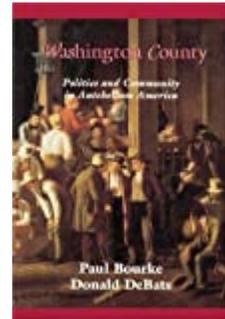


# H-Net Reviews

in the Humanities & Social Sciences

**Paul Bourke, Donald Debats.** *Washington County: Politics and Community in Antebellum America.* Baltimore: Johns Hopkins University Press, 1995. xvii + 407 pp. \$45.00 (cloth), ISBN 978-0-8018-4950-3; \$25.00 (paper), ISBN 978-0-8018-5946-5.



**Reviewed by** Anthony Gene Carey (Auburn University)

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The authors of this ambitious and remarkable book, which is part of a new series entitled *Reconfiguring American Political History*, set a large task for themselves by trying to use an intensive study of an Oregon county as a vehicle for revising interpretations and altering research methods in nineteenth-century political history. As in their previous work [particularly “Identifiable Voting in Nineteenth-Century America: Toward a Comparison of Britain and the United States before the Secret Ballot,” *Perspectives in American History* 11 (1977-78): 259-288], Bourke and DeBats are here concerned primarily with voting practices and voting behavior. Once again, their research centers on extant *via voce* poll books, in this case for the 1855, 1857, and 1859 election in Washington County. Instead of rehearsing the contents of a book published nearly three years ago and already widely reviewed, in this essay I concentrate on assessing how well Bourke and DeBats meet their chosen challenge of reconfiguration.

One of the most striking aspects of the book is the immense dedication and labor required to produce it. Bourke and DeBats have pieced together Washington County’s history from countless fragments of information; the level of detail achieved in everything from fertility rates to the mapping of electoral precincts is astonishing. And all of this detail is presented in readable, oc-

asionally even engaging, prose.

Any historian would be proud to have published such a book, but is *Washington County* likely to reconfigure political history, even local political history? I think not, for several reasons. First and most basic, poll books are to *Washington County* what James Madison’s notes are to studies of the Philadelphia convention. Extant poll books provided the only compelling reason for focusing on Washington County, and scholars who lack similar records—that is, almost everyone else—cannot hope to replicate Bourke and DeBats’s work. Most of what is novel in the book comes from the careful linkage of individual voting records to more familiar and more widely available sources: census returns, land records, territorial papers, and the like. Without the poll books there would be no linkages, and *Washington County* would be just another well-crafted local study—valuable, but unexceptional.

Second, although I would not claim to be skilled, or even competent, in statistical techniques, most of the quantitative methods used in *Washington County* seem ordinary enough, with some innovations adopted for analysis of poll book data. Without minimizing the work involved in gathering the data or dismissing the expertise with which Bourke and DeBats manipulate it, it can be said that quantitative approaches were “new” in the

1960s but hardly promise to reconfigure the field in the next century.

Third, I would guess that broad studies of political culture, based on close readings of texts and nuanced interpretations of symbolic actions, are more likely to reconfigure political history than is any fresh outpouring of studies on voting behavior. Indeed, a shortage of literary sources and the authors' interest in other questions makes *Washington County* weak on matters of ideology. The authors expressly devote only fifteen (pp. 153-167) out of 325 pages of text to party ideology, although shorter comments and discussion (for example, pp. 137-145, 211-216) are scattered throughout the book. The authors' treatment of Democratic, Know-Nothing, and Republican beliefs does not differ essentially from descriptions found in numerous other recent works, and the consideration of "visible partisans" is also rather familiar. Furthermore, despite some interesting and insightful discussion of women, families, and gendered issues, the book will not contribute greatly to the growing literature on gender and politics.

Fourth, the importance of the Washington County story for understanding Oregon or national politics remains, at least to me, unclear. Bourke and DeBats demonstrate that outside events, such as Congress's passage of the Donation Land Claim Law and national Democratic party feuding during Buchanan's administration, significantly affected local society and politics. They also examine, for example, connections between the county's anti-Democratic reformers and similar groups elsewhere. In one of the book's major arguments, they contend that cultural conflicts between Washington County settlers, particularly between native northerners and native southerners, were like "small rehearsals" (p. 3) of the larger sectional conflict that led to the Civil War—a theme supported with massive documentation on the backgrounds and voting behavior of different groups of county residents.

