

## BOOK REVIEWS

*Women & Politics in the Age of the Democratic Revolution.* Edited by HARRIET B. APPLEWHITE and DARLINE G. LEVY. (Ann Arbor: University of Michigan Press, 1990. vi, 289p. Illustrations. \$34.50.)

The women of the Age of Revolution are finally attracting the attention they have long deserved. After all, as this important volume of essays makes abundantly clear, women played essential roles in popular protests and revolutionary movements in America, Belgium, England, France, and the Netherlands, through which they began to enter the Atlantic political world as a more-or-less independent force. They fell far from liberation, but they certainly made progress, for, as Harriet B. Applewhite and Darline G. Levy point out in their introduction, "self-conscious demands for the improvement of women's condition . . . became an ineradicable part of the political agenda" (p. 19).

Applewhite and Levy have produced one of those all-too-rare collections of essays that works, and works exceptionally well. This is in part because strong organizational efforts stand behind the volume: a workshop, an international conference, and continuing, coordinated discussion among contributors—all of which have helped to produce essays of high quality. But the more important reasons for the success of the volume are the shrewd choice of subject and the intelligent approach brought to bear on it. *Women & Politics in the Age of the Democratic Revolution* is a major contribution to an immensely important project—the rethinking of R.R. Palmer's *The Age of the Democratic Revolution: A Political History of Europe and America, 1760-1800*, published in two volumes in 1959 and 1964. Palmer's liberal capitalist Atlanticism, which provided some of the intellectual underpinnings of NATO and the Cold War, is now being challenged in a variety of ways by social historians who wish to draw upon radical scholarship of the past twenty-five years to rewrite the history of Atlantic revolution "from the bottom up."

Limitations of space make it impossible to do justice to this book's many subtle and sophisticated arguments, but perhaps a brief listing of subjects and findings can suggest its range and richness. John Bohstedt explores the place of women in "the informal community politics of [food] riot" in England between 1790 and 1810 and links it to the dynamics of household production and industrialization (p. 24). Dominique Godineau argues that a militant group of *sans-culotte* women stood "at the heart of the revolutionary movement" in France in 1793, forming part of "the mass base of the

democratic revolution” and insisting that “the Sovereign People was composed both of citizens and *citoyennes*” (pp. 61, 64, 68). Applewhite and Levy survey the role of women during the fall of the French monarchy in 1792, and find that their participation in armed processions and mass actions helped to generate new radical-democratic meanings of citizenship, sovereignty, and legitimacy. Wayne Ph. te Brake, Rudolf M. Dekker, and Lotte C. van de Pol treat “Women and Political Culture in the Dutch Revolutions” of the 1780s and 1795. They suggest that women’s position in community networks made them “the bearers of traditions of popular protest and collective action” and hence important political actors (p. 130). Janet Polasky writes that “Belgian women shared actively in every phase of the Brabant Revolution” that erupted in 1789 against the Austrian emperor, Joseph II (p. 156). Gary Kates discusses the campaign for women’s rights and full political equality launched from the Cercle Social in 1790-1791, in which a “cultural revolution” would democratize family relations (p. 165). Alfred F. Young follows the women of Boston through the tumultuous decade that led to the American Revolution, surveying their lives at work and in church, popular gatherings, and politics. He concludes that the revolutionary experience moved some to apply the Revolution’s language of liberty to gender relations. Linda K. Kerber discusses how women in America “carried on the war” after the Revolution by developing a new strain within republican ideology. Some proclaimed that the “new republic required a new woman” possessed of a “noble ardour of independence” (pp. 236, 241). Finally, Laura S. Strumin-gher writes that a group of French women involved in the revolution of 1848 had learned but little from the women who, a generation earlier, had contributed so much to the age of democratic revolution. They saw their cause “as fundamentally new and distinct from previous efforts” (p. 283).

There are two criticisms to be made of the organization of the book. First, it has no index, which will reduce its usefulness. Second, the essays (as summarized above) move backward and forward in time and jump from place to place in awkward fashion. A roughly chronological arrangement would have been preferable, for it alone could have suggested both the direction and the force of momentum in the Age of Revolution. A third, more substantive criticism is that despite their calls for further research, the editors have stuck too narrowly to Palmer’s earlier definition of the sphere of Atlantic revolution. They should have expanded it to include, for example, the modern world’s second victorious colonial war for liberation and its first victorious slave revolt, the Haitian Revolution.

This collection of essays must in any event be judged a striking success. The University of Michigan Press should bring out a paperback edition of the book so that it can reach as many readers as possible.

*Franklin's Autobiography: A Model Life.* By P.M. ZALL. (Boston: G.K. Hall & Co., 1989. xiv, 124p. Chronology, illustration, selected bibliography, index. Cloth, \$17.95; paper, \$6.95.)

P.M. Zall has been associated with J.A. Leo Lemay in two related scholarly projects regarding Benjamin Franklin, the Genetic Text edition of the *Autobiography* and the Norton Critical Edition of the *Autobiography*. In addition, Zall has assembled a valuable collection of Franklin anecdotes in *Ben Franklin Laughing* (1980). So even in a series like the Twayne's Masterwork Studies, one would hope for some distinction when Zall turned on his own to Franklin's *Autobiography*. Beginning readers of Franklin will indeed find this new book lucid and useful. It is also quite clear from the tone and organization of the book that Zall intends this study for college students.

Those with more familiarity with Franklin will not find a great deal to hold their attention, though at points Zall's scholarly background breaks through. He reiterates the point that Franklin's monopolistic attitude toward the details of his own life—refusing to authorize any other biography than the one he himself never finished—contributed to lingering misunderstandings of him. Zall also offers his own speculation about the duration of composition of the first part of the *Autobiography*, suggesting that Franklin worked on it for three years after its opening date in the summer of 1771. On this issue Zall takes a stand rather different from the introduction to *The Autobiography of Benjamin Franklin: A Genetic Text*, which refuses to go beyond previous speculations to the effect that Franklin composed it all rather quickly. Noting the decline of Franklin's English political fortunes in 1774, Zall asks: "Is it any wonder that part 1 concludes with boasting about 'the Stand so generally made throughout the Colonies in Defense of their Privileges . . . ,' a remark more pertinent to 1774 than to 1771?" (p. 24).

Much of the reading of the *Autobiography* in the book treats the remembered life, appropriately, as a sort of fiction, constructed with the skill of a great writer. That presumption at a few points trips up Zall the scholar, who is also aware that the *Autobiography* is also a reasonably faithful record of the real events of Franklin's life. Thus, while considering Franklin's treatment of John Collins, Franklin's friend from Boston and Philadelphia, Zall notes that he and Lemay had found considerable biographical information about every other figure in the *Autobiography* for the Norton Critical Edition. "His may have been a composite portrait, but it is filled with too much concrete particularity to be a mere figment of Franklin's imagination" (p. 39). Zall flirts with a speculation but is recalled to a sounder view. What might seem more surprising is the possibility of the speculation. Franklin himself knew that his beginnings were obscure, so it is only natural that some obscure

people should have been his companions in youth. What is more striking is how many of his early companions—men such as James Ralph, Thomas Godfrey, and Ebenezer Kinnersley—emerged out of obscurity.

There are also some problems that experienced readers of Franklin will have with Zall's study. For one thing, Zall recycles anecdotes and quotations to fill out his text—as if the *Autobiography* and the attendant texts were not ample enough or as if Zall assumes his reader has a short-term memory problem. Zall persists in referring to Franklin's first Philadelphia employer as Sam Keimer; although men with the first name Samuel are often called Sam, Franklin himself never adopted that familiar tone with Keimer in the text of the *Autobiography*, perhaps because the real young Franklin had been engaged in far more deflating shenanigans directed at Samuel Keimer. Zall says that Franklin had "tried to rape Mrs. T., the mistress Ralph had left in his care" (p. 41). Franklin's own characterization (in the text of the Norton Critical Edition) is as follows: "I attempted Familiarities, (another Erratum) which she repuls'd with a proper Resentment, and acquainted [Ralph] with my Behaviour" (*Autobiography*, p. 36). Franklin's behavior was wrong by any standard, but I would question whether "attempted Familiarities" means attempted rape. Discussing the incompleteness of the *Autobiography* as Franklin finally returned to writing it in August 1788, Zall remarks: "The record is pathetic: by October he had finished 112 pages, by the next May only five more, and by April 1790 when he died only another eight pages" (p. 6). But Franklin was terminally ill during this whole period and still engaged in various other projects that demanded his time.

