Over the past quarter century, a new American history has been written. This rewriting of American history has often been associated with the “triumph” of social history within the discipline, but in fact the transformation is much broader than that: the domain of the historical has been vastly extended, inherited narratives displaced, new subjects and narratives introduced.

While at the monographic level, one sees similar developments in various national historiographies, national synthesis—and the idea of a national synthesis—seems to have been less troubled elsewhere than in the field of U.S. history. Admittedly, generalization is risky, especially if one reaches into historiographies with which one is barely familiar. Still, I think that a variety of outstanding national histories (or histories of a people sometimes treated as nations) have been more confident of established narrative strategies. With the exception of the historians of France that I will note, historians of other modern nations seem to have had fewer doubts about the basic framing of a narrative synthesis, and they have not felt compelled to develop new approaches, even though in many cases the other work of the authors involved has been strikingly innovative. Yet the social, intellectual, and political developments that have complicated American historiography are likely, I suspect, to make themselves felt in other national historiographies fairly soon, a point recently made by Jacques Revel, a leading French historian. And that circumstance may spawn a generation of controversy about the politics and strategies of synthesis. If so, the American case may be of more general import and interest.

Beginning in the 1940s, intellectual history became the synthesizing subfield in U.S. history, replacing the political-economic narratives of Frederick Jackson...
Turner and Charles A. Beard. But during the 1970s, the claims being made for a national mind or culture were challenged by social historians. Intellectual history was chastened and transformed by the confrontation with social history. Eschewing their former embrace of synthesis, intellectual historians pulled back to study more precisely defined themes and thinkers. Not only intellectual history but other subfields accommodated social history’s provocation to rethink conventional generalizations. In addition, a professional, even “social-scientific,” concern for precision and specificity of reference collaborated—sometimes with forethought, often not—with a sharpened awareness of difference and conflict that came from social movements outside the academy to undermine older composite narratives.

Neither the frame supplied by Charles and Mary Beard in *The Rise of American Civilization* (1927), with its dramatic narrative of conflict between the “people” and the “interests,” nor the consensual pluralism that succeeded that interpretation in the 1950s survived. If the consensus historians underplayed conflict, the Beards’ approach, for all of its sympathy for the dispossessed, was found to be inadequate as well. Their narrative revealed little feel for the diversity of Americans, and it paid scant attention to non-whites. Most important of all, while their narrative voice was sympathetic, one did not discover the quotidian life or hear the voices of those groups that have found voice in more recent historiography. Judged by newer historiographical expectations, *The Rise of American Civilization* seemed “thin,” compared with the increasingly popular “thick” description that was built, in part, on the enormously influential anthropological work of Clifford Geertz.

In the past quarter century, there has been a proliferation of exciting new research, much of it bringing previously overlooked or explicitly excluded groups and events into the light of history. The number and variety of American stories multiplied. Suddenly, there were histories where there had been none or where the available histories had not been attended to by professional historians: histories of African Americans in the era of slavery and beyond; of Native Americans; of workers at home in their communities, at work, and at play; of women at home and outside of the home and of gender relations more generally; of consumption as well as production; of ethnic minorities and “borderlands”; of popular culture and other “marginal” forms of cultural production; of objects and material culture; of whites and whiteness as historical subjects; of non-state international and intercultural relations; and much more.

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4 Frederick Jackson Turner never completed a major synthesis, but one can see how he might have done that work in his posthumously published *The United States, 1830–1850* (New York, 1935); Charles A. Beard and Mary R. Beard, *The Rise of American Civilization* (1927; 2 volumes in 1, New York, 1930).


By the early 1980s, some commentators inside and outside the profession were wondering whether an American history had disappeared in the onslaught of highly particular studies, often about subgroups in the larger society of the United States. These developments were occurring at a moment when the number of American historians was expanding to an unprecedented degree. Disciplinary expansion both allowed and prompted increased specialization. And that worried some, who began to speak of hyperspecialization and fragmentation. The structure of specialization derived in large part from the impact of a social history that often fused the group-based particularity of focus with ideological commitments to class and identity-based social movements. This pattern of work discouraged the integration of particular histories into some kind of synthesis.8

Traditionalists, perhaps not surprisingly, were unnerved by these developments.9 But even some proponents of the newer history worried. Early on, Herbert G. Gutman, one of the leading figures in the movement to write a history that included all Americans and that recognized differences—class, ethnic, racial, gender—was concerned that instead of enriching and enlarging the usable history of the United States, the new scholarship was failing to do that, perhaps making it in fact less usable. The "new social history," he wrote in the introduction to his collection of pioneering essays in the field, "suffers from a very limiting overspecialization." Take an Irish-born Catholic female textile worker and union organizer in Fall River involved in a disorderly strike in 1875. She might be the subject of nearly a dozen sub-specializations, which would, he feared, "wash out the wholeness that is essential to understanding human behavior."10 Later, in the wake of a national meeting of writers at which historians and history seemed to be largely ignored in discussions of the political and cultural situation in the aftermath of Richard Nixon, Gutman mused aloud in the pages of The Nation over whether the failure of historians to incorporate social history’s findings into a new synthesis had seriously diminished, even evacuated, history’s possible contribution to public debate.11

In the mid-1980s, in what turned out to be a controversial pair of articles, I raised a related question: how might one construct the (to my mind) needed synthesis of recent historiography on the United States.12 There was considerable negative reaction to those articles, coming from two different positions. One

8 For an insightful and quite worrisome examination of recent scholarly practice and its trajectory, see Winfried Fluck, “The Modernity of America and the Practice of Scholarship,” in Thomas Bender, ed., Rethinking American History in a Global Age (Berkeley, Calif., 2002).
9 See, for example, Gertrude Himmelfarb, The New History and the Old (Cambridge, Mass., 1987). This volume includes essays published by Himmelfarb between 1975 and 1984.
position worried about its critique of specialization and its call for addressing a larger public. These arguments were equated with a carelessness about scholarly rigor. The other, and more widespread position, focused on the risks of a national narrative itself. It was evidently feared that such a narrative would, by definition, re-exclude those groups and themes that had so recently been brought under the umbrella of history and would re-inscribe a “master narrative” dominated by white, elite males.

By the end of the 1980s, however, the question of synthesis had become less controversial. The issue became more practical, more professional in some sense: how to do it and how to do it within the parameters of inclusion that had been central to the discussion from the beginning. It was on this note that Alice Kessler-Harris, the author of the chapter on social history in The New American History (1990 edition), addressed the question. In the last section of her essay, with the section title of “The Problem of Synthesis,” she acknowledged the problem and explored various possible ways to overcome “fragmentation” and move toward synthesis.

A different issue emerged in the 1990s. Poststructuralist literary and cultural theory, sometimes broadly and even more vaguely characterized as postmodernism, was and is suspicious of any aspiration toward a comprehensive narrative. It is to this body of theory that we owe the commonplace use and misuse of the epithet “master narrative.”

