How War Began. By Keith Otterbein (College Station, Texas A&M University Press, 2004) 310 pp. $60.00 cloth $25.00 paper

For more than forty years, Otterbein’s published work has been seminal for students of warfare. This book represents his latest survey of, and conclusions about, this dismal but, alas, still important topic.

The information and analyses mustered in this work exemplify the broad database—the general scholarship and “controlled comparison” approach that characterize anthropology, compared to the narrower, more ethnocentrically oriented studies of the self-proclaimed “human sciences.” Anthropologists have long argued that generalizations about humans are impossible without a realistic and logical consideration of humankind in all times, all places, and all “cultures.” In this work, the author carefully marshals anthropological data on deadly violence between hominid social groups, including that documented by primatologists of human’s closest living relatives (chimpanzees), by archaeologists of hominid prehistory, by ethnographers and historians of human ethnology, by historians of literate societies, and data via his special forte, cross-cultural statistical comparisons of modern humans.

In summary, How War Began argues that hominid warfare had two different origins and thus produced two different military organizations, greatly separated in time, basic economy, and sociopolitical context. It argues that warfare first began well over 1 million years ago as a consequence of hominid hunting of big game in small bands and tribes. As big game became rare, and humans began to rely more on small game and plants for food as “settled gatherers,” warfare reached a peak of intensity. Intersocial violence gradually waned, becoming rarer and less important as these sedentary gatherers became village agriculturalists. Several thousand years later, as agricultural chiefdoms and states began to consolidate (often by violent force), professional military organizations (armies) began warfare anew. Otterbein’s graph (12, Fig. 1.2) of his theses (note the plural) indicates a universal peace among “agricultural villagers” beginning just after 10000 B.C. with the “extinction of large animals,” lasting until after 3000 B.C. and the first appearance of agricultural “maximal chiefdom/inchoate early state.” He also argues that after this second advent of warfare, “despotic” states have been most responsible for fostering warfare.

This book’s dual theses violate Occam’s Razor. More importantly, the key data for them is almost entirely prehistoric; that is, it occurs before writing and written records first appeared, c. 3300 B.C. in southwest Asia/Egypt, 2800 B.C. in China, and 300 B.C. in Mesoamerica. It is knowable only via archaeology. As an archaeologist, I will note only that Otterbein’s dual-origin hypothesis is contrary to the known facts of world prehistory. To give just one example, fortifications, war weapons, depictions of battles, weapons traumas on human skeletons, and even horrific mass graves of war victims have been commonly documented
among early prehistoric agricultural villagers worldwide. Also, hominids were hunters of large game hundreds of thousands, possibly 2 million, years before any unambiguous evidence of homicide, let alone group homicides or warfare. However, this disagreement regarding details between anthropologists should not, and cannot, eclipse this work’s virtues and usefulness to social scientists.

This well-written and clear-minded book considers the most relevant data about warfare among ancient and modern humanity. As was said long ago, “Truth emerges more from error than confusion.” This book is definitely not confused. As a well-informed and plausible view of one of humankind’s oldest predicaments, it deserves a respectful reading by all scholars concerned with the roots and nature of war.

Lawrence H. Keeley
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Hearing History: A Reader. Edited by Mark M. Smith (Athens, University of Georgia Press, 2004) 413 pp. $59.95 cloth $29.95 paper

“In these books, is wonderfully preserved, the spirit of each warrior.” So goes satirist Jonathan Swift’s mock-epic engagement between the ancients and the moderns in The Battle of the Books. Like Swift’s combatants, the contributors to Smith’s engaging, provocative, and almost convincing didactic collection contest for the primacy of the ear in sensory history. Smith’s introduction to this collection sounds the call to arms for an aural history: “Quite clearly, . . . historians are listening to the past with an intensity, frequency, keenness, and acuity unprecedented in scope and magnitude” (ix). The battle is not yet won, Smith admits, particularly against the ocular foe (the “visually oriented discipline of history” still babbles on about “perspective” and “focus”), but the forces of the ear are gathering strength. Smith’s collection is not only a tour of the scenes of great aural victories (oops—more oculism), but promises “to identify future topics for research” and “help readers hear the beginnings of a dialogue among scholars of historical aurality (ix, xx).”

