WOMEN'S HISTORY IN MINNESOTA: A SURVEY OF PUBLISHED SOURCES AND DISSERTATIONS.
Compiled by Jo Blatti.

In 1977 I bought a copy of Women of Minnesota, a collection of 16 biographical essays and many thumbnail sketches about notable white women from the settlement period to the present. The book was one of the first such statewide compilations, and it represented an ambitious step beyond individual biography toward a much more comprehensive understanding of women's activities in state and regional history.

"You've come a long way, baby" is surely one of the more revolting advertising slogans of the twentieth century, yet some such exclamation is inevitable when comparing Blatti's comprehensive survey, Women's History in Minnesota, with the earlier volume. The new publication offers a guide to 844 published women's-history sources in a thoroughly organized, annotated, and indexed volume. The vast majority of the material has been published since 1977, thus proving that it is not an overstatement to speak of the "explosion" of women's history in the past 20 years.

The distribution of topics is as interesting as their sheer number. Blatti and her advisers found that standard Library of Congress and historical categories did not adequately represent women's experiences, and so they devised a topical organization by following the documents. Roughly, the bibliography moves in 13 categories from personal identity through immediate social organization, such as the family, to the more formal public sphere. This sounds simple, but as Blatti remarks, "This bibliography's effective shape is one of webs or threads weaving back and forth among private and public spaces or commitments." The bibliography weaves the densest web around a key finding of women's history—the recognition that "much public work of the culture is carried on in ostensibly private family and domestic settings"—by listing some of the same sources under "social life" and "natural sciences and health" and again under "economics and employment" and "law and government."

Interestingly, the largest category in the bibliography is "organizations and clubs," with 192 entries, almost as many as the next two categories, "economics and employment" and "life history," combined. The section on "cultural, ethnic and group affiliation" is disappointingly small (40 entries), but one can reasonably expect it to grow rapidly in the next few years.

This is an eye-opening bibliography, impressive for the quantity, quality, and organization of its sources. It is an essential tool for the next task, which is to rewrite Minnesota's and all states' histories so that women are integral to the story.


BRIGHT RADICAL STAR: BLACK FREEDOM AND WHITE SUPREMACY ON THE HAWKEYE FRONTIER.
By Robert R. Dykstra.
(Cambridge, Mass.: Harvard University Press, 1993. 348 p. Cloth, $47.50.)

Before the Civil War, the persistent and explosive issue of slavery and its extension into the territories made the United States a truly divided house. As fiery rhetoric on both sides inflamed Congressional debate in Washington and settlers took up arms in bloody confrontations on the midwestern frontier, the question of slavery all but shattered the underpinnings of a young America in a way that no foreign threat ever could. But it oversimplifies the matter to view the conflict purely in terms of regional tensions—whether half a nation could hold slaves while the other half was free. The question of African Americans' place in the national tapestry was far more complex, especially when they were not slaves. Many northern midwesterners, even members of various abolitionist communities, held strong antislavery sentiments while also being anti-African American; the question of full citizenship for free blacks was as unsettling as it was unsettled. Whites would carry forth this ambivalence, somewhat modified, throughout the remainder of the nineteenth century. Against this backdrop, Iowa's history of race relations is especially noteworthy.

Bright Radical Star chronicles Iowa's remarkable social and political evolution from being the most racist free state in the antebellum North to one of the most egalitarian. The author's meticulously researched account begins in 1833 when the land was officially opened for settlement, showing how the territorial government, controlled principally by former south-
gains to overturn such prejudices, conservatives broached articles on history, civil rights, education, and contemporary issues is a recent publication on an 1860 Minnesota slave case.
Prairie Populism: The Fate of Agrarian Radicalism in Kansas, Nebraska, and Iowa, 1880–1892.

By Jeffrey Ostler.

(Lawrence: University Press of Kansas, 1993. 256 p. Cloth. $29.95.)

Prairie Populism should appeal to a wide range of readers. For those of us who like a good tale of political history, it delivers a thorough account of Gilded Age politics in Kansas, Nebraska, and Iowa. For those who like to remind themselves that politics is dirty business—and has been for most of our nation’s history—Ostler documents enough mudslinging and name-calling to make even Lee Atwater blush posthumously. And for readers concerned with third-party movements, particularly populism, the author makes several points worth considering.

Late-twentieth-century Americans suffer from confusion about the word populism. To some it means any political movement organized around an appeal to the “common people.” For others it might mean an attempt by demagogues to rally working Americans around a right-wing cause. Some understand the term in its historic context and think of the late-nineteenth-century movement by farmers and workers to wrest justice from the emerging industrial order. Still others think of that plutocrat-populist, H. Ross Perot, and his own “third-party” movement in the last presidential election. For those of us puzzled by this slippery word, Ostler’s book will provide some measure of clarity.

Prairie Populism enters into the venerable debate on American exceptionalism by posing a question: Why was there no populism (meaning a strong third-party vote) in Iowa? To answer this question, Ostler uses Kansas and Nebraska for comparative purposes. In an important part of the book, he demolishes the standard explanation that Kansas and Nebraska went Populist because of hard times, while in Iowa and other “eastern” states, more prosperous circumstances led to the rejection of third-party activity. Using convincing evidence that compares eastern Kansas and Nebraska to Iowa in terms of political, economic, and environmental conditions, Ostler shows that the “frontier” explanation has run its course. Instead, he offers an alternative explanation. “The crucial difference between Iowa and Kansas/Nebraska,” he argues, “was party competition.”

While Ostler does not claim that his findings for Iowa, Kansas, and Nebraska hold true for the rest of the nation, he does attempt to shift the ground of Populist studies from the social to the political. “State political environments,” he contends, “were crucial in determining whether agrarian radicalism took a third-party form.” Ostler does not ignore the social dimensions of populism, and his book contains useful information on women, political education, and movement-building. But his most original contributions concern the development or absence of third-party movements. Iowa’s active Farmers’ Alliance achieved important railroad legislation in 1888 without resorting to third-party tactics. This success by using non-partisan pressure in a competitive political environment left many leaders within the state Farmers’ Alliance opposed to third-party politics. By contrast, no amount of political pressure by the Kansas or Nebraska alliances could move those state legislatures, dominated by unchallenged Republican establishments, to reform on any issue. Thus, successful third parties were born.

Ostler’s political narrative, combined with his excellent economic analysis, make for a convincing argument. I would recommend this book to anyone who has an interest in populism, rural history, or politics. But two questions, among many, arise from reading his study. First, how successful was the Iowa railroad legislation at meeting the demands of Alliance farmers? Ostler never answers this question. If he had looked to Minnesota for comparison, he would have found a “farmers’ railroad bill, passed in 1885, that failed to meet Alliance demands. A resurgent Alliance came back in 1886 and 1890 to demand more reforms. Some analysis of the effects of the 1888 railroad law in Iowa would have strengthened his argument. Secondly, to what extent were fusion and third-party tactics used simultaneously in the same states? Again, Minnesota provides an interesting counterpoint. In 1890 the Minnesota Farmers’ Alliance ran a full slate of candidates for state office as a third party, but at the local level, county alliances ran the gamut of options from straight fusion.