George Washington University

ORMOND SEAVEY

*The Concept of Representation in the Age of the American Revolution.* By JOHN PHILLIP REID. (Chicago and London: University of Chicago Press, 1989. vii, 251p. List of short titles, index. \$32.00.)

John Phillip Reid may justly be called the dean of eighteenth-century legal historians. Monographs and articles flow from his pen with relentless ease, all elaborating a coherent if highly nuanced interpretation of Anglo-American constitutional history in the age of the American Revolution. The most curious aspect of the Reid *projet* is his decision, which I confessedly do not understand, to divide his current writings into two sets of volumes appearing under the imprint of two different presses. The work reviewed here is a companion to *The Concept of Liberty in the Age of the American Revolution*, also published by the University of Chicago Press in 1988; but

both books seem organically bound to Reid's *Constitutional History of the American Revolution*, two of whose three planned volumes have now been issued by the University of Wisconsin Press.

The central theme of this entire corpus of writing is that the constitutional and legal arguments that Americans deployed in rejecting the jurisdiction of Parliament were firmly and indeed correctly grounded in a traditional understanding of the British constitution and the English rights it embodied. This eighteenth-century constitution, Reid repeatedly notes, was still a constitution in which customary rights restrained the arbitrary power of government; it was not yet the constitution of the nineteenth century, where law was fully equated with the command of the parliamentary sovereign.

*The Concept of Representation* extends this basic argument in several fundamental ways. In the customary constitution, representation was constitutional insofar as it checked arbitrary power and expressed the constructive consent of those governed by its acts. If representatives shared the interests of their countrymen and the burdens of the laws they enacted, it did not matter that the franchise was restricted or that numerous towns were not entitled to return members. It was more important to represent property than people, and the practice of county representation somehow sustained the claim that all Englishmen were virtually and universally represented in Parliament.

Judged by these customary standards, the claim that Parliament represented Americans and could thus tax them was unconstitutional both because interests and burdens could not be shared, and because not a single blade of American grass had its member in the Commons. But Reid does not rest the case for the American position on customary English notions alone. As he repeatedly notes, representation was the one element of Anglo-American constitutionalism in which colonial practice had already sharply deviated from the mother country. Americans believed that people, not acres, deserved representation; that consent had to rest on real political connections between electors and representatives; and increasingly, that the purpose of representation was not to restrain government but to enable it to legislate on behalf of community interests.

If these conclusions are not surprising, Reid nevertheless succeeds (yet again) in alerting his readers to the elusive complexities of these constitutional and legal arguments. If his work influences historians less than he might otherwise desire, it will not be because he has failed to deepen our understanding of the customary constitution. The problem is rather that Reid's taxonomic approach is not easy to integrate with the historian's desire to explain change over time. For Reid, the changes that truly mattered are those that came in the nineteenth century, but by constantly contrasting customary

and “future” constitutions, Reid avoids crossing the middle ground of the historical moments that led from the one to the other.

*Stanford University*

JACK N. RAKOVE

*Colonial Delaware Assemblymen, 1682-1776.* By BRUCE A. BENDLER. (Westminster: Family Line Publications, 1989. xvii, 158p. Index, frequently cited sources. \$12.50.)

Family history, or genealogy, is perhaps the fastest growing segment of the research community. Generally, however, genealogists are viewed by historians with some disdain, for they are often amateurs with wildly varying degrees of sophistication in research techniques. In turn, genealogists have viewed historians with suspicion, believing they are pedants writing irrelevant and unreadable tomes on obscure topics. This struggle between the two groups is unfortunate, for skilled genealogists can provide valuable information on individuals and families while skilled historians can provide the context within which to place those same individuals and families. Sadly, far too many genealogists and the publishers who print their works lack any sense of historical discipline—e.g., careful analysis of sources, paleographic skills (particularly for the colonial period), an understanding of old and new style dating, the use of outside readers to verify the accuracy and necessity for the publication, and careful footnoting.

Bruce Bendler, who has attempted to meld history and genealogy, provides in this lamentable work an ideal example of the need for greater scrutiny prior to publication and of the necessity for genealogists to understand and utilize historical methodology. Bendler provides a rudimentary introductory history of the Delaware Assembly (leaving out the Provincial Council, which prior to October 1701 was part of the General Assembly) and an incomplete sessions list (omitting 1682 and 1716, both of which are extant), but most notably he provides biographical sketches of the assemblymen. The essays range from one or two lines (e.g., “Richard Law was surveyed 1000 acres of land in Sussex County in 1688”; “Arthur Vankirk had a son named Arthur. He owned 400 acres of land in Cedar Creek Hundred”) to several paragraphs. Bendler provides life dates where found, the county for which the assemblyman served, the dates he served, and occasionally some genealogical and landholding information, as well as odd bits and pieces of the individual’s political opinions or his local officeholding.

There are several major problems with Bendler’s essays. He clearly did not consult non-Delaware sources to any great degree, thereby failing to

establish the geographical point of departure for many of those early Delaware assemblymen who arrived from Virginia, Maryland, New Jersey, New England, and New York. Secondly, the essays show little understanding of the political dynamics of the period. Thirdly and most significantly, Bendler's work cannot be trusted. In an undertaking of this complexity, errors are bound to occur, but Bendler systematically misreads or misstates his evidence. For example, he claims that Peter Alrichs died in 1694, that his father was Peter Alrichs, that he married Susanna Wessells, that they had five children, and that he served as a justice of the peace in New Castle County from 1682 to 1693. In fact, in 1697, the year in which he actually died, Alrichs served in the Provincial Court, his father was the Reverend Sigfridus Alrichs, he married Maria Wessells, and they had four children. Alrichs served as a justice from 1677 to 1693 and again from 1695 to 1697. Bendler also states that Joseph Booth, for whom he does not provide a birthdate nor a father, died in 1732, that he arrived in Delaware from Scituate, Rhode Island, and that he married (1) Mary, daughter of Richard Richardson, and (2) Frances, daughter of Henry Spencer. In fact, Booth was born in 1659, died in 1733 (Bendler misreads the old style dating), was the son of John Booth, came from Scituate, Massachusetts, and married (1) Frances Cowdrey, widow of Henry Spencer, and (2) Elinor Robinson. He tells us that Robert Clifton married Ann Fenwick, daughter of assemblyman Thomas Fenwick, that William Clarke married Honor Vines, daughter of assemblyman John Vines, and that James Claypoole, son of assemblyman Norton Claypoole, married Mary Cann, daughter of assemblyman John Cann. In fact, Clifton married Sarah Avery, widow of Captain John Avery, and later wife of assemblyman John Kipshaven, Clarke married Honor Huling, daughter of Walter Huling of Salem, West New Jersey, and the James Claypoole that married Mary Cann was the son of Philadelphia assemblyman, James Claypoole, not his brother Norton.

Errors such as these are commonplace throughout the work, but more egregious failings are Bendler's essays on William Dyer and Griffith Jones. The bulk of his essay on Dyer mistakes the son, who was the assemblyman, for his more illustrious father, Captain William Dyer, who was denied a seat on the Provincial Council in 1687, while Bendler conflates three separate Griffith Joneses into one essay. In addition, Bendler's failure to list the members of the 1682 session causes him to omit essays on Nathaniel Walker and Richard Smith.

To make matters worse, Bendler's sources are so generalized as to be useless—e.g., "Sussex County probate, land and tax records" (John Wiltbank), "Kent County probate and land records" (William Winsmore), "Scharf" (various essays), and "Kent County tax and probate records" (various essays).

In effect, Bendler's work will only deepen the antagonism many historians feel towards genealogical publications. The book is also an indictment of the publisher, who, like too many genealogical publishing firms, appears to be all too ready to publish lists of "names" without regard to the quality of the research or the professionalism of the researcher. This cavalier attitude is also evident in the vast outpouring of transcripts or extracts of manuscript sources that are flooding into the bookstores; the result, alas, will be to hinder the publication of scholarly editions, and to further the misinformation found in far too many genealogical books and articles based on these published primary sources.

Bendler states in a brief author's note his hope that this book "will be regarded as a starting point for further and deeper study into the colonial Delaware political scene, and that both historians and genealogists will benefit thereby." While he is correct in believing that further and deeper study will be a benefit, he is seriously incorrect in believing that this book should be a starting point.