These theories have been rather slow to penetrate workaday historical practice among American historians. Levels and types of awareness of them vary: from shocked indignation at the whole idea, to vague awareness and thoughtless dismissal, to intellectual fascination largely in isolation from the making of one’s own histories. In his recent book, Beyond the Great Story: History as Text and Discourse (1995), Robert Berkhofer seeks to force more attention to these issues. Insistently, but not always consistently, he urges historians to recognize the dimensions of the postmodern crisis that surrounds them. He seems more interested in sounding the alarm about the quicksand before us than in guiding us

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around it or safely through. But either way, he intends to challenge the very possibility of narrative synthesis.17

While these worries, proposals, and polemics were being fashioned, the daily work of historians proceeded. Among the products of that work have been a good number of explicitly synthetic volumes. There is, of course, no clear or settled notion of what defines a work of synthesis. I have used a rather generous definition. Some of the books I am calling synthetic might alternatively be designated as monographs—archivally based but exceptionally ambitious books that tackle big questions and seek to frame a large field or to provide an interpretation for an audience well beyond specialists. Others are more obviously synthetic, relying heavily on secondary literature to establish the state of the art in a broad field for a wide audience, including, often, students and the general public. With this diversity of form, purpose, and audience in mind—as well as a concern for a reasonable distribution of fields and periods—I have, with the help of the editors of the American Historical Review, selected a few recent synthetic works for examination.18

The very existence of these books mutes the question of whether we need synthetic works or whether, under the constraints of present historiographical practice, synthesis is possible. In fact, the seeming proliferation of syntheses at present—and their variosity—suggests that the field of American history is at a formative (or reformative) moment that invites synthesis: the quest for new understandings that has undermined established narratives has now, perhaps, prompted new efforts at crystallizing a very unstable body of historical writing into new syntheses.

A different question, however, provides the focus of this essay. What strategies for narrative synthesis are available to historians today? How might we think about the relation between a particular structure of narrative synthesis and the author's purpose or interpretation? How do these different strategies relate to current historiography? What particular work do they do, within the profession and beyond it? And finally I want to ask some questions about the firmness of the boundaries (mostly geographical) that define what is and is not captured in synthetic narratives of U.S. history.

These works do not, of course, cover the whole field of synthetic works. More and other books could have been chosen, but these eleven books (and several others mentioned along the way) at least represent different kinds of history, different


18 None, incidentally though importantly, present themselves as synthetic narratives of the nation, although some to be discussed below certainly reach toward that in practical effect, particularly those authored by Eric Foner (The Story of American Freedom) and by Robert V. Hine and John Mack Faragher (The American West). In fact, I have recommended each to non-historians asking for a literate one-volume history of the United States.
periods, and different themes. Together, the eleven total nearly 6,000 pages of outstanding historical writing. If nothing else, I can conclude that synthetic narrative invites long books.

Because I cannot claim special knowledge in any of the fields being synthesized in these books, I do not propose to do the kind of analysis one would find in specialized reviews. Such criticisms that I have will be framed from the position of my interest in synthetic narrative. I say that in part to be honest about my own limitations in appraising these books but also for another, more positive reason. I want to insist that narrative synthesis is a form of knowledge, indeed, a particularly powerful form of creating, not simply summarizing, knowledge. I hope to get past or under the story enough to probe the implications of different modes of structuring a narrative synthesis. The way different narrative strategies construct that knowledge is important. While inclusion is one of the tests our generation will rightly ask of synthesis, there are other important historiographical issues that are embedded in the question of narrative synthesis.19

The more seriously we consider possible narratives of American history, the more we may be prepared to ask questions that press beyond inclusion. We may even be both bold enough and hopeful enough to worry a little about the language of inclusion, if not the principle. Is there perhaps more than a hint of dominant culture noblesse oblige in the language of inclusion? Might not a more sophisticated notion of the temporal and geographical boundaries of American history, including an awareness of the diasporic stories within American history, complicate and enrich the notion of inclusion?20 Can the historical and historiographical terrain be opened a bit more in a way that enables a deeper, denser, and more complex historiographical exploration of justice and difference at the center of American history? Might democracy be the word, the concept, the commitment that will move us in that direction? As I examine the stack of books before me, I propose to keep these issues in mind and to return to them at the end of this essay.

**Jon Butler's** *Becoming America: The Revolution before 1776* (2000) covers the whole mainland British colonial space and history, and it addresses a wide range of themes. In fact, themes, not time or chronology, organize his story. His brief, often one-word, chapter titles reveal a very distinctive type of synthesis, one immediately accessible to the reader, whether professional or lay: Peoples, Economy, Politics, Things Material, Things Spiritual. It is a reasonable progression, and in each case he brings together a good deal of material. Although his theme is transformation, Butler also claims (following recent historiography) a more inclusive geography, making more of the middle colonies than would have been the case a generation ago.

In some ways, his manner of organizing the material topically bears a relation to

19 I do not propose to go into theories of narrative or even my own notions, but I will here indicate that my understanding has been greatly influenced by the work of Paul Ricoeur, *Time and Narrative*, Kathleen McLaughlin and David Pellauer, trans., 3 vols. (Chicago, 1984–88).

20 Such thinking is not restricted to specialists in the profession exploring the theme of diaspora. The novelist Russell Banks has recently argued that the focus for a synthesis of American history ought to be the African diaspora. See "The Star-Spangled Novel," *Los Angeles Times*, July 2, 2000.
Richard Hofstadter's posthumously published *America at 1750: A Social Portrait* (1971). But what might have worked for Hofstadter, who was setting the scene for a three-volume narrative history of the United States, works less well for the purposes Butler has in his book. If Hofstadter's book was intended to provide a snapshot that would serve as a starting point, Butler's title ("Becoming America") and his stated intentions announce change as his theme. He means to persuade the reader of a broad pattern of transformation that produced a distinctive and modern society in advance of 1776 and that in turn spawned the first modern revolution. Such an argument demands more complex and careful attention to process and cause than his framing of the book seems to allow. While he has surely gathered together a considerable body of material (his notes run to fifty pages), he has not produced a synthetic narrative of change over time, one that sketches a developmental sequence that integrates disparate elements in the interest of a causal interpretation. By bounding each unit of synthesis, Butler is stuck with a structural isolation of topics that undercuts narrative explanation. Given that Butler's theme is transformation, this narrative structure is crippling.

For reasons related to structure and style of argument, Butler's claims for American modernity are quite vulnerable. While there are doubtless some specific ways in which the British North American colonies became "modern" before independence, they were not uniformly modern—over space or in all aspects of life. Many historians would readily grant numerous anticipations of modernity by the middle of the eighteenth century, but few would insist, with Butler, that so much modernity had been achieved so soon, implying that only a few pre-modern anomalies remained on the eve of revolution. Most give a significant role to the revolution.

