three drive motors for a single motor that drove a complex series of belts and clutches; a high failure rate for the single-motor system effectively ended Brush's and Begun's attempts

to dominate the magnetic recording market. Begun's book repeatedly details the troubled development of magnetic recording in terms of mechanical rather than electronic failings. Getting the tape or wire to move at the correct speed was a major obstacle to the successful innovation and diffusion of this particular invention.

By 1950 another, more successful, magnetic sound recorder had emerged, based on AEG's Magnetophone, a polyvinyl chloride tape recorder developed in Germany after Begun's departure and "liberated" by Allied troops in 1945. A refurbished version of this machine was used by John T. Mullen to record Bing Crosby's radio show in the 1947–1948 season and, shortly thereafter, became the basis for Ampex's highly successful recorder.

JOHN BELTON

Timothy Moy. *War Machines: Transforming Technologies in the U.S. Military, 1920–1940.* (Military History Series, 71.) xiv + 218 pp., illus., bibl., index. College Station: Texas A&M University Press, 2001. $39.95.

War Machines: Transforming Technologies in the U.S. Military, 1920–1940, is not as broad as its title might suggest. Timothy Moy does indeed propose a broad thesis, that institutional culture plays a large, though seldom acknowledged, role in technological innovation. But he addresses only two very particular case studies of military innovation between the world wars. The longer reviews the Army Air Force's development of the technology for precision bombing; the shorter examines the U.S. Marine Corps's development of the technology for amphibious assault. What primarily shaped these innovations were, in Moy's view, the particular cultures of the air force and marines and their internal politics rather than the more commonly cited technical or industrial imperatives.

The book is divided into two parts, each of four chapters, plus an introduction (Ch. 1) and a conclusion (Ch. 10). As we might expect of a study derived from a Berkeley history of science dissertation, *War Machines* rests on a reasonably solid research base. The two stories are told separately, however, and differ significantly in both structure and documentation. Part 1, "Precision Bombing," alternates chapters on doctrine and technology. Chapters 2 and 4 follow the air force adoption of strategic bombing as a mission to win independence from the army. Judging by the endnotes, both chapters rest chiefly on secondary sources and are less analytical than assertive. Chapters 3 and 5 trace the air force effort to find the bombsights and bombers that its self-chosen mission required. These chapters offer a more extensively documented and persuasive narrative than those on doctrine, although the two are not very well integrated. Part 2, "Amphibious Landing," in contrast, integrates doctrine, training, and technology in a single narrative. It begins with the marines adopting advanced base defense and amphibious warfare to define missions that would sustain institutional independence, then follows the interaction of doctrinal evolution, training exercises, and technological development. These chapters are based (again judging from the endnotes) much more heavily on archival research than those in Part 1.

Moy is a fine writer, but he sometimes lapses into phrasemaking at the expense of explanation. Referring to the air force as a "high-tech" organization, for instance, he claims that "the airmen's institutional self-image as futuristic and gallant knights of the sky shaped the vision of strategic bombing they articulated and embraced. Daylight bombing grew out of this high-tech culture" (p. 172). How bombing squares with a "knights of the sky" self-image he never explains. Nor does he ever fully address the specifics of one of his major categories, high-tech versus low-tech innovation. This becomes all the more troublesome when he extends the distinction by characterizing the air force as high tech, the marines as low tech. Quite apart from the anachronistic usage, this contrast tends to become more label than analysis.

However well chosen Moy's cases, two seems a slender base to sustain his thesis. One feature of his argument that I find particularly problematic is the almost complete absence of internal opposition. Neither airmen nor marines spoke with one voice about the choice of a single mission to define their service. Moy mentions the advocates of pursuit aviation only in passing, for instance, and is completely silent about any dissidence among marines. This places him in the company of a long line of historians and social scientists who have studied controversies over military innovation exclusively from the viewpoint of those perceived to be on the winning side. In his conclusion Moy does briefly discuss the alternatives each service might have chosen; but however well done, it is too little and too late.

BARTON C. HACKER

■ Recent (1950–)

Allan A. Needell. *Science, Cold War, and the American State: Lloyd V. Berkner and the Bal-*

ance of Professional Ideals. (Studies in the History of Science, Technology, and Medicine, 10.) xii + 404 pp., illus., bibl., index. Amsterdam: Harwood Academic Publishers, 2000. $60, £40 (cloth): $28, £19 (paper).

Lloyd Berkner (1905–1967), radio engineer and ionospheric physicist, was among a small circle of power brokers who helped bring American science and the American state closer together during World War II and the early years of the Cold War. In this exemplary biographical study, Allan Needell, a historian at the Smithsonian Institution's National Air and Space Museum, gives a well-documented account of Berkner's life and career and a nuanced examination of how American scientists and engineers defined and balanced the interests of their professions and of the national security state during those crucial years.

A product of the Midwest prairie, Berkner was an accomplished radio operator before entering the University of Minnesota to study electrical engineering and to train as a Naval Reserve aviator. The Bureau of Standards hired Berkner as a radio engineer in 1928, just in time for him to participate in Richard Byrd's highly publicized Antarctic expedition. The adventure helped form what Needell calls Berkner's "technocratic vision" that justified science both as a heroic exploration of the unknown and as a prime mover of technology and human progress. Berkner energetically pursued this vision of science, first at the Bureau of Standards and then at the Carnegie Institution of Washington (CIW), where he directed internationally respected research on ionospheric physics.

World War II honed Berkner's skills in instrument building and organization. On active duty he served as the naval liaison at the MIT Rad Lab to produce radars and helped install them on naval aircraft. He also came into close contact with Vannevar Bush, I. I. Rabi, Jerome Wiesner, and other influential scientists who shared his enthusiasm for collaboration with the military. A rising star at the end of World War II, Berkner worked to build a science-state partnership for the ensuing Cold War, which he saw as a continuation of the struggle between democracy and dictatorship.

Berkner helped bring scientific expertise to bear on American defense and foreign policy in the 1940s and 1950s as executive secretary of the military's Joint Research and Development Board and as a consultant to the State Department and the CIA. He drafted the famous "Berkner report" on science and American foreign policy, directed Project Troy on the overseas operations of the Voice of America, and participated in the heated debate over continental defense against a possible Soviet nuclear attack. Berkner's election to the National Academy of Sciences in 1948, despite the lack of an advanced degree, undoubtedly aided his agenda, as did his assuming in 1951 the presidency of the Associated Universities, Inc. (a consortium that ran the Brookhaven National Laboratory).

In the 1950s, Berkner helped initiate the International Geophysical Year (IGY), which led to the launching of the Soviet *Sputnik* in 1957. Did Berkner propose the satellite component of the IGY as a cover for U.S. satellite intelligence programs? Needell finds no smoking gun but presents strong circumstantial evidence that intelligence was indeed a factor in Berkner's thinking. The connection apparently posed no moral challenge for Berkner, who maintained an abiding faith in the harmonious goals of American science and state—what he called the "unity of democracy." Not everyone shared Berkner's faith, however. The geophysicist Merle Tuve, Berkner's boss at Carnegie, for example, feared government control of science and fought against Berkner both at the CIW and in national science policy. In the end, Needell sees Berkner as a kind of broker who brought American scientists together with the state through both strategies of insulation (he often hid national security considerations from them) and overt integration of interests and action.

Based on rich archival sources, *Science, Cold War, and the American State* is a superb social and political study of American science and technology rendered through Lloyd Berkner's life and times. Needell writes with clarity and achieves a nice balance between narrative and contextual analysis, which makes the book suitable for teaching courses. For scholars, the study raises fascinating questions for further research. One wonders, for example, to what extent Needell's intriguing hypothesis—that Berkner's activism derived from his nonacademic background—can be applied to other scientist-statesmen. Above all, the book's focus on the ease with which Berkner moved between the worlds of science, government, and national security points to the need to reformulate the well-known debate between the historians Paul Forman and Daniel Kevles over who was using whom in the U.S. science-state relationship during the Cold War. Neither the science nor state community was monolithic, and further comparative studies of mediators on the scene such as

Berkner should help deepen our understanding of the dynamics of that partnership.

ZUOYUE WANG

David Leverington. *New Cosmic Horizons: Space Astronomy from the V2 to the Hubble Space Telescope*. xii + 507 pp., frontis., illus., figs., tables, app., bibl., index. Cambridge/New York: Cambridge University Press, 2001. $85 (cloth); $29.95 (paper).

New Cosmic Horizons was written by a project manager, originally trained as a physicist, who worked in the European space world and in business for about twenty-five years and then returned to academia to complete his Ph.D. It is a well-written, comprehensive compilation of major scientific results in space astronomy obtained during the latter half of the twentieth century. As the book jacket explains, "it explores the triumphs of space experiments and spacecraft designs and the amazing astronomical results that they have produced." It is particularly useful because it does not just concentrate on American contributions in this area, as important as they have been, but tries to redress the transatlantic balance by including scientific work in the Soviet Union and Europe, notably the European Space Agency. The blurb claims that David Leverington relates the changes in space astronomy programs in these various countries to their "changing political imperatives." This is done very sketchily, however, and merely by way of a backdrop to his main objective.

Practicing astronomers, be they amateur or professional, and historians of science who could use a survey of the major milestones in space astronomy will find this a useful guide. It is seriously marred as a reference work, however, by the total absence of any reference to primary or secondary material in the text. There is a bibliography of what Leverington calls "general sources used in the preparation of this book" (p. 476), which includes standard histories of space and of space science. None of these books, nor any scientific papers, are cited in the body of the argument. Everything that is said, or claimed, has to be taken on the authority of the author, an extraordinary approach that can only undermine and discredit what was otherwise a laudable objective.

JOHN KRIGE

Constantin Roman. *Continental Drift: Colliding Continents, Converging Cultures*. Foreword by **John F. Dewey.** xvi + 211 pp., illus., index. Bristol/Philadelphia: Institute of Physics Publishing, 2000. $40, £26.

This is an interesting and charming book—even if not strictly an essay in the history of science. The dissident author studied earth sciences in Romania during the beastly Ceauşescu regime but managed to get out by attending a conference in Newcastle (U.K.) and never returning until after the end of Eastern European communism. Yet he remained a Romanian patriot and is presently a professor *honoris causa* in Bucharest, while residing with his family in salubrious Glyndebourne.

Constantin Roman must, by his account, surely be one of the world's most upwardly mobile earth scientists. Starting in England with only £5 in his pocket, by ability, persistence, and charm, and using Newcastle as a stepping-stone, he became acquainted with the right people (especially the late Keith Runcorn) and obtained a scholarship to Peterhouse, Cambridge, to do a Ph.D. on the tectonics of the Caucasus and across into Central Asia, using seismic data to identify plate boundaries and movements. On this basis, and studying areas of compression and tension, he proposed the existence of two nonrigid "buffer plates"—Sinkiang and Tibet—between the Indian and Eurasian plates. This was an iconoclastic suggestion in the early 1970s. Later, after getting his doctorate under Edward Bullard, Roman became an oil industry consultant and, I infer, made good money.

Primarily, the book is about the madnesses of dictatorships and bureaucracies—and also the lovely life of a research student at Cambridge. When it came to Kafkaesque bureaucracy, the British authorities could be quite as obdurate as their Romanian counterparts: you can't have a work permit (and then residence) unless you have a job; you can't have a job unless you have a work permit. The difference, though, was that Roman could enlist support via his influential Cambridge contacts, and eventually he broke the logjam by getting an acquaintance at the *Telegraph* to offer him a kind of pseudo-job (as a research assistant, on matters Romanian, to his contact there). He was tenacious, resourceful, and bright, and seemingly charming to boot. It worked!

Roman displayed similar qualities as a researcher. When he was well into his Ph.D. work, Bullard drew his attention to a paper emanating from Peter Molnar and his group at MIT that dealt with the same topic and arrived independently at essentially the same theory. The American paper had been refereed and accepted and

was shortly to be published. Bullard warned his student that if this happened before Roman submitted his thesis he could only expect to get an M.A. So with bounce and initiative Roman dashed up to London and persuaded *New Scientist* to publish the main arguments of his thesis before the MIT paper got into print. This is presented as a coup, and so it was. Roman's Ph.D. was saved. But while that was all very well for the Cambridge "chaps," one may wonder what the Americans thought about the matter. We're not told.

We're not told a few other things either, particularly what happened between Roman and his first supervisor, Dan McKenzie (who also was a referee for the Molnar article, which was why Bullard knew about it). We are, however, told much about the delightful "lotus eaters" at Cambridge and the life there that is open to all—providing they have the right energy, brains, and charm. Roman got where he did by his ample possession of these qualities.