Doing aural history is not easy; sounds are like writing on water. They cannot be duplicated in their original context. As Douglas Kahn wisely cautions in his essay, “as a historical object, sound cannot furnish a good story or consistent cast of characters” (37), to which Bruce R. Smith, at the end of the book (a “coda”) adds, the history of sound must be “a triumph of mind over matter” (390). Smith (whose shadow hovers over the entire book and whose voice is heard at its beginning and its end) has the last word, however: “If hearing were demonstrably impor-

tant to people in the past, and if they left evidence to that effect, then the topic demands and deserves our attention” (402). But the problem of finding evidence of past sounds, as opposed to their importance at the time, is not so easily elided.

The first set of essays (Part I) deals with the theoretical possibilities and (by implication) the theoretical obstacles to aural history. R. Murray Schafer argues that “the general acoustic environment of a society can be read as an indicator of social conditions which produce it” (6). Jacques Attali warns that “our sight has dimmed; it no longer sees our future . . . Now we must learn to judge a society more by its sounds. . . .

By listening to noise, we can better understand where the folly of men and their calculations is leading us” (10). Peter Bailey reminds that in the metaphors of our literature as in life itself, “noise is a common compound signifier” and “the changing function of hearing and sound within the combinations and hierarchies that pattern sensory perception as a whole” define identity as well as express it (27). Although some sounds have not had “a good press,” historians must pay more than “lip service” to the role of sound in history (33, 34). Steven Connor has done just that—connecting aurality and modernity, “the self and the ear” (55). Hillel Schwartz agrees: “Historical research” can reveal past “soundscapes” (52), in particular how and why they changed over time. In effect, aural history can be highly and effectively depictive, something that historical novelists (a breed of aural historian not represented in the collection) have long assumed.

Part II reprints some of the classics of European sensory history, starting with Charles Burnett’s piece on sound, noise, silence, and other epistemologies of medieval thought: a selection from Bruce R. Smith’s delightful and incisive re-rendering of the sounds of rural and urban sound in early modern England (though the selection hardly does the book justice); D. R. Woolf’s essay on the Elizabethan valuation of “eye and ear” (115); and James H. Johnson and Alain Corbin’s comments on music and bells, among other contributions.

Part III, “Sounds American,” offers more recent pieces, most excerpts from book chapters, including Richard Cullen Rath’s musings on the relationship between styles of worship and church acoustics; Leigh Eric Schmidt’s attempt to link the “calling” of itinerant preachers to its primary auditory expression and the “noises of revival” (235); and Shane White and Graham White’s exploration of the sounds of slavery, among other excellent excerpts. The closing section is a dialogue between the editor and two of his friendly critics, a colloquy that does not match the quality of the selections and could have been omitted.

The sum of the parts is worth more than the whole. The selections are so valuable in themselves that one need not join the editor in combat over the relative merits of hearing over seeing.

Peter Charles Hoffer
University of Georgia
Behind an unrevealing name, the population register is an original data-collection system that differs considerably from the mainstays of countries such as the United States—the separately administered censuses and vital-registration systems. Its principle is the constant updating of an initial or periodic enumeration by adding new births and in-migrants and removing deaths and out-migrants. Population registers make it possible to keep count of the resident population at all times, thus opening up possibilities of complex analyses. They have existed in a number of European countries (in Scandinavia, the Netherlands, and Belgium among others) and in Japan and parts of China at various times since the eighteenth or nineteenth centuries. Since their use was meant to serve local administrative or religious purposes, their preservation has been spotty, and their analysis by historical demographers has so far received less attention than the English or French projects based on parish records or the American studies of the census. The present volume constitutes a triumphant notification that this situation is changing. The wealth of individual data opened up to longitudinal analysis on a comparative basis constitutes an extraordinary opportunity to provide a second breath to the field of historical demography.

The collaborators have each adopted comparable methodologies with data on communities from rural Sweden, Belgium, Italy, China, and Japan, over various lengths of time (from a low of 12 years in one Chinese area to a high of 146 years in a Japanese one), for a total of almost 1.5 million person-years of observation (100,000 life histories in 100 rural communities, to quote the blurb on the cover). The database is undergoing further development. The information bears on individuals and their positions in households over time, and can be used to test hypotheses at the individual level from a longitudinal perspective. The focus is not on levels of mortality and their change, but on patterns of response to crises.

The underlying assumption is that the effect of economic stress, indexed through changes in food prices, can be documented by rises in mortality, thus providing a measure of the standard of living. Higher mortality provides an indication that particular persons are losing out in the “competition for resources.” The comparative mortality response in relation to age, sex, and relationship with other members of a household thus suggests differences in the social organization of the various communities in the study. Although the extent to which this set of assumptions tends to “explain” changes in mortality risks by access to food rather than by more complex epidemiological factors (for which no data are available) is open to question, nevertheless, it provides a powerful theoretical underpinning to the examination of “life under pressure.”

