

## BOOK REVIEWS

*Friends in the Delaware Valley. Philadelphia Yearly Meeting, 1681–1981.*

Edited by JOHN M. MOORE (Haverford Friends Historical Association, 1981. 273 p. Appendices, index. Cloth \$8.95, paper \$4.95.)

*Philadelphia Quakers, 1681–1981.* By ROBERT H. WILSON (Philadelphia Philadelphia Yearly Meeting of the Religious Society of Friends, 1981. 132 p. Illustrations, \$6.95.)

The first of these two books commemorating the tricentennial of Quakerism in the Delaware Valley is a collection of eight essays. *Friends in the Delaware Valley*, writes editor John Moore, deals with “critical periods and significant aspects of the history of Philadelphia Yearly Meeting” (p.1). In fact, a group of four essays spans the whole period 1681 to 1955. The other four are topical, treating women Friends, relations with Indians, Quaker activity in Japan, and the American Friends Service Committee. Of these, only the inclusion of the essay on Japan begs for justification, whether it was extraordinary or typical of Yearly Meeting activity, or whatever, is not clear.

The essays vary widely in character. All are written by persons familiar with their subjects, but some clearly reflect the interests and discipline of professional historians whereas others appear antiquarian. Editor Moore comments in the introduction that “activities and accomplishments have been emphasized more than beliefs and doctrines.” And it is precisely those essays that include beliefs and doctrines as well as social history and comparisons with society at large that succeed. Some of the others resemble chronicles and single-dimension denominational histories.

Arthur Mekeel’s essay on the period 1681–1789 benefits from early Friends’ involvement in government and society in that period. That involvement lends variety and richness to his account which is more difficult to come by in nineteenth- and twentieth-century Quakerism. Mekeel’s account is most interesting in such dramatic episodes as the American Revolution. But he also includes institutional topics like changes in the disciplinary code of the Meeting and the geographical spread of meetings.

J. William Frost’s essay on the period 1790–1860 focuses on the Hicksite separation of 1827, a truly critical event in the Society’s past. Frost’s essay is the best brief explanation of the schism in print, and possibly the best of any length. The author knows the social and intellectual condition of the Society and the religious and social currents that swept over America and England in the early nineteenth century. He brings this knowledge to bear in accounting for the differences that erupted in the Society. In earlier

Quakerism, a variety of theological doctrines and different emphases coexisted. But exposure to doctrines and movements like evangelicalism, rational Christianity, and Biblical criticism destroyed the former tolerance and Friends succumbed to a desire to define and delimit "true" Quakerism. Frost is to be commended as well for informing the reader of the differences between nineteenth- and twentieth-century movements which, because they share the same name, confound the reader's understanding of the past divisions within Quakerism.

After the schism, the history of the Yearly Meeting almost necessarily becomes the histories of two Yearly Meetings, Orthodox and Hicksite, and the format becomes comparative. Frost and then Edwin Bronner for the period 1861–1914, compare the two with respect to their experiences, innovations, and rates of change. Women's roles, philanthropy to freedmen and Indians, pacifism, and temperance are the major respects in which the activity of the two are described and compared. Like Frost, Bronner commendably treats the changes in Friends' professions and theology, including those among English Friends.

With Herbert Hadley's concluding chronological essay, quality declines considerably. The account lacks cohesion and is occasionally obscure and elliptical. Beginning with Hadley, the authors variously tend to stay close to the minutes of the organization they describe. The result is sometimes a chronicle of the creation of committees, appointments, comings and goings of persons, and memorials to worthy laborers. While topics appear and disappear from the pages, variety is really an illusion because this is narrow denominational or institutional history. Margaret Bacon's essay on women Friends stands above the others, but it would profit from comparisons of Quaker women's lives with women at large in America.

*Philadelphia Quakers* professes to be "simply a tercentenary family album." It is a happy example of just that. Whereas the first book appears not to know exactly what it is about and the audience it is addressing, this one aims at Friends wanting to reminisce and any person wanting an introduction to Quakers in Philadelphia. It hits both. More than half pictures and other illustrations, it is delightfully eclectic, treating among other things meeting houses, schools, Quaker resorts, furniture makers, silversmiths, quilting, botany, golfing, as well as William Penn, Indians, and the American Revolution. The narrative occasionally shows some wry humor — the long-time Quaker retreat at Atlantic City, The Chalfonte-Haddon Hall, ultimately ends up as the city's first gambling casino, we are informed. Perhaps in an attempt to relate to the best known features of American history, there is too much treatment of Benjamin Franklin and the American Revolution. Both were at times hostile or dangerous to Friends. The organization of topics is

occasionally elusive and one may question at least two interpretations of past episodes. But these are small faults which do not diminish a good read.

*University of Arizona*

JACK MARIETTA

*Inside the Great House: Planter Family Life in Eighteenth-Century Chesapeake Society.* By DANIEL BLAKE SMITH. (Ithaca, N.Y.: Cornell University Press, 1980. 305 p. Illustrations, tables, figures, index. \$17.50.)

Like much else Edmund Morgan has written, *Virginians at Home* has dominated and even defined its field for years. But unlike his other works, it has done so by default more than by merit. For three decades it has been almost our only account of eighteenth-century southern family life.

Now, at last, it has found a successor, and a remarkably appropriate one at that. Daniel Blake Smith's *Inside the Great House* seeks, as *Virginians at Home* sought, to penetrate the intimate affections and familial sensibilities of the planters and their households. Setting himself against prevailing winds of scholarly fashion which emphasize the assessment of demographic and social structure by systematic study of the wills and inventories of the population at large, Smith tries, as Morgan tried, to discover the "emotional texture" of Chesapeake domesticity by impressionistic interpretation of the personal documents of the region's elite.

Even as Smith shares with Morgan a preoccupation with the interior life of the plantation, though, he surpasses his predecessor in every respect. *Inside the Great House* is denser and more intricate in design and richer by far in the range of its research. It is at once more sweeping and more probing. It asks more provocative questions and offers more imaginative answers. It is, in short, our new standard, and an indispensable point of departure for all further inquiry.

All that acknowledged, it is also a work of disturbing irresponsibility to its own arguments and evidence; and if it stands as our standard for long, it will instill confusion as surely as it will inspire insight. *Inside the Great House* may be too full of the feel of early southern family life, and too prolific of suggestive ideas about that life, ever to be eclipsed as utterly as it eclipses *Virginians at Home*, but its power is precisely in its parts. It is more a melange than a satisfyingly sustained composition.

It does purport to have an argument, and the argument can be rudely recapitulated. Smith traces the trajectory of planter family experience through three phases. The first, which spans the seventeenth century, is marked by

emotional disengagement and structural instability. The second, which runs roughly through the first half of the eighteenth century, is a "golden age" of ordered patriarchy. And the third, which dominates the later eighteenth century, is a period of advancing privatism in personal and familial values. The shift from the second to the third phase especially appears to Smith an "unmistakable reorientation," indeed a "transformation." Hitherto "muted" affections are allowed overt and even extravagant expression. An "overriding concern for order and clear lines of authority and obedience" gives way to a "growing belief in the autonomy of sons and daughters" and a distinct child-centeredness. And formerly open, extensive families turn their psychic energies inward to "focus on an intimate, sentimental family unit that stood apart from the larger society as a private enclave."

But it would be a mistake to take these putative developments more seriously than Smith himself does. He certainly never substantiates the sequence he asserts, and he often displays an indisposition even to try. A page after outlining the "dramatic shifts" of the eighteenth century, for example, he concedes the "conservative" quality of family life. Discounting the decisive metamorphoses he has just described, he declares that "family values persist even in times of profound social change"; they resist amendment altogether or adapt "almost imperceptibly."

Time has always been a trial for historians, and for family historians more than most. On the one hand, they share the predilection of the historical profession for finding change. On the other, they actually encounter rather less of it than other historians in the sources they study.

The need to relieve that tension has impelled many a family historian to force his evidence into one or another overarching social theory that would enable him to discern some sort of evolutionary order in his discordant data. Certainly it impels Smith. The privatization he proclaims is an elemental expression of the premier paradigm of contemporary social science, namely, linear modernization.