But I wonder about Cambridge. It is the privileged (but accessible) tip of a huge social and economic pyramid, supported by a massive base of taxes, endowments, and, ultimately, the exploitation of third-world lands and peoples and, formerly (and to some extent even now), of British workers. Roman knew that his home country was a mad dictatorship. He got out, and into what was then undoubtedly a better place. But what of those nameless ones today who suffocate in containers in their desperate struggle to get into Britain, or the refugees who are now incarcerated in alien detention centers in the Australian deserts? The West welcomes some, but not all. *Continental Drift* says nothing about such matters, but much about winning supporters through contacts, energy, and persistence.

DAVID OLDROYD

Seth R. Reice. *The Silver Lining: The Benefits of Natural Disasters*. 213 pp., illus., figs., tables, index. Princeton, N.J.: Princeton University Press, 2001. (Cloth.)

Disturbance, claims Seth Reice, is "paradoxical" (p. ix). What kills also rejuvenates. Disturbances provide the creative destruction in nature's economy; the silver lining is enhanced biodiversity. What matters is the right mix of disturbances and the extent to which ecosystems have adapted to them. The ideal is what Joseph Connell developed into the "Intermediate Disturbance Hypothesis" (p. 35) in 1978: enough disturbance to keep the pot bubbling, not so much that it boils over. What the popular media often characterize as "devastation" resulting from eruptive natural events is actually valuable and even necessary.

The Silver Lining seeks to bring these "fresh insights of disturbance ecology to a broader, nontechnical audience," with the added goal of showing "the importance of living with uncontrolled natural systems and process" (pp. ix–x). The core of this brief text is a pithy tutorial on disturbance ecology, dynamic equilibrium in ecosystems, and their contribution to biodiversity, as variously measured. The presentation is concise and lucid; the intended reader is indeed someone with almost no knowledge of the field. While Reice exploits a range of examples, he collapses his illustrations primarily into fires and floods. The text opens with Black Saturday, the blowup that climaxed the Yellowstone conflagrations of 1988.

But short veers into simplistic. Problems seep in when the author goes beyond the basics of what is no longer a new theory, one that has in truth become a bit shopworn. At least with respect to fire, he is consistently incorrect about policy, history, and the ecology of humans as fire agents. The federal policy of total fire suppression did not arrive in 1940 (it came much earlier and hardened after the 1910 fire season). Bambi's mother did not die in a fire (a hunter killed her). It's the "U.S." or "USDA" Forest Service, not the "National" Forest Service. The current policy is not one of "Let It Burn" but of "appropriate response" and "fire by prescription." The 1988 Yellowstone fires, while startling the media and the urban public, served to educate both about the character of wilderness fire and have created an audience perhaps more sophisticated than Seth Reice supposes.

The deeper trouble with the book is that its paradox of disturbance fails to address people. The author's heartfelt but naïve solution to restoring damaged ecosystems is to "leave them alone" (p. 203). Stop harming them, then get out of the way and permit them to repair themselves; in particular, let nature ramble and roar over those preserves as it will. There is almost no middle ground, no Intermediate Human-Nature Disturbance Hypothesis that allows for human disturbance as a legitimate ecological agency. The author declares flatly, "I know of no case in which human-induced disturbance of the environment has enhanced biodiversity or ecosystem services" (p. 179).

I know of scores. Certainly with respect to fire, an immense catalyst, there is ample evidence that anthropogenic burning has profoundly shaped the character—and biodiversity—of many landscapes. Ethnobotanists in

eastern Amazonia, for example, have estimated that perhaps a third of the total species present are the result of traditional swidden cultivation. Remove people, and the region's biodiversity declines. Instead, in *The Silver Lining,* people exist on the extremes: they either nuke landscapes or abandon them to nature parks.

What is notably absent, too, is the silver lining that bonds nature's economy with society's. But the link between biodiversity and bullion here goes largely unexplored. One exception is an account of the Conservation Reserve Enhancement Program that allows North Carolina to lease stream buffers from farmers along four sensitive watersheds. It is, Reice proclaims, a "win-win situation: the farmer gets paid and the ecosystem is protected from pollution" (p. 174). The silver lining that makes this possible, however, gets obscured in the stormclouds of environmentalist exhortations.

STEPHEN J. PYNE

Richard A. Rajala. *Clearcutting the Pacific Rain Forest: Production, Science, and Regulation*. xxiv + 286 pp., illus., bibl., index. Vancouver: University of British Columbia Press, 1998. $75.

Richard Rajala's *Clearcutting the Pacific Rain Forest* is a thoughtful examination of how North Americans treat their forests and those who work them. He ably demonstrates how both were increasingly exploited from 1880 to 1965 in the Douglas fir habitat of British Columbia and the states of Washington and Oregon.

The book is divided into two parts that examine separate aspects of the exploitation of the forests and forest laborers. The first part—"Machines, Managers, and Work"—chronicles the technological changes that transformed this heavily forested area into the equivalent of a factory floor. Rajala chronicles the myriad new technologies seized on by lumber companies as hand tools and animal power gave way to steam donkeys, aerial skidders, chain saws, Caterpillar tractors, aerial photography, grapple yarders, and IBM punch-card computers. For the reader who is less than proficient in silvicultural technology, informative illustrations and photos accompany Rajala's lucid descriptions. All of these technological changes, Rajala claims, were an attempt by larger lumber companies to implement a factory regime on the previously chaotic forest floor. For Rajala the efficiency expert Frederick W. Taylor is at least as important as the pioneering conservationist Gifford Pinchot for understanding North American forest history. Rajala is clearly more interested in economic determinism than environmental determinism.

The second, more substantive, part of the book—"Clearcutting, Conservation, and the State"—examines the social, scientific, and political context that allowed the clearcutting of the Pacific Northwest Douglas fir forests. In his tripartite historical division, Rajala chronicles the emergence of clearcutting as the dominant lumbering method between 1880 and 1930. The forest "researcher" (one uses the term advisedly) J. V. Hoffman did immeasurable harm in this era with his 1913 "seed storage" theory of Douglas fir recovery. Hoffman's claim that clearcuts and a quick slash and burn would lead to a natural seed regeneration of Douglas fir was eagerly seized on by lumber companies and their forester allies. These self-serving justifications led to an expansion of clearcutting until the combined economic and environmental disasters of the 1930s derailed the trajectory of exploitation. In the 1930s clearcutting gave way to selective logging and nascent efforts to regulate lumber extraction. Regulatory attempts eventually came to naught owing to determined opposition and, eventually, wartime exigencies. Rajala's description of regulatory failure at the federal level (in Oregon and Washington) in the U.S. and the provincial level (in British Columbia) in Canada is the one section of the book in which the comparison between the two countries offers some truly novel insight. From 1940 to 1965 the grim march toward sylvan destruction reemerges. Patch logging (clearcuts interspersed within intact forest), at best a partial solution, eventually gave way by the mid 1950s to broadscale clearcuts quickly replanted with economically valuable species. These forest plantations represented the epitome of efficient lumber extraction and the nadir of habitat for hikers, spotted owls, and others dependent on biodiversity.

Of particular interest to historians of science is the manner in which the forestry sciences were directed by corporate needs. In Rajala's overview, good forestry science consistently fails to drive out bad science. The forest researcher Leo Isaac (the antithesis of Hoffman) discredited "seed storage" and by 1943 had developed a workable theory of natural reseeding of certain cut areas. Yet his work was routinely ignored by regulators, foresters, and, of course, large lumber corporations—who, one is led to believe, would advocate phlogiston theory if it increased their profits. Rajala uses Harry Braverman's theory of "deskilling" and the neo-Marxist school of labor process analysis to bear hard on the actual work of lumbering and to argue strongly that the ex-

ploitation of workers and of trees is connected. This thesis, although not a perfect mesh, does strongly suggest that contemporary logging problems may have much longer roots than is currently expressed in the rather simplistic formulations of "jobs vs. owls." Rajala's application of labor process theory to the forest floor is illuminating in the same manner as the suggestions of an earlier generation of historians that the exploitation of women and of natural resources might have a common ideology. As such, Rajala has written a keystone book for anyone interested in the context of natural and human exploitation.

MARK MADISON

John Alcock. *The Triumph of Sociobiology*. x + 257 pp., illus., figs., tables, app., bibls., index. Oxford/New York: Oxford University Press, 2001. $27.50.

This book is a manifesto for what John Alcock calls "orthodox sociobiology," the systematic study of the biological basis of all social behavior following the premise that behaviors and their mechanisms evolve under the primary influence of natural selection acting on individual differences in genetic success. Sociobiology focuses narrowly on finding adaptive explanations for social behaviors while attempting a grand synthesis of biological and social sciences. Alcock's book is largely defensive, aimed at refuting criticisms and a perception that, twenty-five years after its proclamation, sociobiology is moribund. Alcock's defense relies not on an extensive review of relevant literature but on the persuasiveness of arguments illustrated with favored examples. He emphasizes sociobiology of human cultures and prescribes practical applications for restructuring societal institutions.

The major argument against orthodox sociobiology is summarized in Edward O. Wilson's book *Consilience* (Knopf, 1998): "We know that virtually all of human behavior is transmitted by culture. We also know that biology has an important effect on the origin of culture and its transmission. . . . Culture allows a rapid adjustment to changes in the environment through finely tuned adaptations invented and transmitted without correspondingly precise genetic prescription. In this respect human beings differ fundamentally from all other animal species" (pp. 126–128). This argument denies that human social behaviors are best understood as biologically evolved adaptations and places them outside the domain of the genetical theory of natural selection.

Alcock's major claim for sociobiology's triumph is its use of comparative methodology to identify ultimate causes of social behaviors. Over the past twenty-five years, evolutionary biologists have developed rigorous systematic methodologies for making comparative inferences in evolutionary research. Such explicitly phylogenetic approaches provide more powerful tests of ultimate causality in evolution than the comparative methods available when sociobiology was first proposed. Although the success of sociobiology would seem to lie in its ability to use the strongest comparative methods to test its major claims, Alcock's examples rely entirely on archaic methodology that no contemporary evolutionist should accept.

The hypothesis of adaptation predicts that a character enhances its possessor's ability to utilize resources for survival or reproduction relative to alternatives against which it has been tested evolutionarily. Phylogenetic analysis of species possessing the character and their closest relatives identifies the comparisons appropriate for testing this hypothesis. Hypothetical adaptations are mapped onto phylogenies to identify their origins, antecedent conditions, associated characters, environmental contexts, and selective regimes. Alcock forfeits the power of evolutionary study by arguing that sociobiologists "can only use data from unrelated species known to experience similar selection pressures" (p. 73) in the "independent convergence test" of adaptation. No animal species are completely unrelated, nor are their behaviors adaptive in an absolute sense. Even comparisons of analogous characters across species must consider phylogeny because homologous genes and developmental programs can be utilized for analogous roles in different species. Without a firm grounding in phylogenetic principles, Alcock's methods cannot effectively test hypotheses of evolutionary causality.

Alcock spends much effort defending sociobiology against the criticism that its major claims constitute untestable adaptationist stories. David Barash's sociobiological interpretations of mating behavior in bluebirds receive particular attention because Stephen Jay Gould criticized them as untestable. Alcock shows that Barash and others have empirically rejected some explanations in favor of better ones, although the hypotheses tested and the knowledge gained reside entirely in proximate mechanisms, not the ultimate causes that constitute sociobiology's main challenge. Gould's criticism therefore withstands Alcock's counterarguments.

Readers unfamiliar with evolution should con-

sult recent books such as Brian Hall's *Evolutionary Developmental Biology* (Chapman & Hall, 1998), Jeffrey Levinton's *Genetics, Paleontology, and Macroevolution* (Cambridge, 2001), and John Avise's *Phylogeography* (Harvard, 2000) to see evolutionary disciplines that have triumphed over the past twenty-five years. These disciplines contrast with sociobiology in their methodological rigor and profound reformulation of major theories. By contrast, Alcock's sociobiology seems essentially unchanged from the 1970s. If twenty-five years of sociobiological study have produced only a repetition of its initial conjectures, their effectiveness as testable hypotheses seems doubtful.

Alcock acknowledges "human evolutionary psychology" as a new subdiscipline of sociobiology. Indeed, orthodox sociobiology seems to be relocating from biology to psychology departments. My interpretation of this move is that sociobiology has not kept pace with evolutionary biology and therefore has sought refuge in a traditionally nonevolutionary discipline that has not yet incorporated the current critical standards of comparative evolutionary research.