But the most serious problem is not with the phenomena he notices or does not notice, even if there is some real unevenness on this point. Rather, it is Butler's teleology of the modern, combined with his exceedingly loose, elusive, and, as is so often said today, undertheorized definition of modernity. Add to this an unnecessary but apparently irresistible tendency to claim American uniqueness and "firsts" for nearly everything he identifies as modern in America. He names a number of phenomena that he considers evidences of the modern—polyglot, slaves, cities, market economy, refined crafts and trades, religious pluralism, and "sophisticated politics." Without further historical specification and theoretical precision, one can indulge in *reductio ad absurdum*. With the exception of religious pluralism, all of these qualities probably described Athens in the age of Aristotle at least as well as the British colonies. In fact, I suspect that Peter Linebaugh and Marcus Rediker, relying on their recent book *The Many-Headed Hydra: Sailors, Slaves, Commoners, and the Hidden History of the Revolutionary Atlantic* (2000), would argue that the

Atlantic world provides a better example of modernity on those terms than does the colonial mainland.²⁴

He makes many claims for American distinctiveness. In the end, however, it is diversity, which he tends to equate with multiculturalism, that for Butler makes Americans modern. But if we look around, we cannot but wonder about his claims for a uniquely polyglot society. This assertion may be quite vulnerable from any sight line approaching a global perspective. Can he fairly claim that New York City harbored a level of diversity “never before gathered together”?²⁵ Might not this be as plausibly said of Constantinople during the period covered by Butler's book? And did not the Ottoman Empire—of which Constantinople was the capital—far exceed the religious and ethnic diversity of the British colonies?

My point here is partly one of fact, of care in making comparative statements without comparison. More important, however, are the criteria of the modern. Few, if any, major political bodies in the past half millennium more successfully accommodated diversity than the Ottomans, yet that achievement has never brought them recognition for a precocious modernity. One needs greater definitional and descriptive specificity to make the argument he claims. Because of the breadth and generality of synthetic narratives, it is especially important to be clear about key concepts.

Similarly, he tends to claim the realization of “Americanness”—here equated with some vague notion of modernity—for events that, however interesting in themselves, hardly sustain his assertion that they designated “the American future.”²⁶ For example, writing of the French Huguenots, a group he knows well, he notes their assimilation, and he calls this “American.”²⁷ Well, of course it is, but so are the endogenous marriages that continue for various groups well into the twentieth century—sometimes because of racial difference and even legislation (as in the case of African Americans) or out of choice, as in the case of Scandinavians in the upper Midwest. Or to take a more ominous subject, it seems a bit fatalistic to say that colonial encroachment on Indian land “predicted” nineteenth-century relations with the Indians.²⁸ Oddly, such a claim, while taking the moral high ground, nonetheless erases the postcolonial history of the United States by denying contingency and thus diminishing both the capacity and moral responsibility of all later actors or potential actors. The twin and linked teleologies of “modern” and “American” produce a distorting and de-historicizing synthesis.

If there is a problem with the sort of synthesis Butler has written, what precisely is it? He makes historical claims about patterns and meanings of development on the basis of a narrative structure that effectively isolates and de-historicizes his themes. By not constructing a developmental narrative that integrates the various themes now separated in distinct chapters, the process and complexity of development is obscured. While his chapters are full of relevant and interesting details of

²⁵ Butler, Becoming America, 9.
²⁶ Butler, Becoming America, 36.
²⁷ Butler, Becoming America, 22. One of Butler's previous books is The Huguenots in America: A Refugee People in a New World Society (Cambridge, Mass., 1983).
²⁸ Butler, Becoming America, 68.
everyday life, they never get integrated in any individual, institution, or place. In the absence of a narrative of change to explain and interpret, he resorts for a theme to repeated assertions of “modernity.” The issue is not so much the claim for an eighteenth-century American modernity—although I am myself drawn to much more complex, nuanced, and contradictory discussions of that theme—as it is the incapacity of the particular model of synthesis he deploys to advance that theme or argument.

Philip D. Morgan’s Slave Counterpoint: Black Culture in the Eighteenth-Century Chesapeake and Lowcountry (1998) is at once similar to Butler’s and quite different. Both focus tightly in each chapter on a particular topic or theme; there is little play among the different themes in both cases. While Butler’s themes propose a reorganization of material, thus giving an impression of freshness, Morgan’s quite important questions are phrased in well-established ways. While Butler’s structure works against his theme of transformation, Morgan’s similar structure better fits his goals for the book, partly because transformation plays a smaller role in his analysis than one might expect.

Slave Counterpoint addresses nearly all the issues raised by a half century of vigorous scholarship on the beginnings of slavery, the practices of racial slavery as a labor and social system, and the nature of African-American culture in early America. It is a book of enviable learning: with a seeming total command of the historiography and an impressive knowledge of a substantial archival base, Morgan proceeds to pose (or re-pose) difficult historiographical issues. Again and again, he offers compelling answers. Want to know what scholarship has disclosed about slavery and African-American culture in the eighteenth-century Chesapeake and Lowcountry? Look to Morgan’s synthesis of a generation of scholarship.

To have done that is to have done a great deal, and he has done it magnificently. Yet one gets the sense of a summary volume, a volume driven by the past, by past questions. Synthesis can either cap a phase of scholarship or initiate another. I think Morgan’s book falls into the former category, while Ira Berlin’s new book, Many Thousands Gone: The First Two Centuries of Slavery in North America (1998), which also relies on a generation of scholarship and addresses many of the same issues, has the potential to become a new starting point. Berlin has captured the shift to an Atlantic perspective that has increasingly characterized scholarship by early modern Europeanists, Africanists, Latin Americanists, and historians of British North America. In this sense, his work, at least the early parts that sketch out and populate the Atlantic littoral, points forward.29

In a dramatic opening section, Berlin, relying more on secondary literatures than does Morgan, locates his story in very broad understandings of time (periodization) and space (the Atlantic world), the dimensions of which are shadowy, almost invisible, in Morgan’s account. He locates Africans in an Atlantic history connecting four continents and in a rich and growing historiography reaching out from Europe, Africa, Latin America, and North America.30 One

30 Berlin’s powerful evocation of the Atlantic builds on many predecessors. At minimum, mention should be made of Philip D. Curtin’s The Atlantic Slave Trade: A Census (Madison, Wis., 1969); and The
wishes Berlin had sustained this perspective in the later sections. But even if he narrows the story to the territory that later became the United States and loses the multiple histories implied by his portrait of the Atlantic world, the beginnings of stories, whether novels or histories, are heavy with intention and implication that can, I hope, be built upon.  

In fact, the four Atlantic continents remain an always changing aspect of American and African histories. Attending to, or at least recognizing, that larger and continuing extended terrain of American history would enrich the story of the making of African Americans and America, a historiography that is at present too much captured by an implicit and too simple assimilation or “Americanization” model. Nonetheless, Berlin has provided a powerful image of the creation of the Atlantic world and of the origin of modern slavery within it.

Morgan has a quite different strategy. His domain is not the Atlantic but the South, or two regions of the South, which he is anxious to reveal as differentiated. Thus his is a comparative history, comparing two regions within the South. Suggesting a certain scientific aspiration, he refers to his delimited space as a kind of laboratory, a site for an “indirect experiment.” This approach offers him much. He is able to focus tightly on his questions and generally achieves sharply phrased answers. Yet, like any good scientific laboratory, his field of inquiry is almost hermetically sealed. A two-hundred-page part of the book titled “The Black World” begins with a fifteen-page section on “Africans.” Yet it is in only one paragraph at the beginning and a few other scattered references that one reads anything about Africa. His story rarely strays east (or south or north or west) of the Maryland/Virginia and South Carolina boundaries.