But Smith's evidence and insights relate obliquely at best to his modernization model, and ultimately he acknowledges as much. In his concluding chapter he blithely abandons large parts of the model, leaving it exposed for what it was all along, a ramshackle stage on which to mount an array of intriguing observations.

And in the end those observations, and the fullness of the family life they evoke, represent the real strength of Smith's study. *Inside the Great House* may be infuriatingly indifferent to the demands of its own declared design, but it has arresting things to report about the rigidity of southern sex roles, the genesis of gender identity in planter families, and the sexual division of responsibility for child rearing. It may be maddeningly unconcerned for the

logic of its largest propositions, but it offers a host of illuminating hunches about the sources of self-confidence in children, the nature and extent of their dependency and autonomy, and the place of parental intrusion in their affairs.

Exactly as he set out to do, Smith takes us inside those early gentry families, perceptively and provocatively. He poses pregnant questions about ratios between emotional control and release. He is acutely sensitive to the prerogatives of patriarchy. He does some suggestive calculations of the shifting balance between conjugal family attachments and ties to wider networks of kin, friends, and neighbors. If he is at his worst when detailing grand dynamics he cannot demonstrate and often cannot even keep straight, he is at his best when retailing the rich record he has unearthed, enabling those eighteenth-century planters to speak for themselves across the years.

*University of Pennsylvania*

MICHAEL ZUCKERMAN

*For Want of Trade: Shipping and the New Jersey Ports, 1680–1783.* By JAMES H. LEVITT. The Collections of the New Jersey Historical Society, Volume 17. (Newark, N.J.: New Jersey Historical Society, 1981. xii, 224 p. Maps, illustrations, appendices, bibliography, index. \$21.45.)

New Jersey — the keg tapped at both ends, emptying westward into the port of Philadelphia and eastward into the port of New York. It is not a new image. Nor is it anything but an accurate one. For no other sector of life was it more accurate than for overseas shipping and trade. The import and export trade of colonial New Jersey centered not in its own official ports of entry (POE) Perth Amboy, Burlington, and Salem, but in POE New York and POE Philadelphia. (It might be worth reminding ourselves that the colonial POE were not places but regions, established for purposes of collecting duties and keeping records.) Addressing the impact of all of this, Levitt himself repeats the well-known statement of Gov. William Franklin who, in 1774, reported to the Secretary of State for the Colonies, William Legge, Earl of Dartmouth, that the records of the New Jersey POE were useless if one wanted to learn anything about New Jersey's shipping or trade (p. 141; and C.O. 5/992, fol. 42, PRO). These records

can be of very little if any use in forming an Idea of Quantity of our produce sent to foreign markets. New York and Philadelphia are in reality the Commercial capitals of East and West Jersey, and almost all of the Articles we import for Home Consumption are from one or the other of those Cities, of which no Entries are or can well be made at our Customs House, consequently we have no way of coming at an exact account of them

Historians of colonial New Jersey have long recognized what Franklin's words — and the reality that stood behind them — have meant for their work. The naval officer shipping lists (NOSL) of the three New Jersey POE, and all of the other accounts compiled in the custom houses of those POE, recorded only a tiny fraction of New Jersey's maritime activity. The commercial and financial transactions of the merchants of the colony with regard to international trade, shipbuilding, and shipowning were entered into the records of POE New York and Philadelphia. Thus one cannot take the measure of New Jersey's maritime activity by counting up the entrances and clearances, the imports and exports, the ships built and registered in the records of POE Burlington, Salem, or Perth Amboy. Conclusions about New Jersey's shipping and trade based on the records of its own POE simply are not valid. Levitt has based his analyses of New Jersey's shipping and trade on the records of those three POE. His conclusions are not valid.

To be fair to Levitt, he does say in his book much of what I have said above. But he consistently fails to follow through on its implications. The result for the reader is a confusing "see-saw" effect. On one page (p. 36), Levitt tells us that by law and by custom no entries were made in the New Jersey NOSL of goods carried or ships moving between the New Jersey POE and POE Philadelphia or POE New York. Yet throughout his book he uses his computerized compilation of the data from the three New Jersey POE to show how small or non-existent was the trade of those POE (e.g., p. 34). Obviously considerable shipping activity existed in all three of New Jersey's POE, almost all of it moving across the Delaware and the Hudson, and none of it recorded as such. There is simply no way of telling using the records that Levitt used.

Equally obvious is that Levitt is caught in a bind that has ensnared all-too-many students of the colonial economy. We must abandon analysis of colonial economic history rooted in the units of colonial political history. New Jersey, the keg tapped at both ends, existed as part of, and indeed as a bridge between two economic regions, the Delaware River Valley and the Hudson River Valley. The people at the time realized this and behaved accordingly. Only when colonial historians write economic history on the appropriate regional bases will we begin really to understand what happened in the early American economy. Only then will we be able to avoid the dilemma that so hobbles Levitt's book.

*University of Maryland, College Park*

JOHN J. MCCUSKER

*A People in Revolution: The American Revolution and Political Society in New York, 1760–1790.* By EDWARD COUNTRYMAN. (Baltimore: Johns Hopkins University Press, 1981. 388 p. Illustrations, map, appendices, index. \$24.50.)

The language of historical criticism is unfortunately, if unavoidably, formulaic. The adjectives that pepper book reviews, whether in praise or blame, are so predictable and the rhetorical devices that accompany them are so inflated that readers often look between the lines to discern a reviewer's real point of view. I begin my review of Edward Countryman's *A People in Revolution* with this observation only because I want my assessment of this extraordinary book to be read quite literally. This book *is* extraordinary. It *is* a landmark in the historiography of the American Revolution. And it *does* deserve close reading by anyone with even a passing interest in what the Revolution was all about. It is the most strikingly original book on the Revolution to appear since Gordon Wood's *The Creation of the American Republic* — and, in some ways, it surpasses even that masterpiece.

Countryman proposes to look once again at the Revolution in that most maddeningly complex of provinces, New York. Carl Becker set the terms of historical debate on the Revolution in New York in 1909. Since that time, many historians have attempted to settle the "home rule/who should rule at home" question; a few have even tried to transcend it. Countryman, no less than his predecessors, is to some extent a prisoner of previous historiography, although he wisely minimizes references to Becker and those scholars who followed him into the thicket of Revolutionary New York. Still, it must be said that the simplest reading of Countryman would show that he has offered a sustained and overwhelmingly persuasive case for the view that the Revolution was a struggle over who should rule at home in New York as much as it was a battle over home rule in the Empire. But it must also be quickly added that the nature of Countryman's evidence, the structure of his argument, and the sophistication of his methods distinguish the book from all its predecessors, whether concerned with New York or any other colony.

Perhaps the best way to indicate the singularity of Countryman's accomplishment is to compare his work with that of scholars in the two currently dominant schools of Revolutionary historiography. One of those schools, associated with Merrill Jensen and his students, has consistently maintained that neither the Revolution nor its aftermath can be comprehended without careful attention to economic interests, social experience, and the conflict they engendered. Scholars of this school have tended either to minimize the importance of ideas or to regard them with suspicion. In contrast, Bernard Bailyn and his students have argued that ideas must be taken seriously and

that such conflict as did exist in Revolutionary America deserves less emphasis than the overwhelming fundamental consensus which provided the basis for the creation of the American nation. Members of both schools, particularly Gordon Wood and Jackson Turner Main, have significantly advanced our knowledge of the Revolution as both process and event. And yet a lingering dissatisfaction remains because no scholar has managed to combine the two perspectives in a manner that would permit us simultaneously to take ideas seriously and to see their relationship to material circumstances. Wood's prescriptive statements notwithstanding, rhetoric and reality remain quite separate in recent work on the Revolution. Or at least that was the case until the appearance of Countryman's book, for he has given us the best of both worlds and has, as a consequence, transcended the conventional historiographic categories in breathtaking fashion.