Sociobiology has served evolutionary biology mainly by revealing the limits of reductionist selective arguments, thereby directing us to move beyond them.

ALLAN LARSON

Leslie R. Alm. *Crossing Borders, Crossing Boundaries: The Role of Scientists in the U.S. Acid Rain Debate*. x + 147 pp., tables, bibl., index. Westport, Conn./London: Praeger Publishers, 2000. $50.

In the debate over acid rain, American and Canadian scientists learned hard lessons about the making of U.S. environmental policy. Leslie Alm's brief study of the controversy is a postmortem on how researchers viewed the science and policy relationship that unfolded—or suffered, as it were—during the period 1979–1990. President Carter officially recognized acid rain as a serious environmental problem for the United States in 1979. By 1990, policymakers had reached a compromise of sorts with the Clean Air Act Amendments. In between were scientific assessments, congressional hearings, industry rebuttals, and the changing interests of three successive administrations. Canadian politicians and scientists played a quasi–special interest group role in the U.S. political process. The Canadian Coalition on Acid Rain (CCAR) positioned itself as an "information broker," providing policymakers with data from studies by U.S. researchers but ignoring Canadian studies so as to downplay criticisms of flawed data and their government's domestic policy failings on the issue.

For his study, Alm interviewed 129 Canadian and U.S. acid rain researchers and asked them questions about the relations between science and policymakers, advocacy on the part of scientists, and the possibility of objectivity in research. The interview responses, along with his analysis of exchanges between politicians and scientists at congressional hearings, led Alm to support the "two worlds" argument. Scientists and policymakers hold competing perspectives on how science should inform a policy debate. Scientists focus on the uncertainty of science and balk at making statements that imply policy actions, while policymakers seek out advice that offers grounds for action. Of the scientists interviewed, 75 percent believe that scientists have only a weak to moderate influence on policymakers. Despite this, 73 percent agree that scientists should advocate policy positions. On the question of objectivity, the responses become more provocative: 67 percent of natural scientists agree that scientists can be objective, while only 43 percent of social scientists believe it possible. These differences appear to hold despite nationality. Drawing on these data, Alm recommends that social scientists act as mediators for natural scientists, making it "possible that the science could move forward with more clarity and natural scientists could preserve their relative independence and objectivity from further erosion" (p. 126). The difficulty that natural scientists have in accepting value constraints on their strong views of objectivity would be balanced by the ability of social scientists to frame scientific issues in policy contexts.

Alm's assessment of natural scientists as more committed to objectivity and social scientists as less so does not necessarily lead to an assertion that social science is a more palatable form of information for policymakers to digest than natural science. Social science statistics on crime or drug abuse, for example, can be just as contentious. Alm's vision of a perfect discourse community of natural and social scientists leaves us with a perplexing view of scientific discourse. For science to be effective, natural and social scientists must not only achieve consensus on the data but also come to "some type of consensus on the acceptable boundaries of advocacy and the true nature of objectivity" in order to communicate better with policy makers (p. 128). There have been moments in the history of acid rain policy when small groups of scientists

achieved something close to consensus in preparing reports, only to have policymakers rewrite or tone down their findings. This kind of power dynamic confounds Alm's prescription. Some readers might long for further descriptive analysis of the multiple views of objectivity and advocacy held by individuals and groups of scientists, such as CCAR, as they waged acid rain battles. As it stands, this little book raises a host of issues for those interested in the science and policy relationship.

AMY C. CRUMPTON

D. Wade Hands. *Reflection without Rules: Economic Methodology and Contemporary Science Theory*. xii + 480 pp., figs., bibls., index. Cambridge/New York: Cambridge University Press, 2001. $95 (cloth); $34.95 (paper).

This fine book is a comprehensive and careful survey of the current situation in the methodology of economics. It is directed primarily at economists and students of economics. Indeed, the economist who reads it with the care it deserves will have a better grip on matters of methodology in economics than most philosophers of science, but philosophers and historians of science will also find the work rewarding and interesting. Though a few examples may be beyond the economically untutored reader, they are not essential to the exposition, and other examples are accessible and enlightening: the brief discussions of Paul Samuelson's revealed preference approach and Julie Nelson's criticism of Gary Becker's work on economics of the family are good instances. Chapter 2, "The Methodological Tradition in Economics," is a calm and brief introduction to its subject.

Until quite recently practicing scientists, economists among them, and philosophers of science presupposed several strong theses about science and knowledge: that all genuine knowledge is scientific; that the objects of this knowledge are eternal and free of context; that all knowledge and all science is one, unified at least by a common scientific method; that, though not infallible, this method is progressive—it approaches truth as a limit—and operates in independence of the nonscientific beliefs and values of its practitioners. These principles (they are essential components of what Philip Kitcher aptly baptized "The Legend") cannot be presupposed today. Those who would accept any of them must be ready to meet serious criticisms. The import of this contemporary *Methodenstreit* as it affects economics is the dominant theme of *Reflection without Rules.* The reader is left with the conviction that the science of economics and the study of its methodology are inseparable and that it was only dedicated and myopic attachment to The Legend that kept this messy business at bay. Nor can there be any return to the age of innocent division of labor—economists to economics, philosophers to methodology.

There are too many topics treated in the book even to list in a brief review. There are (among many others) calm and clear accounts of Thomas Kuhn's work; of logical positivism (to be distinguished from its successor, logical empiricism); of the deep effects of the collapse of The Legend on Walrasian equilibrium models; of Daniel Hausman's work on the nature of economics; of the strong program, which strives to reduce scientific activity to the effects of the social interests of scientists; of Alvin Goldman's reliabilism and social epistemology (in which economic methods are applied in a quite general epistemological setting); and of the important differences between pre- and post-Legend views of science and knowledge.

The question of values in science arises for economics in a special and pointed way, for economics studies many sorts of values. It also, at least since Adam Smith, deals in the unintended collective consequences of individual intentional actions. Economics evidently consists in the largely unintended consequences of the intentional actions of individual economists, and, once The Legend is discredited, economics is seen to navigate the very currents that it studies. This opens the door to the virtuous possibility of self-criticism in tune with the strong naturalism of contemporary epistemology.

It is not the least virtue of *Reflection without Rules* that its author wears his impressive erudition gracefully and well. The book is sophisticated and at the same time naive in the best sense. It will fortunately be the standard reference for work in the methodology of economics for some time to come.

JOHN M. VICKERS

Jessica Snyder Sachs. *Corpse: Nature, Forensics, and the Struggle to Pinpoint Time of Death: An Exploration of the Haunting Science of Forensic Ecology*. 288 pp., index. Cambridge, Mass.: Perseus Publishing, 2001. $25 (cloth).

Early in Jonathan Demme's 1991 film *The Silence of the Lambs,* the novice FBI agent Clarice Starling brings a cocoon pulled from a corpse's throat to the Museum of Natural History for identification. There she finds two entomologists playing chess with live beetles in the dim light

of their lab. Interrupting their game to slice open the cocoon, they identify it as that of *Acherontia styx,* the death's-head moth, a species that lives only in Asia. The specimen would have been hand-raised from imported eggs, they note with the special awe of connoisseurs. "Somebody grew this guy, fed him honey and nightshade. Kept him warm. Somebody loved him."

The scene with the chess-playing bug experts is easy to read as so much Demme grotesquerie. But the cocoon proves to be crucial evidence, a symbol of the killer's desire for metamorphosis that eventually leads Agent Starling to him. The moths fluttering about Buffalo Bill's home are what reveals him to Clarice; in a very real sense, the mystery in Demme's film is solved not by the FBI but by a couple of insect lovers who know a great bug when they see one. As such, Demme's film gives us a rare cameo of what Jessica Snyder Sachs calls "forensic ecology," the budding field that promises to unlock some of death's deepest secrets.

Sachs's *Corpse* is the first book-length study of the role anthropologists, entomologists, and botanists have begun playing in murder investigations. A thick but readable stew of lurid crime history and sophisticated science, *Corpse* describes how these specialists have become the new "Mod Squad" of forensics by reading crime scenes in ways the putative "experts" cannot. Anthropologists can identify victims from bones; entomologists can use the life cycles of the eggs, maggots, and pupae on a corpse to estimate time of death; and botanists can do the same by examining the flora under and around a body. Together, they have quietly revolutionized forensics.

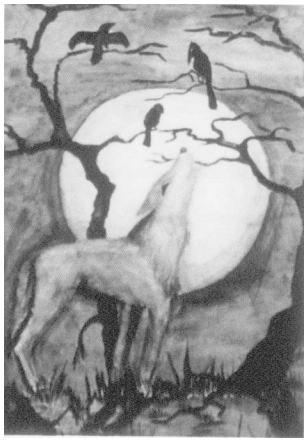
With unflappable poise and no little poetry, *Corpse* tells the riveting if gruesome story of how forensic ecology has taught us to use parasites and plants—those parts of a crime scene that used to be trampled down or brushed away—to calculate that all-important fact: time of death. According to Sachs, homicide investigators have historically had an especially hard time pinpointing time of death. Modern chemistry and microbiology have made cause of death fairly simple to determine, but time of death has remained elusive. The traditional indexes of rigor mortis, algor mortis, and livor mortis are notoriously imprecise, as are more recent additions to this catalogue such as stomach contents and the potassium level of the eye's vitreous humor. Body fat, age, size, health, ambient temperature, and manner of death can all affect these indicators. Moreover, once a body has been dead more than a day or two—once decomposition has truly begun—all bets are off. Until now, that is. Forensic ecology, Sachs shows, is doing what pathologists could not. Able to calculate time of death weeks, months, and sometimes even years after decease, forensic ecologists are gradually earning the respect and even admiration of the police, lawyers, and academics who once scoffed at their butterfly nets and eccentric ways.

Sachs writes about difficult material with near-perfect pitch. Whether narrating a murder case, describing Renaissance theories of spontaneous generation, explaining the life cycle of maggots, or detailing the process of human decomposition, her prose is crisp and engaging, her point always clear. The result: a book about some of the most unspeakable aspects of death that delights rather than disgusts—not because it dwells morbidly on death, but because it finds in forensic ecology an inspiring and fascinating story of scientists who have been able to see past the gruesome circumstances of their work to a future where murder is easier to solve.

ERIN O'CONNOR

Richard F. Mould. *Chernobyl Record: The Definitive History of the Chernobyl Catastrophe*. xviii + 402 pp., illus., figs., tables, index. Bristol, England: Institute of Physics Publishing, 2000. $57, £35.

The nuclear power industry will long remember 26 April 1986. The catastrophic events of this day would destroy whatever belief the public



Art inspired by the Chernobyl disaster of 1986 (reprinted from Richard F. Mould, Chernobyl Record: The Definitive History of the Chernobyl Catastrophe, *plate 6).*

might once have had in the safety of nuclear reactors, a belief that had already been weakened by the Three Mile Island accident some seven years earlier. The site of this nuclear debacle lies 146 kilometers north-northwest of Kiev, the capital of the Ukraine, near the Belarus-Ukraine border. Four reactors made up the Chernobyl Nuclear Power Plant; the first unit had been built in 1970 and the fourth was completed late in 1983. Workers engaged in an unsanctioned and poorly conceived experiment lost control of the reactor in unit 4. The ensuing accident not only destroyed the reactor but spread radioactive contamination globally because the graphite-moderated reactor had been built without a special containment structure, which had been deemed unnecessary. Initially Soviet authorities denied that an accident had occurred, but once the plume of contamination crossed national boundaries, containing its political ramifications was no longer possible. The days and weeks that followed revealed how fragmented, uncoordinated, and often ill conceived was the international reaction to a nuclear accident of this magnitude. Some nations immediately instituted countermeasures, such as the distribution of stable iodine tablets; some did not. Some nations began to confiscate and then destroyed potentially contaminated foodstuffs; others did not. The Soviet Union initiated heroic steps to move over a quarter of a million individuals thought to be in harm's way to safer areas and to mobilize over three quarters of a million workers, largely soldiers, to contain the damage. Both efforts were seriously compromised by inadequate preparedness and indecision.