*Biographical Dictionary of  
Early Pennsylvania Legislators*

CRAIG W. HORLE

*Chainbreaker: The Revolutionary War Memoirs of Governor Blacksnake, as told to Benjamin Williams.* Edited by THOMAS S. ABLER. (Lincoln and London: University of Nebraska Press, 1989. xvii, 306p. Illustrations, appendixes, note on orthographies, references, index. \$29.95.)

Toward the end of his long life the Seneca called Governor Blacksnake (ca. 1753-1859), "nephew" of the famous Cornplanter as well as of Handsome Lake, recited an account of his participation in the Revolutionary war to a Seneca neighbor named Benjamin Williams. Williams, who spoke and wrote English, recorded this tale, together with a narration relating to some important events that took place before and after the period of conflict, in two versions. These two manuscripts are important texts because they provide a Native view of Seneca concerns during that period when the American nation was being formed and they offer insights into the workings of those Indian "nations" suddenly swallowed into the belly of a great fish after the Revolution.

Abler's editing plus his comments on these manuscripts are remarkable successes. The two texts have been skillfully combined and then divided into six sections covering the most important episodes described. The opening remarks from the narrative have been lodged in an appendix and replaced



by Abler's carefully crafted introduction, which effectively sets the stage for Blacksnake's telling of his story. Also impressive are Abler's long introductions to each of these six sections. Each of these provides the reader with sufficient historical and anthropological background, notes, comments, and interpretations to be able to read the precise transcription of the Williams text with ease. Abler's introductions, plus the other sections that he provides, would stand alone as an important contribution to our understanding of the Native participation in the American Revolution. His history of these events shows how a particularly modern problem is created—namely, the formation of a modern political state that must deal with the various native peoples living within its borders.

This is an important history of this era, draped over the fragile skeleton of Blacksnake's story. While showing just how much of Blacksnake's story may be real, Abler never hides his awareness that "For the most part the series of battles and encounters [Blacksnake] reports cannot with certainty be tied to known historical events" (p. 81). In reviewing the narrative and attempting to make sense of it, Abler clearly indicates that Native participation in the Revolution remains poorly known and difficult to document. Many suggest that it was probably overrated in importance and urge scholars to use new approaches to study this subject, such as the creation of more accurate Native population counts through genealogical research. The numbers of colonial volunteers and Indian allies participating at any given time or place may never be accurately known, but the clues provided by documents such as this are the only hope that we have for historic reconstruction as well as for generating specific information about the individual Seneca participants in these events.

A number of important aspects of history become quite clear in reading this book, in particular the lack of "political" unity among and within each of the Six Nations. Although the Oneida decision to back the rebels at the Battle of Oriskany is noted as a chief factor in splitting Native loyalties, at no time had there ever been a concerted military effort on the part of all the Iroquoian peoples. This absence of united action, even within a single Native "nation," reflects the sociopolitical organization of these peoples, and was a significant factor in limiting their potential in a major conflict.

Abler carefully describes the bravery and skill of the Seneca and other Native participants in these military actions. His estimates of Native numbers, however, provide the key to determining whether these peoples could have had any impact in the war beyond the terrorization and temporary disruption of frontier settlements. At one point Abler suggests that the Seneca had at most 1,000 fighting men (p. 90), yet a figure of under 400 might be considered more probable (cf. W. Engelbrecht, *American Antiquity* [1987]). The lower figure renders more dramatic the thirty or more Seneca killed at

Oriskany and in subsequent fighting, and also helps explain why the Seneca role in the war is seen as minimal by many historians.

The specificity of the commentary on the Blacksnake text provides a good basis for exploring the many questions left unanswered in this volume. Readers will find this interesting book to be a necessary supplement to A.F.C. Wallace's *Death and Rebirth of the Seneca* (1969).

West Chester University

MARSHALL JOSEPH BECKER

*Benjamin Silliman: A Life in the Young Republic.* By CHANDOS MICHAEL BROWN. (Princeton: Princeton University Press, 1989. xvi, 377p. Illustrations, note on sources, index. \$29.95.)

Imagine a biography of a major antebellum American statesman—e.g., Joel Poinsett of South Carolina, Benjamin Silliman's exact contemporary—that ignores its subject's views on slavery and states' rights. Doing so illustrates the major (and extremely serious) flaw of this biography of the longtime Professor of Chemistry at Yale, long recognized as one of early nineteenth-century America's leading men of science. Quite simply, Chandos Brown seriously downplays Silliman's scientific work to emphasize his social views and his relations with his friends and relatives, and the book reminds one of the clichéd précis of *Hamlet* that ignores the Prince of Denmark.

Brown tries to justify his focus by claiming that "Silliman's contributions to science, as such, were negligible," and compares Silliman unfavorably with "Baron Cuvier, Sir Humphry Davy, Roderick Murchison, Louis Agassiz, Charles Lyell, and even Charles Darwin." Should historians then slight Poinsett's views on government just because Calhoun's and Webster's and "even" Lincoln's were so much more important? What about Silliman's fame among his contemporaries? More important, much recent work in the history of science demonstrates the value of studies of scientific "practice" and "influence" as well as "contribution." As Brown notes that he chose to write a "cultural biography" (rather than a "scientific biography") largely because of Silliman's "*representativeness*," rather than his "*exceptionality*" (Brown's italics), he probably would admit the validity of these objections. An unkind reviewer thus would wonder if Brown wants to have his cake and eat it too.

Perhaps the best example of Brown's method and approach (and of his book's problems) occurs when he quotes from and paraphrases an entry in Silliman's *Journal of Travels* in Europe that describes a visit to a lead mine. As Brown mentions, Silliman emphasized the minerals he saw and wrote

that "I had the satisfaction of seeing, in their native situations, the most important ores of lead; the beautiful crystallizations of flour spar, and calcareous spar; the sulphat of barytes, native copperas, and extensive strata of limestone filled with the most beautiful petrifications" (p. 167). But rather than relating this passage to Silliman's important mineralogical studies (which he barely skims over), Brown focuses instead on Silliman's asides about the miners he saw and provides an exegesis on Silliman's thoughts on social issues. Silliman's views on such subjects may be as representative as Brown claims they are, and are probably worth studying. But one expects the biographer of an individual whose contemporaries saw him as a scientific leader to pick up on (at least) that which they recognized as important.

Brown thus devotes twelve pages to a paraphrase of Silliman's *Letters of Shahcoolien*—a juvenile social commentary written in the guise of a Hindu traveler through America—but none to Silliman's mature and highly influential *Elements of Chemistry*. Despite its title, Brown's chapter on "The Acquisition of Science" almost ignores Silliman's reading in chemistry. Instead, Brown again follows Silliman's letters from the period quite closely to emphasize such topics as the Silliman family's relations with its slaves. (Indeed, I often asked myself if Brown's purposes would have been served better by a highly selective and well-annotated edition of the Silliman family papers.) Brown also bases his report of Silliman's world-renowned analysis of the 1807 Weston meteor almost entirely on this correspondence. He does mention that "nine articles concerning [ the properties and origins of meteoric stones] had appeared in English and continental scientific journals in the previous decade," and claims that Silliman "was familiar with the work of" his European contemporaries. But he cites none of these articles, and gives no detail of his work. Similarly, Brown's discussion of Silliman's attempts to sell artificial mineral water in New York and other cities emphasizes the marketing and financial problems that he faced, rather than the chemical techniques he used to produce his waters and the theory that informed his work.

Toward the end of the book, Brown discusses (without much concern for its scientific content) the emergence of a nationwide correspondence network that centered on Silliman. He then traces, almost as an afterthought, how it led to the *American Journal of Science* that Silliman established in 1818 and edited for years, which did more than anything else to help American science professionalize itself. But Brown indexes the journal only under Silliman's name, and downplays the journal's content to emphasize the financial problems it caused Silliman. And although Silliman lived to 1864, established a major research school in geology and mineralogy, and trained such distinguished chemists of the next generation as Benjamin Silliman, Jr. (his son) and James Dwight Dana (his son-in-law), Brown closes his book in 1820.

Had historians of science dozens of scientific biographies of Silliman to consult, these remarks would be irrelevant. But Silliman has attracted only two book-length biographies to date, the most recent of which appeared in 1947. One hopes that cultural historians of the early national period will find much of value in this volume. Historians of science certainly will not.