Any summary of so daring and inventive a book as *A People in Revolution* must necessarily impose an unwarranted simplicity upon a very complex interpretive scheme. In the most general terms, however, it can be said that Countryman has sought to discover the relationship between socio-economic relations and politics in New York between the onset of resistance to Great Britain and the ratification of the Constitution. During that thirty-year period he finds that New Yorkers divided with great consistency and that such division, if deeply probed, was often class-based. In the 1760s, he writes, New York was a "troubled, unstable, class-ridden province." During the years of the Revolution itself a workable coalition of groups did emerge among New Yorkers of varied economic status, political experiences, and social loyalty. Victory in the war, however, permitted the coalition to fragment. The fragments of the coalition realigned in a consistently partisan state assembly that became the focus of intense controversy over issues of policy that spoke directly to what the precise impact and significance of the Revolution for New Yorkers could and would be. By the mid-1780s, "a combination of conservative institutions and radical policies led to the stabilizing of the new order." The "new order" was one in which a liberal state, governed through partisan politics, took responsibility for ordering a bourgeois society. By 1790, Countryman contends, the "violent and rapid" process of Revolution had "created a New York that was new in leadership, institutions, population, consciousness, and geography." Class conflict played a significant, though not exclusive role in this process of transformation, as did ideology and previous political experience.

Because Countryman's argument is so wide-ranging and because he understands the Revolution as a social as well as a political event, he has had to cast an equally wide net in gathering evidence. In so doing, he reveals a breadth of theoretical, literary, and methodological skills that other historians



will justly admire. Theoreticians as seemingly antagonistic as Samuel P. Huntington and Lenin provide the basic organizational principles for the analysis; their writings illuminate the evidence without being obtrusive. And the evidence itself is wonderful. Countryman is as comfortable discussing crowds and ritual behavior as he is analyzing the results of a quantitative study of voting behavior in the New York Assembly. He is as surehanded in delineating the structure of social and economic relations as he is writing the history of political parties and institutions. And he is as good with texts and ideas as anyone now writing on the Revolution. His brilliant discussion of Alexander Hamilton is simply the best available.

In short, this book not only demonstrates the explanatory weakness of the prevailing historiography of the Revolution, it is also a creative application of all the best methods that historians have developed in recent years. Underlying Countryman's literary grace and technical skill is the conviction that genuine historical understanding can never be the province of one or another sub-discipline. Countryman's example convincingly establishes a maxim many historians of the Revolution have acknowledged but too few have been willing to attempt to prove: an event as widely and as profoundly experienced as the Revolution demands that social, economic, intellectual, and political history all be brought to bear upon one another in systematic fashion. By fulfilling so intimidating an agenda Edward Countryman has produced a book from which all historians, regardless of field, can learn a great deal. *A People in Revolution*, despite its relatively narrow geographic focus, is simply one of the best books ever written about the American Revolution. It deserves to be one of the most influential.

*Princeton University*

DOUGLAS GREENBERG

*Princetonians, 1776–1783: A Biographical Dictionary.* By RICHARD A. HARRISON. (Princeton: Princeton University Press, 1981. xli, 498 p. Illustrations, appendix, index. \$35.00.)

This splendid volume, the third in a series that had its beginnings a decade ago, provides a wealth of information about the one hundred-fifty-seven men who attended Princeton University (then the College of New Jersey) during the turbulent era of the American Revolution. To a remarkable degree, these Princetonians achieved distinguished careers. From their ranks came three members of the Constitutional Convention, fourteen congressmen, eleven senators, and five cabinet members, as well as two score others who filled

prominent positions as governors, judges, and legislators in their respective states. Among the most notable figures on the national scene were John Armstrong, Jr. (1776) of Pennsylvania and New York, Jonathan Dayton (1776) of New Jersey, William Branch Giles (1781) of Virginia, Edward Livingston (1781) of New York and Louisiana, and Nathaniel Macon (1778) of North Carolina.

Recruited mainly from the upper echelon of American society, the undergraduates came from eleven states, with the largest representation from New Jersey (45), Pennsylvania (28), New York (23), and Virginia (21). Compared to earlier classes, there were fewer entrants from New England and many more from the South. Chiefly because of the unsettled times — which diverted a third of the young men into military service, strained the finances of many of their families, and even disrupted the work of the College — only eighty-two of those who matriculated remained to take their degrees. Formerly nearly ninety percent of those enrolled had graduated. Not surprisingly, in view of the drastic decline in the proportion of graduates, far fewer members of this contingent entered the professions — law, medicine, and the ministry — than had their predecessors. Especially striking was the decline in ministers from over forty percent of the earlier classes to about twelve percent. In fulfillment of Princeton's avowed mission, however, all but one of the clergymen from these years affiliated with the Presbyterian Church. It is remarkable that only four men became educators, by far the most notable of whom was Ashbel Green (1783), the President of the College from 1812 to 1822.

Aided by generous financing, in part from the National Endowment for the Humanities, the editor and his fine staff have obviously spared no effort in compiling this biographical dictionary. The sketches, varying in length from less than a page to the fifteen pages devoted to Ashbel Green, are fastidiously researched, ably composed, and fully documented. Reproductions of the portraits of thirty-six of the subjects add life to the volume. A useful appendix lists the students conveniently by place of birth, state of primary residence, occupation, and public office. There is a generous, detailed index. To cap it off, the book is so handsomely designed as to make browsing in it a delight.

Fittingly enough, this volume is dedicated to the memory of the late Professor Wesley Frank Craven, distinguished historian of colonial America, who during the years of his "retirement" contributed so much to the success of this magnificent enterprise.

*Rutgers University*

RICHARD P. MCCORMICK

*The British Soldier in America. A Social History of Military Life in the Revolutionary Period.* By SYLVIA R. FREY (Austin. University of Texas Press, 1981. xii, 211 p. Appendix, bibliography, index. \$25.00.)

*Goodmen The Character of Civil War Soldiers.* By MICHAEL BARTON (University Park The Pennsylvania State University Press, 1981. vii, 135 p. Appendices, sources, index. \$12.95.)

War, its strategy and tactics as well as its political and diplomatic implications, has long been a central concern of historians. Biographies of wartime leaders, civil and military, also have traditionally occupied an important place in historical literature. From David Ramsay's contemporary history of the American Revolution and the early biographies of Washington to Russell Weigley's recent study of Eisenhower's generals, American historiography has paid due notice to the military and political implications of military confrontation and to the roles played by important individuals while the republic has been at war.

More recently, historians, some utilizing social science techniques, have explored the social and intellectual dimensions of warfare that lay beyond the battlefield and well outside the purview of international strategic conclaves. Bell Wiley's pioneering studies of the Civil War soldier were among the first to study the impact of warfare on the common soldier. The provocative work of John Shy has served to open many new paths of inquiry for students of the military in the revolutionary era. Charles Royster's studies of the revolutionary generation at war are a model for anyone concerned about war as a force in intellectual and cultural development. Important, too, has been the work of a host of historians who have examined the social composition of the nation's early fighting forces through demographic and economic analysis. Together, these studies have markedly changed a discipline that once defined itself in terms of battlefield strategy and international power politics.

The books under review here are part of this subcurrent in military history. Sylvia R. Frey and Michael Barton are both interested in the collective personality of soldiers, though for very different reasons. By examining the institutions and environment that shaped the life of the common soldier in the British army during the American Revolution and the "adjustment of soldiers to it," Frey seeks to abstract group personality patterns that will "provide at least flashes of insight into the life and mind of the eighteenth-century soldier" (p. xii). Barton is far more ambitious. Using content analysis — the method for which he carefully explains in three appendixes — of 419 published diaries and letters written by Civil War soldiers and officers, he attempts to examine and explain national character. His work, "a quantitative sociolinguistic case study in historical psychological

anthropology" (p. 4), seeks in particular to measure the American commitment to self-control, a concept that he identified as central to the pervading Victorian mentality of mid-nineteenth-century America. Analyses of the soldier as a instrument of power politics these are not.

