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## Looking Back Rereading and Rewriting History

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*American Retrospectives: Historians on Historians.* Edited by Stanley I. Kutler. (Baltimore: Johns Hopkins University Press, 1995. Pp. x, 341. Notes. Clothbound, \$48.50; paperbound, \$16.95.)

Some years ago editor Stanley I. Kutler commissioned for *Reviews in American History* a series of retrospective reviews of the important books in a variety of fields. He has collected here twenty-four of those reviews. They consider works published as early as Richard Morris's *Studies in the History of American Law* (1931) and as late as Nathan Huggins's *Black Odyssey* (1977). Not surprisingly, the collection emphasizes earlier works, examining only a few of the "new" histories. Anyone who reads *Reviews in American History* regularly will be familiar with these essays and will no doubt be grateful to have them collected here.

The essays were single in assignment—to "explain the present vitality and usefulness of the work" and to "explore the original reception and impact of the works" (p. ix)—and are varied in execution. Some of the essays serve as excellent summaries of the contents and arguments of the book under review. Donald B. Cole's essay on Arthur Schlesinger, Jr.'s *Age of Jackson* (1945) does this nicely, demonstrating the extent to which this is a big-theory book in its anti-Turnerian emphasis on class. Other essays focus more fully on the book's influence. Alan Brinkley argues that Richard Hofstadter's *Age of Reform* "is the most influential book ever published on the history of twentieth-century America. For more than a decade after its appearance in 1955, its interpretation shaped virtually every discussion of modern American reform. For longer than that, its methodological innovations helped recast the writing of history in many fields" (p. 45). Brinkley is persuasive and his argument is borne out in the collection as a whole: no other author is mentioned more often.

Like the books that they reviewed, the essays by James T. Kloppenberg on Morton White's *Social Thought in America* (1949) and Aviam Soifer on Willard Hurst's *The Growth of the American Law*

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(1950) are really directed at specialists in philosophy and law. Also, like their authors, they offer fascinating if sometimes overly abstruse meditations.

Some of the essays focus on only one book of the many produced by a prolific historian. Merle Curti's *Growth of American Thought* (1943), Bernard De Voto's *Year of Decision* (1943), and Mary Beard's *Woman as Force in History* (1946) are reviewed without reference to their other writings. This is hardly Beard's best book and Suzanne Lebsock offers a clearer statement of Beard's argument than Beard herself did. The selection of *The Image* as the Daniel Boorstin book to be reviewed is peculiar. Stephen J. Whitfield's essay on this book persuades me that the work is interesting without convincing me that it is either especially important to or emblematic of Boorstin's body of work. In some cases the historian only wrote one remarkable book. Eleanor Flexner's *Century of Struggle* (1959) has been essential reading for historians of women because of its "simple elegance and its scholarly reliability" (p. 117) not because it profoundly influenced the historiography of women's history. Jon Gjerde examines the fullness of Paul Gates's writing on American land policy and the continuing relevance of that work. Similarly, Thomas K. McCraw looks at Alfred Chandler's formative contributions to business history and locates them in the context of the author's life. McCraw did not trace the influence of Chandler's work on other historians but does demonstrate his affection and admiration for Chandler, his Harvard Business School colleague. I missed this kind of personal context in the other essays.

Some of these books inspired a school (even a factory) of research. As Martin Ridge remarks about John D. Hicks's *The Populist Revolt* (1931), "far from being the last word on Populism, it proved to be an evocative book that produced a variety of valuable case studies" (p. 29). Some of the books reviewed are in such a school: Robert Starobin's *Industrial Slavery* (1970) in response to and development of Kenneth Stampp's studies, for example. In other cases the book stands almost alone. As James Campbell and James Oakes wrote about Winthrop Jordan's *White Over Black* (1968), "it cast a narrow historiographical shadow" (p. 282). This is the one book in the collection that I still assign to students and find amazingly provocative, admittedly in its briefer *White Man's Burden* (1974) form.