His comparative method has impressive rigor. Yet one senses that not only does his approach trap him within a particular place, he is also caught within a very confining net woven from the existing historiography. As Walter Johnson pointed out in a review of the book in this journal, his questions are smaller than the stories he has unearthed. Much like another important book on African-American history, Herbert G. Gutman’s *The Black Family in Slavery and Freedom* (1976), this book, for all its synthetic aspirations, cannot capture some of its best material within the tightly bounded historiographical questions and issues that frame it.

As in the case of Berlin’s book, Morgan’s is quite explicit about time and space. There is a well-thought-out chronology of change, and one of his major arguments is that the South, and thus the black as well as white experience, was not uniform over space. He shows real and important distinctions between the experience of

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slavery in the Chesapeake and in the Lowcountry. Yet by treating both the temporal and spatial aspects of the story as sites (and very limited ones) rather than as processes of historical making, he weakens the capacity of his local analyses to explain change over time and, to a lesser extent, space. His major explanatory claims appear in the introduction. They are not only brief but also separate from the rich stories he tells and the analyses he makes of historiographical questions. The expansiveness of Many Thousands Gone, by contrast, evokes a strong sense of change, of process. It achieves a narrative synthesis of the movement of Africans onto the Atlantic and into the Western hemisphere. The difference between this approach and the tightly controlled analysis crafted by Morgan is striking.

Like Morgan's, Michael Schudson's book, The Good Citizen: A History of American Civic Life (1998), is organized around fairly established questions—especially one big question. Has American civic life deteriorated over the course of the past three centuries? Naturally, the question is of a different order than those driving Morgan's analysis. It has not been generated by disciplinary scholarship. It arose out of American public life. Schudson thus draws on history and other disciplines to address directly a public question, one endlessly repeated today and, as he shows, in the past.

Schudson himself, we should note, is not a historian. He was trained as a sociologist, and he teaches in a Department of Communication. While he reveals an impressive command of the relevant historiography, historians are not his primary reference group or audience. Although I am sure specialists will find some of his formulations to be of considerable historiographical significance and likely to encourage new lines of research, his intention, again, is different: his audience is a general one, and he seeks to bring historical knowledge to bear on a civic issue.

What he is doing points toward the most important work that one kind of successful narrative synthesis can do, for the profession and for the public. By openly declaring his address to a public issue and for a public audience, Schudson participates in a very important tradition of historical writing. Some of the very best professional historians of the United States in this century have done precisely that: Frederick Jackson Turner, W. E. B. Du Bois, Charles Beard, and Richard Hofstadter all focused on issues, worries, or preoccupations of fairly general interest to write synthetic works that importantly rephrased fundamental themes in American history. This mutual enrichment of public and professional discourse is perhaps the ideal cultural work of narrative synthesis. Let us hope that historians can do this more often and more effectively. Yet as I make this point, I realize that all of the historians just named, including Schudson himself, were either trained as social scientists or did not recognize a significant boundary between history and the


(other?) once more expansive social sciences. Is this a mere coincidence, or is it an issue to be addressed by the profession?

While I would not place Schudson’s book in the same class as the scholarship produced by the short list of great historians, he has written a fine book. It is a book about change over time, and he establishes three eras of citizenship and participation, each clearly defined. He does not devote much attention to how each configuration changes into the next, but he effectively characterizes their differences, even in some very brief summaries, as in the following paragraph from early in the book:

Another way to characterize the past three hundred years of political change is to say that the type of authority by which society is governed shifted from personal authority (gentlemen) to interpersonal authority (parties, coalitions, and majorities), to impersonal authority (science, expertise, legal rights, and information) . . . The geographical center of politics has shifted from the countryside to the cities to the suburbs and perhaps, today, to “technoburbs,” “postsuburbs,” or “edge cities,” or whatever we name our newer habitations. Correspondingly, the kind of knowledge a good citizen requires has changed: in an age of gentlemen, the citizen’s relatively rare entrances into public discussion or controversy could be guided by his knowledge of social position; in the era of rule by majorities, the citizen’s voting could be led by the enthusiasm and rhetoric of parties and their most active partisans; in the era of expertise and bureaucracies, the citizens had increasingly to learn to trust their own canvass of newspapers, interest groups, parties, and other sources of knowledge, only occasionally supported by the immediacy of human contact; and in the emerging age of rights, citizens learn to catalog what entitlements they may have and what forms of victimization they may knowingly or unknowingly have experienced.37

This paragraph reveals the argument and the narrative strategy that Schudson uses to undercut the widespread notion of civic decline: rather than a story of decline, it is one of restructuring, one that recalibrates citizenship and civic practice in relation to changing values and social experiences. What some, including me, see as the erosion of our public life and the thinning of American political culture, he presents as a complex rearticulation of expectations and institutions. Whether one fully agrees with Schudson or not, the book and the point of view it ingeniously argues constitutes an important contribution of contemporary civic life. And a narrative strategy of restructuring (as opposed to the usual rise or fall scenarios) deserves a place in the historian’s menu of narrative types.

“Presentist” purposes may, however, carry the danger of anachronistic readings. Schudson is vulnerable on this score, especially in his consideration of the colonial period. He too easily asks how democratic any phase of political life was. A commitment to explore the fate of democracy in our past—something I endorse—surely includes recognizing when democracy is not an available concept. He might better have asked how the legitimation and exercise of power worked. Indeed, such a deeper historicism would complement his anti-anti-Whig approach.

Similarly, while a then-and-now binary invites sometimes interesting questions and offers some illumination of past and present, it also invites problems. Again, one sees this risk in Schudson’s work. False categories of judgment are explicitly or implicitly brought to bear. Speaking of the first generation to live under the

Constitution, he observes that little political knowledge was expected of voters, “at least little of the sort of knowledge that today’s civic moralists urge upon people.” Voters then were expected to have “local knowledge—not of laws or principles, but of men.” The binary obscures the role of principles in the past and knowledge of men in the present. Most important of all, it diverts our attention from the principles that it was thought would aid voters in judging character. Sometimes, by focusing so much on the party system that we worry about today, he overlooks those important issues that eluded the parties or that parties avoided. Substantive issues—the reason citizenship and civic life are important—are marginalized in his account of the different concepts and patterns of public life. The result, whether intended or not, is a form of consensus history.

“Progress or decline is not the real question,” Schudson concludes. He converts that question into one of restructuring that points to his core argument: there must be a fit between forms of citizenship and forms of everyday life, between values and institutions, between aspirations and commitments. It is that historically informed understanding that allows him in his conclusion to speculate in quite promising ways about an evolving pattern of citizenship that may yet serve our collective hopes and needs. Still, his conclusion leaves me uneasy. Like the journalistic coverage of politics today, the substance of political conflict is subordinated to discussion of the “health” of the system, of the institutions and practices.

By contrast, the tensions, conflicts, and substantive issues that made politics so important in the development of the United States and in the lives of individuals are at the center of Eric Foner’s *The Story of American Freedom* (1998). Foner’s book has an uncanny resemblance to one that at first glance might seem utterly unrelated: Richard Hofstadter’s *The American Political Tradition and the Men Who Made It*. Of course, Foner inverts the point Hofstadter sought to make. If Hofstadter famously played down conflict and (less remarked upon) paid little attention to the social making of political ideologies, Foner emphasizes conflict and the changing historical construction and reconstruction of the idea and ideology of freedom. Foner’s work is much more explicitly sensitive to social history, even if it parallels Hofstadter’s in its interest in ideology and the limits and possibilities of American political culture.