Frey's *The British Soldier in America* offers a fascinating glimpse into eighteenth-century military life. Life was harsh at best. Death was ever present. Disease, not the ball and bayonet, was responsible for eighty to ninety percent of military fatalities. Cramped living conditions (as many as thirty-two soldiers in a room in Boston in 1775), standards of personal hygiene that eschewed bathing and called for bedding to be changed once a month, malnutrition and inadequately supplied uniforms, which when available hindered circulation while repelling neither the sun nor rain, combined with primitive medical treatment to make dysentery, smallpox, and a host of other maladies the scourge of every soldier. Life in camp was tedious. Close-order drilling, guard duty, and a dreary array of tasks ranging from chimney cleaning to burying old meat filled a day that began at sunrise and ended at sunset. In the field, fatigue replaced tedium. Bearing sixty-pound packs and frequently force-marched over demanding terrain to meet the enemy, soldiers often faced the horror of battle wet, cold and hungry. Soldiers assigned to the southern campaigns found service particularly exhausting, being forced to hazard frequent stream crossings and to endure debilitating diseases, long marches, and the inadequacies of a greatly overextended system of supply.

Yet the great majority of soldiers remained loyal. Though not statistically measured in this study, desertion seems not to have been a serious problem. Paroled prisoners of war and soldiers otherwise separated from their units seem to have travelled great distances to rejoin British forces. Frey argues that the institutional totality of military life, especially its paternalistic and authoritarian structure, left soldiers dependent on the army for social, personal and psychological satisfaction while also instilling personal loyalty. Kinship ties, actual or implied, were important too. Though evidence is limited, it does seem that sons followed their enlisted fathers into military service, often serving in the same unit. That men served in the same unit throughout their careers built esprit and solidarity as well. The conscious effort by regimental recruiting officers to draw enlistments from particular villages or regions also contributed to internal cohesiveness and loyalty by drawing upon pre-existing kinship ties and personal relationships. Equally important was the experience of having survived together the trauma of the battlefield.

Understanding the loyalties of British soldiers is a speculative business at best, there being no diaries or letters extant, and Frey concedes as much. Still she argues that a particular "publick spirit" existed among soldiers

derived in part from the fact that they came to the army from "respectable family and occupational" backgrounds. Several problems arise here, not the least of which is the assumption that soldiers from less than "respectable" families and occupations were not susceptible to the same institutional and psychological pressures that shaped their social and economic betters. But even conceding that to be the case, I am not persuaded that the common soldier, typically thirty years old, ten years in service, and single, would have claimed the "respectable" antecedents that Frey credits to him.

Her analysis of the composition of the rank and file is based on an admittedly limited number of recruitment records drawn from the Fifty-eighth Regiment of Foot and the elite Coldstream Guards which cover in the first case a span of nearly fifty years ending in 1800 and in the second a three-year period during the American Revolution. Frey is clearly aware of the limitations of her data and resists over-generalization. However, her conclusion that these units were composed of individuals quite different from the "scum of society" so often described as having filled the ranks of professional armies in the eighteenth century is unconvincing. Recruits were, as Frey says, most often drawn from regions adversely affected by technological and demographic changes in English society. Many had been textile workers and shoemakers, though a significant proportion were displaced common laborers. In contrast, workers in the more stable coal and iron industries, for example, seldom volunteered for military service. In short, recruits were the under- and unemployed, burdens on an English society insensitive to the social implications of industrial change. If not the "scum of society," a phrase that begs for definition, recruits would hardly have been described by their contemporaries as the sons of "respectable families." Nor is it likely, since most apparently enlisted at age twenty, that they would have derived any discernable psychological benefit from a brief apprenticeship in a craft clearly in decline.

Nevertheless, Frey's is a well researched and eminently readable book which, though it falls short of defining the collective personality of the common soldier, does offer important insights into the institutions and environment that shaped the British regular's life in North America.

Michael Barton's *Goodmen: The Character of Civil War Soldiers* is less about soldiers than the language they used and its implications for understanding regional differences on the eve of the Civil War. Barton argues that Northerners and Southerners, officers and enlisted men, shared a common value system more concerned with the "prosaic values" of moralism, progress, religion, achievement, and patriotism than the "ideological values" of freedom, equality, individualism, and democracy. Nevertheless, Southerners, especially officers, exhibited those values more emotionally than did

Northerners. Similarly Southerners, again especially officers, expressed a greater concern with character (particularly expressed in terms of individualism and will) and used a more emotional style of expression. In an analysis of how, rather than what, soldiers wrote, Barton describes Northerners as more methodical and less expressive than Southerners. He notes that Southern officers were less methodical and more verbose than the other groups, while Northern enlisted men were most laconic. The conclusion that Barton draws from all of this? That Southerners were, as Tocqueville said, "more given to act upon impulse" than were Northerners.

Barton's analysis relies on methods well-established among the practitioners of content analysis. With the confidence of a scientist he dissects the language of printed letters and diaries in search not of insights into the Civil War experience but of the collective personalities of the North and South. Words, not their context, are the basis for his analysis. And insofar as one is inclined to accept that approach to history, the work is well done, though it does seem that the convenience of printed sources more than the search for "scientific truth" guided the decision to rely only on printed sources.

At one point, Barton reminds the reader, quoting William Blake, that "To generalize is to be an idiot. To particularize is alone distinction of merit" (p. 57). Curiously it is at that point that Barton leaves behind his own propensity to particularize and plunges headlong into wholly unsubstantiated generalizations. His suggestion that the mere frequency of southern letters of condolence extant in printed sources offers conclusive evidence that Southerners "were more emotionally expressive and more concerned with character than Northerners" or that child rearing techniques (what they were is not discussed) must have had a bearing on the development of regional personalities because "common sense" tells us that "as the twig is bent, so the Tree's inclined" (p. 69), borders on the absurd. He is on thin ice too when he tries to use a psychological anthropology model to explain the South's greater emotionalism as an expression of a society "having relatively lower accumulations of goods and lower technologies" than the "bourgeois" North (p. 77). How Barton then explains the laconic style of the North's still mostly rural citizens is unclear. Certainly the plantation south fits this model poorly, even if one accepts, as Barton does, Genovese's argument that Southerners were pre-capitalists and Richard Brown's thesis that the North was more modern than the South. Barton may have shown that Civil War diaries reflect a southern propensity to reject an order and discipline increasingly pervasive in northern society, but far more research, using something more than content analysis and a handful of secondary sources, is necessary before he can explain why such differences existed.

Readers concerned about what, rather than how, the Civil War soldier

thought about his condition and circumstances will still find Bell Wiley their best guide.

*Texas A&M University*

LAWRENCE DELBERT CRESS

*Foundations of Power: John Marshall, 1801-1815.* By GEORGE LEE HASKINS and HERBERT A. JOHNSON *The Oliver Wendell Holmes Devise History of the Supreme Court of the United States, Volume II.* (New York: Macmillan, 1981. xiv, 687 p. Illustrations, tables, index. \$60.00.)

Congress in 1955 established the Oliver Wendell Holmes Devise Fund to publish a multi-volume history of the Supreme Court from the proceeds of Justice Holmes's estate. *Foundations of Power: John Marshall, 1801-1815* is the fourth of the projected eleven volumes to appear in the series. Reviewers accorded the three previous volumes a mixed reception. Some scholars charged these initial volumes suffered from excessive detail, often impenetrable prose, and a cavalier disregard for contemporary historiography. Other reviewers praised the encyclopedic detail. The volumes literally bulged with new material. This latest volume has all of the virtues and few of the shortcomings of its predecessors.

This is a book of separately authored parts; it is not a book of dual authorship. George L. Haskins in Part I analyzes the external political forces that operated on the Court, Herbert A. Johnson, in Part II, examines the internal and doctrinal development of the Court.

It was the Court's — and ultimately the nation's — good fortune to have the leadership of John Marshall. The new chief justice took office at the same time the Jeffersonian Republicans captured the popular branches of government. The Republicans intended to curb what they viewed as the last bastion of Federalism by repealing the well-conceived Judiciary Act of 1801, impeaching Judge John Pickering and Justice Samuel Chase, and by attempting to narrow the jurisdiction of the high court. These threats to the federal power, the authors observed, coincided with new demands placed on the Court by the nation's growing commerce, expanding borders, and strained foreign relations.