Finally, while some of the books reviewed here were probably meant for a particular audience (Merrill Jensen's for historians of the Revolution), others were intended to cast new light on all of the American past. Reviewed here by Robert M. Collins, David Potter's *People of Plenty* (1954) invites the reader to rethink the effect of abundance on the development of America. According to reviewer David W. Blight, Nathan Huggins bids us to retell the whole story in light cast by the slave experience. Robert Dawidoff, giving the kind of whole-souled review that every author would die for, claims that

Merle Curti in *The Growth of American Thought* “wrote the intellectual history on which most of American history of the last forty years has been based” (p. 85).

The best essay in the book may be Kenneth Cmiel’s on Robert Wiebe’s *The Search for Order* (1967). Cmiel rethinks the “search.” He looks again at Wiebe’s modernization theory and rightly notes that “so preoccupied was he with the system-building itself that he ignored the ways that the system interacted with human beings.” And, Cmiel continues, Wiebe left out “friendship, love, family, neighborhood, work.” Cmiel detects in Wiebe a deep sadness about the inevitability and toll of modernization and comments that Wiebe forgot that “there are more things in the world than order” (p. 308).

The organizing principle of Kutler’s book is not evident nor is the principle of selection (why these from the *Reviews in American History* series and not others), but Kutler does articulate a rationale for commissioning the reviews in the first place and for publishing this gathering up: many of these books were for him profoundly important and, he says, “they remain essential to my understanding of the American past, subject, of course, to the obvious caveat that their successors have ably enhanced and expanded their contributions” (p. x). In this thrust and parry Kutler reveals both the text and the subtext of this collection. These are important books; nearly every one of them has been required reading for American history graduate students in the last thirty years. Only a few of them, however, would currently be read or assigned in a class or grappled with in our own writings—they are too old, or outdated, or superseded. This kind of change is in the nature of the discipline. Even so, Kutler laments that historians increasingly “cite only the work of their peers and contemporaries, often failing to recognize how that work had evolved from apparently long-forgotten historians, some of whom had produced the pioneer inquiry on a subject” (p. ix). Unfortunately, he doesn’t comment explicitly on the irony of historians ignoring their own historical past.

Like many collections, this one probably will not generally be read from beginning to end nor in one extended sitting, but will profitably be dipped into by graduate students and teachers who want to recall a specific book or be reminded of a particular argument. When read altogether, however, the essays present a fascinating retrospective on the larger field of American history and offer a provocative commentary on the current state of historical writing.

The collection demonstrates how fully each book was embedded in the ideas and events of its time. Most of these books would certainly offend a modern reader’s sensibilities about gender and race. Louis P. Masur notes that Bernard De Voto reflects his era’s attitudes about Indians in the American West. The books are time related and reflective in other ways, too. Arthur M. Schlesinger, Sr., took as the dominant image in his *The Rise of the City, 1878–1898* the

White City of the 1893 Columbia Exposition in Chicago, but, as Terrence J. McDonald comments, the book appeared in 1933 when few Americans resonated with such a hopeful, optimistic, even romantic view of the city. If Schlesinger noted the irony, he didn't mention it, even in his introduction, but it is impossible not to be struck by it today. By contrast, William Appleman Williams's indictment of American foreign policy, first published in 1959 and reviewed here by Bradford Perkins, did speak to (and perhaps even helped to crystallize) some discontents of his day. His work found an enthusiastic if not overly generous reception as a result. Flexner had the misfortune of publishing her book on women's political history just as some American women were declaring that the personal was political and were taking their scholarly (and personal) questions in another direction, as Carol Lasser's review aptly points out. In his book *The Concept of Jacksonian Democracy* (1961), Lee Benson's distrust of appearances and his quest for scientific precision mirror the early 1960s' rejection of artifice and the period's slightly desperate embrace of science. As Daniel Feller notes, "Jabbing italics, bolstered by an imposing display of social science terminology and numerical data, intimidated even where they did not persuade" (p. 275). Wasn't Benson in a way doing in his writing what architects Renzo Piano and Richard Rogers were doing in their design of the Pompidou Center in Paris—putting the construction methods on the outside so that the beauty was in the function not in the form?