*Worcester Polytechnic Institute*

MICHAEL M. SOKAL

*The War of 1812: A Forgotten Conflict.* By DONALD R. HICKEY. (Urbana and Chicago: University of Illinois Press, 1989. xiii, 457p. Maps, illustrations, note on sources, index. \$32.50.)

The causes, context, and consequences of the War of 1812 have intrigued historians, if not Americans in general, since Henry Adams scrutinized the administrations of Thomas Jefferson and James Madison nearly a century ago. Canadian territory, national sovereignty, maritime rights, political partisanship, and republican ideology have shared the blame and occasionally the credit for a war remembered for producing a new spirit of national self-confidence with neither territorial gain nor diplomatic concessions. Donald Hickey's new study focuses on the context of war, offering a strong narrative encompassing military and diplomatic strategy as well as political and economic developments.

Hickey believes that the War of 1812 was a mistake borne of an unrealistic perception of America's importance on the world scene and a naive insistence on maritime rights. Jefferson's rejection of the Monroe-Pinkney treaty with Great Britain set an unfortunate and unnecessary course toward commercial restriction, war, and economic dislocation. Had cooler—that is, Federalist—heads prevailed, peace, prosperity, and Anglo-American accord might well have characterized the first decades of the nineteenth century. Hickey has little patience with Republican domestic policy, either. He compares Republican intolerance of antiwar sentiment to the Sedition Act of 1798, condemns the administration's fiscal and manpower policies, and castigates Madison for failing to incorporate Federalists into his cabinet. The Hartford Convention, he argues, was a "triumph of moderation" (p. 280), led by men justifiably frustrated by the national government's failure to defend the region. That New Englanders largely refused to cooperate with the Madison administration's efforts to raise men, money, and material except on their own terms hardly enters Hickey's analysis at all.

Time, distance, leadership, and weather shaped the military stalemate that existed when hostilities ceased. War on the Continent allowed the American

army to replace its Hulls and Wilkinsons with Perrys and Jacksons before Britain mounted its most serious assaults. Extended supply lines tipped the military balance toward territorial defenders, giving the strategic advantage to American forces during most campaigns. Except in Maine and around the Chesapeake Bay, American forces repeatedly frustrated or defeated British campaigns on American soil. The loss or presence of dynamic leadership shaped the war's outcome, too, but so did the elements. Hickey's sound account of engagements on land and sea offers a subtle reminder of the impact of wind and weather on warfare during the Napoleonic era.

The United States's most important victory came in the Belgian city of Ghent, Hickey contends, not on the battlefields of North America. A distinguished team of American negotiators, including Henry Clay and John Quincy Adams, easily dispatched a demanding but second-rate British delegation. The British reneged on a promise to secure land for their Indian allies and backed away from an insistence on *uti possidetis* before agreeing to peace terms preserving the *status quo antebellum*. An equally significant accomplishment may have been the Republican leadership's transformation of a "futile and costly struggle" threatening national survival into a "glorious triumph" over "the conqueror of Napoleon and the Mistress of the Seas" (p. 309). Success on that front, Hickey begrudgingly concedes, gave the Republicans the high moral ground, doomed the Federalist party, and created the myth of the "Second War for Independence."

Hickey intended his history of the War of 1812 for "generalists and specialists alike" (p. xii). Both will find much of interest, though specialists may long for more insight into the war's impact beyond issues of national policy and strategy. How, for example, did opposition to the war manifest itself in the daily life of New England? As Hickey suggests in a "Note on Sources," much work remains to be done on this forgotten war.

*University of Tulsa*

LAWRENCE DELBERT CRESS

*Common Houses in America's Small Towns: The Atlantic Seaboard to the Mississippi Valley.* By JOHN A. JAKLE, ROBERT W. BASTIAN, and DOUGLAS K. MEYER. (Athens and London: University of Georgia Press, 1989. x, 238p. Tables, maps, illustrations, glossary of structural forms, bibliography, index. Cloth, \$50.00; paper, \$25.00.)

*Common Houses in America's Small Towns* represents an ambitious contribution to the ongoing study of the cultural landscapes in the eastern United States. The contribution rests not only in what this book attempts to achieve

through its geographical and statistical methodologies, but also in the indirect way in which it identifies the limitations of certain modes of architectural fieldwork. Based firmly in the cultural geographic tradition of Fred Kniffen, Wilbur Zelinsky, and Peirce Lewis, *Common Houses* forthrightly states its hypothesis: "Dwelling types and dwelling characteristics vary systematically both within and between the principal cultural regions of the Eastern United States" (p. 19). The authors' goal is to test five cultural geographic corollaries related to this hypothesis: "cultural change, regional variation, convergence, diffusion, and taste" (p. 19). Jakle, Bastian, and Meyer pursue their objective in an orderly and thoughtful manner—first by providing a concise literature review, then introducing their methodology, study towns, and aggregate findings. At this point the organization of the book shifts to the assessment of regional characteristics by building type. The heavily illustrated book concludes with a synthesizing chapter, glossary of building types, and bibliography.

The principal strength of *Common Houses* rests in its efforts to apply diffusion theory to an enormous landscape composed of twenty towns located in four culture regions of the eastern United States. Counting houses and categorizing their basic features constitutes tedious work, but it is the sort of labor that is necessary to advance the level of generality pursued by the authors. Because field research focused almost entirely on the exterior of houses, the typologies generated for common houses are limited. The attention to roof types, elevations, porch forms, and massing enables the authors to piece together a number of interrelated variables composing the core elements of a typology of sixty-seven dwelling types.

The central difficulty with *Common Houses* rests at the interpretive intersection of architectural variables, fieldwork, analysis, and interpretation. When Jakle, Bastian, and Meyer are working with basic externally visible architectural elements, they are on limited but solid ground. When their discussion moves toward plan types such as double-pile or shotgun dwellings, however, they go well beyond the limitations of their fieldwork methodology. As the authors and most fieldworkers in American vernacular architecture know, the interior arrangement of a house is often at odds with the exterior evidence. A cottage that appears to be two rooms deep from the outside may contain a single undivided space; a bungalow elevation may mask an eighteenth-century core. Moreover, buildings move through time and are consequently susceptible to plan alterations and changes in fashionable trim. The result is that even simple houses are often complex reflections of taste, social aspiration, acculturation, and economic ability. These contextual complexities make the detailed investigation of house form an archaeological pursuit well beyond the scale or intention of *Common Houses*. The authors would have done better to have left aside aspects of plan and to have kept

their focus on the sorts of characteristics that can be reliably gleaned from the exterior evaluation of houses.

Where Jakle, Bastian, and Meyer make their most praiseworthy contribution is the manner in which they have created a large-scale comparative study. The best of vernacular architecture studies in the United States continue to emphasize geographically limited studies exploring issues of competence and performance in building, building form and regional culture, and the economic and social history of architecture and community. *Common Houses* broadens the purview of such studies to accommodate an interregional perspective. While this work is not entirely satisfying in the way in which it accomplishes its goals, it importantly allows us to measure, reassess, and reexamine the evidence used by Fred Kniffen and his students to construct the models of cultural diffusion that still prevail in the study of American vernacular architecture. Thus, when the authors state in their conclusion, "Ours is an attempt at rediscovery by clearly classifying dwellings according to form in order to see them as structure types variously repetitive from place to place" (p. 206), they simultaneously are being overly specific and under-representing the scope of their attempt. As an interpretive guide to the ordinary architecture of small towns in the eastern United States, *Common Houses* should be read with a critical eye. As an invitation to the refinement of new methodologies and cross-regional perspectives, *Common Houses* stands as a potentially exciting prospectus for future study.

*University of Delaware*

BERNARD L. HERMAN

*Silent City on a Hill: Landscapes of Memory and Boston's Mount Auburn Cemetery.* By BLANCHE LINDEN-WARD. (Columbus: Ohio State University Press, 1989. xi, 403p. Illustrations, appendix, bibliography, index. \$49.50.)

Since the 1960s historians have found considerable evidence for changing social attitudes in the material culture surrounding death and dying. Led by the late French scholar Philippe Ariès, they have persuasively shown how Western notions of dying, death, and the afterlife can be used as keys to understanding the mentalité of diverse societies in various historical periods. The cemetery has become an important focus in this deciphering of the meaning of death. Scholars who have analyzed cemeteries have connected changing environmental and design patterns to evolving notions of religion, community life, domesticity, and individualism. A recent addition to this

growing body of literature is Blanche Linden-Ward's study of Boston's Mount Auburn cemetery.