Chernobyl Record sets out in admirable detail these actions and, to a lesser extent, the steps taken by other national and international agencies to minimize the accident's effects. It is an important contribution to the documentation of the Chernobyl fiasco and should be an integral part of any library interested in the uses of nuclear energy and the hazards stemming from such uses. The author, Richard Mould, is an English health physicist who has been involved in studies of the Chernobyl accident since shortly after it happened, either as an advisor, a consultant, or a participant. He writes clearly and with authority and sensitivity. The book is generously endowed with well-chosen figures, photographs, and tables and organized so that it can be read with understanding by the nonspecialist. The opening chapter, for example, offers a brief but helpful discussion of the units of measurement of ionizing radiation and a succinct summary of the health effects of exposure, particularly the immediate or acute ones. The succeeding chapters deal with the design of nuclear reactors; the explosion and subsequent release of radionuclides; the measurement of ground, water, and food contamination; and estimates of the size of the exposed population and the collective effective doses in the three republics of primary concern, Belarus, Russia, and the Ukraine. The book's penultimate chapter, "The Legasov Testimony," is particularly interesting since it recounts the experiences of Valery Legasov as a member of the government commission sent to appraise the seriousness of the accident and to institute those steps necessary to limit its physical and biological effects. Legasov's account is exceptionally candid, detailing the lack of preparation for an event of this magnitude at both the local and the ministerial levels, the failure of the Soviet scientific community to develop good diagnostic and control systems for their reactors, and the virtual indifference of highly placed nuclear engineers to issues of safety. Legasov would commit suicide two years after the accident, no less a victim of the tragedy than those twenty-eight nuclear workers and firemen who succumbed within three months of the explosion.

Arguably the weakest sections of the book are those chapters dealing with the health effects of the accident. This is not unexpected, since the totality of those effects is still poorly known and may remain so for some years to come. Much that has previously been written about radiation-related health effects as they pertain to Chernobyl has been speculative, frequently based on data of dubious generality, and often stated from a self-serving perspective. As yet, with the exception of the few hundred individuals who experienced some of the symptoms of acute radiation sickness and the widespread psychological trauma, the only unequivocal effect involves the increase in thyroid malignancies seen in all three affected republics, especially among the young. This reservation aside, Mould has produced an admirable treatise, a lasting contribution to the understanding of the causes and consequences of what one hopes was the last major nuclear accident.

WILLIAM J. SCHULL

Michael Friedewald. *Der Computer als Werkzeug und Medium: Die geistigen und technischen Wurzeln des Personalcomputers*. (Aachener Beiträge zur Wissenschafts- und Technikgeschichte, 3.) 497 pp., illus., figs., tables, bibl., index. Berlin: Verlag für Geschichte

der Naturwissenschaften und der Technik, 1999. €38.50.

In the age of the Internet the computer has become an easy-to-use tool and medium for anybody who wants to do word processing, use spreadsheets, communicate, get information from the net, or simply play. Common use of files and cooperative use of connected machines make the computer and the net a digital medium. In this published version of a doctoral thesis written at the University of Technology in Aix-la-Chapelle, Michael Friedewald describes the intellectual and technical roots of the personal computer (PC).

This book is based on numerous archival sources from the United States, especially from the Stanford University Library, as well as on extensive study of published sources. It considers not only the technology but also the ideas, models, concepts, and innovations that preceded the use of the PC as a medium. These had been developed long before the true microcomputer came onto the market.

That development can be traced back to Vannevar Bush's fictitious Memex, the first conception of a personal machine that would be a tool for supporting scholarly tasks with an intuitive man–machine interface and associative data organization. Even then, in 1945, Bush stressed the importance of the information problem and proposed its solution through a machine. In the 1950s real-time computers (Whirlwind) were built at MIT, as were complex man–machine systems for air defense (SAGE project). Both projects were among the first that aimed not at further development of computer technology but at realizing applications with the help of machines.

Cybernetics and artificial intelligence laid the foundation for Joseph Licklider's vision of a computer tool for intellectual work. Licklider used the time-sharing concept to achieve the interactive functioning of computer work. In the 1960s, as well, Douglas C. Engelbart at the Stanford Research Institute saw the "individual as a user, generator, and retriever of information" and the computer as a means for communication between people. The man–computer interface became the focus of attention: better tools for data input and output (e.g., the mouse) were developed, and working in front of the display became common practice. Early in the 1970s Engelbart developed an "Online-Text System" for word processing and document organization that included a so-called Journal, a permanent, centrally stored archive of documents accessible to everyone through the just-established Arpanet.

The efforts of the Xerox Palo Alto Research Center (PARC) and its staff—among them Robert W. Taylor and Alan C. Kay—brought new ideas (e.g., instead of time-sharing, every user got his own computer) and developments in hardware (e.g., graphical displays), netware (e.g., the client–server concept), and software (the "What you see is what you get" concept or an object-orientated programming language). The true microcomputer arose in the 1970s—for example, in Apple Computers, founded by Steve Jobs and Steve Wozniak.

Friedewald shows that the universal interactive machine of today is the product of more than forty years of interdisciplinary research in computer technology, software development, cognitive psychology, cybernetics, and information science. This detailed and well-illustrated book complements other related works by exploring the subject from a specific perspective: the computer as a tool for intellectual work. The full history of the latter will also encompass efforts toward the mechanical organization of information (especially scholarly information) and communication pertaining to libraries and documentation.

THOMAS HAPKE

Agatha C. Hughes; Thomas P. Hughes (Editors). *Systems, Experts, and Computers: The Systems Approach in Management and Engineering, World War II and After*. (Dibner Institute Studies in the History of Science and Technology.) 513 pp., illus., tables, index. Cambridge, Mass./London: MIT Press, 2000. $50.

Systems, Experts, and Computers presents the papers from a 1996 conference, sponsored by the Dibner Institute, that considered the economic and social impact of systems engineering, the techniques that combine mathematics, economics, computers, and management. Underlying most of the contributions is the thesis that systems engineering has had an impact as fundamental and pervasive as the more familiar technological developments of World War II: radar, the atomic bomb, and electronic computers. This book is a substantial contribution to the understanding of systems engineering and manages to exhibit most of the strengths of an edited conference volume while embracing few of its weaknesses.

The papers give a broad, cross-sectional look at the interaction of systems engineering and society. They not only treat the central develop-

ment of the discipline through the patronage of the United States Air Force but also consider ideas that might not be immediately connected to a traditional view of the topic. The book contains chapters devoted to genetic codes, Swedish contributions to systems engineering, and the implications of technological management on French politics. Some of the papers seem to be more concerned with the development of technology than with the impact of systems engineering, but their presence serves to emphasize the breadth of topics that fall under the rubric of systems engineering.

The volume treats a number of issues that have been discussed in narrower fields and brings them together in a way that allows the reader to compare and contrast their evolution. Chapters discuss the development of radar-based fire control systems under the OSRD, PERT, and other planning procedures, the founding of the RAND Corporation, concern over software reliability, the emergence of electronic data processing in the federal government, the growth of mathematical weather modeling, and the flourishing of global social projections such as those made by the Club of Rome. The authors make no attempt to explain the technical nature of these methods, which may prove frustrating to those with no training in systems engineering or operations research, but the multitude of topics serves to emphasize common themes of power, authority, and decision making.

Though almost all of the essays refer to developments during World War II, most devote their attention to the Cold War era. There are few references to the pre-1940 antecedents of systems engineering. The authors give passing mention to the contributions of Frederick Winslow Taylor, the Cowles Commission, Henry Ford, and the Applied Mathematics Panel but are silent about early pioneers such as Karl Pearson, Oswald Veblen, Mordecai Ezekiel, and Lewis Fry Richardson. Their absence underscores the fact that this volume is concerned with the border that separates technological management from political decision making.

For the most part, the book avoids the disjointed feel of conference volumes. Part of this is due to the thorough introductory essay by the editors. A few of the papers make references to ideas that were apparently discussed at the meeting but not included in the book. Many of the papers could have benefited from cross-references to similar ideas in other chapters. But these are minor quibbles. The book will serve as a foundation for the exploration of the systems approach to management.

DAVID ALAN GRIER

Toby Smith. *Little Gray Men: Roswell and the Rise of a Popular Culture*. xii + 199 pp., bibl., index. Albuquerque: University of New Mexico Press, 2000. $24.95.

Without question, UFOs are part of popular culture; indeed, one might even talk of them as *a* popular culture. Without question, Roswell is part of the UFO scene; but it is far from the whole thing, nor is it even the central issue. Still less did the Roswell "culture" spawn humankind's preoccupation with possible alien visitors from outer space or the literary genre of science fiction. Yet if this book is to be believed, Roswell has been the center from which these and yet more matters arose: "through the early years, the single event of Roswell, even without a crumb of proof, augmented every image, word, and sound produced by the mass media" (p. 7). To call this an enormous overstatement would be a large understatement.

Little Gray Men is unfortunately full of such sweeping assertions, so broad as to be implausible on their face and yet unsupported by argument or evidence. Toby Smith even defines popular culture as "communication that may be informative but that principally provides pleasure for the participants" (pp. 6–7), a description that many scholars of culture or communication might find arguable. Some assertions are not only sweeping but puzzling as well—for example, reference to "the Pavlovian nature of popular culture" (p. 8) or a "rhomboidal, zinc-tinted" television set (p. 9). Many more examples can be found without venturing beyond the book's introduction.

Chapter 1 asserts that Roswell is "a touchstone for our times." Chapter 2 is about the rocket pioneer Robert Goddard, who does have a connection to Roswell "the Place" (as the author has it) but a most tenuous connection to Roswell "the Incident," which is supposedly the theme of the book. The UFO Encounter gathering at Roswell in 1997 is described in Chapter 3, and the allegation that Roswellian aliens were transported to Wright-Patterson Air Force Base constitutes Chapter 4. Books about the Incident are mentioned in Chapter 5, films in Chapter 7, science fiction on television in Chapter 8; in between, Chapter 6 is about the science fiction writer Jack Williamson, whose connection to the Incident is less than noteworthy.

The book has no formal bibliography or specific notes, though there is a short chapter entitled "Sources." Thus, when one reads on page 2 "A book came out, in 1980," there is no quick or easy way to find out what that book is. So

Little Gray Men is not a scholarly book. It is in fact a grab bag of trivia connected, not substantively, but only semantically, through the name "Roswell."

As a piece of journalism, one might describe the book as impressionistic, were it not so misleading. No one could gather from *Little Gray Men* that the popular culture of ufology has a serious side to it, that competent scientists and social scientists have concerned themselves with issues arising from claims about UFOs in general and, in some small part, from claims about the Roswell incident. It is as though one chose to write about the popular culture of cancer by gathering trivia about cranks, quacks, and survivors while saying nothing about mainstream treatments or the underlying scientific knowledge.

Ufology has a vast literature, some of it the give-and-take of quite disciplined controversy among competent and informed people. That this book has been entirely ignored by ufologists illustrates that it is, in Wolfgang Pauli's phrase, "not even wrong."

HENRY H. BAUER

Karl T. Pflock. *Roswell: Inconvenient Facts and the Will to Believe*. Foreword by **Jerry Pournelle.** 331 pp., illus., figs., apps., index. Amherst, N.Y.: Prometheus Books, 2001. $25.

Karl Pflock has done a thorough job of deflating the widely held myth that an alien spacecraft (or perhaps more than one!), with aliens aboard, crashed in New Mexico in early July 1947. To bolster his case he spent a great deal of time and effort in tracking down "eyewitnesses," unearthing obscure documents, and untangling the tangled story from its beginning up to the present. Primary sources are used wherever possible, and the relevant affidavits, formerly classified reports, and so forth are not only cited fully but in many cases reproduced in the text or in one of the appendixes.

Pflock often goes into very fine and intricate detail indeed, but as a result he makes a convincing case. For example, it is fascinating to "follow the trail back" and watch the purported hundreds of eyewitnesses dwindle to a handful, none of whom produced any hard evidence of extraterrestrials. Equally engrossing are matters such as how the myth spread and grew ever more complex, the role of the media in the whole affair, and the enshrinement of "Roswell" as an accepted fact by many.

The Roswell incident took place just a couple of weeks after the famous first sighting of UFOs by Kenneth Arnold near Mount Rainier on 24 June 1947. The media played the latter story for all it was worth, and for months afterward newspapers large and small ran prominent stories about the literally hundreds, if not thousands, of flying saucer reports that suddenly poured in from "reliable witnesses" (though curiously never from professional astronomers). Roswell got on this bandwagon early, so it is not surprising that it achieved widespread fame. Moreover, that prominence has lasted, as one can confirm by visiting many a bookstore or public library.