Kutler's collection asks what our right relationship to these books ought to be but does not really answer the question. Should we remember the works and their authors as Americans often think about pioneers: with fondness and perhaps a little condescension (appreciating them for what they did in their time but now finding them vaguely quaint or simplistic)? Kutler intimates that most of us take a whiggish view of our own disciplinary past and feel justified in doing so. Clearly, our discipline has been propelled by a commitment to the new—new methods, new sources, new questions, new interpretations. Many of us who do American history today would not have found an intellectual home among professional historians in the past but flourish in the present as a result of other historians' willingness to break away from the trails laid out by earlier historians—some of them among those here reviewed.

The book demonstrates how often in the last sixty years the discipline has been invigorated by something new—an economic interpretation where political ones have dominated; an emphasis on conflict where consensus has prevailed; a reformulation of the questions. If we do not pay attention to these earlier works because of the substance of their findings, we would do well to pay attention to what has energized the profession in the past.

The first lesson: think bigger; imagine in larger terms; take risks. Most of the books reviewed here are remarkable for their ambi-

tion and their vision. These authors generally had big ideas and looked at them in a big way. "Let's rethink all of this," they seemed to be saying to themselves. Asking how all reformers thought was certainly a foolishly extravagant question, yet it was also a provocative and interesting one. Making generalizations about the development of American thought seems to be a fool's errand, but aren't we grateful that someone was willing? Of course contemporary historians, committed to the importance of social, gender, race, and class categories in history, are bound to be skeptical of generalizations about "Americans." But we continue to be hungry for generalizations, if only to sharpen our wits and intellectual acumen to say nothing of our research. Historians must find ways to make broad generalizations while at the same time maintaining the integrity of the particulars. As Dawidoff says, "The general survey such as Curti's reminds the historian that there is a larger picture into which the special foreground must sooner or later fit" (p. 86).

The reviewers repeatedly commend their authors for their largeness of vision. Reviewer Stephen Botein praises Richard Morris for working "in bold strokes for the benefit of lay readers." Chandler's work "represented a monumental historiographical achievement: a *tour de force* of comparative research and conceptualization, and a matchless example of a business historian's willingness to generalize" (p. 105). "If [*Rise of the City*] is not the history of the city that we need today it is still the place to start for a refreshing sense of the possibilities that the American city once represented. If we no longer see those possibilities it may be less the result of our improved historical practice than of the diminishing size of our moral universe" (p. 269). Cmiel repeatedly praises Wiebe for the genius of his synthesis.

Contemporary historians have gotten very careful, too careful. Too many books and articles are published for the sake of tenure or promotion and authors are unwilling to risk rejection or bad reviews. We might do well to listen when De Voto declares, "the reverence for fact that is the necessary condition of research has too often become a screen for timidity" (p. 187). We should listen to Suzanne Lebsock's judgment about the intellectual struggle that reading Mary Beard's work requires. It is worth it, she argues, because "Beard took on questions of enormous importance." Lebsock continues, "To study Beard is to grapple with fundamental issues, for history, for feminism, for the future of us all" (p. 180). And we should take seriously Dawidoff's charge that Merle Curti's kind of thinking has "been abandoned by a profession that lost its bearings in specialization and has sought to recover them on too small, too high, and too undemocratic an intellectual plane" (p. 85).

When historians write small, our audiences are small. David M. Kennedy compares Allen's *Only Yesterday* (1931) with *The Great Crusade and After* (1930) by William Preston Slosson. Both books

covered the 1920s and both appeared at about the same time. "Where Allen was selective, anecdotal, judgmental, and unfailingly interesting, Slosson was comprehensive, scientific, objective, and a trifle boring. If Allen's prose lilted musically along, Slosson's soldiered stolidly forward" (p. 83). Which would you rather read? Kennedy is critical of the public that has for sixty years opted for Allen over Slosson. He sees in the choice a somewhat deplorable public preference for "style to substance." A little harsh on the reading public, I think, and a little too forgiving of Slosson. The American public may be consumed by materialism and a television culture. Nonetheless, there is out there a large history-reading public whose historical interests are not currently being satisfied by professional historians but by writers willing to hazard stories that speak to significant issues. No doubt these popular historians make some mistakes along the way. Professional historians also make mistakes, but we can't let our fear of them paralyze us. In our carefulness, in our precision, we have abdicated our authority to speak and to be heard outside of the academy on important issues. That is one of the lessons of this collection.