While Hofstadter was alternately comic and ironic, bitterly so at times, in *The American Political Tradition*, Foner’s *Story of American Freedom* is strikingly fair and straightforward. Yet the underlying hope is similar. As James Oakes has perceptively noted, Foner’s narrative is undergirded by an unstated but firm liberal ideal of freedom, one that at once shares in an Enlightenment universalism and

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38 Schudson, *Good Citizen*, 81.
40 See, for example, his summary judgment of the party system at Schudson, *Good Citizen*, 132. Put differently, it bears at least a formal relationship to the theories of pluralism popular in political science during the 1950s.
41 Schudson, *Good Citizen*, 313.
accommodates current concerns for inclusion and regard for difference. I would even argue that Hofstadter's own liberal position was closer to Foner's than one might at first suspect. Both appraised American political culture and its prospects from the position of a richer, more textured liberalism than we usually recognize in current debates.

In thinking about the core issue in Foner's narrative, therefore, it seems fair to consider it to be the quest for a democratic liberalism, insisting on the relevance and indispensability of the modifier inserted before liberalism. One might thus characterize Foner's as a democratic synthesis, which, as I suggested above, offers a stronger and more egalitarian standard of judgment than commonplace invocations of inclusion. It offers as well the implication of voice and empowerment.

To Foner, as he indicates in his introduction, “abstract definitions” of freedom are not the focus. His concern is “with the debates and struggles through which freedom acquires concrete meanings, and how understandings of freedom are shaped by, and in turn help to shape, social movements and political and economic events.” The result is a narrative that is at once focused yet always open to an examination of larger issues, structures, and events that intersect with and often drive his story. It is a dynamic story, filled with actors, with agents making freedom and using freedom. He selects key events or controversies of different eras, events that are widely contested (slavery, labor and property, the role of the state, social movements). Of course, coverage is selective; the gain is the richness deriving from a series of concentrated focal points. In each case, he examines the conflict, the parties contending, and the stakes. He does not hesitate to declare justices and injustices, to name winners and losers, and he does so from a consistently democratic perspective. Foner thus achieves inclusion without the dilution consequent with the faux openness characteristic of talk radio and without the postmodern hesitations that undermine moral judgment.

_The American West: A New Interpretive History_ (2000) by Robert V. Hine and John Mack Faragher spans the whole of American history, from “the European invasion” until the present. The book is written in the spirit of Frederick Jackson Turner. Instead of lamenting the ambiguity of Turner's conception of the frontier, which after Turner got reduced by rigorous historians to a place, the West, Hine and Faragher embrace its fullness. For them, the frontier is both a place and a process. and they recognize that it is not only impossible but limiting to separate

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44 For just such a contemporary theorization of liberalism, see Ira Katznelson, _Liberalism's Crooked Circle: Letters to Adam Michnik_ (Princeton, N.J., 1996). Interestingly, this work also comes from a Columbia scholar, however much it is openly acknowledged to have derived largely from his experience at the New School for Social Research. Perhaps the relevant context for this liberalism is the city of New York, with its cosmopolitan character and free-for-all quality of political contestation. For a brief statement of Hofstadter's relation to liberalism, see Thomas Bender, “Richard Hofstadter,” in _American National Biography_, John A. Garraty and Mark C. Carnes, gen. eds. (New York, 1999), 11: 1–4.


46 In Eric Foner, _Reconstruction: America’s Unfinished Revolution, 1863–1877_ (New York, 1988), where chronological compression allows for a richer analysis, one can see more fully the method and its achievements.

and sharply distinguish between the two aspects of the concept. That openness allows them to tell the history of the United States as a story of successive frontiers, including a fascinating rethinking of American regionalism as urban-centered at the end of the twentieth century.48 In fact, the chapter on the postwar era is a tour de force—imaginative, original, and quite compelling.

In Turnerian fashion, they argue that “westering defined America’s unique heritage.”49 To a very impressive degree, they give substance to this claim, but recent historiography makes that claim, even for western history, problematic. As Hine and Faragher show, in the nineteenth century as well as today, the West (and the United States) was formed by migrations from west to east and south to north, and even in a limited way north to south, as well as east to west. The notion of westering is so strong in American and European history and culture, it is difficult to construct an alternative narrative structure, though no less important for the difficulty.50 This worry does not, however, undercut another summary point they make: the “frontier is our common past.”51

The book is grounded in social history. Of all the books under consideration here, _The American West_ is probably the most sensitive to the categories of experience and groups previously excluded from mainstream narratives of American history. Their work goes well beyond mere representation of such groups and categories; previously invisible groups, whether Native Americans, migrating women, African-American settlers, working people, or the people of the borderlands, are actors who contributed to the shaping of history. But there are limits to this achievement. While there are multiple positions and voices represented in their narrative, only rarely does their narrative bring the reader inside group life. There is not much inquiry into the interior experience and subjective meanings shared by the various groups identified and recognized.52

While the story could have been situated in a wider context, one that revealed the global reach of the empires or, later, the importance of global markets, in its particular geographical focus the book consistently avoids privileging the English line of settlement. Other settler efforts are considered and sometimes compared. As is often the case with synthetic histories, however, there is a tendency to do the work of inclusion at a particular moment, and then lose the group at issue. For example, there is a good discussion of the origins of racial slavery, but the later


49 Hine and Faragher, _American West_, 531.


51 Hine and Faragher, _American West_, 560.

52 In fact, they concentrate this kind of analysis in one chapter, a fascinating one in “A Search for Community,” but it is limited in its cases, and it segregates such analysis from the greater part of the narrative. Hine and Faragher, _American West_, chap. 12.
extension of the plantation system and internal slave market that was a part of the frontier movement is not adequately recognized.

At times, the transnational themes they develop are extremely illuminating. They refer to what would later be characterized by theorists of the global cities as a "dual economy" in describing the role of foreign migrants, especially Chinese, in the nineteenth-century California agricultural economy. Likewise the interplay of national and international in their discussion of the Zimmerman telegram inviting Mexico to ally with Germany in World War I and in their discussion of San Francisco's "commercial hinterland." But, as in the case of Butler's book, there is a bit of parochialism in making claims of distinction. Perhaps such assertions can be demonstrated, but more rigorous definitions and empirical research than we have here are required to establish, for example, that the United States is today the world's most multicultural society. How would it compare with Indonesia, the world's fourth most populous nation, whose citizens speak more than 100 languages and live on almost numberless islands?

The social-history approach, whatever its success in representing difference, has in this instance under-represented national political institutions and policies. The development of the West, as Richard White and other historians of the West have pointed out, was profoundly indebted to what western Republicans now call "big government," for water, transportation, Indian removal, and, more recently, direct investment, as in defense contracts and installations and aerospace industries. The political economy and the role of markets, as has already been suggested, do not get the attention they deserve. We often overlook how much industry was in the West, and how much western industries—from milling and meatpacking to mining—were integral to the industrial system of the United States. And we forget how much the astonishing productivity of western agriculture enabled the formation of a large urban industrial labor supply. More of these dimensions of western history might have been included if only in the interest in enabling the story better to tell the national experience.