*Silent City on a Hill* is a lavishly illustrated and intelligently written discussion of the establishment, in 1831, of the first American "rural" cemetery. Moving far beyond the usual antiquarian interest in burial grounds, Linden-Ward spends more than half of her book setting the stage for the creation of Mount Auburn. She traces the evolution of the graveyard from its medieval Christian origins as a barren spot next to the church through its transformation into a picturesque site of natural beauty, civic pride, and sentimental grief. In these beginning chapters she both delineates the Catholic and Protestant theological underpinnings of the romantic cemetery movement and articulates the economic and social changes that motivated a rethinking of how to house the dead. She sees the American cemetery as having its ideological and aesthetic origins in Europe. By first explaining the appeal of the English garden during the eighteenth century, Linden-Ward sets the background for the horticultural fascination of the Bostonian founders of Mount Auburn. The French concern for establishing a place for melancholic expression and philosophical musings as well as a site to demonstrate civic pride was reflected in the American fascination for secluded burial plots and commemorative funeral statuary. At the same time, however, Linden-Ward carefully points out how American cemeteries did not merely reflect European trends. Mount Auburn, for instance, maintained its picturesque and rural quality long after the Parisian Père Lachaise's avenues became crowded with burial tombs.

Linden-Ward successfully accomplishes the difficult balancing act of setting the cemetery in a wider context while providing a thorough discussion of the details of Mount Auburn's history. She is particularly skilled at unearthing the conflicts of interest that shaped the history of the cemetery, including political problems in Boston prior to its founding and rival claims to the role that experimental gardening should play on the grounds. Major figures in the cemetery's establishment, like physician Jacob Bigelow and horticulturist Henry A.S. Dearborn, are woven into the overall story without overwhelming the book's social/cultural orientation. Likewise, the analysis of the neoclassical, Egyptian, and Gothic revival grave markers and the history of the various architectural improvements are well-integrated into the text. Never falling into the trap of treating cemeteries as art objects not used by people, Linden-Ward describes the public abuse of the cemetery that motivated the founders to limit access, explains how the proper Beacon Hill society dinner frequently ended with a stroll through Mount Auburn, and shows how the isolation of the burial ground suited *men's* need for controlled mourning. In the epilogue, Linden-Ward evaluates the role that Mount Auburn played in the founding of other rural cemeteries. Her examination



of the continued church control over Catholic burial practices that precluded the establishment by Catholics of romantic, wooded cemeteries is particularly interesting.

*Silent City on a Hill* is a model study of the cultural and social significance of the cemetery. My only regret is that the book's high price tag will limit its appeal. We can only hope that soon a paperback version will appear so that undergraduates studying the American way of death can have this exceptional summary of the evolution of the rural cemetery.

*University of Utah*

COLLEEN McDANNELL

*The City Beautiful Movement.* By WILLIAM H. WILSON. (Baltimore and London: Johns Hopkins University Press, 1989. x, 365p. Illustrations, maps, note on sources, index. \$38.50.)

Historians have traditionally viewed the city beautiful movement as the mother of the planning profession, and yet for many in that profession this parentage has proved an embarrassment. With its emphasis on grandiose boulevards and pompous neoclassical civic centers, the city beautiful crusade has been regarded as a best-forgotten ancestor that lent little nobility to the profession's heritage. In *The City Beautiful Movement*, William H. Wilson forcefully challenges this standard viewpoint, lauding the goals and achievements of turn-of-the-century planners. For him, the city beautiful movement was not a foolish effort to mask urban problems with neoclassical façades but a serious initiative to confront those problems and to improve the city. Thus, he vigorously rejects much conventional wisdom and expresses unabashed enthusiasm for the movement.

In the first four chapters of this work, Wilson examines the origins and ideology of the city beautiful crusade. Although historians have often viewed the nineteenth-century park designer Frederick Law Olmsted as at odds with city beautiful principles, Wilson shows that Olmsted, in fact, laid part of the ideological foundation for the movement. Also significant was the municipal improvement movement that emerged during the late nineteenth century and encouraged citizens to commit themselves to upgrading their cities. Wilson, however, feels the Chicago World's Fair of 1893 did not instigate the city beautiful movement; visitors to the World's Fair did not rush home and immediately demand change. Instead, the fair caused certain architects to reconsider urban aesthetics. Moreover, city beautiful propagandists later exploited fond memories of the exposition by claiming that their movement would recreate the virtues of the much-acclaimed fair.

In the remaining sections of the book Wilson presents case studies of the city beautiful movement in Kansas City, Harrisburg, Seattle, Denver, and Dallas. For example, he writes of J. Horace McFarland and Mira Lloyd Dock of Harrisburg who battled for parks, improved streets, and pure water in Pennsylvania's capital. Wilson makes clear that these city beautiful advocates sponsored a program that balanced the utilitarian and the scenic. They were not narrow aesthetes but citizens with a broader perception of urban problems than critics of the movement have been willing to admit.

Wilson's study will undoubtedly serve as the authoritative account of the city beautiful crusade for many years to come. In previous studies of the movement, Daniel Burnham and his plans for San Francisco and Chicago have been the center of attention. By considering less celebrated applications of city beautiful planning in cities like Harrisburg and Seattle, Wilson's account proves especially rewarding. One might question his narrow focus on the American origins of the movement, ignoring to a large extent the European roots. The dominant planning figure in Kansas City, and to a lesser extent in Dallas, was George Kessler, a German-trained landscape architect, and the driving force on the Kansas City park board was its president August Meyer, a graduate of German universities. Yet Wilson finds the roots of Kansas City's city beautiful campaign in the Olmsted of Boston rather than their counterparts in Berlin. Moreover, Wilson's appreciative study of the city beautiful movement at times reads more like a defense brief than a balanced reexamination of the topic. On the whole, though, Wilson deserves the thanks of historians for reasserting the merits of a crusade that has been much maligned.

*Purdue University*

JON C. TEAFORD

*City at the Point: Essays on the Social History of Pittsburgh.* Edited by SAMUEL P. HAYS. (Pittsburgh: University of Pittsburgh Press, 1989. xvi, 473p. Illustrations, bibliography, index. \$29.95.)

The purpose of this collection is to synthesize the considerable secondary literature on Pittsburgh and to assess how that knowledge relates to our broader understanding of the processes of urbanization and urbanism. The contributors are almost all affiliated—as present or former faculty, or advanced degree recipients—with the University of Pittsburgh and Carnegie-Mellon University where most of this scholarship was generated.

Thirteen essays cover most of the topics one would expect. Particularly valuable are those by Paul Kleppner (politics), Richard Oestreicher (labor),

Joel Tarr (infrastructure), Edward Muller (regionalism), John Ingham (elites), and Roy Lubove (social welfare). The city's industrial history is alluded to repeatedly, and some passing references are made to technological change in the workplace. The omission of chapters on the economic history of the city and on the technology of the iron and steel industry, however, was disappointing. This may reflect the absence of a historian, and hence graduate seminars, at either school in those areas. Nonetheless, the subjects are critical to Pittsburgh.

The two most suggestive essays are the final chapters that place Pittsburgh in a national and international context. In "Pittsburgh: How Typical?" editor Hays draws out a number of the broader themes of the volume which are applicable to urban history generally: centralization vs. decentralization, enhanced personal opportunities and options, and persistent inequality and class. Hays also addresses the question of the distinctiveness of the city, although less successfully. He argues that Pittsburgh's population was more conservative than other cities in such cultural and social areas as the role of women, religious practice, and the importance of education—a tendency he relates to the strong traditional ethnic culture of the population. However, the assumption that Pittsburgh for the past century has been an exceptionally "ethnic" community has not been convincingly demonstrated. For example, among the nineteen cities with 250,000 people or more in 1910, Pittsburgh ranked twelfth by percent foreign-born and percent of foreign stock. What may have made Pittsburgh somewhat unique was that no single ethnic group dominated the foreign-born population. Germans, Irish, Poles, Italians, and Russians had roughly equal numbers in Pittsburgh; this characteristic, combined with the lack of growth in the twentieth-century steel industry, reinforced stable, ethnically conscious neighborhoods and slowed acculturation and assimilation. These aspects might have been explored in the essay on immigration and population by Nora Faires, which was rather disappointing.