Haskins and Johnson conclude that Marshall prevailed because he fashioned a distinctively American rule of law. His decision in *Marbury v. Madison*, which established the doctrine of judicial review of acts of Congress, was crucial to this process. Haskins insists that Jeffersonians, including the President, realized as twentieth-century scholars have not, that the central

importance of the opinion lay in its establishment of the idea that the courts protected individual rights rather than settled political controversies. This, Haskins argues, broke from the eighteenth-century belief, to which Jefferson subscribed, that the judiciary was an appendage of the administration of government and therefore implicated in the political controversies that stirred the popular branches.

Haskins also successfully argues that Marshall's promotion of nationalism complemented the idea of an independent rule of law. The Court's decisions in *Fletcher v. Peck* and *Huidekoper's Lessee v. Douglass*, for example, reinforced the underlying conception of *Marbury* that a fixed set of nationally-binding principles should exist beyond the immediate reach of politics. Haskins also shows that the Marshall Court appreciated that a diminished federal presence in certain politically-charged areas, such as the common law of crimes, would ultimately enhance its power to settle questions of private common law.

Johnson in Part II analyzes the operations of the Court and the role of the Justices in developing a national private common law. Marshall replaced the Justices' practice of issuing seriatim opinions with "opinions of the Court." When Marshall did not deliver these opinions, the most senior member did. Yet Johnson offers provocative evidence that when these senior justices delivered opinions, as they did in the important cases of *M'Ilvaine v. Cox's Lessee* and *Stuart v. Laird*, it did not follow that they authored them. Far from coercing and bullying his colleagues, as some scholars have speculated, Marshall, according to Johnson, realized the importance of exploiting the Justices' values of seniority and deference in order "to obtain acquiescence from those of his associates who outranked him in age and service to the Republic, if not in professional knowledge and energy" (p. 385).

Johnson's pathbreaking analysis of the Marshall Court's development of private common law constitutes the most significant contribution of this volume. In dealing with illegal trade and prize cases, which formed the largest (thirty-two percent) component of the Court's business, the Justices distinguished between law and public policy. The former involved private property rights that were subject to confiscation only by judicial action; the latter entailed an overarching national purpose against which claims of private individuals had to be measured. This same balancing of vested rights and public interests occurred in cases involving marine insurance, American citizenship, and the formulation of American international law. The Court also promulgated private common law doctrines designed to promote business expansion and commercial development. Finally, Johnson argues, the Justices' most notable achievement involved the formulation of procedural and jurisdictional rules that formed the "foundations . . . used for broader and



more obvious national purposes" during the explosion of constitutional developments in the Court between 1819 and 1824 (p. 646).

This definitive history of the early Marshall Court is not without problems. The authorship of separate parts creates some repetition of both fact and argument, especially in the presentation of the cases. Johnson clearly has the better grasp of the recent historiography, and Haskins's sharply anti-Jeffersonian analysis of the political attack on the Court might have benefited from attention to Richard Ellis's *The Jeffersonian Crisis*, which offers a richer and more subtle analysis of the political composition of the Republican party. That *Foundations of Power* requires 687 pages to treat fourteen years of the Court's history suggests that the directors of the Holmes Devise remain doggedly committed to a narrative encyclopedic approach. Justice Holmes, however, would undoubtedly have found it suitably monumental — in girth, price, and scholarship.

*University of Florida*

KERMIT L. HALL

*The Papers of Daniel Webster: Correspondence, Volume IV, 1834–1839.* Edited by CHARLES WILTSE and HAROLD D. MOSER. (Hanover, N.H.: University Press of New England, published for Dartmouth College, 1980. xxvi, 562 p. Illustrations, index. \$27.50.)

Charles Wiltse and his staff have added another significant volume to a rather ground breaking collection. When American historical scholarship from the end of World War II until 1980 is evaluated, nothing will stand higher than the great edited works of the correspondence of American political leaders done under the auspices of the National Historical Publication Commission and the National Endowment for the Humanities. During the course of the last generation, projects were begun on Benjamin Franklin, Alexander Hamilton, Thomas Jefferson, Woodrow Wilson, and the like. By 1980 many of these works had run to over thirty or more volumes and had established a level of excellence far beyond anything accomplished before in historical editing. These volumes are a far cry from the nineteenth-century efforts by Jared Sparks on the papers of George Washington or Calvin Colton's edited volume of the papers of Henry Clay. Leaving out incriminating paragraphs or changing spellings, these nineteenth-century editions were more historical fabrication than historical editing. Lyman Butterfield, Arthur Link, and Julian Boyd, by contrast, have engaged in extraordinary efforts to print every letter sent or received by their correspondents, to

present each letter as accurately as possible, and to identify every individual and event mentioned in the correspondence. One unforeseen consequence of this massive effort has been that few of the projects have been completed and the chance remains that few of these projects may ever be completed, now that the National Historical Publication Commission has been virtually abolished and the National Humanities Foundation will have to get along on reduced funding. Thus the Adams Papers project, for example, which started off so conspicuously in 1961 (and was reviewed by John F. Kennedy in the *American Historical Review*), is a shambles and, if it is ever completed, will be quite different from what was originally intended.

Given the recent funding crisis, the Webster Papers may become a model for other projects. Charles Wiltse has previously edited an almost complete Webster Papers. But this earlier project was on microfilm (and is now available at many major research libraries). *The Papers of Daniel Webster* is to be a thirteen-volume selection of important correspondence. This volume is equal in scholarship to the standards of the other major series. Thus for the general reader interested in Webster or for the scholar surveying the Jacksonian era there is a good sample of Webster writings. For the specialist, these volumes act as an index to the three collections of Webster materials on microfilm.

Volume IV of Webster's correspondence deals with the years 1835–1839, a period in which the Massachusetts senator was at the height of his political power. Twice during this period Webster unsuccessfully attempted to capture the Whig nomination for president. The key to his failure in 1835 lay in Pennsylvania. While he had strong support in New England, most Whigs had great doubts that the former Federalist would generate enough interest elsewhere to become a viable candidate. Thus, Webster regarded an endorsement by the Whigs of Pennsylvania as crucial to his nomination. For several years prior to 1835, Webster courted the Anti-Masons, who made up the largest segment of the anti-Jackson forces in the state. By pacifying the Anti-Masons of Massachusetts and by taking the strongest position against Freemasonry of any presidential candidate, Webster hoped to win Pennsylvania's Anti-Masonic nomination. But the convention nominated William Henry Harrison in December, 1835 and Webster's candidacy collapsed.

Much of the correspondence which does not deal with politics is concerned with Webster's financial affairs. Although the great orator had little money himself, he attempted to live as well as his far richer associates. To accomplish this, Webster borrowed money from the wealthy of Massachusetts, who were in turn in his political debt. Realizing how dependent he was, Webster constantly attempted to build up his fortune, largely through land speculation. Perhaps fifty percent of the letters in this volume deal with this side of

Webster, a side about which most general readers know little. These were particularly important years for Webster economically as well as politically. During 1835 and 1836, the senator borrowed heavily to finance a number of land purchases — only to be hurt badly by the Panic of 1837. By the end of 1839, Webster had seen his political dreams shattered and his financial dreams destroyed as well.

This volume offers the reader a multi-dimensional view of Jacksonian America through the eyes of one of its most important figures. It also provides numerous insights into the nature and development of American politics.

Temple University

HERBERT ERSHKOWITZ

*Race and Manifest Destiny The Origins of Racial Anglo-Saxonism.* By REGINALD HORSMAN. (Cambridge, Mass.: Harvard University Press, 1981. 367 p. Index. \$22.50.)

In this study of *Race and Manifest Destiny*, Reginald Horsman of the University of Wisconsin at Milwaukee challenges the conventional view that antebellum myths of Anglo-Saxon superiority rested on a cultural, essentially "benign," conception of race, that only later in the nineteenth century yielded to more strictly biological and hereditarian definition. During the American Revolution, Anglo-Saxonism nourished the colonists' cause of freedom, Horsman argues. However, by 1850 this same myth, now with a racial twist, fed an aggressive nationalism. Faith in the virtues of free institutions thereby "became a belief in the superiority of the Anglo-Saxon branch of the Caucasian race" (p. 4).