An intellectual tradition that goes back at least to Richard...
Cantillon, Adam Smith, and Thomas Malthus has placed the Chinese and the English, or Western European, demographic systems in opposition. Even though the classical economists had limited knowledge of Asia, the Malthusian perspective continues to fascinate historians of Chinese demography. Several chapters of the book start with page-length quotations from Malthus’ *Essay on the Principle of Population* (London, 1798). Analysis of the data bank confirms sharp contrasts in domestic organization: Household formation in the East does not depend on marriage, and households are more complex. However, demographic behavior does not confirm Malthus’ intuition that the individualistic West is more vulnerable to mortality crises than the East because it lacks welfare systems and family solidarity.

The volume contains valuable descriptions of the interactions between society and mortality in the various sites of the project. The chapters that examine various types of relationships comparatively and test hypotheses concerning them in the literature (gender differences, mortality in infancy, childhood, and old age) constitute major contributions, despite the negative finding that few generalizations can be extended to all the communities, or even to the East or the West. Does the presence of grandparents protect children? Are married women less vulnerable than single ones? What are the risks to mothers of small or large families or of young girls with older sisters or brothers? Most chapters end with a disappointing but realistic warning about the diversity of human experience, the dependence of demographic patterns on local features of the economy or social organization—and, of course, on the need for yet more data.

The Eurasian Project promises to be an epoch-making new turn in historiography and demographic methodology. Another volume on reproduction is announced, and more are planned on nuptiality and migration. The project will provide much new material on the behavior of individuals and communities. Some of it is bound to be challenged, since the communities in the studies may be neither typical nor representative. But the work will constitute an indispensable reference for years to come, and its influence should extend to the study of contemporary demographies.

Etienne van de Walle
University of Pennsylvania

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*The Laws of the Roman People: Public Law in the Expansion and Decline of the Roman Republic.* By Callie Williamson (Ann Arbor, University of Michigan Press, 2005) 506 pp. $75.00

In this extraordinary book, Williamson takes on a daunting and demanding subject—the character and consequences of Roman expansion in Italy over a period of 300 years, the incorporation of Italic peoples
into the Roman system, and the resultant tensions and pressures that culminated in the fall of the Republic. No brief review can begin to do justice to the richness and complexity of this work. Topics tumble upon one another impressively but, at times, confusingly. The treatment is complex and convoluted, often turning back upon itself, falling into repetition, and occasionally reversing its own conclusions. Logic and consistency are not its strongest points. But the ambitious investigation merits praise. A principal theme knits together its disparate discussions—the engagement of Romans from all social classes, ethnic backgrounds, and political associations in the process of promulgating legislation. That process could promote consensus, balance, and concord. But it could also provide a forum for dissent, conflict, and fragmentation.

Williamson’s choice of topic is inspired. The generation of laws by the Roman people constitutes a persistent and repeated (though not steady) feature of Republican history. Williamson’s thorough collection of the laws, gathered in an invaluable series of tables and organized by subject, period, sponsors, issues, and institutions (among other things), demonstrates the remarkable variety and compelling significance of legislative activity over three centuries. Williamson does not dwell on minutiae but endeavors to draw larger conclusions about Roman politics and society in the building, and then dissolution, of consensus among the leadership and between the elite and the populace. One of this study’s refreshing features is that it concentrates more on what held the Republic together than on what tore it asunder.

The book is replete with acute observations. For instance, Williamson shows that the introduction of the secret ballot did not represent demagogic activity on behalf of the populace but a means whereby the elite protected its own ascendancy. She rightly lifts key legislation of the late Republic (for example, in 67, 63, 59, and 58 B.C.E.) from the realm of mere politics to the broader issues of leadership, public welfare, and the assimilation of wider elements of society into the governing process. She pointedly suggests that the dramatic increase in legislative activity during the last generation of the Republic indicates that the process and the system had gone awry.

Yet the elaborate and imposing structure of this work does not quite hold together. Long discussions of Rome’s efforts to integrate Italians into its institutional system, of the mobility of the Italians and the fluidity of their means of livelihood, of the effects of military service, of the variety of Roman voting assemblies, and of shifting patterns of leadership, all have only a loose connection with law-making activity. Williamson’s fertile intellect is everywhere present, but her writing could use more discipline. Her admirable efforts to present the contexts of legislation tend to engulf the legislation itself.

Williamson deserves credit for placing public law onto center stage. Her instinct that it bears an important relationship to a host of key developments in the history of the Republic (especially the absorption of It-
aly, the extension of citizenship, and the interplay between Roman leadership and the plebs) is surely right. But the book more frequently asserts that relationship than explores or establishes it.