If Hine and Faragher encompass both the full geographical and temporal dimensions of western history, Linda Gordon's microhistory builds out from a very delimited western space, the Sonoran highlands of Arizona, to develop a highly innovative narrative synthesis that locates itself at the various and causally interrelated scales of town, region, nation, and the transnational. Her work reminds us that there is a difference between a mere local study and a microhistory. The local histories of villages, towns, and cities, so common in the 1970s, tended to use global concepts but within artificially bounded fields of inquiry. One of the most famous of them all, Kenneth Lockridge's study of Dedham, Massachusetts, offered an isolated inwardness as a principal finding, although it was a finding that derived

54 Hine and Faragher, *American West*, 395–97, 414. This story could be greatly expanded. San Francisco was closer to Asia than to Europe, a simple geographical point that usually eludes us. For an outstanding study of this relationship, see Ian Tyrrell, *True Gardens of the Gods: Californian-Australian Environmental Reform, 1860–1930* (Berkeley, Calif., 1999).
56 Richard White, "It's Your Misfortune and None of My Own": A History of the American West (Norman, Okla., 1991).
mainly from a methodology not only local but firmly bounded. By contrast, Gordon's *The Great Arizona Orphan Abduction* exemplifies a microhistory that enables the historian to synthesize the threads of local life, many of which are translocal in origin and implication. Unlike Hine and Faragher, she gets inside the subjective experience of local life, even the experience of very ordinary people, without getting trapped inside that world and without implying that the larger world of the region, the nation, and even transnational economic and religious institutions were beyond the ken of her study of a seemingly local conflict.

Mostly, her account is the story of the arrival and fate of Catholic orphans from New York who were to be placed in Catholic homes. The homes were Mexican as well as Catholic, and that was the problem and the focus of conflict. The conflict played out along class, ethnic, religious, and gender lines, and it eventually reached the Supreme Court. It is a compelling and very human narrative, but one that also addresses a whole range of analytical and interpretive issues of broader interest to historians. Bringing the issues of gender, class, and race into relation with each other allows for an appraisal of their relative importance in this particular historical explanation. I think that her story reveals class to be more important than her conclusion argues, but the real point to be made is that only a narrative synthesis that brings diverse threads together will enable the historian and the reader to make this kind of judgment.

These complex ends are achieved in part by her adoption of an imaginative literary strategy. Gordon's book is constructed of two types of chapters. One is quite often a broad frame for local events. In these chapters, her perspective as narrator is exterior to the action. The issues addressed are frequently structural and, as often as not, extend beyond the community. Here, one gets an analytical explanation of the relation of local experience to larger national and international cultural, political, and economic developments. Between these chapters, she has crafted others that get inside the culture of the community, providing wonderfully rich, thick descriptions of daily life and the development of the conflict. With oral histories as well as fragmentary documentary evidence, she brings the reader very close to the experience and voices of the community. The play between these accounts and the more conventional chapters produces an unusual but powerful synthesis.

Whether a microhistory qualifies as a synthesis, even by my generous definition, may be debated. But the singular relevance of this book for the discussion of synthesis concerns not scale but its literary ambition, the literary experiment that gives structure to the book. Those who would write other syntheses—at various scales—will, I hope, be encouraged, even inspired, to experiment with novel narrative strategies in the interest of more powerful representations of the past.

Quintard Taylor presents a third version of western history, *In Search of the Racial Frontier: African Americans in the American West, 1528–1990* (1998). He offers a broad synthetic account that characterizes the experiences of African Americans over a very long period of time. While the book does not ignore

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relations among different groups in the West, particularly and inevitably between blacks and whites, but also between blacks and Native Americans, the contribution of the book is otherwise.\textsuperscript{59} He is mapping and making visible as a whole a history that has been largely unknown or studied in very specific instances and places. Drawing on a substantial body of scholarship, most of it published in the past quarter century, he aims to "reconstruct the history of African American women and men" in the West over five centuries, although mostly his focus is the nineteenth and twentieth centuries.

Taylor's central themes are the quest for community by blacks and the relative degrees of freedom and opportunity they find in different times and places. The conjuncture of the map of African-American presence and the conventional history of the West that his story brings out compels rethinking of both African-American and western history. He makes the point, for example, that the issue of Texas independence in 1836 was not simply, as myth, even the more recent multicultural version, would have it: Anglos and Tejanos in Texas confronting a despotic government in Mexico. It was also an Anglo effort to preserve slavery.\textsuperscript{60} More broadly, the map literally reveals that African Americans in the West were overwhelmingly city and town dwellers, and it is that fact that unifies their experience.

The kind of synthetic narrative that he has constructed provides an invaluable service at a particular moment, crystallizing a generation of scholarship, making generalization possible. His work not only informs the public of the dimensions of previously unrecognized histories, it also provides a base for the next generation of scholarship. In a similar way, another recent synthesis, one that focuses on a more narrowly defined but also more developed area of scholarship, reveals the harvest of recent scholarship on work and workers. American Work: Four Centuries of Black and White Labor (1998) by Jacqueline Jones at once brings this rich scholarship to a wider audience and proffers a fresh way of framing the field.\textsuperscript{61}

If The American West, In Search of the Racial Frontier, and American Work cover very long chronological spans, books by David M. Kennedy and Fred Anderson address short periods. Their focus is also quite different, since both concentrate on political and military history. Kennedy’s Freedom from Fear: The American People in Depression and War, 1929–1945 (1999) addresses what might well be called “high politics,” while Anderson’s The Crucible of War: The Seven Years’ War and the Fate of Empire in British North America, 1754–1766 (2000) brings social history and high politics into fruitful play, finding in that interaction the terms of his central argument about the nature of power in the British Empire.

At the outset, both books locate their stories in a broad international context. Kennedy’s book begins at the close of World War I, and the first character introduced is Lance Corporal Adolf Hitler, who was in a military hospital recovering from a poison gas attack when he heard the news of Germany’s surrender. The international context thus suggested is obviously central to the half

\textsuperscript{59} He explicitly recognizes the issue of intergroup relations, but he equally explicitly indicates that such is not his aim here. See Quintard Taylor, In Search of the Racial Frontier: African Americans in the American West, 1528–1990 (New York, 1998), 18–19.

\textsuperscript{60} Taylor, In Search of the Racial Frontier, 39.

\textsuperscript{61} Jacqueline Jones, American Work: Four Centuries of Black and White Labor (New York, 1998).
of the book devoted to World War II, but it is not nearly so much developed as it might be. The geography of Washington, D.C., even that of the White House, and the biographies of three men—Franklin D. Roosevelt, Winston Churchill, and Hitler—are more important to Kennedy's story than the world beyond the borders of the United States or, for that matter, than the American people of his subtitle.