Horsman's taxonomy of the racial idea, in outline at least, resembles that in such earlier studies as Thomas Gossett's *Race* (1963): the evolution of Anglo-Saxonism in Britain after 1530, its revival in the histories, literature, and philology of the Romantics, and its transformation in the "scientific" theories of polygenists and phrenologists. Although he also adopts the familiar theme of a change from a cultural-environmental to a biological-hereditarian conception of race, he pushes this analysis to new conclusions. Whereas others have assumed that the polygenist theory of Creation of the American school of ethnology was anathema to most Christian Americans, he argues that this scientific racism gained wide currency by 1850. He then proceeds to read all references to the "blood" of inferior and superior peoples, and to "innate" qualities, in this light.

In chapters dealing with Indian removal, the Mexican War, and the expansionism of the 1850s, Horsman further argues that white America applied this new racialism to any who stood in its way. Hints of the new

attitude can be found in the years immediately after 1815, spurred by the easing of Anglo-American tensions, the clash of settlers and Indians on the western frontier, and the south's desire to put slavery on firmer intellectual foundations. The social upheavals of the 1830s and 1840s completed the process, as "elaborate racial hierarchies" laid the basis for a "new aristocracy" (p. 301) of white Americans. In this regard (although Horsman does not explore the point), racial Anglo-Saxonism joined the cult of femininity as an antidote to potentially destructive tensions based on class, wealth, or section. In the case of race, the final payoff was conviction that inferior peoples not only must adapt to superior institutions but must finally be exterminated.

Although Horsman shows conclusively that Anglo-Saxonism was widespread and that many whites had little use for Blacks, Indians, and Mexicans (points few have doubted), his case seems weakest at its central point: the biologizing of the racial idea. On this issue he appears to undercut himself, arguing sometimes that the crucial issue is a stress on "innate racial differences," while at other times conceding that antebellum spokesmen continued to confuse "race, language, culture, and nationality" (p. 302). His insistence on the term "racialism" instead of "racism" itself seems to argue for differences between earlier and later conceptions. Nor do the many references to "blood" and "race" during the Mexican war clinch the point. Even if traditional Christian ideals were less a barrier to polygenist views of Creation than some have thought, these "scientific" arguments seem notably absent from policy debates, which continued to stress free institutions and a millennial mission. Nor also do these examples alter the previous conclusion (of Frederick Merk among others) that Anglo-Saxonism set limits to expansionism at least during the Mexican war. For these reasons among others, this well-written and extensively researched study of antebellum racialism should stir considerable debate.

*Swarthmore College*

ROBERT C. BANNISTER

*The Pennsylvania Antiwar Movement, 1861-1865.* By ARNOLD M. SHANKMAN. (Cranbury, N.J.: Associated University Presses, 1980. 236 p. Illustrations, bibliography, index. \$15.60.)

For those with a well-developed and lovingly nurtured interest in the American Civil War, Professor Shankman's apologia for publication of this study is probably quite unnecessary. For the rest of us, the statement that "there are many aspects of the conflict yet to be examined" seems a prelude to yet another of those Civil War studies that traverses all too familiar ground

at so little new an angle as to be totally unworthy of any serious attention. But this tightly organized book does prove the truth of the author's contention and does, in fact, break new ground. The book does, however, disappoint. What it does it does well, but it does not attempt to cover all the ground that the reader might fairly begin to expect it will, and it does not always draw together the diverse and frequently merely anecdotal information it presents into meaningful and relevant argument. As a result, it often leaves many aspects of the conflict it might well have examined more clearly identified but still unexamined.

In general two significant things are accomplished in *The Pennsylvania Antiwar Movement*. The author, by carefully organizing the book in very strict chronological format, manages to provide an orderly and comprehensible analysis of the complicated issues involved in the changes that the Democratic party in Pennsylvania underwent during the years of the Civil War. And the author, by basing his research on a solid sampling of contemporary newspaper articles and editorials, and private letters and journals, offers a survey of the widespread violence and threats of violence directed against those in Pennsylvania who in these years were viewed by many as less than "patriotic" in their support of the war.

Among the most striking of the specific areas in which the book makes some significant contribution, perhaps the most interesting is in the brief but strongly argued section dealing with General George McClellan's presidential candidacy of 1864. Shankman notes that McClellan's "repudiation" of the peace plank of the Democratic platform did not cost him nearly as much support among the Copperheads in Pennsylvania as it seemed to have in Ohio. To explain this fact, Shankman points to a number of closely inter-related factors including an especial appetite for political spoils, a belief among some in the binding nature of the platform on the views of the candidates, racism, and the simple fact the General was a native Pennsylvanian.

Despite its accomplishments, the book is too often simplistic in its treatment of often complex issues, frequently lacks summation and relevant conclusion, and leaves too much work undone. Shankman does not provide the study of a movement but rather a survey of public, and to a lesser extent, private antiwar sentiment, particularly among "Peace Democrats." The book generally overlooks objections to warfare and the bearing of arms on religious or moral grounds. Certainly, especially in Pennsylvania, such an aspect of an "antiwar movement" is worthy of central attention.

*The Pennsylvania Antiwar Movement* is nonetheless a useful book. It makes a solid contribution to a better understanding of the intricate mosaic of Pennsylvania politics during an important period in the state's history. It

also opens the study of an aspect of violence during the Civil War that has before been much overlooked. One can hope that Professor Shankman's work will produce further studies of the conflict that he has examined in this book.

*Swarthmore College Peace Collection*

J. RICHARD KYLE

*Shadows of the Storm*, Volume I of *The Image of War, 1861-1865*. Edited by WILLIAM C. DAVIS. (Garden City, N.Y.: Doubleday & Co., 1981. 464 p. Illustrations, index. \$35.00.)

Photographs of the Civil War have fascinated Americans almost as much as the war itself. During the conflict (the first in America to be extensively scrutinized by the camera) there was a steady demand in the North for "war views" in many forms, *cartes de visite* for albums and "stereo" views for stereoscopic viewers being the most popular. Immediately after the war, two of its photographers, Alexander Gardner and George Barnard, brought out the earliest attempts at "photographic histories" of the war. Gardner's *Photographic Sketch Book of the War* (1866) and Barnard's *Photographic Views of Sherman's Campaign* (1866), although commercial failures, have come to be regarded as genuine artistic achievements and have been recently reproduced in new paperback editions. The first attempt at a comprehensive photographic history of the 1861-1865 conflict, however, was the mammoth ten-volume project of New York's Review of Reviews Company, known as the *Photographic History of the Civil War* edited by Francis T. Miller and published in 1911. *Shadows of the Storm*, the first volume of a projected six-volume enterprise titled *The Image of War* and sponsored by the National Historical Society, follows in this historiographical tradition of Civil War literature.

Unlike Miller's ten-volume compendium, the current projected multi-volume series edited by William C. Davis does not claim total coverage of the conflict. The editor and his numerous assistants recognize that far too much of the war took place in areas where no photographers were ever present. Instead only "images of war" are offered, perceptions of the struggle as seen through the lens (and eye) of the photographer.

In addition to this interpretative guideline, the editors (Davis was assisted by, among others, the late Bell I. Wiley, a well-known social and cultural historian of the Civil War period and William A. Frassanito, noted historian of nineteenth-century Civil War battle photography) have at least three other objectives. They are anxious, for example, to rectify the primitive quality of

photographic reproduction and to correct the numerous erroneous captions in the Miller work which has served as a standard reference text since 1911. Equally important in the *Shadows of the Storm* is an editorial attempt to reassess the often inflated position of Matthew Brady among Civil War photographers and to assign proper recognition, where due, to heretofore neglected figures such as David D. Woodbury, T.C. Roche, James Gibson and others. Finally, as Brady's contributions are placed in a more appropriate perspective, the editors have elevated the largely ignored work of Confederate camera men whenever their work could be found. In addition to including more images of the early Southern war experience than this writer has seen in similar volumes of this genre, an entire chapter is devoted to the work of J. D. Edwards, a Yankee from New Orleans who extensively photographed Confederate military installations in various areas of the South.