The work does not qualify as interdisciplinary in the usual sense. Political and institutional history stands at its core. But its new approach (via scrutiny of legislative process and products) to the old questions of what integrated and what undermined the Roman Republic, even if it does not successfully answer those questions, should stimulate fresh endeavors—a salutary and welcome result.

Erich S. Gruen
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Envoys and Political Communication in the Late Antique West, 411–533. By Andrew Gillett (New York, Cambridge University Press, 2003) 335 pp. $75.00

Scholars have long recognized the symbiotic relationship between diplomacy and warfare. In this thought-provoking book, Gillett uses this link as an entry point into the profound political and social changes of the period between 411 and 533 C.E. This period encompasses the establishment and consolidation of barbarian kingdoms in regions that had formerly been provinces of the Roman Empire, ending with Justinian’s wars of reconquest in North Africa, Italy, and Spain (6). Gillett’s basic argument is twofold. First, with the emergence of new rulers and kingdoms in the post-Roman world, diplomatic relations became more complex and multilateral than they had been under the Empire (3, 7, 54, 64). Indeed, the surviving evidence vastly underrepresents the scale and regularity of embassies in the period, with the result that scholars have tended to place undue emphasis upon specific accounts of diplomatic missions, and ignored the context within which these individual events took place (1–2, 34, 74, 204, 275). Second, as a consequence of the expanded significance of embassies, the role of envoy became a more important marker of status and prestige within local communities, and individuals were more likely to acknowledge or stress this aspect of a public persona than previously (4, 16–17, 3, 276).

Gillett illustrates these propositions in four chapters that provide case studies of a series of fifth- and sixth-century texts. Those texts celebrate or employ the figure of the envoy as a motif, and attach it to broader claims about the individual’s position in the transforming social and political milieu of the late antique West. Preceding these studies is a general introductory chapter outlining the historical development of embassies and envoys in the Graeco-Roman world. A final chapter details the set of shared ritual, ceremonial, and administrative practices that
As consciously interdisciplinary, Gillett’s project raises certain methodological questions. His stated aim is to focus upon “the interaction of sources, their genre, and their historical setting” (ix). Such an approach treads the line between the literary scholar’s project of textual criticism on the one hand, and the historian’s emphasis upon the text’s context and the information that it provides on the other (2). The two projects need not be mutually exclusive, but the risk of attempting to do both simultaneously is that neither is done effectively. True, the distinction between literary topos and historical reportage is difficult to distinguish in the best of times; they are subtly interwoven. But, in interpreting a particular presentation of an envoy or embassy as both, Gillett creates difficulties for both his exposition of the literary aspects of the work and the historical conclusions that he draws from it. Throughout the book, there is tension between two slightly different interpretive stances. On the one hand, Gillett argues that these texts are unique literary creations, thematically different from other examples of their genre. On the other, he assumes that they can be interpreted as representative of a general ideology, which emphasized the importance of the envoy in the period. The argument appears to be circular: Specific cases are interpreted as reflecting a general context, which is used to explain the uniqueness of these cases (for example, 84–86, concerning Sidonius Apollinaris’ portrait of the emperor Avitus as an envoy).

This criticism is not to deny the value of Gillett’s work. Scholars of the post-Roman world will find much to ponder. Gillett offers a challenging insight into an exciting subject and asks important questions of interesting texts. He offers specialists a series of propositions with much broader implications for the post-Roman world, and provides a general audience with much methodological food for thought.

Cam Grey
University of Pennsylvania


Using an interdisciplinary approach, Hartman answers one of the crucial questions that economic historians ask, Why did Western Europe singularly usher in the modern world? Previous historians have offered two theories predicated upon the idea that Western Europe was a unique and dynamic center of political and economic change. One theory holds that Europeans were more inventive due to their new system of national states; the other places the impetus upon Europe’s early capitalist structures. Building on Hajnal’s theory, Hartman argues that “a prior and dis-
tinctive development” occurred before these two “interdependent master-processes” (3). What laid the foundation for major economic and political changes of the emerging modern world, according to Hartman, was the distinctive European family and household system, “whose most crucial feature, late marriage for women, appeared in the manorial regions of northwestern Europe at the end of the Roman Empire” (243). A reliance on female labor and late marriage for women “generated a set of shared experiences, attitudes, and values that profoundly affected how people in the region thought and behaved” (243). Hartman persuasively argues that this “crucial feature” deserves to be placed at the center of all of Western history for a proper understanding of the past. Indeed, she suggests that sixteenth-century religious upheaval, the scientific revolution, the growth of nation-states, the Enlightenment, witch hunting, homophobia, and the emergence of capitalism and democracy can be fully understood when this distinctive Western phenomenon is considered.