One of Kennedy's aims is evidently to urge upon Americans a greater attention to and sense of responsibility in the larger world, yet with the exception of the excellent discussion of the differing explanations of the economic crisis offered by Herbert Hoover and Roosevelt, there is surprisingly little incorporation of international elements into the dynamic of the story. For all the importance of the larger world, for Kennedy, as for many Americans, whether professional historians or not, the international is a sort of "other," something "over there," if I may reverse the title of one of Kennedy's earlier books.62

Kennedy also pays little attention to social history, not even to social histories that have sought to better explain the politics of the interwar years.63 Nor does the book address intellectual history, the history of science and technology (except briefly in connection with war production), the states, education, urban history, and much more. In fact, the book would have been more accurately described by the title of William E. Leuchtenburg's classic, F.D.R. and the New Deal, 1932–1940, which is here superseded and extended into the war years.64 So titled, adding the war to the New Deal, one could have no objection to this extraordinarily well-written, deeply researched, and compellingly argued book. But is it a history of "the American people"?

Freedom from Fear is a masterful narrative on the terms it has assumed for itself. Yet having said that, historiographical questions remain. Kennedy apparently assumes that three voices are the important ones; not many other voices are heard, even though each of a small clutch of additional figures is presented very effectively as a full human being: Lorena Hickok, Eleanor Roosevelt, Harry Hopkins, Raymond Moley, Herbert Hoover, John L. Lewis, and A. Philip Randolph, among a few others. History for Kennedy, unlike for the other authors of these syntheses, is made by select leaders, not by ordinary people. What is remarkable, therefore, is the illusion of synthesis that is achieved. The book was published in a series that promises narrative syntheses of the defining periods of American national history. Most so far published accept traditional definitions of periods, and they are framed as political history, but none is so severely restricted as this one, which won the Pulitzer Prize in part because it was recognized as a work of grand synthesis. Dramatic changes in the historiography of the American field make it seem anachronistic. Yet its success makes the point that political history in the grand

62 David M. Kennedy, Over Here: The First World War and American Society (New York, 1980). The point Kennedy makes about Americans could be turned against his own book, which assumes the same divide he finds among Americans generally. He complains in the text that Americans held tight to "the dangerous illusion that they could choose whether and when [I would add how] to participate in the world." David M. Kennedy, Freedom from Fear: The American People in Depression and War, 1929–1945 (New York, 1999), 386.

63 The only exception I spotted in the footnotes is Lizabeth Cohen, Making a New Deal: Industrial Workers in Chicago, 1919–1939 (New York, 1990).

style, focusing on a few elite figures, can still claim, at least for the general public, to be a narrative history of a people.

Fred Anderson's *Crucible of War* again engages us with the question of elites and ordinary people, and it provides a promising approach. While Kennedy seems quite confident of the importance of a few leaders, Anderson seems to be ambivalent, and that ambivalence enriches his history. Although I think the principal contribution of *Crucible of War* to our understanding of the British Empire is grounded in the social history of the political and military experience of ordinary Americans, the dramatic focus, as with Francis Parkman's great nineteenth-century narrative, is on two great leaders of the French and Indian War, the marquis de Montcalm and James Wolfe. Yet, as Alan Taylor has insightfully insisted, Anderson has rewritten the story of their confrontation in a way that diminishes these actors, especially Wolfe. To be sure, Anderson's book goes beyond Parkman in its respect for Native Americans, their agency, and their role in the empire (and the role of the empire and war for them). He also modifies Parkman on a point that is central to the book's contribution to imperial history: unlike Parkman, Anderson not only notices but makes much of the division between English colonials and English metropolitan. These differences in expectation and experience make the war in his view a "theatre of intercultural interaction."

Like Butler, Anderson seeks to diminish the role of 1776 in understanding the development of what became the United States. Historians, he argues, will better understand the creation of the United States by closely examining the Seven Years' War and, more generally, by challenging the usual tendency to "take as our point of reference the thirteen rebelling colonies, not the empire as a whole." Yet, even as he argues the importance of getting behind the Revolution of 1776 so that one can discover the eighteenth century as it was experienced, the revolution remains a touchstone for him. More than anything else, he wants the reader to recognize that the shots fired in the Seven Years' War were the ones with implications around the world. But he keeps de-historicizing his story to use it to diminish the shot of lesser implication (in his view) heard 'round the world in 1775.

When one begins the book, there is a sense of excitement. Here is a history of the United States ready to take the globe as its context. Before the narrative even begins, the reader is presented with a portfolio of maps. Only two of eight describe the British colonies; no more than four of them consider North America at all. The portfolio begins with a world map, revealing the global distribution of the battles that marked the Seven Years' War. There are also maps of the Indian subcontinent, Central Europe, and the Caribbean. The introduction promises a book that will make the world, or at least the full extent of the British Empire, its context and subject. We are told that "if viewed from Montreal or Vincennes, St. Augustine, Havana, Paris or Madrid—or, for that matter Calcutta or Berlin—the Seven Years' War was far more significant than the War of American Independence."

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68 Anderson, *Crucible of War*, xv.
69 Anderson, *Crucible of War*, xvi.
Yet once the narrative is begun, it immediately narrows. We get very little of Asia (although Manila makes a brief but important comparative appearance), the Caribbean, Africa, and continental Europe. Of course, other European powers are part of the narrative, but they only have walk-on roles. We learn little of them at home or about the ways leaders or ordinary citizens interpret events, while we are, by contrast, led through elaborate accounts of high British politics. The preface, presumably written last, sketches an extraordinary agenda for what would be a stunning book. Unfortunately, Anderson did not write the book he there described.

Still, judged in terms of what it did rather than what it proposed to do, it is an outstanding work of craft. It will no doubt be our generation’s account of the Seven Years’ War. As military history, it is superb, and it contributes importantly—but not so grandly as some of the opening rhetoric promises—to the non-controversial but still unclear issue of the causal relations that connect the Seven Years’ War to the coming of the revolution.

Anderson in fact offers a rich Anglo-centric narrative that explores and explains the different meaning of the war both as strategic event and as experience for the British of the metropole and in the colonies. It is written with verve and confidence—and a seemingly complete command of the materials, primary and secondary. One of its themes is the misperception of events by political elites; with the exception of William Pitt, surely Anderson’s hero in this story, they fail to understand the different meaning of the war and empire for ordinary soldiers and colonial subjects. He thus makes cultural issues the heart of the book. Military and political elites play a dramatic role in the narrative, but causation for Anderson—and here he points to important newer developments in military and diplomatic history—is to be found in the culture of everyday life.70 In making this point, he not only offers an important interpretation of the war (building in part on his previous book on Massachusetts soldiers), he also reveals the empire to be less solid, more a matter of continuous negotiation, than historians often consider such entities, whether empires or nations or states.71

More effectively than Anderson, Ira Berlin, referring to the earliest history of Afro-European North America, and Daniel T. Rodgers, addressing the early twentieth century, incorporate the Atlantic, or at least the North Atlantic, into their narratives of American history. Berlin and Rodgers write very different kinds of history and focus on different periods. Berlin’s is a social history, while Rodgers has written an intellectual history, or, perhaps, a history of political culture. Yet both Berlin and Rodgers recognize the complex webs that route movements—of people, of ideas, of money, of things—in the Atlantic world. The transnational terrains that Berlin and Rodgers evoke establish larger and truer frames for national histories than do notions of bounded and self-contained regions or nations.