In order to accomplish these tasks, the volume is organized into nine chapters, each containing an introductory essay by a Civil War specialist followed by an assemblage of images that the editors have deemed pertinent to the topic of that chapter. What the Civil War photographer missed, the contemporary historian writing for this volume was also instructed to ignore. "What [the photographers] did not cover with their negatives," editor Davis tells us, "*The Image of War* does not cover in its text. The chapter narratives, written by the foremost Civil War historians of our time, aim not at a history of the war, but prepare the reader for the images to follow."

To be sure, the editors have selected several outstanding historians (e.g., the late T. Harry Williams, Wiley, Albert Castel) for this difficult but demanding task. Unfortunately, few use the abundance (650 images) of photographic evidence assembled as actual historical evidence. Although William Frassanito is among the book's photographic consultants, few of the historians writing the chapter introductions to the volume's various categories of photographic data (e.g., "The First Bull Run," "The Navies Begin," "The War Moves West") follow the model of his brilliant exegeses of similar data in his interpretations of *Gettysburg: A Journey In Time* (1975) and *Antietam: The Photographic Legacy of America's Bloodiest Day* (1978).

The photographic resources gathered in this first volume (ultimately the six-volume set is to contain over 4,000 images) come as close as perhaps is possible to being representative of the types of photographs actually made during the period, 1861–1865. Hence the vast majority of them are military or political in subject matter, individual or group portraiture in photographic genre. What shall the historian make of this enormous corpus beside what it may reveal about name and rank of the famous and the forgotten men of the blue and gray? In future volumes, one hopes the editors will press their historians to take this methodological imperative as the main thrust of their

interpretive essays. The title of this volume, "Shadows of the Storm," is a most appropriate one for the difficult analytical task posed by its hundreds of separate portraits and landscapes. To date, much of the cultural, social, symbolic, even psychological meaning of American Civil War photography still remains obscure and largely uninterpreted. *The Image of War* series has the great potential to make a major contribution toward both significantly expanding Civil War historiography as well as the methodology of photographic history scholarship.

*University of Notre Dame*

THOMAS J. SCHLERETH

*Historic Buildings of Centre County, Pennsylvania.* By The Historic Registration Project of Centre County Library; GREGORY RAMSEY, coordinator. (University Park: The Pennsylvania State University Press, 1980. ix, 222 p. Maps, illustrations, appendices, glossary, bibliography, index. \$15.00.)

Conceived as a bicentennial project for Centre County, Pennsylvania, this work enjoyed a five-year metamorphosis from a modest catalog of historic properties to a balanced, valuable study of the county's old buildings. To read through the volume is to discover the diversity of Centre County's built environment: villages nestled into the contours of the Bald Eagle Mountains, remote churches and meeting houses clinging to survival, and Victorian towns with tree-lined streets and eclectic mansions. Out of these elements emerges a pleasant mosaic of a predominantly rural Pennsylvania county. Identification of the county's historic buildings remains central to the book's purpose. It cites the usual raft of old houses, of course, but also includes a wide variety of structures ranging from mills and barns to prison and university quarters, and even includes a cave, less a shelter than a tourist attraction.

Coherent organization and abundant illustrations mark the book's strength. A shaded 1874 atlas map of the county introduces each of the four chapters, which deal with geographical-historical groupings of townships and boroughs. Historical maps of towns and villages with old roadways and rail lines offer valuable points of reference for readers of the text, while a simplified highway map provides clear directions for visitors to the sites. In addition, a physiographical map indicates the geographical underpinnings of the nature and direction of the county's settlement and transportation patterns.

Equally valuable are the photographs. The careful selection and concise captions of views, interior elements, exterior shots, and archival photos



collectively relate much of the county's architectural history. Individually, however, they also illustrate the text's architectural discussions and often indicate a sensitive eye for both significant details and sweeping vistas. For this, Alison Taggart, who took over eighty percent of the 231 contemporary photos in the book, deserves great credit, as does Gregory Ramsey, who shot nearly all of the remainder. Their ability to record relationships between buildings, whether adjacent to one another or separated by tilled fields, and between villages and landscape, is insightful and delightful. Views across open pastures, along village streets, and over clustered rooftops evoke the spirit of pastoral life. This glance at an idyllic past puts the book at the forefront of a new concern of professional preservationists, the built environment of rural America.

Although a joint project, the text possesses a remarkable continuity of style and strikes an appropriate balance between local history and architectural criticism. Because of Centre County's deeply interior location, the county understandably did not stand in the vanguard of architectural innovation. Its architectural significance rests at the opposite extreme: loyalty to out-of-fashion styles, naive juxtapositions of old and new forms, and unique, unschooled expressions on the part of provincial Pennsylvanians. Because much of that architectural expression has meaning only within its historical context, a record of relevant local events is essential. Local histories can easily degenerate into genealogies and community responses to national crises. Happily that is not the case here. The authors focus both on families important in the founding and development of localities and on industries and economic events contributing to change in the county, but they avoid the trivia and boosterism that diminish the value of many local histories. Valuable extensions of the text include appendices on governmental preservation criteria and sources and illustrated glossaries of architectural and preservation terms, which nonspecialized readers will especially appreciate.

Most of the credit for this excellent work must go to Gregory Ramsey, who spearheaded a drive among the Bellefonte Historical Sites Commission to gain recognition for — and preservation of — Centre County's rich but obscure architectural heritage. His initial goal was to develop a bicentennial catalog of significant county buildings and place them on the National Register of Historical Places. The registration project quickly evolved both thematically, to include districts as well as individual structures, and administratively, to include the Centre County Library and county commissioners. The library provided space for the program and procured matching funds from the State Library for gathering archival materials essential for the project. The commissioners allocated a small operating budget, but most importantly took advantage of the federal CETA program to pay the staff.

Although a great deal of help came from community volunteers, Penn State students, and generous merchants like the Centre Film Lab, the foot soldiers in the mission proved to be the CETA workers. Hired by the county and paid by Washington, these dedicated people emulated earlier WPA workers on such 1930s projects as the Historic American Buildings Survey, and like their predecessors, many have gone on to important positions in the preservation field. Unfortunately the CETA program is now disbanded and the registration project's office is closed. Through the wisdom and generosity of the Centre County Commissioners, however, we do have this handsome volume to illustrate what can be done with much work and little money to document and publish the material culture of out-of-the-way but important parts of our country.

*West Chester State College*

RICHARD J. WEBSTER

*Their Sisters' Keepers. Women's Prison Reform in America, 1830-1930.* By ESTELLE B. FREEDMAN. (Ann Arbor University of Michigan Press, 1981. viii, 248 p. Illustrations, appendices, bibliography, index. \$18.50.)

Freedman's book traces the growth and change in the women's prison reform movement from its beginning in Philadelphia when Quaker women began visiting women incarcerated in the Arch Street Prison. This carefully researched and well-written book, winner of the 1978 Hamilton Prize, examines the successes and failures of the movement, offers biographical information on its leadership, and contrasts the 1830-1930 reform impulse to the contemporary feminist one.

The author concentrated her research on Indiana, New York, and Massachusetts where most prison reform occurred, but readers of this magazine will find some information about Pennsylvania. Four Pennsylvania women are cited in biographical sketches. Additional information is available on Abby Hopper Gibbons, a Philadelphia Quaker, who helped organize and administer a home for discharged women prisoners. Gibbons joined with Josephine Shaw Lowell in 1870 to mount a successful campaign which established separate prisons for women, to be run by women.

The first generation reformers' goal of separate women's prisons providing differential feminine care fell short of their ideals in design, personnel, and inmate population. The structures usually resembled traditional prison buildings, although some did have homelike cottages. Inadequate space and facilities plagued all institutions, as did the serious problem of attracting qualified

staff. Inmate populations that seldom fit the mold of young, first offenders contributed to the difficulty of transforming prisoners' lives. Despite limitations, the new prisons offered an option to women who otherwise would have served in even less desirable circumstances.