The final chapter by Herrick Chapman compares the Pittsburgh experience with heavy industrial cities in Britain, France, and Germany. He focuses on the issues of elites, labor activity, ethnicity, and politics. This is a thoughtful and insightful piece, particularly for American historians who are unfamiliar with European urban and social history.

All of the authors identify areas for further research, and a number of recurrent themes emerge. Because of Pittsburgh's symbolism in the industrial age, most research has focused on the period 1850-1920, with the antebellum and post-World War I periods suffering relative neglect. Topics of more recent interest to historians have not yet been treated: consumerism and material culture, recreation and leisure, the family, changing opportunities for women, and the middle class. Several authors call for more attention to the interactions among class, ethnicity, race, and gender; more still needs to

be done with social mobility, particularly taking into account those interactions.

Although the volume does not purport to be a history of Pittsburgh, there is considerable discussion of the city's physical expansion and the role of topography, geography, and neighborhoods. In fact, Hays suggests that topography may have influenced the perceived conservatism of the population. It was all the more disappointing that there was not a single map in the entire book, either of the Pittsburgh region or of the city and its communities which are referred to repeatedly by name.

These essays will be particularly useful for those seeking a summary of the scholarship and the salient issues in the various areas of urban and social history, anyone interested in Pittsburgh, and historians interested in the uniqueness of place.

*Lehigh University*

ROGER D. SIMON

*Secret Ritual and Manhood in Victorian America.* By MARK C. CARNES. (New Haven and London: Yale University Press, 1989. x, 226p. Illustrations, appendixes, index. \$27.50.)

By looking at nineteenth-century fraternalism with a new question, Mark Carnes breaks important ground for the history of gender in the United States and for cultural history generally. Fraternalism has been seen previously as paralleling popular Protestantism and affirming middle-class values. Carnes has the historical wit to ask: why should middle-class men become so intensely interested in the performance of *secret* rituals during this period? What can explain the phenomenal success of the Red Men's new Adoption degree of 1868 (the ritual Carnes examines in most detail) in which a paternal figure reacts to the initiate with lethal anger before embracing him into the "bosom" of his new family of half-naked fraternalists? The achievement of Carnes's book lies as much in the way he leads us toward answers as in the answers themselves.

By establishing at the outset both the seriousness of ritualism and the ways in which its themes seem antithetical to the new industrial values, Carnes sets the stage for his demonstration of the relevance to fraternalism of Victor Turner's analysis of the role of liminal values in social life. Carnes documents the unanimity with which ritualists described the progressive initiation into their higher degrees as a process both of revelation and concealment, suggesting that the truths fraternal symbols mediated were too terrible to approach directly. He then develops the paradox that in an age of increasing

theological liberalism, revision of rituals moved from an earlier optimism toward a darker view (reminiscent of but also clearly divergent from Calvinism) of fallen men in search of an unfathomable God. The tendency in revision to exclude Christ is but one of the signs that evangelical Protestants had reason to be hostile to fraternalism as a competing alternative religion.

In a brilliant shift in Chapter 3, Carnes moves that competition center stage. He uses the National Christian Association's campaign against fraternalism, and, more generally, evangelicalism's dependence upon women workers to open up the importance of gender in the opposition to fraternalism. The central argument of his book emerges: feminization in home and church created a need for a counter-acculturation for the young middle-class men who for a time during the nineteenth century found such intense satisfaction in fraternal rituals.

In developing the logic of his interpretation so forcefully, Carnes may underestimate the continuing, if limited, usefulness of earlier perspectives on the lodges' appeal. I would guess, for example, that the excitement of the exotic and mysterious within the rituals helped attract those young men whose notion of respectability would make it impossible for them to be comfortable pursuing disreputable forms of excitement. My need to guess this, however, reminds us of a still largely unanswered question: who were these young men who spent so much time in ritual performance and what kinds of careers did they have? Carnes postulates a situation for white Protestant "middle-class" males regardless of occupation, which makes the attraction to fraternalism seem a very general one. But there is at least one moment when he suggests that those drawn to fraternalism may have come disproportionately from those with more marginal status and limiting experience, such as clerks spending a lifetime doing much the same thing. Addressing the question of social composition entails a very different kind of investigation, and one inappropriate for Carnes's focus here. Making us see in a new and compelling way the significance of fraternal ritualism is achievement enough, and one from which social and cultural historians have much to learn.

*Vassar College*

CLYDE GRIFFEN

*The Waterloo Mennonites: A Community in Paradox.* By J. WINFIELD FRETZ. (Waterloo: Wilfrid Laurier University Press for Conrad Grebel College, 1989. xxiv, 391p. Illustrations, tables, appendix, bibliography, index. Cloth, \$29.95; paper, \$19.95.)

Fifty years ago American and Canadian Mennonites, like the Amish today, were nearly all farmers in counties where their ancestors had been among

the earliest settlers. After World War II Mennonites left the farm for factory, office, or profession. Soaring real estate values in many North American Mennonite areas accelerated their movement from the land. Amish and Mennonites in Waterloo County, Ontario, experienced the same pressures for changes as their kinfolk in Bucks or Lancaster Counties, Pennsylvania.

J. Winfield Fretz has studied every aspect of the Waterloo Mennonite community's varied response to change in ethnic identity, understanding of religious faith, the work of the church, family life, education, farming, other occupations, leisure, health and welfare, politics, credit and mutual aid. As the first president of a Mennonite college within the University of Waterloo, he observed firsthand the crucial decades in this process of change. His book is important as a study of a religious community in multicultural Ontario, but equally as a bench mark for studies of ethnic and religious groups in Canada and the United States.

As Fretz noted, "no other Mennonite community . . . has the ethnic and organizational diversity of the Waterloo community." The 16,000 Waterloo County Mennonites are the heirs of three distinct migrations. In the aftermath of the American Revolution Mennonites from eastern Pennsylvania began a chain migration that drew relatives and friends to Ontario for some fifty years. They had the formative influence, and ties with Pennsylvania remained strong long afterward. A numerically smaller migration, between 1824 and 1860, brought Amish families direct from Europe. Mennonite refugees from the Soviet Union began coming in the 1920s with their own traditions and culture.

Although Pennsylvania Mennonites bought one entire township in 1803 and Alsatian Amish purchased another in 1824, Mennonites always formed a small minority in Waterloo County. They comprised only 12 percent of the population in 1881 and 5 percent a century later. As a self-conscious minority, dominant in certain townships, they initially maintained cultural boundaries in language, dress, and custom as well as religious faith. Time blurred these distinctions, but in every generation the majority accepted change. Old Order Mennonites, however, rejected any change and went their own conservative way from 1889, and congregations in the Waterloo-Markham Conference sought to slow the pace of change after 1939. Fretz has illuminating chapters on the relative success of the more conservative groups in maintaining the family farm, financing land purchases within the community, colonizing new rural neighborhoods, and developing their own school systems.

The historical chapters provide background for understanding more recent developments, which are the focus of the book. Viewed from the 1980s, the Old Orders appear as guardians of the tradition and the norm from which the majority departed. But a deeper probe into nineteenth-century Waterloo

County would suggest greater continuity and diversity. There can be no question that "farming has been the predominant way Mennonites earned their living," as it was for other North Americans. But Mennonites always had a variety of farm-related occupations. Many Mennonite-owned corporations grew organically from small family businesses processing milk, dressing poultry, or making sausage or cheese for market. The change was in scale.

The first Mennonite settlers in Waterloo County built mills and distilleries and laid out Eby's Town (later Berlin, now Kitchener). Their first pastor, Bishop Benjamin Eby, had a major interest in a furniture factory and a weekly newspaper. His sons continued the publishing business. Jacob Yost Shantz (1822-1896) was Berlin's leading industrialist. Although the percentage of Mennonites in the Berlin population was very low by 1871, his biographer noted "the involvement of Shantz and his numerous Mennonite business associates in town cultural and business life for several decades" (Samuel J. Steiner, *Vicarious Pioneer: The Life of Jacob Y. Shantz* [1988], p. 60). Waterloo Mennonites had diverse economic interests even then.

Fretz has given us a major study of the Ontario plain people. *The Waterloo Mennonites* will be a valuable source for anyone interested in the Pennsylvania Germans on either side of the border.