This unique early Western European pattern—first instituted to serve the landholding needs of patriarchs—unintentionally spurred several developments that contributed to the emergence of a modern mentalité. The ordinary peasant family’s decision to keep females working at home and postponing marriage until their mid-twenties, provided an environment in which men and women had more choice in spouse selection. It also increased female survival rates and encouraged “life-cycle service”—a strategy devised by families when the need for labor lessened. Young female domestic servants moved frequently from place to place, often under yearly contracts, which offered women “greater personal responsibility for their fates,” as well as many opportunities to meet potential mates—outside of parental control (57). “Life-cycle service” led to an early reliance upon the nuclear family as the chief residential form because extended wage-earning employment for both sexes promoted pooling of resources and a desire to establish new and independent households. Hartman suggests that for most “ordinary young persons, their release from patriarchal networks that continued to hold them fast in so many other [early marriage] societies can be recognized to have come not from the outside but from their own families’ collective deliberation and action” (100).

This “collective deliberation and action,” and the unstable nuclear families that it produced, ultimately led to a pervasive and persistent Western belief in the importance of gender difference, a sexual hierarchy favoring men, and women’s resistance to male dominance. The peculiar Western European marriages that developed because women married later, amplified women’s agency—in both the home and beyond—as it

served to undermine the authority of men as strict patriarchs. “Since new brides and grooms were typically the only resident adults in brand new households, shared decision making in running those households was more likely to occur from the outset, especially as women often brought resources they themselves had earned to the creation of those households” (32). Unlike the early-marriage system’s multi-family households in the rest of the world, these Western European nuclear families fostered innovative decision making—in both long- and short-term planning for the increasingly fragile family’s existence—that would be fraught with friction and conflict. These unstable gender relationships and weakened kinship connections in Western Europe greatly influenced institutions and power relationships outside the home and provided the foundation for the vast political and economic changes of the modern era.

Hartman’s well-written synthesis of existing research in economic, social, and cultural history, women’s studies, sociology, anthropology, ethnography, psychology, and demography provides an overarching theory that will encourage further research. Such a tremendous undertaking also has its pitfalls. Making sense of nearly a millennium of history utilizing research from every available field necessarily leads to overlooking important works, such as those by Reis and Kamensky. That being said, Hartman should be applauded for undertaking such a huge task and producing this thought-provoking “subversive” view of our Western past.

Debra Meyers
Northern Kentucky University

Strange Histories: The Trial of the Pig, the Walking Dead, and Other Matters of Fact from the Medieval and Renaissance Worlds. By Darren Oldridge (New York, Routledge, 2005) 198 pp. $33.95

Oldridge had the genial idea that with a series of studies of events or patterns from late medieval or early modern European history, he might persuade the “general reader” that strangeness is in the eye of the beholder. He argues that our sense that people in earlier times believed the weirdest things, behaved cruelly, and could not have explained themselves if they had tried is based on our self-congratulatory ignorance. In Oldridge’s view, witch hunting, voluntary suffering (by saints or by ordinary people), and the judicial trial of animals—to take three prominent examples—all made good sense in their time, even if they no longer do.

To argue this case, he presents ten chapters that highlight the bizarre, the alien, and the unconventional.

His general point is well taken and well worth explaining to general readers. Most historians would agree that our own culture and that of others, in other places and in other times, do (and did) have the power to make many things seem normal and rational that appear indefensible if measured by other standards. Thus, he describes the physical reality of demons and angels; souls that walk the earth after separation from the body; the reasons why animals might be held responsible for crimes; the evil magic and supernatural deeds of supposed witches; the belief in werewolves; the belief in raptures, inspiration, and demonic possession; the notion that suffering might be beneficial; and the reasons for killing heretics. In many of these cases, the strangeness that Oldridge describes derives from biblical literalism, a stance that he assumes (wrongly) cannot be found in the world of today. But in this form, Oldridge has constructed an argument that general readers may find attractive and understandable.