Even though poorly equipped for the task of reforming criminals, the keepers attempted to fulfill their duty through a mixture of feminine reform and traditional penal discipline. This program was intended to retrain inmates to the reformers' model of middle class womanhood. Recognizing the need to provide prisoners with skills appropriate for life on the outside, reformers instituted several types of programs, including academic classes, industrial and domestic skills. For a variety of reasons, domesticity prevailed at every institution. Sadly this did not prepare prisoners for life in urban, industrial America where most returned upon release. Character training focused on piety, purity, and maternity in conjunction with self-discipline and self-sufficiency. In contrast to other prisons, those for women relied more heavily on systems of merits and demerits to enforce discipline rather than alternate methods such as corporal punishment.

The legacy of women's prison reform is a mixed one. In sex-segregated prisons, the boundary between keepers and prisoners, based on class and power, revealed the difficulty in the ideology of sisterhood. After the 1920s, the women administering the prisons did not profess belief in a common womanhood, nor were they as openly critical of men's prisons as earlier reformers. Though run by women, these institutions no longer existed to serve women. Instead, they supported the male-dominated prison system and adopted its values. The willingness of women's prisons to accommodate to the larger penal system helps explain the resiliency of the separate-but-equal ideology.

The feminist movement of the 1960s and 1970s spearheaded an attack on the separate-but-equal principle, calling it into question for the first time in almost a century. Feminists sought to minimize incarceration for women, to expose abuses in both male and female prisons, and to advocate the decriminalization of victimless crimes for which so many women are imprisoned. Freedman concludes that feminist monitoring of police and court practices, and even the maintenance of single-sex prisons, serves women's best interests. Until full equality for women is achieved, Freedman asserts, "their sisters' keepers" must continue their watch.

*Steelmasters and Labor Reform, 1886-1923*. By GERALD G. EGGERT. (Pittsburgh: University of Pittsburgh Press, 1981. xvii, 211 p. Bibliography, index. \$17.95.)

The names of such industrial giants as Carnegie Steel, U.S. Steel, and Philadelphia's Midvale Steel have filled the lore of modern American history. They bring to mind great economic capacity, shrewd business acumen, massive labor conflicts, and self-satisfied capitalists. Morgan, Schwab, Perkins, Carnegie, Gary, Frick: these men are familiar to even the beginning student. Gerald Eggert's *Steelmasters and Labor Reform* takes us behind the names into the corporate boardrooms where men haggled over everyday policies. His primary interest is the reform of labor relations from within management; the efforts of company officials to meet public criticism from the outside and blunt union activism. In so doing, the book contributes a dimension frequently overlooked in business and economic history: the genuinely humane efforts of some executives to treat workers with greater compassion. Indeed, Eggert shows that the people in the boardroom were not a homogeneous group with a uniformly balance-sheet approach to employee relations. Rather, labor policies were often points of contention even within the upper echelons of management.

The common thread linking Carnegie Steel, U.S. Steel and Midvale in what the author calls "reform from above" is the career of William Brown Dickson, who moved through the highest levels of all three companies. Dickson, like other Carnegie proteges — Schwab, William Corey and Alva Dinkey — came up through the mills, learning the steel-making process from the bottom. These men were not, however, typical of the workforce, but were set apart by middle-class and native American family backgrounds, educations, and contacts within the company that prevented them from spending long, frustrating years on the shop floor. As these self-proclaimed self-made men rose in the Carnegie corporate hierarchy, their experiences as workers moved into the background, and only Dickson retained some sympathy for the plight of the laborer. His battle to end the seven-day workweek and the twelve-hour workday caused him to lose favor with his colleagues and eventually leave the company. After a six-year retirement, he joined a new corporate venture at the Midvale Steel and Ordnance Company. There, Dickson attempted to push collective bargaining and industrial democracy, albeit through a rather weak company union. Still, Dickson was really never quite able to achieve meaningful reform. U.S. Steel ignored its commitment to ending the long hours whenever suitable, and his fellow managers at Midvale never took the company union seriously or gave workers any choice in shaping policies.

The author, unfortunately, disregards any changes not directly initiated

by Dickson, casting doubts on his claim that the book is not a biography but "an account of labor-reform-from-above in the steel industry" (p. xvii). Welfare capitalism and profit-sharing are mentioned but not adequately discussed. Furthermore, he fails to place reform from above in a broader historical context. As Stephen Meyer's recent study of Ford Motors has shown, employers were becoming increasingly aware of such problems as absenteeism, soldiering and turnover even among unskilled workers, and reform from above was often aimed at coaxing workers into a more steady and disciplined routine. Because Eggert looks only for those forms of worker resistance equated with "weak and waning" unionism, he does not place the few efforts at humane reform in the context of the more all-encompassing new managerial perspectives that also included scientific management, technology and bureaucratization as a sort of carrot and stick approach to creating a modern labor force. In fact, there is no mention of Taylorism or scientific management, which are probably more closely associated with Midvale Steel than with any other firm.

Without discounting the importance of Dickson's support for important reforms, the reader is left with a skewed perspective on corporate management. The labor policies of Carnegie, Gary, and the like, are described as bound to tradition and opposed to change. In fact, however, they were far more dynamic. And it is certain that no matter how sympathetic Dickson was to human reforms, he would have rejected the more meaningful changes brought about by mass-production unionism.

*Temple University*

KEN FONES-WOLF

*People, Poverty, and Politics: Pennsylvania During the Great Depression.* By THOMAS H. COODE and JOHN F. BAUMAN (Lewisburg, Pa.: Bucknell University Press, 1981. 276 p. Bibliography, index. \$24.50.)

Two California State College historians joined their talents to produce this work. Their basic goal is to "illuminate the broad impact of the Great Depression on the social, political, and economic life of Pennsylvania." The result is a kaleidoscopic treatment of various episodes, events, and local developments in the Keystone State, selected specifically to achieve this purpose. In ten relatively short chapters, the reader is given a view of the "sociopsychological malaise of unemployment and poverty" in the state, the jobless marches in the mill districts of Philadelphia, rural distress in Snyder County, the thrift garden projects and cooperative stores in Allentown, Indiana County depression politics, the plight of miners and the activities of

the United Mine Workers of America, federal relief developments in southwestern Pennsylvania, and New Deal projects and housing programs in black Philadelphia

This book in reality is a series of studies rather than one. Three of the ten chapters were written by persons other than the authors, and the attempted wedding of diverse writing styles and frequent changes of locale and subject matter occasionally prove to be jarring. Much repetition inevitably occurs: for example, miners' problems are handled in chapters 1, 6, and 7, aspects of Pennsylvania politics in chapters 5, 7, and 10, the practice of "doubling up" is described three different times, Harry Hopkins constantly appears afresh, as do his roving welfare reporters. As a consequence, the reader emerges with a certain sense of discontinuity despite the authors' valiant efforts to overcome it. Frequent bridging phrases or sentences are necessarily introduced to prevent the reader from losing the primary threads of the story. Sometimes this succeeds, sometimes it doesn't. Perhaps the two best organized and executed chapters in the book, "The Rural Dimension" and "The Great Depression in Allentown," were written by outside contributors Donald D. Housley and Ernest B. Fricke, respectively.

Still, this book has much merit and clearly supports its main contention that the New Deal in Pennsylvania consistently breeched "the isolation which kept rural, ethnic, racial, and such occupational groups as miners and textile workers out of the mainstream of modernizing trends in America" (p. 17). This conclusion underscores what other recent New Deal historians have been claiming, quite properly re-directing some of our attention from the national aspects of New Deal activities to the formative impact which they had on local and state communities. The authors' examinations of the various rural, smalltown, and city environments in the Keystone State therefore contain more than illuminating insights into Depression Pennsylvania. Their efforts represent a laudatory attempt at history "from the bottom up," giving the grand design of the national New Deal added meaning by analyzing its local content. For this reason, alone, this volume deserves a place on the reading lists of collegiate Pennsylvania history courses and on the social studies shelves of high school libraries.

A *very* useful bibliographical essay appears at the end of the narrative, indicating that the authors know the scholarly literature well. It includes government records, manuscript collections, and dissertations. The authors' use of oral history is also to be recommended. A very practical index concludes the volume. The sale price of \$24.50, however, is unconscionable and unfortunately will severely limit the book's distribution and availability.