*Bluffton College*

RICHARD K. MACMASTER

*Where the Sun Never Shines: A History of America's Bloody Coal Industry.* By PRISCILLA LONG. (New York: Paragon House, 1989. xxv, 420p. Maps, illustrations, bibliography, index. \$24.95.)

This well-written survey of the American coal industry demonstrates how British technology and industrial organization were transplanted in the United States by British immigrants to these shores during the mid-nineteenth century. In an excellent summary the author shows how modern industrial capitalism first emerged in the anthracite fields where production and distribution were vertically integrated from mine to market. The focus of the book then shifts to the Colorado coal fields where, Long argues, the emergent corporate system evolved into one of social and political repression and led to the Ludlow Massacre of 1914. With an outraged public baying at his heels, John D. Rockefeller, Jr., the major operator in Colorado, recognized that reform was necessary in order for capitalism to retain its ideological hegemony. He therefore initiated a strategy of "corporate liberalism," better understood as company paternalism, becoming a pioneer in public-image manipulation as well as remaining a corporate mogul.

The author states that "in these pages, readers will find more than a glimpse of women's history, labor union history, business history, the history of work and of technology, and the history of working-class culture" (p. xxi). This diversity of "glimpses" represents both the strength and weakness of this book. Women often are completely ignored in books about miners, and Long's attempt to weave them into the fabric of coal field life is interesting, refreshing, and appropriate. But Long tries to do too much, and when this multitude of "glimpses" are blended into the chronology, conceptual and organizational clarity suffer.

This study is represented as a history of the American coal industry, but the analysis is confined primarily to the Pennsylvania anthracite and Colorado bituminous fields. Naturally, the question arises whether or not these fields are truly representative of the industry as a whole. I would contend that while the anthracite fields do have historical significance, unique geological and technological considerations alone render them unrepresentative. The Colorado fields were comparatively small and peripheral during this period. Therefore, evidence cited from the Appalachian and Central Competitive fields to support an analysis based on the anthracite and Colorado mines raises even more specific questions. Take, for example, the discussion on work rules. Mining anthracite coal differs significantly from mining bituminous; so, references to work rules in West Virginia or Ohio do not necessarily have direct meaning in anthracite.

A stridently argued secondary thesis is Long's contention that the British miners who established the United Mine Workers of America (UMWA) were ethnocentric and racist, and that their attitude hindered the formation of working-class solidarity required to wage an effective struggle against the new corporations. However, the author provides neither the dynamic that explains internal politics, nor does she take into account the precursor unions that provided the social-political context for the UMWA's emergence. In this analysis the British are simply conservatives who want to maintain their traditional control of work and the union. The radical unionists are the ethnic miners, the Irish in anthracite and the Italians in Colorado. This counters generally accepted knowledge and requires more analysis than is given here before the assertion is actually proven. By this account, Colorado's ethnic miners were sold out during the strike of 1913-1914 by the British-controlled UMWA for ethnocentric reasons. While the antagonism between ethnic coal miners and the UMWA must be acknowledged, it is also important to recognize that the union's phenomenal growth between 1890 and 1920 was a direct result of its success in organizing the newer immigrant groups from eastern and southern Europe, and that could not have occurred by ignoring non-British miners.



To write an overview of the American coal industry that focuses on two unique peripheral districts, rather than the Central Competitive and southern Appalachian fields where the great majority of miners were employed, is bound to distort that history and undermine its usefulness.

*West Virginia University*

RONALD L. LEWIS

*Pure Food: Securing the Federal Food and Drugs Act of 1906.* By JAMES HARVEY YOUNG. (Princeton: Princeton University Press, 1989. xiii, 312p. Illustrations, index. \$29.95.)

There is still ample room for conflicting interpretations of the activities that led to the Food and Drugs Act and the Meat Inspection Amendment of 1906. But James Harvey Young has assembled, in his most recent book, a masterful and coherent summary of all the key elements and participants in that complex struggle.

The origins of food and drug regulation in this country involve a convoluted and shifting intertwining of disparate allies and opponents. Their interactions, amid the changing economic circumstances and developing technology in an industrializing America, require a scorecard that, despite some notable studies—especially Oscar Anderson's *Health of a Nation* (1958) and Young's own earlier work—has remained largely blank. *Pure Food* reexamines and incorporates all the published material, contemporary and scholarly, and for the first time provides us with a completed guide to these activities. If the private plans of some of the players remain unrevealed, only the discovery of a cache of unsuspected manuscript material is likely to shed much more light upon them.

This said, there are limitations to Young's study. The flyleaf quotation from Daniel M. Fox is certainly correct when it emphasizes "the brilliance and persuasiveness of its critique of previous work on the problem. . . ." This is both its achievement and its limitation, for Young has not unearthed that cache of new material. But Young manages to make quite clear even the most complex machinations, by building upon his own earlier studies and the work of other researchers who have explored primary materials. To his credit, he is rarely content to accept blindly the assessments of other historians in the field; he has obviously re-read and weighed the contemporary published material used by them. And often he is able to make better use of those sources than the original researcher.

For the historian without specialized knowledge of this subject, Young's final chapter, an extensive historiographical essay, should be the most interest-

ing part of the book. He pays particular attention to the revisionist issue of business control of legislation, raised by Gabriel Kolko and others. But here Young is sometimes too willing to accept the conclusions of others.

For example, he completely endorses Ilyse D. Barkan's inadequately proven assertion that food and proprietary medicine producers were able to dictate to the Congress at will. Although Young is unfailingly gracious when his assessments differ from my own (*Fair Play in the Marketplace* [1986]), he mechanically repeats a criticism leveled by a reviewer. My argument that the *commercial* sophistication first achieved in New York soon "spread to encompass the rest of the country" is misinterpreted to mean that I belittle medical, chemical, and political developments after 1886.

There are other, minor, problems in *Pure Food*. Young tends to draw too sharp a dividing line between "professionals" and "businessmen," and, personally, I would have liked him to explore in more detail ethical questions faced by the various medical, pharmaceutical, and chemical professionals. Young provides a complete citation for his sources only the first time they are used, rather than the accepted practice of providing a complete citation for the first use in each chapter. Thus, in trying to backtrack an incomplete footnote, the reader may be forced to review several prior chapters. He does not include a bibliography.

These questions aside, there is no doubt that Young has made a significant contribution to a comprehensive understanding of the process by which early food and drug laws, and especially the laws of 1906, were enacted. Better than any other work, *Pure Food* clarifies the roles played by the various participants in this struggle.

*Kingsborough Community College*  
*City University of New York*

MITCHELL OKUN

*Domesticity and Dirt: Housewives and Domestic Servants in the United States, 1920-1945.* By PHYLLIS PALMER. (Philadelphia: Temple University Press, 1989. xvi, 241p. Figures, index. \$29.95.)

At first glance, Phyllis Palmer's exploration of American housewives and domestic service between 1920 and 1945 seems uncomplicated and straightforward. She says that she wants to determine "by what concrete historical processes women learned to do housework in certain ways and learned their places as mistresses and servants, and how these concepts fit into general social and legal attitudes toward women's work and women workers" (p. xii). She pursues her goal in seven clearly defined topical

chapters. Chapter 1 offers a brief, largely statistical, review of domestic service between the world wars, coupled with an explanation of Palmer's theoretical framework. Chapter 2 explores the image and expectations of middle-class housewives (designated by Palmer as MCHs) in the period. Chapter 3 analyzes how these MCHs organized housework and why they needed domestic workers to help them. Chapter 4 looks at the working conditions of servants and their attitudes toward the MCHs who employed them. Chapter 5 examines the government-funded domestic service training courses of the New Deal. Chapter 6 explains the failure of servants and employers to reform domestic service. Chapter 7 "theorizes" about important psychological associations of MCHs with dirt, sex, and the home.

Palmer insists that her conclusions challenge existing interpretations of housework between the wars. Other scholars, she says, believe that MCHs, finding it difficult to hire domestic workers, assumed more responsibility for doing housework, either because of new technological innovations that made housework seem easier, or because of a new "obsessive concern" with their homes and families. Palmer, on the other hand, concludes that MCHs clung to their servants, despite an apparent lessening of household labor, for psychological reasons—namely, a belief that by dumping still disagreeable tasks on racial and social inferiors, they confirmed their own self-worth as women.