Of course there *was* something found in New Mexico in July 1947, and there were indeed secret projects going on at that time and place, although aliens and their spaceships played no part in matters. Instead, as Pflock demonstrates pretty clearly, the explanation is much more mundane (but I won't give the game away here).

While this book is a thorough refutation of the Roswell affair, one may ask why we should read it, let alone buy it. After all, isn't this just another case of mass delusion, of interest to psychiatrists, sociologists, and the like, but not to many more?

One answer is that the Roswell myth is recent, and indeed still forming, so that we can examine the process in almost as much detail as we like and from almost any aspect we choose. And while this immediacy may deny us historical perspective, it does provide us with a point of view that no future generation can have.

Another answer is that no reader of *Isis* is immune from the possibility of being asked sometime, somewhere, by someone, "Do you believe in flying saucers?" "Are aliens visiting Earth?" or, more to the point, "What about Roswell?" As the last is the most famous and widely believed-in topic in all of ufology, it might be useful to keep Pflock's book handy. There should be little fear of the subject going out of date, as half a century later the television series *Roswell* is now in its third successful year, public interest is renewed after every TV "documentary" on the topic, and the International UFO Museum and Research Center is still the main attraction in the city of Roswell.

As Pflock points out, the Roswell affair is yet another example of how myth can take precedence over reality and how the will to believe can be strengthened by evidence to the contrary. In this respect we might all glance in a mirror, for similar delusions can be held not only by the uninformed but also by scholars; no doubt many readers can recall, with wry chuckles, examples in their own fields.

Perhaps this New Mexican fable will never die. However, at least we can do our best to

speed it on its way, and this book can be a big help in such an effort. In short, buy this book, read it, keep it available, and above all donate a second copy to your local public library.

RONALD A. SCHORN

Alice B. Kehoe; Mary Beth Emmerichs (Editors). *Assembling the Past: Studies in the Professionalization of Archaeology*. (Based on papers presented at the First Joint Archaeological Congress, 6 January 1989, and the American Anthropological Association annual meeting, 16 November 1989, Washington, D.C.) viii + 241 pp., bibls., index. Albuquerque: University of New Mexico Press, 1999. $49.95.

This volume addresses issues of professionalization and professional development primarily in American archaeology (ten of the twelve papers). To put it simply, professionalization is the process of creating boundaries between a discipline and other disciplines, amateurs, and the general public through organizations, training, certification, and so on. The effect of this process is to place some individuals, or whole categories of people, on the outside, but it also forces some individuals to create alternate roles within the boundaries.

Robert Burkitt was dropped by the University of Pennsylvania Museum as a contract explorer because the museum moved toward more structured (and legal) ways of collecting in Guatemala. Augustus Le Plongeon was marginalized because of his increasingly extreme interpretations; his positive contributions were ignored. Daniel Wilson made important contributions to understanding British prehistory but was not able to get a permanent position and had to immigrate to Canada. Not as well connected as his contemporary, John Lubbock, he has not received as much attention as Lubbock from modern scholars.

Women interested in archaeology prior to World War I often found themselves excluded from all-male networks and organizations and from fieldwork (Matilda Coxe Stevenson was an exception). They created their own anthropological organization and found ways to contribute to the field through archival research (Zelia Nuttall), financial support of field expeditions (Mary Hemenway), development of research institutions (Alice Fletcher) or archaeology programs (Abby Leach), and site preservation (most of the women named above). By midcentury, when fieldwork was more open, women archaeologists such as Marian White often chose alternate career strategies. Some women were in contexts where they could make interesting archaeological observations, as did Marietta Wetherill in Chaco Canyon, but this hardly qualifies them as "pioneer archaeologists," as the senior editor claims of Wetherill (p. 13).

Early in the professionalization process, regional competition was a factor that structured collecting and research. In the search for a curator at the Field Museum in Chicago, city leaders shunned the New York–connected scholar (Franz Boas) and went with the Washington-connected one (William H. Holmes). In New Mexico, Edgar Lee Hewett spent the first decades of the century fighting against Harvard and the Smithsonian for control of southwestern archaeology. The issue of regional competition in archaeology deserves more examination, as the Smithsonian and Peabody Museum made many enemies during their massive collection programs.

Two chapters take a broader view of the development of subdisciplines of archaeology. Biblical archaeology moved from the mainstream of American cultural life to the margin because of the fundamentally theological approach to research taken in the post-1950 period. In classical archaeology, training discouraged initiative and kept the field from adapting well to changes in broader patterns of scholarship and funding.

This volume does not address the issue of whether or how professional development in archaeology differs from that in other sciences, although some similarities are briefly noted. One aspect of archaeology that will always make it different from most other scientific disciplines is the strong role that amateurs play in basic research. There were no amateur archaeologists in 1890, but at least by the 1920s professionals had drawn boundaries and created the category. On one side were professional archaeologists who worked for museums and colleges and had their own organizations, networks, and publications; on the other side were amateur archaeologists who worked at archaeology in their spare time, owned and sometimes sold artifacts, and had separate organizations, networks, and publications. This multifaceted boundary was and still is shifting and porous. Ultimately, the story of boundary definition in archaeology, which is covered episodically in this volume for the earlier period of professionalization, will need to include the relationships between amateurs and professionals after the discipline had matured.

ANDREW L. CHRISTENSON

Ruth O'Brien. *Crippled Justice: The History of Modern Disability Policy in the Workplace*. xiv

+ 302 pp., notes, index. Chicago: University of Chicago Press, 2001. $19 (cloth).

Historians of science are familiar with the many significant contributions of scientists who lived with disabilities. James Joule, Johannes Kepler, Pierre Janssen, Louis Pasteur, James Sumner, and, more recently, Stephen Hawking were challenged with physical disabilities. Kepler also experienced blindness, as did Galileo, William Hyde Wollaston, Robert Wilhelm Bunsen, and Leonhard Euler. Among notable deaf scientists were Charles Bonnet, Anders Gustaf Ekeberg, Annie Jump Cannon, Henrietta Swan Leavitt, Charles Nicolle, and John Warcup Cornforth. Historically and in contemporary life, scientists and other people with disabilities have faced a constant barrage of stereotypes and negative attitudes while attempting to succeed in the workplace.

In *Crippled Justice* Ruth O'Brien examines disability as part of the human condition and shows how society has not always been open to change or difference, particularly with regard to policy as it relates to employment. While not focusing on science per se, the book traces the legislative development of the history of employment provisions in disability policy from World War II to the present and summarizes what has been learned from the experience in the United States with rehabilitation and the attempts to "normalize" people with disabilities. This approach, O'Brien explains, focuses on accommodation by people with disabilities to society rather than the other way around. The book further describes how the psychoanalytical principles underpinning rehabilitation during the postwar years were challenged by a new set of values as proponents fought for rights-oriented policy as reflected in the Rehabilitation Acts, their subsequent amendments, and the culmination of this movement in the Americans with Disabilities Act (ADA) in 1990. O'Brien argues that public support for the cultural values of self-sufficiency behind vocational rehabilitation and a lack of support for the assumptions behind the rights orientation are responsible for an ineffectual ADA. In describing these movements, she presents a comprehensive picture of disability policy; this volume not only offers a cogent analysis, but also serves as an informative reference work.

O'Brien's meticulously researched work demonstrates that despite decades of governmental policy and court decisions, the picture of disability rights in the realm of employment remains both unclear and unfair. Legislative ambiguity and bureaucratic decisions continue to present obstacles since the passage of the ADA. *Crippled Justice* is an illuminating resource for anyone interested in equity issues and the inclusion of people with disabilities in the workplace. Societal attitudes toward disability are the true handicaps; this book is for those who believe in meaningful inclusion of people with abilities saddled by ignorance and injustice.

HARRY G. LANG

Roberto J. González. *Zapotec Science: Farming and Food in the Northern Sierra of Oaxaca*. xii + 328 pp., illus., maps. Austin: University of Texas Press, 2001. $50, £34 (cloth); $24.95, £16.95 (paper).

Do farmers in the southern Mexican highlands practice science? The anthropologist Roberto González argues that they do in his well-written and solidly researched account of Zapotec farmers' cultivation of corn, sugarcane, and coffee. This book speaks to enduring questions concerning the nature of science through its focus on the often-overlooked sophistication of traditional farming. Historians of science interested in agriculture, international development, and definitions of science more broadly will find in *Zapotec Science* a rich store of wide-ranging questions and provocative answers.

González aims to move beyond the limitations of many sociological and anthropological studies of traditional societies. Many ignore international "cosmopolitan" influences, and most that do deal explicitly with these treat the traditional and the cosmopolitan as separate spheres. González finds no reason to treat these categories as nonoverlapping, as he documents how Zapotec farmers have experimented with and adopted techniques, implements, and crop varieties from many parts of the globe. And the flow goes both ways. Many so-called cosmopolitan scientists are beginning to recognize the importance of traditional knowledge for solving problems in industrialized nations. But though the boundaries may be blurred, González argues that the conceptual bases of Zapotec agriculture are "culturally incommensurable with those predominating in industrialized societies" (p. 3).

In Chapter 1, González elaborates his position relative to other anthropological work on local science and traditional ecological knowledge and poses comparative questions about Zapotec and cosmopolitan science. Chapters 2 and 3 richly detail the daily practice of Zapotec agriculture and its environmental and geographical, cultural, and historical contexts. González em-

phasizes campesinos' craft, the refined skills that underlie every aspect of home, community, and agricultural life.

González's research comes to the fore in the middle chapters on maize, sugarcane, and coffee cultivation. Here he clearly illustrates how Zapotec farming is founded on five fundamental values: household maintenance (the agricultural enterprise spans from the fields to the home), mutual aid among families, personification of the earth and supernatural beings, an understanding of physical work as normal and desirable, and an emphasis on the quality of food over its quantity. For each crop he shows how present agricultural practices have emerged from an evolving balance between local crop development over hundreds or thousands of years and carefully weighed periodic opportunities to adopt new practices or plant varieties. Sugarcane cultivation, for example, illustrates how Zapotec farmers' decisions grow out of the five core values rather than a more narrow "economic rationality." Zapotec communities do not consume cheap, white, processed sugar; rather, they produce and use an older, less processed, more expensive form called panela. Panela's persistence in their diet is due to its role in reciprocal social relationships and to Zapotecs' preference for high-quality, good-tasting food. The book concludes with a critique of Green Revolution development strategies and a lively discussion of the fertile borderlands between traditional and cosmopolitan knowledges.

González's definition of science, closely related to Bronislaw Malinowski's, centers on practical knowledge. González argues that Zapotec farmers practiced science as they experimented with new techniques and implements from beyond Oaxaca's borders. Although traditional and cosmopolitan knowledge are conceptually distant, the author places great weight on what they share: both are systematic searches for practical knowledge. Traditional agriculture offers materials and methods that cosmopolitan scientists increasingly need.

This book's strength lies, not surprisingly, near González's expertise: his careful description and analysis of Zapotec farmers' practices, logic, and values. As with all ambitious and worthwhile projects that pull together scientific enterprises that are culturally, geographically, and conceptually distant, González's book leaves questions unanswered. His characterization of cosmopolitan science is at times contradictory and often narrow. While Zapotec scientists are said to consider a variety of procedures and techniques and "adopt what seems to work according to their own categories, and reject what does not," cosmopolitan scientists operate according to "one view, one procedure" (p. 259). A more thorough characterization of cosmopolitan science would strengthen the comparisons with Zapotec farming and would complement the direct treatment that González provides for cosmopolitan science's exceptions, sustainable agriculture and alternative medicine. This would also help to clarify differences between natural scientists and development economists, whose values are often not distinguished. Furthermore, cosmopolitan science seems to dwell outside of Mexico. What would it look like to widen the lens to include the Mexican urban academic scientific community? How does this book speak to the wider literature on scientific practice that largely focuses on cosmopolitan science?

Zapotec Science is an important contribution to our understanding of nonindustrialized agriculture outside of the United States. It will, I hope, prompt historians to delve into the historical relationships among people who employ agricultural knowledge toward divergent goals: scientists, farmers, and government leaders dwelling in different national and professional cultures.

KARIN MATCHETT

■ Sociology & Philosophy of Science

Giuseppe Giordano. *Tra paradigmi e rivoluzioni: Thomas Kuhn.* (Biblioteca di Studi Filosofici, 4.) 206 pp., index. Soveria Mannelli: Rubbettino, 1997. L 20,000 (paper).