The first section of Berlin’s Many Thousands Gone, a portrait of the Atlantic littoral, describes a world framed by cities and the sea, little divided by national

70 Anderson, Crucible of War, 453–54.
71 See Fred Anderson, A Peoples’ Army: Massachusetts Soldiers and Society in the Seven Years’ War (Chapel Hill, N.C., 1984).
boundaries, which did not yet organize any of the four Atlantic continents. Berlin's opening tableau describes the emergence of the Atlantic world as an ever-expanding historical terrain, where the African presence is pervasive on the sea and in the cities, including Lisbon, where they made up 10 percent of the population in the sixteenth century. He evokes a world defined by a network of cosmopolitan cities populated by creolized peoples. African people were not only omnipresent, they were often crucial cultural and economic brokers, helping to knit this new world together. Berlin lets go of this powerful frame and image in his later chapters, where he narrows the focus to regional difference within the bounds of British North America. Still, the book's protean beginning remains in the reader's mind, inviting others to realize its narrative logic and moral meaning.  

In Atlantic Crossings: Social Politics in a Progressive Age (1998), Daniel T. Rodgers also achieves a richer historicism by expanding the space of analysis. One small indication is in the subtitle. He refers to "social politics," not the more usual "welfare state." His approach, examining relations in space as well as over time as fields of contingency, makes the welfare state a problematic common term. When he uses the more general and more mobile term "social politics," he effectively historicizes the concept, lineage, and practice of the welfare state. The development of a social politics has other possible paths and outcomes besides evolution into the national welfare state. The national welfare state thus becomes a historically and place-specific invention rather than a universal or, worse, the teleological endpoint of American liberal narratives—an endpoint surely upended by the politics of the last quarter of the twentieth century.  

Following the pioneering work of James T. Kloppenberg, who also assumed a Euro-American context for progressivism and social democracy, Rodgers approaches this age of reform as at once a transnational and national issue. A variety of reforms—from urban planning to social insurance to regulation of capitalism—are examined as products both of general, transnational ideas and of particular, national political cultures. The complex narratives thus developed by Rodgers and Kloppenberg—ones that recognize, especially in the case of Rodgers, the historicity of the balance between national and transnational—are a major advance in the narrative synthesis of a national history.  

Both Rodgers and Kloppenberg impress on the reader that ideas could cross the Atlantic in either direction. This is salutary; American intellectual history is too often thought by Europeans and Americans as well to be either insignificant or derivative, not quite up to equal participation in an international world of ideas. This common point is handled differently in each book. While Kloppenberg notes
direct interaction, he seems more interested in demonstrating a homological relation or a kind of convergence. Rodgers, by contrast, focuses on the specific transit of ideas and emphasizes the way intellectuals and reformers on either side of the Atlantic drew selectively on these ideas, depending on personal taste and local circumstance. The result is a fundamental and valuable reorientation of the way we might understand intellectual history.

The conceptual opening they have created invites a yet more radical understanding of the territory and movement of ideas. Let me go back to the title of Rodgers's book. I think that "Atlantic Crossings" projects too narrow an understanding of the implications of the book. It emphasizes the movement of people and ideas back and forth across the Atlantic. To that extent, it recalls a much older Anglo-American historiography of "trans-Atlantic influences." Rodgers goes well beyond this historiography in showing that, in important respects, Europe was partly Americanized and the United States was partly Europeanized by the phenomena he describes. But his really important accomplishment is to get away from the "influence" model, to displace the linear A to B notion of intellectual history. But he could have gone farther yet. There is more to the circulation of ideas than this framing recognizes. It is more than an Atlantic crossing, more than a link between Western Europe and the United States. The whole Atlantic, South Atlantic as well as North Atlantic, and, indeed, increasingly, parts of the Pacific world better describe the extent of the intellectual network his book evokes.

In regard to urban development and reform, an important theme in Rodgers's book, it is clear that there is a global conversation at work. Rather than the linearity of steamship crossings (the dustjacket illustration) between the port cities of Western Europe and New York, I imagine a Great Bazaar of urban ideas, technology, and aesthetics hovering over the Atlantic, with many traders and buyers. This exchange is not, of course, symmetrical, and that itself is an issue, but participation was nearly global in 1900. Progressive ideas, especially those dealing with urban reform and technologies, traveled through many circuits and with different voltage, but nearly the whole world was connected, not only Western Europe and the United States. Simply look at the cities of Eastern Europe, Latin America, parts of Africa, Central and East Asia. Surely, they were part of an international conception of urbanism—and of urban commercial culture. The remnants of the era make it clear that New York and Chicago, no less than Lyons, Cairo, Buenos Aires, or Shanghai, were local instances of a global process of city-making.

These last comments suggest what I take to be the next challenge of narrative synthesis. But before I conclude, let me briefly review what has been accomplished by the cohort of synthetic histories considered here. These books reveal, even verify, the capacity of narrative synthesis to achieve inclusion and to respect issues of identity. Moreover, it seems possible in synthetic narratives to combine structure

and agency and to consider causal explanation without sacrificing the explication of subjective meaning—and vice versa. The volumes here examined reveal many narrative strategies and quite different relations to a wider reading public. There is no single model, and no one volume (yet) does all the things we might fairly expect in a realized synthesis.

In addition, these books, both in what they do and do not do, suggest to me the value of embracing a narrative core that is a more explicit and deeper exploration of democracy and difference, freedom and empowerment, contest and justice. Such a focus promises a sharper analytical history, one more historical and less susceptible to teleology, whether of modernity or anything else.

It seems plausible to propose that a wider canvas, a supranational context, may in fact enhance the examination of these issues. The work of Hine and Faragher, Berlin, Gordon, and Rodgers in particular enables one to imagine an even more radical synthesis of national history, one that operates on multiple geographical scales, from narratives smaller than the nation to supra-national ones—thus identifying the nation as a product of history as well as an object of historical inquiry. Such a framing of national history will increase awareness of the complexity of the multiple axes of historical interaction, causation, and identity formation.

While I mean these concluding comments to suggest an ambitious new agenda for the discipline, we must not overlook an already existing and compelling example. Decades ago, David Brion Davis embarked on a multivolume history that considered all these issues. He brought them together in his majestic synthesis that explores slavery and freedom in the Atlantic world, a history of nearly global reach that is also—and I emphasize this fact—a history of the United States. My point, then, is that such histories can be written, have been written, and I trust that more will yet be written.

The present moment seems especially propitious for such histories. The relation of the nation to both subnational and transnational solidarities is very much in question. It is a public concern as well as an object of interdisciplinary scholarly inquiry. Historians surely have an open invitation to rethink the boundaries of national histories. Colonial historians have been moving in this direction for some time, redefining their field as the Atlantic world long before the globalization talk. Likewise, Rodgers and Ian Tyrrell, both of whom work on the modern period, moved in this direction fairly early and for a different reason: their concern about the claims of American exceptionalism.

With these various concerns at work, we may fairly expect a movement of American historians and other historians as well toward a wider sense of their fields. National histories will not be so firmly bounded, and the assumption of their national autarky will be softened by the recognition that national histories are

76 David Brion Davis, The Problem of Slavery in Western Culture (Ithaca, N.Y., 1966); The Problem of Slavery in the Age of Revolution, 1770–1823 (Ithaca, 1975), with the final installment yet to come.


embedded in yet larger histories. And all of this will demand yet more ambitious strategies of narrative synthesis.

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