But if Palmer's goals and conclusions seem straightforward, her means of achieving them are complex. For instance, utilizing theories of "postmodernist" racial and class oppression, she insists that race, class, and sexuality are relational, not absolute, concepts, and that the oppression of domestic servants by MCHs must be explained not in simplistic terms of race and class but within the framework of "notions of womanhood, of whiteness and nonwhiteness, and of middle-classness as well as working-classness" (p. 14). Palmer also informs readers that she will evaluate domestic service and American life from her perspective as a feminist and former civil rights worker. Thus, she may state without qualification, "This book rests on the proposition that Western culture depicts all women as potential seductresses and monsters" (p. 150). Then, just as one is trying to determine how such assumptions might skew Palmer's evaluation of the issues, she goes on to admit having a guilty conscience. This, we learn, stems from a number of "personal dilemmas," foremost of which seems to be a conflict between her "commitment to a . . . multiracial, multicultural women's movement" and her reliance on domestic servants to clean her own house (p. xi).

Some readers will find Palmer's theoretical framework, political agenda, and personal involvement appealing; others will think that one or more of these ingredients compromises her work. Palmer does leave too many questions unresolved and too many issues undeveloped to be fully satisfying. She

fails satisfactorily to justify the years 1920 to 1945 as a distinct period, and she underestimates the importance of personal relationships between employers and workers as a tool for understanding domestic service. Palmer's book is stimulating in its approach and use of sources, but many readers will harbor doubts about the accuracy of its conclusions and speculations.

*University of Arkansas*

DANIEL E. SUTHERLAND

*In Defense of American Liberties: A History of the ACLU.* By SAMUEL WALKER. (New York and Oxford: Oxford University Press, 1990. xiii, 479p. Bibliographic essay, index. \$24.95.)

Those who attempt to write accurate analytical histories of organizations are confronted with formidable challenges. Such work requires the creation of a delicate balance between the biographical elements of significant personalities and the policies they created. Samuel Walker, a professor at the University of Nebraska at Omaha and a member of the American Civil Liberties Union (ACLU), has struck that proper balance in this superbly researched and clearly written study. The result is a fascinating portrayal of the ACLU's defense of personal liberties in the context of the continuing American struggle to balance the conflicting demands for security and personal freedom in a curiously ambivalent society.

The author's treatment of the ACLU's activities in seven chronological-topical divisions, beginning with its origins in 1917-1919 and continuing through the present, seems justified by the evidence he presents. It enables him to describe the elements of continuity and change in the challenges to civil liberties and the ACLU's response to them during the past seventy years. Especially germane is his depiction of the impact of the tension generated in the ACLU by the forty-year debate that culminated in its decision to adopt an absolutist stand on matters of censorship and church-state relations. That struggle, and a continuing debate about whether the ACLU should concentrate its limited resources against a finite number of problem areas through organized ongoing projects or simply confront civil liberties challenges on an ad hoc basis, underscores an important conclusion: the ACLU is not and never has been a monolithic organization.

The author treats frankly what he regards as the less praiseworthy aspect of the organization's behavior such as the expulsion of Elizabeth Gurley Flynn from its Board of Directors during the McCarthyite scare of the fifties, and the recent attempts of some of its more influential chapters to dominate policymaking. He also presents a balanced discussion of the ACLU's critics,

noting that they range in nature from the "cheap shot" types like George Bush in the 1988 presidential campaign to the members of religious groups, political conservatives, and some liberals who find an absolutist position on First Amendment freedoms incompatible with their more limited perceptions of the scope of civil liberties in American society.

The author's judicious, incisive treatment of the leaders who shaped the ACLU's policies is one of the strong points of this work. His depiction of Roger Baldwin, the founder of the ACLU, as someone whose lifelong dedication to civil liberties was matched by his inability to share leadership of the organization with his associates, is one of many such candid assessments that Walker presents. In so doing, he effectively underscores the diversity of personalities and leadership styles that have found acceptance in the ACLU.

One point of criticism is in order. This reviewer takes issue with Walker's statement that none of the American socialist leader Norman Thomas's biographers treat his defense of civil liberties adequately. The Fleischman, Seidler, Johnpoll, and Swanberg biographies of Thomas, as well as a volume on his writings by this reviewer, devote extensive treatment to this aspect of his career. All, moreover, emphasize its central position in his hierarchy of values.

In summation, Walker has written what should be regarded as the standard work on the ACLU. He leaves no doubt that the expansion of civil liberties in American society has evolved out of the conflicts and controversies of real life, and that the ACLU has been the catalytic agent for much of that growth. Whether one agrees with its absolutist position or not, it is impossible to deny its impact on a society that is constantly struggling to balance order and liberty.

*Wichita State University*

JAMES C. DURAM

*Mind's Eye, Mind's Truth: FSA Photography Reconsidered.* By JAMES CURTIS. (Philadelphia: Temple University Press, 1989. x, 139p. Illustrations, note on sources, index. \$29.95.)

Archaeologists dream of brushing away the layers of sand and silt of an undisturbed site to reveal artifacts placed there centuries ago. *Mind's Eye, Mind's Truth* explores the degree to which the photographs of the Farm Security Administration differ from an objective view of an undisturbed site. The 80,000 photographs made by the FSA photographers under the direction of Roy Stryker have proved a particularly tempting site, providing the basis

for books ranging from the scholarly *Documenting America, 1935-1943* to the poetic aesthetic approach of *Let Us Now Praise Famous Women*. James Curtis also makes use of scholarly and aesthetic perspectives while, in a limited way, emulating monographs like *Walker Evans at Work* in showing us previously unpublished shots the photographers made in the process of arriving at the images that later became cultural icons.

But this is neither a monograph nor a simple historical investigation. It is a book that combines research and visual analysis of several key images produced by Walker Evans, Dorothea Lange, Arthur Rothstein, and Russell Lee in an attempt to deal with the problem of photographic objectivity. There are many aspects to this problem, but there is only one essential problem. Photographs are not undisturbed archaeological sites. They are artifacts created by human observers with very human preconceptions and sensibilities.

Each of us who has engaged in the conspiracy of the snapshot knows the problem. When we embrace on cue, only to disengage when the flash has stopped burning, we know the elemental urge to make the picture better than simple observed occurrences. Curtis details the use of several overt and subtle stylistic techniques used by FSA photographers in their attempt to make persuasive images.

Analyses of a limited number of major images by each of the photographers helps Curtis to distinguish their styles and motivations. Lange and Evans are seen as talented individualists, with Evans disposed toward simplification of formal design, and Lange more inclined to consider the social implications of her work. Lee and Rothstein are depicted as photographic functionaries more willing to satisfy Stryker's requests for images that would more directly support administration policies.

Curtis's choice of photographs will likely please those looking for good quality reproductions of many of the major photographic icons of the era. Lange's *Migrant Mother*, nine of Evans's photographs from *Let Us Now Praise Famous Men* (which were not actually done on FSA assignment, but appear in the archive), as well as Rothstein's *Fleeing a Duststorm* appear with many less well-known images that Curtis uses to support his analyses. The juxtaposition of Evans's work with large and small format cameras provides a clear insight into the effect of equipment on imagery. The inclusion of one of Lange's studio portraits illustrating her stylistic use of gesture to suggest intimacy provides a resonant insight that can inform our reaction to her FSA photography.

*Migrant Mother* and five other exposures made of Florence Thompson and her children are the central focus of an essay on Lange. From the other five images Curtis constructs a series of inferences about Lange's style and attitudes. He argues that Lange's published image showing only two young

children and a nursing baby more closely adhered to the standards of family size to which her middle-class audience subscribed than would inclusion of all seven of the subject's children. He also indicates some of the basic visual concerns, like simplifying background elements, that might have influenced Lange's composition.

Curtis and the book's designer, Richard Eckersley, should be commended for their exemplary integration of image and text. Like the photographers whose work he discusses, Curtis exhibits stylistic sophistication in his persuasive use of images. His lucid text is valuable as an informed and frequently insightful response to many of the icons of Depression-era America. Although personal conjecture is at times poorly distinguished from more objective analysis, *Mind's Eye, Mind's Truth* provides an important step in the process of gleaning historical insights from photographs, once we understand that photographic objectivity is a myth.

*Saint Joseph's University*

DENNIS WEEKS

## ERRATUM

In the photographic essay, "Eight Views of the Historical Society of Pennsylvania," *PMHB* 114 (January 1990), the caption to the illustration of the Patterson Mansion should read that the mansion was ". . . formerly owned by General Robert Patterson."

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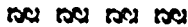
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