Unfortunately, for the scholarly reader, Oldridge is often too vague, too general, or too self-contradictory to be fully convincing. This problem derives in part from treating the period from 1100 to 1700 as an undifferentiated unit, but also from a bland assumption that “most Christians” accepted what a few dogmatic theologians may have asserted. As Oldridge probably knows, it is hard to know what “most Christians” thought. Another difficulty stems from Oldridge’s desire to describe all of premodern Europe. Virtually all of his sources, however, are English translations, and the bulk of them derive from England. Even with his English sources, moreover, he sometimes seems to think that Puritan theologians spoke for everyone. Even if the redoubtable William Perkins (1558–1602) had a broad influence, he was surely not a generic or representative Christian.

Another problem depends upon the author’s frequent failure to recognize that many supernatural topics provoked debate in early modern times and were not simply sources of uniform conviction. In fact, there were few topics on which everyone in early modern Europe agreed. For example, not everyone in the sixteenth century accepted the flight of witches. Indeed, the tradition of the venerable tenth-century “Canon episcopi” was strong enough to provoke vigorous debate on just this point. Nor did everyone accept the efficacy of maleficium (harmful magic). Certainly jurists did not all agree on the punishment of felonious animals. Even when he treats well-known skeptics, Oldridge oversimplifies. Johann Weyer did not really leave “the foundations of witchcraft intact” (93), unless he merely means Weyer’s belief in the devil. Similarly, when dealing with the early-modern Christian belief that suffering might be good for the soul (Chapter 8), Oldridge mixes evidence from Catholic practices of self-mortification (for example, extreme fasts and self-flagellation) with Protestant beliefs that if God caused difficulties, Christians should accept them as a needed chastening. To regard
these notions as belonging together is a mistake; they depend upon different ideas of how to please God.

It does not help that the book is marred with typos, misspellings, and mistaken biblical references. A useful idea for a book of general interest has been spoiled through carelessness.

H. C. Erik Midelfort
University of Virginia


This book is remarkably ambitious and erudite, even daunting. It has a time span from c. 1830 to the present and features eight categories of causation, and some of them are as mysteriously broad as “Mind” and “Ideas.” The concept of causation is itself a contested one. How could any mortal historian get this much material under his control and make it accessible?

The answer is that Kern has a single and simple thesis that he uses to organize every chapter—“a specificity–uncertainty dialectic” to describe the change from the Victorian to the modern. In the chapter on sexuality, he writes, “The increasing specificity and complexity, as well as probability and uncertainty of sexual desire applies especially to knowledge of homosexuality” (187). Or, as knowledge becomes more specific and complex, awareness of its lack increases. This conclusion is repeated for every category studied. Kern is aware that his argument is “as simple as a cliché,” but finds its elaboration “endlessly surprising” (26). Readers will inevitably differ in detail about what they do or do not find surprising, but the repetition of the general thesis for every chapter makes for monotonous predictability, an ironic result for a book that emphasizes the theme of uncertainty.

Kern devotes much attention to quantum theory, yet concedes that it “plays only an infrequent and marginal role in historical and literary matters” (365). In spite of his concession, the drift of his book has the effect of blurring the difference between Werner Heisenberg’s technical uncertainty principle in physics and a loosely used metaphor. Similarly, Kern acknowledges that explanation, at least in part, can include purposes and reasons, but he emphasizes the role of biochemical entities in neuroscience as “the most basic causal explanation” (15). Nevertheless, he is aware of the distortion involved in his using parts of novels as if they were “criminological or psychiatric case histories” (16). He is good at anticipating objections.

Kern’s idea for this book began with his interest in the literary shift from fictional realism to fictional modernism in James Joyce, Marcel Proust, and Virginia Woolf, suggesting “a cultural pivot for a history of
causality” (2), because these authors diminished the role of plot and character. The notion of the pivot raises the question of whether Kern’s approach is another version of the Whig interpretation of history, ruled implicitly by an idea of progress. Again, Kern is himself aware of it as a temptation and defines his thesis as a “gradational change,” rather than a turning point, though he tends to undercut it when he asserts that a fictional character’s murderous act in an André Gide novel is “an event of enormous cultural historical significance” (4).

Kern recognizes that the modernists wrote as if they represented a progressive change, an idea that works in science and medicine, but not in art. The literary modernists had successors, for example, who hoped to reinvigorate plot and character, as they did in the English novel. Kern barely mentions American philosophers of uncertainty, such as the pragmatists William James and John Dewey, whereas he bestows much attention on the contemporary Continental proponents of “the linguistic turn.” The drift of Kern’s book shows that the idea of progress is alive and well, and living in Paris under the name of postmodernism.