The second lesson to contemporary historians is to write better. To be sure, not all the books reviewed here are models of good writing. In a wonderful flash of good writing, reviewer Thomas Slaughter notes that reading Merrill Jensen "is about as much fun as slogging one's way through a Valley Forge winter in bare feet" (p. 143). But most of these books are noteworthy for their attention to the writing. Cole admires Schlesinger's "ability to evoke the past in human terms," and argues that "Schlesinger comes closer than anyone else to conveying the feeling and emotion of the era" (p. 73). Whitfield lauds Boorstin: "It is so rare for a historian to be clever that Boorstin's astonishing erudition at least dampens the suspicion that he is *only* clever. His books yield surprising and obscure information, related with such sprightly wit and vividness that many a larcenous lecturer has cherished them for introductory courses (and beyond)" (p. 220). And Cmiel praises Wiebe's "extraordinarily graceful prose" (p. 294).

Contemporary historical scholarship demonstrates the allure of research. Our own pleasure in data—and the oft-reported dread of writing—may blind us to the need to give life, voice, and meaning to our data when we turn it over to others. When our students complain that this or that work is boring, don't we sometimes agree—whether or not we admit it to them? Aren't we asking a lot of each other and of the reading public by writing in such deadly and deadening ways? That is another of the questions these essays raise.

Finally, the third lesson is that we must write with passion. We know why our work is important, why a focused study of a well-defined subject is really relevant to much else, but we don't like to tell anyone, or take the risk of showing it. The conventions of contemporary historical scholarship require that the life be squeezed out of it or into the conclusion or epilogue. Giving the reader a reason to

read the book may seem like pandering, and expressing our personal stake may make us feel too exposed; but don't most of us do our work because we really care in some fundamental way and because it touches our own deepest questions and issues? What makes many of the books reviewed in this collection worthy of reconsideration is that they tell passionate stories or they tell their stories passionately. Of course we know the distortions of passion, and as historians we know the effects of passion. But as historians we should know that the disguise of passionlessness is also a distortion.

Each of these books is fired by some passion or another. In the judgment of reviewer Alex Lichtenstein, Robert Starobin's commitment to the black liberation movement informed *Industrial Slavery* in an entirely responsible way. Starobin's political commitment, Lichtenstein argues, "informed [his] scholarship without distorting it" (p. 223). Merton L. Dillon contends that Dwight L. Dumond, a student of Ulrich B. Phillips, wrote from a commitment to liberal values and an abhorrence of radicalism in any form. Did these commitments blind them? Of course. Kenneth Koch observed that a way of seeing is always a way of not seeing. Dumond did misunderstand Garrison. The Beards' commitment to economic explanations blinded them to religion. At the same time, Starobin's political commitments also helped him see. If his "numbers and economic theory were shaky his general conclusions were not" (p. 229).

Even, or maybe especially, would-be scientist Lee Benson had a passion in his work: "Benson not only demanded a reexamination of old shibboleths like 'Jacksonian Democracy,'" Feller writes, "he wanted historians to adopt a new way of thinking and of writing, and indeed a new purpose" (p. 272). Isn't that passion, too? We needn't choose between dispassionate and distorted. We have many choices in between, and we can exercise good judgment on our own and in our community of scholars. If we want our work to matter, to fuel interest in the past, to cast new light on some issue or another, we have to stop banking our fires.

Three lessons and lots of thoughtful writing—that is quite a harvest from one book. Of course, the essays vary in quality. An index would have been helpful. I would like to have read a concluding, interpretive essay by editor Kutler. That is by the by. This book is provocative, insightful, and not a bad rendition of our own disciplinary story. Oh, yes, and a call to action that I hope we heed.