Thomas Kuhn was not only the greatest historian of science but also one of the most influential philosophers of the twentieth century. Faced with such a significant character, Giuseppe Giordano has decided to focus on Kuhn "the philosopher," touching on the historian only indirectly. The book is roughly divided into two parts. The first one (Chs. 1–2) is devoted to a reconstruction of the genesis of Kuhn's most important ideas, focusing in particular on the essay "The Essential Tension" and on *The Structure of Scientific Revolutions.* In the second part (Chs. 3–5) Giordano considers the reception of Kuhn's work within the philosophy of science community. Since the philosophical debate is almost entirely a post-*Structure* phenomenon, the narrative proceeds more or less in chronological order, covering Kuhn's career from the beginning to the end. The story unfolds almost entirely in the realm of ideas, ignoring institutional matters such as Kuhn's role in the creation of a com-

munity of professional historians of science or his time as president of the Philosophy of Science Association.

The better part of the book is the first. Here Giordano tells us how Kuhn turned from physics to the history of science and, most important, how the concept of "paradigm" evolved from a pedagogic device to a pathbreaking philosophical idea. He also sketches the intellectual background to Kuhn's work, especially the received views on science and on the role of history embodied in the logical positivism of the 1950s. Unfortunately, the author does not make use of recent work on Kuhn, Carnap, and the neopositivists by Peter Galison, Michael Friedman, and others. As a result, the contraposition between the "old" and the "new" philosophy of science is a fairly conventional and dated one. Developments in the historiography of science in the last two decades are also ignored: one would have liked to read something on Kuhn's influence on the new sociology of science, the birth of the micro history of science as a reaction to Kuhn's macro approach, and so forth.

But of course no author can discuss everything, and Giordano has explicitly decided to focus on the relationship between Kuhn and the philosophers of his time. Chapter 3 deals with the controversy between Kuhn and Karl Popper. Giordano argues that whereas Popper never really questioned his own theses, Kuhn benefited from the debate, which prompted some significant changes in his position. In this chapter and the following one (on Kuhn's views on the relationship between history and philosophy of science), Kuhn's thought is presented in its dynamic evolution, as he adjusted and reacted to criticism. In both chapters Kuhn is set at center stage, with the other characters playing supporting roles.

The last chapter discusses Kuhn's "mature" views on theory change and scientific progress. It is a pity that nowhere in the book are we provided with a rigorous formulation of crucial concepts such as "progress" and "scientific rationality." This is the main defect of the book, the philosophical depth and rigor of which is sometimes less than satisfactory. Other examples are a confusion between scientific realism and the correspondence theory of truth (p. 68), a sloppy formulation of the problem of induction (pp. 94–95), and the lack of a serious discussion of the Duhem-Quine problem (which is relegated to a footnote). Because of these problems, Giordano's book is valuable chiefly as a concise summary and discussion of the overall significance of Kuhn's philosophical work. The commentary is frequently interrupted by long quotations from Kuhn's texts, and the footnotes also quote widely from the secondary literature (in some chapters, the quoted parts add up to almost 50 percent of the text). This makes Giordano's book a peculiar piece of work, rather like a conflation of a textbook and a Kuhnian anthology.

It must be recognized that to write yet another book on Kuhn is a challenging task. Kuhn's work has been dissected, criticized, and interpreted a number of times, and a truly novel analysis would require a truly novel approach. Other scholars have stretched the interpretation of known texts and facts until they have become "new" texts and facts. Giordano does not aim to be controversial and admirably abstains from such flamboyant exercises. However, he does not pursue the other route either: that of digging deeper into the past in order to discover something genuinely novel. A new book on Kuhn should be based, at the very least, on serious archival research in Kuhn's papers at MIT, and Giordano has not done that. Furthermore, he relies on a relatively small fraction of Kuhn's published writings, invariably the most widely known and celebrated ones. It is not surprising, then, that he ends up with a very familiar picture of Thomas Kuhn and his place in twentieth-century philosophy of science.

FRANCESCO GUALA

Isabelle Stengers. *The Invention of Modern Science*. Translated by **Daniel Smith.** (Theory Out of Bounds, 19.) [iii] + 185 pp., index. Minneapolis/London: University of Minnesota Press, 2000. $19.95 (paper).

This book, dedicated to Bruno Latour, provides a good sense of what his version of science studies looks like as a philosophical perspective. Isabelle Stengers, a philosopher of science at the Free University of Brussels, is perhaps best known in the anglophone world as Ilya Prigogine's coauthor of *Order Out of Chaos* (New Science Library/Shambhala, 1984), a popular defense of Epicurean naturalism in light of twentieth-century nonequilibrium thermodynamics. Prigogine's idea of the "dissipative structure"—that is, a physical order whose stability depends on disorder elsewhere in a system—has functioned throughout Stengers's work as a root metaphor for post-Newtonian cosmology, the history of the laboratory sciences, and, now, the science studies practices of someone like Latour.

A clear mission of this book is to draw anglo-

phone historians and philosophers of science into the francophone intellectual orbit. The text is not only sprinkled with references to classic and recent work in anglophone science studies, but it even begins by discussing the significance of Karl Popper, whom the French have begun to translate only in the last twenty years, and Thomas Kuhn, a figure traditionally dismissed as a belated Gaston Bachelard. However, by page 14 Kuhn yields the floor to Leibniz, who appears as the patron saint of the philosophy of "pure difference." Leibniz is suited to this role if Newton, rather than Locke, is seen as his main opponent. Instead of the baroque defender of innate ideas and cosmic teleology, the French Leibniz advances the principle of sufficient reason as the doctrine that nothing is superfluous in nature. We therefore do God a disservice if we suppose (à la Newton) that appearances are always "mere" and hence reducible to some neat mathematical formula that captures endlessly repeatable events.

The francophone image of Leibniz orients the philosophical impulse that animates Stengers (and Latour). But the standard-bearer of the image has changed with fashion, so that Stengers has now replaced Michel Serres with Gilles Deleuze as the Leibnizian *du jour.* Nevertheless, their common message was delivered with admirable austerity in the "Irreductions" that form the appendix to Latour's *Pasteurization of France* (Harvard, 1988). If everything is meaningful as it is (and not a disguise for something else), then there is an endless opening for mediators who create new events for others to interpret. These inquiries will eventuate not in an overthrow of a world that enslaves us but an acceptance of the world that we have helped to create in those very inquiries. Here Stengers recalls Latour's common cause with the Edinburgh Science Studies Unit, who urged philosophers to "leave the world alone" by adopting a more anthropological perspective that takes exotic forms of life at face value as loci of alternative rationalities.

To justify the Latourian turn, Stengers cleverly invokes Leibniz's original diplomatic efforts in the religious wars of seventeenth-century Europe as a model of conduct for today's "Science Wars." At the same time, she conveniently omits Leibniz's reputation for opportunism and equivocation, a point no less relevant for today. However, to her credit, Stengers offers an explicit presentation of the epistemic stance of Latourian science studies. It is captured by the Epicurean word "humor," which is set against "irony." Whereas the ironist embeds the object of study in some larger or higher context that undermines the object's status, the humorist sets aside any prior distinctions between herself and the object, whose identities are then coproduced in the course of inquiry. By entering this humorous mode, both scientists and science studies practitioners agree not to let self-definitions, normative ideals, senses of destiny (a.k.a. "modernity"), and other delusions of grandeur (a.k.a. "power") impede their ability to come to grips with their common reality, which is quite worth understanding in its own right.

That scientists should stop believing their own metaphysical hype sounds like disarmingly good advice, especially when given with Stengers's philosophical sophistication. However, Stengers is not simply offering a method for understanding science akin to, say, Edmund Husserl's phenomenological bracketing. She is also legitimating the professional identity of the science studies practitioner, who is, after all, best placed to inject the requisite humor into science. An apt comparison here is with the psychoanalyst who gains the patient's confidence by admitting that she too has undergone analysis: "We have never been rational." This admission helps put the patient at ease, which leads to a fruitful session—and many return visits. (It is no accident that the other person to whom Stengers dedicates this book, Deleuze's longtime collaborator Felix Guattari, was a psychoanalyst.) In any case, the image of the science studies practitioner as therapist suits the contract research environment in which most of Latour's followers ply their trade. Science studies reveals its indispensability to science policy by giving voice to agents whom policymakers have repressed at their own risk.

Despite its clear legitimatory function, *The Invention of Modern Science* contains some nice conceptual touches, such as a comparison of Darwinism's antiteleology with science studies' denial of scientific progress and a recognition that the demarcation problem in the philosophy of science implicitly concedes the success of "pseudo-scientists" in producing scientific effects. Nevertheless, I am left with a sour aftertaste from Stengers's repeated attempts to purge what might be called "the spirit of '68" from science studies. Like Latour in *Pandora's Hope* (Harvard, 1999), she distances science studies from anything that might resemble "revolutionary" or even "reformist" politics, be it Marxist or feminist. Perhaps this rejection of *rive gauche* intellectualism should be humorously regarded as a stage in the professionalization of science studies, a process that the field has already revealed in the sciences. Or maybe the expulsion

of social criticism from science studies is not such a laughing matter.

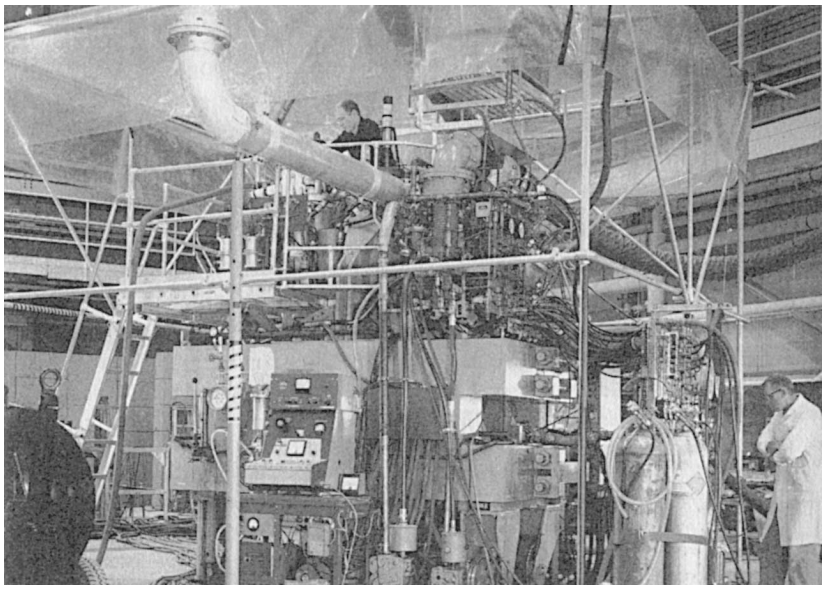
STEVE FULLER

Robert P. Crease. *Making Physics: A Biography of Brookhaven National Laboratory, 1946–1972*. xii + 434 pp., illus., figs., apps., bibl., index. Chicago/London: University of Chicago Press, 1999. $38, £30.50.

Making Physics chronicles the early life of Brookhaven National Laboratory, the first post–World War II entry in the United States Atomic Energy Commission's pantheon of national, multipurpose scientific research institutions. Built around instruments thought too large for any single university or corporation to build and operate alone, the national laboratories have functioned during much of the second half of the twentieth century as repositories for resources, both human and material. Studies of these institutions have highlighted a multitude of issues in postwar science and technology, from the evolving character of scientists and scientific research to instantiations of alternately rocky and cozy partnerships between the laboratories and the government that funds them.

Brookhaven, emphasizes the SUNY–Stony Brook philosopher of science and official Brookhaven historian Robert Crease, was fashioned explicitly to model university-style inquiry. The programs focused heavily on basic research, and the atmosphere encouraged flexibility for both experimental and theoretical groups. Run by a consortium of nine eastern universities, Brookhaven has been home to some of the nation's most powerful accelerators and research reactors, including the Cosmotron, the AGS, the BGRR, and the HFBR (the Alternating Gradient Synchrotron, the Brookhaven Graphite Research Reactor, and the High Flux Beam Reactor, respectively—expansion and cross-referencing of acronyms in the index would greatly aid in combating the confusion invariably resulting from Big Science alphabetitis). Brookhaven has had its share of both rivalries with other laboratories, nationally and internationally, and community relations issues with its neighbors in the Long Island town of Upton, New York—the latter a subject to which Crease devotes not just lip service but an entire chapter.