Cushing Strout
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Dictatorship in History and Theory: Bonapartism, Caesarism, and Totalitarianism. Edited by Peter Baehr and Melvin Richter (New York, Cambridge University Press, 2004) 308 pp. $65.00 cloth $22.99 paper

Bringing together historians and political theorists, Dictatorship in Theory and History traces the evolution of the terms “Bonapartism,” “Caesarism,” “dictatorship,” and “totalitarianism” in the nineteenth and twentieth centuries. The emphasis throughout is on fundamental disagreements about the meanings of these often politically salient but always highly contested terms—including whether the two Napoleons represent a uniquely French, a distinctly modern, and a specifically military phenomenon, whether they constituted the end or the fulfillment of the Revolution, what kinds of domination was involved in their rule, and even whether they created similar or different types of regimes. At the same time, the terms are held together by a curious and troubling combination of authoritarian and popular politics and the power that Napoleon and Caesar themselves hold over our imaginations.

Dictatorship coheres remarkably well for an edited volume precisely because it is about a plurality of meanings. The chapters progress from Napoleon I’s coup d’état to the reactions of German conservatives; to Alexis de Tocqueville and Karl Marx; to Napoleon III’s surprising contributions to local government and democratic citizenship; to Max Weber, Antonio Gramsci, and Carl Schmitt; to parallels with Charles de Gaulle; and finally to the gradual decline of the terms “Bonapartism”
and “Caesarism” by way of Arendt’s theory of totalitarianism.¹ In a puzzling violation of an otherwise chronological sequence, the volume ends with two excellent chapters that link shifts in the meaning of dictatorship in Rome to changing uses of Caesarism in later European history, topics that might have better served as a bridge between the historical Caesar and Napoleon I. The discussion of eighteenth- and nineteenth-century British fears of Caesarism in the second of these chapters also raises questions about its relationship to a fear of oriental despotism that merit further exploration, as well as underscoring the book’s overwhelming emphasis on French and German concerns. The upshot is that although much remains to be said on the topic, this book has much to say.

Aside from addressing specific topics of interest to historians, Dictatorship succeeds in addressing the concerns of political theory. The very slipperiness of the terms in question seems to inspire re-evaluations of classic theorists, forcing reconsideration of such issues as the status of “The Eighteenth Brumaire” in Marx’s writings, the political origins of Weber’s “charisma,” and Gramsci’s relationship to Marx.² Simultaneously, the intimate relationship between dictatorship and constitutionalism, particularly of dictatorship in defense of constitutionalism, is a recurring theme clearly worth examining in greater depth.

Although more about political theory than political science, this book is relevant for students of comparative democratization as well. The long “First Wave” of nineteenth-century democratization encompasses a period in which most European countries fell uncomfortably between traditional systems of rule and modern representative democracy. Nineteenth-century France offers particularly difficult problems of regime classification, as indicated by the great difficulty that many classic studies of democratization have in accounting for its uneven journey to the Fifth Republic.³ The essays in this volume serve to highlight some of the conceptual issues at stake and, especially in the discussion of Napoleon III’s contributions to French democracy, suggest an incrementalist solution to the problem of nineteenth-century democratic development.

Philip J. Howe
Adrian College


This splendid sequel to the author’s earlier studies of Cheshire is solidly based on an impressive variety and volume of sources—family and estate records, tax returns, wills, inventories, and genealogies, many of them tabulated and printed in the text. What emerges is a detailed history of individuals and families over three centuries and a useful account of how the local economy functioned, including authoritative descriptions of sailclothmaking, linen manufacture, and salt production. A chapter on Quaker families confirms the value of their kinship links and personal integrity for business.

Two main arguments are advanced in the book. First, in the dairy economy of the Northwest between 1500 and 1750 most farmers were customary or leasehold tenants who held their land at fixed rents during an inflationary period and thereby built up equity at the expense of the Crown, the Church, and some landed families. Since they practiced partible inheritance, their children acquired capital to start the small local businesses that were the bedrock of the Industrial Revolution. Second, this diffusion of capital created an independent, regional business community, often Non-Conformist in religion and linked to the Atlantic economy. Its “business culture” (entrepreneurial would have been a better term) was able to resist the rentier and professional “gentry culture” that dominated the South. The Industrial Revolution was driven by innovating and risk-taking businessmen, not by technology.

This book exemplifies the strengths and weaknesses of local history. It mines rich sources, raises important questions, and advances a plausible case. But there is a yawning gap between the particular and the general. National trends are discussed in a simplistic terminology with too many important issues, such as banking and credit, omitted. The crucial question of the extent to which entry fines compensated landowners for fixed rents is also left in limbo. The arguments are not systematically developed or weighed against alternatives; they are presented as assertions without an adequate demonstration of their mechanism or relative importance.