The authors acknowledge, however, that their findings do not allow them to generalize about "the policy issues that engaged state legislatures and Congress" (p. 15), where the play itself was staged. Do scholars need to look to Washington County to understand the dynamics of the sectional conflict? Is extensive evidence from the Oregon frontier required to show that the cultures of the Upper South and New England were different? If we have Charles Sumner and Preston Brooks, do we need James H. McMillan and Andrew Jackson Masters, whose deadly altercation provides the opening and closing scenes for

*Washington County*? The authors' largely successful efforts to set Washington County in context enrich their story, but the applied context helps explain the local story infinitely more than the local story contributes to understanding the context.

Fifth, the heart of the book is Chapters Seven through Ten, in which Bourke and DeBats exhaustively analyze poll book data and other sources to arrive at the conclusion that neighborhoods were central in the political and social experience of Washington County residents. The authors make many fine points about the organization and manner of voting, the differences between committed partisans and occasional voters, the importance of turnout, the infrequency of party-switching, the impact of persistence and immigration on voter participation rates and electoral outcomes, and the reasons for roll-off and split-ticket voting. The breadth and depth of this analysis is, I believe, unparalleled for antebellum America. Many scholars have, however, made similar arguments before, albeit with less technical sophistication and far less conclusive documentation.

On page 216, the authors turn to the question, "What relationship existed between the socioeconomic and cultural attributes of voters and their partisan behavior?" The answer proves disappointing, perhaps even to Bourke and DeBats. The data show some differences between opposing "visible partisans" and, unremarkably, that greater wealth was related to greater political participation, but at bottom "general social characteristics, whether taken individually or collectively, were not associated in a meaningful way with the partisan choice of the electorate at large" (p. 246). Where to go now, given this negative finding?

To neighborhoods, the groupings that linked ordinary voters to "visible partisans" and structured society and politics. Many readers will recognize this theme, presented here in somewhat different form, from the authors' previous work [cited above]. Bourke and DeBats muster massive evidence and employ interdisciplinary theory to demonstrate that Washington County was organized into relatively distinct neighborhoods, which displayed relatively distinctive partisan leanings and political styles. Kinship networks were dense, family members tended to vote alike, and "politics for many people became an expression of their membership of [in?] a community" (p. 275).

Bourke and DeBats take pains to say that they are not attempting to "explain" (p. 275) voting behavior by reference to neighborhoods, a disclaimer that I found

odd. Although I understand the pitfalls inherent in the task, if voting behavior can not be explained in Washington County, with all the authors' expertise and the mountain of information they have accumulated, where can it be explained? The authors conclude that "what may be recovered of the culture in which these people lived suggests that the physical and social networks to which they belonged provided the essential settings in which their choices about everything else, including politics and public affairs, were made" (p. 322). Pardon me, but this claim seems like common sense; few historians will either be surprised by it or inclined to dispute it.

Which brings me to the sixth reason why *Washington County* seems unlikely to reconfigure American political history. Suppose the impossible, that historians could produce a hundred similar studies for counties across the Union, and suppose further the highly improbable, that these studies would not contradict one another. Then scholars would have access to an enormous body of information on how individual voting behavior is largely unexplainable, on how neighborhoods structured political life but did not determine voter choice, and on myriad voting practices.

Many traditional concerns of political history, however—such as how parties formed and disintegrated at the state and national level, why some issues were salient and others not, how decisions were made in Congress and state legislatures, and why state and national elections were won and lost—would remain largely unaddressed. Perhaps reconfiguring political history means abandoning or greatly deemphasizing these concerns, but surely many scholars will resist such a reconfiguration. How could the findings of a hundred county studies be incorporated into syntheses and textbooks? Certainly the major points could be summarized

in overviews of grassroots politics, but it seems to me that a political synthesis of the Middle Period constructed around the politics of neighborhoods would be both unmanageable and unsatisfying.

Grand themes boldly advanced reconfigure fields; I state this as a matter of fact rather than of right. *Washington County* might, to pursue this point, be compared to Alan Taylor's *William Cooper's Town*. Both are local histories that rest on immense research and aim to address big issues. As Taylor tells it, the people of Cooperstown and its environs were engaged locally in what was a national and international struggle over class, status, wealth, democracy, and the meaning of the American Revolution; readers can readily grasp how politics was grounded in society and how politics mattered. While Bourke and DeBats to some extent employ North-South conflict as an organizing theme, they ultimately emphasize the peculiarities of neighborhoods, the complexities of voting behavior, and the perils of generalization. Perhaps Bourke and DeBats are truer to their subject and more nearly "right" about Washington County than Taylor is about Cooperstown—whatever "right" might mean, a question too large to tackle here. Nonetheless, I predict that scholars such as Taylor, who present grand themes capable of structuring the way historians and the general public understand whole eras, will inevitably triumph over more narrowly empirical works in any battle to reconfigure American political history.

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