Capturing a quarter century of the research, politics, technology, and humanity that make up an institution is no mean feat. This is especially the case with Brookhaven, whose programs spanned fields from chemistry to medicine to nuclear engineering. Crease's somewhat novel tactic is indicated by his subtitle, "A Biography of Brookhaven National Laboratory"; he attempts



Bubble chamber at Brookhaven Laboratory, 1960 (reprinted from Robert P. Crease, Making Physics: A Biography of Brookhaven National Laboratory, 1946–1972, *p. 240).*

to treat his subject as one might a living entity and to combine many strands into a single narrative. Crease relies heavily on interviews with laboratory leaders, scientists, and personnel conducted by himself and others during the 1980s and 1990s. In addition, he has combed Brookhaven's archival documents and made productive use of numerous secondary sources, particularly Jack Holl's 1997 study *Argonne National Laboratory* (Illinois, 1997), Peter Galison's *Image and Logic* (Chicago, 1997), and Spencer Weart's *Nuclear Fear* (Harvard, 1988). However, as it employs almost no archival documents beyond the Brookhaven collection, this tale might more accurately be called an autobiography, if an occasionally critical one, since not only the narrative, but also the sources consulted, ensure that it is from Brookhaven's point of view.

In the introduction Crease explicitly states that he has not set out to write a history of science. Addressing the relative merits of his approach, he asserts that risking parochialism is acceptable in order to "illustrate the particular kind of pleasure the scientific life provides in a 'university-style' atmosphere" (p. 5). Indeed, in this goal the book succeeds admirably. For its chosen audience, "those around who are still interested in stories about modern-day *technites* and think that such stories reveal an essential part of the meaning and history of what they do" (p. 6), Crease's engaging narrative does indeed convey the exciting challenges that persuaded individuals to participate in the research program at Brookhaven. Historians, for their part, can greet *Making Physics* with the cautious embrace they should afford any well-written, self-reflective autobiography, for the work contains an invaluable wealth of information about an important institution relevant to many aspects of post–World War II society.

ELIZABETH PARIS

Susanne Uebele. *Institut im Bild, Volume 2: Bauten der Max-Planck-Gesellschaft zur Förderung der Wissenschaften*. (Veröffentlichungen aus dem Archiv zur Geschichte der Max-Planck-Gesellschaft, 11.) 292 pp., frontis., illus., figs. Berlin: Archiv zur Geschichte der Max-Planck-Gesellschaft, 1998. (paper).

This volume is a picture book presenting 440 photographs of more than 80 Max Planck Institutes and research sites of the Max-Planck-Gesellschaft zur Förderung der Wissenschaften (MPG). As such it represents a continuation of Glenys Gill and Dagmar Klenke's *Institut im Bild, Volume 1: Bauten der Kaiser-Wilhelm-Gesellschaft zur Förderung der Wissenschaften* (1993), which, along with several other associated volumes of this series on the Kaiser-Wilhelm-Gesellschaft (KWG) and the MPG, I recently reviewed in *Isis* (March 1999, *90*:387–388). The photographs run the gamut from the Aerodynamische Versuchsanstalt in Göttingen to the Institut für Züchtungsforschung in Kreis Hameln. The vast majority of the photographs show the exteriors of institute buildings or an institute as a whole; there are, by contrast, only a small number of photographs of a given institute's floor plans, of a building's interior, or of individual scientists. In conjunction with the other volumes in this series, the present volume constitutes a useful source for appreciating the development of the KWG and MPG.

DAVID CAHAN

Stephen Hilgartner. *Science on Stage: Expert Advice as Public Drama*. (Writing Science.) xvi + 211 pp., figs., tables, bibl., index. Stanford, Calif.: Stanford University Press, 2000. $49.50 (cloth); $18.95 (paper).

"The play's the thing," according to Hamlet (*The Tragedy of Hamlet, Prince of Denmark.* Act 2, Scene 2). Stephen Hilgartner agrees, and he has taken the notion of performance—public drama—and used it as an extended metaphor and analytical tool to explore ways in which scientific advice is generated, how advisory bodies seek to present themselves, and how they achieve (or fail to achieve) credibility.

Hilgartner focuses on three reports from the 1980s of the National Academy of Sciences that deal with diet and health. These were not typical reports, for they generated huge controversies that swirled around them. The reports' potential influence on doctors and patients, on the food and agriculture industries, and even on parents who admonish their children to drink their milk and eat their vegetables was also huge.

Like most NAS products, the three reports were written by committees. The authors of the first one managed to fend off critics. The second was, to use Hilgartner's theatrical term, virtually "upstaged" by its critics. And the third was never published at all. The draft of this report, a 1985 study that dealt with recommended dietary allowances (RDAs) for foods, proved so disputatious that the effort was canceled by the president of the academy. All three cases are interesting and well presented, but the cancelled RDA report naturally has the most dramatic interest and is the most instructive.

Credibility is the central issue in scientific advice. Even a body as august as the National Academy of Sciences cannot assume that its advice will be accepted at face value by policymakers, the public, or the scientific community itself. Hilgartner examines the techniques the academy used to enhance the credibility of its reports. These techniques included the literary structure of the publications as well as the maintenance of a sharp distinction between the "front stage" and "back stage" components of the advisory group's "performance." Backstage, in the committee's deliberations, there may be sharp disagreements, but out front, the report is presented to the world as the product of consensus. Sometimes, however, these stage management techniques fail and internal dissension emerges into public view. Hilgartner's account of the saga of the RDA report illustrates how leaks and "unauthorized performances" (i.e., committee members speaking to the press without official sanction) effectively killed the report.

Hilgartner carries the theatrical metaphor well beyond what one might expect in a scholarly book on science policy. In Chapter 4 he takes the academy's announcement canceling the 1985 RDA report and recasts it in explicitly dramatic form—a ten-page script for a play entitled "A Letter from the Chairman: A Play in Three Acts," complete with stage directions and a Greek chorus. Surprisingly, it works. While it may never be performed on Broadway, "A Letter from the Chairman" effectively makes Hilgartner's point. The episode illustrates how the academy, faced with a breakdown of its regular procedures, managed to portray the report's cancellation as "completely normal, . . . a logical consequence of the situation, not an aberration" (p. 128).

From my own standpoint as a political scientist, I might have liked a bit more emphasis on the interests the various committee members represented. Hilgartner acknowledges the roles vested interests (i.e., the various food industry critics) play in the controversies that surrounded the three reports, but in maintaining his focus on information management by the academy, he fails to devote much attention to them. Similarly (and probably understandably), he does not deal at all with a key category of scientific advice—that rendered privately by scientific advisors to policymakers.

These are minor criticisms, however. On the whole this is an interesting and well-written book that contributes a unique and valuable perspective to the literature on scientific advice given to the public.

ALBERT H. TEICH

■ Reference Tools

R. W. Hunt; A. G. Watson; W. D. Macray. *Bodleian Library Quarto Catalogues IX: Digby Manuscripts*. Reproduction of the 1883 catalogue by **W. D. Macray.** Notes on Macray's descriptions of the manuscripts by **R. W. Hunt** and **A. G. Watson.** 196 pp., apps., indexes. Oxford: Bodleian Library, 1999. £60.

On 30 December 1634 the Bodleian Library in Oxford received an important gift. Fourteen trunks bearing a mass of manuscripts bound in no fewer than 237 "handsome calf" volumes arrived from London. They bore the coat of arms of Sir Kenelm Digby (1603–1665), courtier and philosopher, who was to be prominent three decades later in the early Royal Society. At least half of the volumes contain material of scientific and philosophical interest and have been extensively quarried by twentieth-century scholars of "medieval" science. Many of them are from Merton College and have cast light on "Merton science." Most of the manuscripts, as has only recently been fully appreciated, were bequeathed to Digby by Thomas Allen (1542–1632) of Gloucester Hall. (Digby's own collection, probably acquired during his sojourns in Pisa and Florence, included one by Galileo on the "flux and reflux of the sea" and another by Jean Riolan on Fernel, dated 1588.)

Allen was reputedly one of the "magi" (another being Thomas Harriot) who had served Henry Percy, ninth earl of Northumberland (1542–1632), during his long imprisonment in the Tower after the Gunpowder Plot. Digby's father had been executed for alleged complicity in that plot when Kenelm was only an infant. Allegiance to the old religion, while conforming outwardly to the English church, constituted a strong bond between Allen and Kenelm. It may also explain the untiring zeal with which Allen sought out manuscripts from the purged libraries of Oxford colleges and from dissolved religious houses throughout the land. He thereby rescued them from being totally destroyed or surviving only as binding material for the flood of volumes from the printed press for which librarians now had to find shelf space.

Digby's decision to present the Allen manuscripts to the Bodleian was rather sudden. It came very soon after he came into their possession and just after he had them sorted and bound. Those of scientific and philosophical interest had, for example, been classified as astronomical, astrological, alchemical, mathematical, or medical. The decision to part from "the library

which with so much cost and labour I haue raked together" followed his resolve, after the death of his wife in 1633, to devote the rest of his life to penitence, prayer, and meditation.

William Dunn Macray, the compiler of the original 1883 catalogue of the Bodleian Digby manuscripts, succeeded in tracing only a few of them to the Allen bequest. With A. G. Watson's researches, spanning a quarter of a century, Allen's status as one of the most important contemporary English collectors of medieval manuscripts has secured full recognition. The "Notes" in the present volume supplement Macray's own descriptions of individual manuscripts and help to indicate the importance of the collection as a resource for historical research. There is an index of incipits, as well as an index of the manuscripts cited in the original catalogue and its notes. Watson has appended a catalogue of Allen's own manuscript collection that was compiled during Allen's lifetime and consulted by Macray but has never before been published.

The revised edition of the catalogue will greatly assist the study of the Digby manuscripts. It also adds much to our knowledge of the English collectors who saved manuscripts from wanton destruction after the dissolution of religious foundations and the purging of university libraries that followed the Henrician Reformation.

P. M. RATTANSI

Linda Ehrsam Voigts; Patricia Deery Kurtz (Editors). *Scientific and Medical Writings in Old and Middle English: An Electronic Reference*. Ann Arbor: University of Michigan Press, 2001. $54.50, £45 (CD-ROM).

It is better to exist than not to exist. That stage in the ontological argument can apply also to reference works: even if they display no more than a basic competence, their mere existence is extremely welcome. Of few scholarly fields can this be more true than of vernacular scientific writing in medieval England. Like their Latin counterparts, scientific and medical writings in Middle English are often anonymous or bear spurious attributions. They can be securely identified only by incipits. Their sheer mutability defeats traditional editorial techniques. Hence the fundamental need for an approach to the corpus that focuses on individual manuscripts rather than on texts.

Scholars of medieval Latin material have long enjoyed the benefits of "TK," the *Catalogue of Incipits of Mediaeval Scientific Writings in Latin* (Mediaeval Academy of America, 1963), edited by Lynn Thorndike and Pearl Kibre (even though TK is still essentially text based). And they will soon enjoy the yet greater "searchability" of "eTK," the revision in electronic form promised by its editor, Peter Murray Jones (*Gazette du livre médiéval,* 1999, *35:*53–56). No medieval vernacular has been comparably served. True, the relatively few scientific writings in Old English have each been studied intensively; and the Anglo-Norman material has been rescued from oblivion almost singlehandedly by Tony Hunt. But apart from a few major translations from the Latin, the Middle English corpus—far more substantial than the Old English or the Anglo-Norman—has remained, by comparison, poorly understood, largely because many of the manuscript witnesses have yet to be found and listed, let alone scrutinized.

All this is about to change. If the editors of the work under review had merely scratched the surface in their cataloguing they would have earned our warmest thanks. But they have done much more than that. Since 1985, Linda Voigts and Patricia Kurtz have spent some ten years recording information on texts surviving in more than 1,200 manuscripts (including, for the sake of completeness, the Old English corpus) held in libraries and private collections in fifteen countries. Prologues and "embedded texts" are treated individually. The scope of the project is admirably broad and free of anachronism. To quote the editorial introduction: "We define medieval texts on medicine, science, and technology as those writings found in scientific and medical manuscripts. This is not a circular definition, for these books contain not only texts that are still judged to be medicine and science, but also many other writings considered science and medicine in the Middle Ages but not today [for example, computus and chiromancy]." The result is indexed by incipit, author, title, translator, TK equivalence, manuscript, and subject. Keyword searching of all these indexes, and of the bibliography, is of course easy. Invaluably, an "Index of Raw Data" allows the user to view incipits in the order in which they appear in the manuscripts. Even for a computer semiliterate such as myself, the database proved straightforward to install and navigate. Since it has been so many years in the compiling, its technology and design now seem a little primitive. But both are more than adequate to their task. Overall, this is a magnificent achievement; and, as the corrections and supplements pour in to the editorial office, "eVK" will become even more detailed and comprehensive. Its existence places the entire subject on a new footing.

PEREGRINE HORDEN

American Science Leaders. Santa Barbara: ABC-CLIO, 2000. $49 (CD-ROM).

This compilation of biographical sketches of 400 "leaders" of American "science" could become a favorite resource for students at the secondary school level. Easy to navigate, with useful and quick summary information, it should appeal to those accustomed to instant feedback in a variety of predigested forms. Included in the list are 380 men and 20 women, not more than a quarter of whom started their careers before 1900. The earliest is Benjamin Banneker; the latest, Jason Lanier.

Various features make it easy and absorbing to search or browse the disk. One can search by text, attribute, or subject, with the results displayed for further actions (sorting, displaying, saving, printing). For example, one can search by some or all of the following "attributes": occupation, birth and/or death date, sex, ethnicity, and birthplace. One can then view the results along a "timeline," which is unfortunately anchored mostly by political events or wars rather than great events in science and technology. "Birthplace" leads to one of 85 basic, colored, easy-to-read "maps" of states and countries of origin. "Notebook" permits cutting, pasting, and annotating selected text from the disk or adding one's own to compile an original document. "Ethnicity" yields 375 Caucasians, 14 Afro-Americans, 11 Asians, and 2 Hispanics. Most biographical sketches come with a photograph or portrait of the scientist and have bibliographical references for further information. Highlighted text links to other biographies, geographical locations, or a glossary of 294 terms to help with unfamiliar terminology. There is a 71-term "subject index," and appropriate "help" is available. But there are shortcomings. Glossary explanations are limited, and it's not clear why some terms are included but others not. A relative lack of inclusion of specialized terms limits technical understanding, and hence the educational value of the CD.

Although the sketches are described as "in-depth profiles," most are quite short and have few bibliographic references. References, dating to 1996, are a mixture of popular and scholarly articles, books, and websites; Asimov's *Biographical Encyclopedia of Science and Technology* seems to be cited more often than the *Dictionary of Scientific Biography*. Credits describe the author, Amanda de la Garza, as "a professional writer, web page designer, and editor," further reinforcing the impression that this is not a scholarly work. A perennial problem with such compilations is who's included and who's excluded. The absence of explicit selection criteria, as in this case, only augments inevitable questions about bias and representativeness. No scientist before Banneker is listed, and a number of pre-Revolutionary notables are omitted. Many eminent scientists from 1800 to 1860 are omitted. Ironically, O. C. Marsh is in, but E. D. Cope is out. Barry Commoner is the one environmental biologist, Rachel Carson the sole ecologist. Nearly 25 percent of the list are physicists, who, despite their acknowledged importance in science, make up less than 10 percent of the scientific community. Nobelists are well represented—and the Nobel Prize may be a major selection criterion for the twentieth-century scientists on the list. There are 18 inventors, 1 promoter, 1 explorer, and 1 industrialist among the "scientists," who also include 33 "medical" people, including 8 surgeons. It is clear from the list of occupations that the term "scientist" is broadly inclusive; the consequent blurring of common distinctions between science, engineering, medicine, and invention further raises questions about what it means to be a "leader of science."

It would have been useful to have more attributes (e.g., education, religion) included, thus making the sketches more uniform and comparable and the search combinations more varied. A complete and separate bibliography would have been useful. So, too, would have been a Mac version, especially given Apple's connection with education and its appeal to scientists and engineers. All in all, this disk is a good pilot project with obvious promise. Much still remains to be done to make it a reputable educational tool for higher education. At the moment, it's sadly too truncated for other than the most superficial college or university work.

DONALD DEB. BEAVER

Friends of the United States Air Force Academy Library. *The Genesis of Flight: The Aeronautical History Collection of Colonel Richard Gimbel*. xii + 372 pp., illus., app., bibl., index. Seattle: University of Washington Press, 2000. $60.

This is a thinking person's coffee-table book. Beautifully produced in glossy quarto, it delights the eye at the same time as it introduces a sampling of an outstanding collection and educates the reader in the rich history of flight before 1914. It is also computerized. The accompanying compact disc offers a multimedia presentation of the featured items, with search capability

Man and woman in balloon, from the Gimbel flight collection (reprinted from The Friends of the United States Air Force Academy, The Genesis of Flight: The Aeronautical History Collection of Colonel Richard Gimbel, *p. 288).*

and additional information. The combination is impressive.

The volume is divided into sections corresponding to major areas of the collection: printed books (further subdivided 1489–1850 and 1851–1914), manuscripts, prints, other holdings, seals, and numismatics. Each of the three hundred or so selected items is presented on its own page with a large illustration and up to a half-page of explanatory annotation written by the distinguished scholar responsible for the selections in that section.

There is a substantial introductory essay describing the collection, the collector, and the history embodied in the materials. Each section is also introduced by a one-page essay written by the responsible scholar. Contributors include Clive Hart, Tom Crouch, Paul Maravelas, Ellen Morris, Dominick A. Pisano, Dolly Pitman, and Edward Rochette.

As implied by the diversity of the collection, it is not limited to the strictly scientific development of flight. Popular culture, mythology, advertisements, and fine arts are well represented. I am unaware of any other collection with comparable breadth. The Gimbel Collection truly offers "one-stop" research opportunities, as well as rare items generally unavailable elsewhere except in reproduction. The accompanying multimedia compact disc is an excellent tool, searchable by keyword, and it should be given more prominence. The reader comes upon it by surprise, housed on the inside of the back cover; it is not mentioned in the bibliographic information, on the book jacket, or in the table of contents. Immediately following the index, there is a page that offers a description of the CD and instructions for its use, but even this page is not noted anywhere else and can be found only serendipitously. Unfortunately, the CD does not incorporate a complete listing of the collection.

Readers will find this book and its CD a useful introduction to aeronautical history, a window into the widespread social interest in flight, and an indication of the richness and depth to be found in the Gimbel Collection. It may also help in demonstrating to one's students, friends, and family what fascinations are to be found in the field. It is a delight to the eye, and the mind, but it is not a "catalogue" of the collection. It is merely a very instructive sample and an implied invitation to further research at the United States Air Force Academy Library, where the full Gimbel Collection of over 20,000 items is housed. The online catalogue for the full academic library, of which the Gimbel Collection is a prized component, may be accessed at *http://www.usafa.af.mil/dfsel/index.html*—but the Gimbel Collection is not catalogued separately.

BAYLA SINGER

■ Collections

Michael Tad Allen; **Gabrielle Hecht** (Editors). *Technologies of Power: Essays in Honor of Thomas Parke Hughes and Agatha Chipley Hughes*. xx + 339 pp., illus., index. Cambridge, MA: MIT Press, 2001. $24.95, £16.95 (paper); $62, £42.95 (cloth).

John M. Staudenmaier, S.J.: Disciplined Imagination: The Life and Work of Tom and Agatha Hughes. **Gabrielle Hecht and Michael Thad Allen:** Introduction: Authority, Political Machines, and Technology's History. **W. Bernard Carlson:** The Telephone as Political Instrument: Gardiner Hubbard and the Formation of the Middle Class in America, 1875–1880. **Eric Schatzberg:** Culture and Technology in the City: Opposition to Mechanized Street Transportation in Late-

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Robert Jütte; **Motzi Eklöf**; **Marie C. Nelson** (Editors). *Historical Aspects of Unconventional Medicine: Approaches, Concepts, Case Studies*. xii + 288 pp., tables, index. Sheffield: European Association for the History of Medicine and Health Publications, 2001. £34.95.

Robert Jütte: Alternative Medicine and Medico-Historical Semantics. **Claudine Herzlich:** Patients, Practitioners, Social Scientists and the Multiple Logics of Caring and Healing. **Marijke Gijswijt-Hofstra:** A Sense of Gender: Different Histories of Illness and Healing Alternatives. **Martin Dinges:** The Contribution of the Comparative Approach to the History of Homoeopathy. **Gerda Bonderup:** Danish Society and Folk Healers 1780–1825. **Sofia Ling:** Physicians, Quacks and the Field of Medicine: a Case Study of Quackery in Nineteenth-Century Sweden. **Motzi Eklöf:** Doctor or Quack: Legal and Lexical Definitions in Twentieth-Century Sweden. **Riitta Oittinen:** Health, Horror and Dreams for Sale: Patent Medicine and Quackery in Prewar Finland. **Michael Stolberg:** Alternative Medicine, Irregular Healers, and the Medical Market in Nineteenth-Century Bavaria. **Phillip Nicholls:** The Social Construction and Organisation of Medical Marginality: the Case of Homoeopathy in Mid-Nineteenth-Century Britain. **Sigríður Svana Pétursdóttir:** Homoeopathy in Iceland. **Elisabeth Hsu:** Figuratively Speaking of 'Danger of Death' in Chinese Pulse Diagnostics. **Gry Sagli:** Chinese Medical Concepts in Biomedical Culture: the Case of Acupuncture in Norway. **Barbara Wolf-Braun:** 'The higher order of the natural laws and the wrong of world of hysterical mediums': Medicine and the Occult 'Fringe' at the Turn of the Nineteenth Century in Germany.

Karl A. Kottman (Editor). *Millenarianism and Messianism in Early Modern European Culture*. Vol. 2. Catholic Millenarianism from Savonarola to the Abbé Grégoire. xiv + 108 pp., index. Dordrecht: Kluwer, 2001. €80, $69, £49.

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Arien Mack (Editor). *Technology and the Rest of Culture*. x + 395 pp. Columbus: The Ohio State University Press, 1997. $24.95.

Robert Heilbroner: Introduction. **Robert McC. Adams:** Social Contexts of Technology. **Leo Marx:** *Technology:* The Emergence of a Hazardous Concept. **Langdon Winner:** Technology Today: Utopia or Dystopia? **Arno Penzias:** Technology and the Rest of the Culture: Keynote. **Alan Trachtenberg:** Introduction. **Elizabeth L. Eisenstein:** From the Printed World to the Moving Image. **David E. Nye:** Shaping Communication Networks: Telegraph, Telephone, Computer. **Sherry Turkle:** Computational Technologies and Images of the Self. **Nicholas Humphrey:** Introduction. **Marvin Minsky:** Technology and Culture. **Peter Galison:** Three Laboratories. **Joshua Lederberg:** Science and Technology: Biology and Biotechnology. **Ira Katznelson:** Introduction. **Alan Ryan:** Exaggerated Hopes and Baseless Fears. **Paul Gewirtz:** Constitutional Law and New Technology. **Rosalind Williams:** Introduction. **George Kateb:** Technology and Philosophy. **John Hollander:** Literature and Technology: Nature's "Lawful Offspring in Man's Art." **Robert L. Herbert:** The Arrival of the Machine: Modernist Art in Europe, 1910–25. **Robert McC. Adams:** Introduc-

tion. Comments by Jerry Berman and Daniel J. Weitzner, Robert Heilbroner, William H. Janeway, Ira Katznelson.

Ellen Frankel Paul; Jeffrey Paul (Editors). *Why Animal Experimentation Matters: The Use of Animals in Medical Research*. ix + 224 pp., index, tables, figs. New Brunswick: Transaction Publishers, 2001. $24.95 (paper)

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Clark A. Miller and Paul N. Edwards: Introduction: The Globalization of Climate Science and Climate Politics. **Paul N. Edwards:** Representing the Global Atmosphere: Computer Models, Data, and Knowledge about Climate Change. **Stephen D. Norton; Frederick Suppe:** Why Atmospheric Modeling is Good Science. **Simon Shackley:** Epistemic Lifestyles in Climate Change Modeling. **Chunglin Kwa:** The Rise and Fall of Weather Modification: Changes in American Attitudes Toward Technology, Nature, and Society. **Clark A. Miller:** Scientific Internationalism in American Foreign Policy: The Case of Meteorology, 1947–1958. **Paul N. Edwards; Stephen H. Schneider:** Self-Governance and Peer Review in Science-for-Policy: The Case of the IPCC Second Assessment Report. **Clark A. Miller:** Challenges in the Application of Science to Global Affairs: Contingency, Trust, and Moral Order. **Dale Jamieson:** Climate Change and Global Environmental Justice. **Shelia Jasanoff:** Image and Imagination: The Formation of Global Environmental Consciousness.

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