Richard Grassby
Funkstown, Maryland


The occult sciences enjoyed a kind of renaissance in late Elizabethan England as print and translation made ancient, medieval, and earlier Re-
naissance texts available to would-be English adepts. Despite advances in astronomy, physics, chemistry, and anatomy, the occult sciences of astrology, alchemy, and magic continued to attract practitioners at the end of the seventeenth century. Witness Isaac Newton’s alchemical experiments. Baconian empiricism may ultimately have triumphed, but in the last decades of the sixteenth century, it was not at all evident that the future did not belong to those following in the footsteps of Dr. John Dee, mathematician, astrologer, alchemist, cartographer, and magus.

Dee’s most famous, or infamous, successor was doubtless Simon Forman, a generation younger than Dee, a largely self-taught adept who, during years spent in bouts of manual labor alternating with school teaching (and a little over a year spent at Oxford), managed to create a growing reputation as a practitioner of astrological physic. By the 1590s, his astrological casebooks show that he was seeing more than 1,000 patients a year, distilling his own medicines, and manufacturing sigils and amulets that permitted possessors to access astral powers. Since he was living in London and later that decade in Lambeth, his activities became known to the College of Physicians, who interviewed him, found him totally deficient in knowledge about medicine and astrology (as Kassell argues, the College had no objection to astrology per se), and had him imprisoned on several occasions for practicing medicine without a license. There is no evidence that Forman was killing larger numbers of patients than the licensed physicians, but success in the practice of medicine was never the issue. Forman eventually moved to Lambeth, beyond the College’s jurisdiction, and ignored the College’s continual summonses. When he died in 1611, his papers passed to his friend Richard Napier, a fellow astrological physician, and, on Napier’s death in the 1630s, to Elias Ashmole.

Given that there are modern studies of Forman, which Kassell duly acknowledges (her footnotes testify to the fact that she has apparently read everything of relevance, concerning both Forman and the occult sciences), Kassell asserts at the outset that she has not written another biography but rather has attempted to recover “his voice for the histories of medicine and the occult sciences” (3). This proved a formidable task, both because Forman wrote so much (a listing of his extant manuscripts runs to eight pages), and because he was anything but a systematic thinker. Forman borrowed and bought everything that he could concerning astronomy, astrology, and physic, alchemy and the distillation of drugs, the manufacture of the philosophers’ stones, geomancy, and “all depe and subtille artes” (228). Although he believed astronomy to be the queen of the sciences, and astrology the operative means to determine God’s will and intention in disease and healing, he never developed a theoretical or unified understanding of the relationship between the various strands of occult knowledge that he purveyed. He was concerned only with practice. He cast horoscopes as a prognostic tool, prescribed drugs and alchemical distillations based on Paracelsian practices, summoned spirits (never successfully) and employed astral magic. Because he
rarely noted outcomes, he could only point to his successful practice and growing wealth as evidence of his efficacy.

Kassell’s study, if not easily digested, is a deeply learned work. She embeds Forman’s commentaries in the Renaissance world of the occult sciences, noting the sources that he used, ranging from the cabala to the apocryphal “Life of Adam and Eve.” How a busy medical practice allowed time for Forman to copy and comment on so many texts can only be explained by a voracious appetite for practical occult knowledge. Kassell does not attempt to rationalize such a melange, but she does explain what contemporaries understood by the various occult sciences and practices and what Forman hoped to learn from his prodigious reading and writing.

Paul S. Seaver
Stanford University


De Krey long ago established himself as a leading authority on late seventeenth-century London with a pioneering book on the City’s Whig and Tory parties during the reigns of William III and Queen Anne. He has now written a synoptic account of religion and politics in the Stuart capital from the eve of the Restoration through the judicial campaign that smashed the first Whigs in the early 1680s. Although essentially a political narrative, it is informed by a meticulous investigation of the social composition and geographical distribution of the capital’s dissenting and conformist communities. Thoroughly researched and carefully argued, it is obviously the product of years of thoughtful research.

De Krey begins with the groundswell of opposition to republican government and army rule in London that played an instrumental role in the king’s return. He argues that an overwhelming majority of the City rallied to the king in reaction to sectarian radicals; many Londoners saw Charles II as the only alternative to continuing political instability. But this solidarity fractured with the construction of a narrowly defined Church and the expulsion of Presbyterian clergy from their benefices in August 1662. The intolerance of the official Church gradually gave rise to a “dissenting identity” that united moderate Presbyterians with more radical non-conformists. It also unsettled London politics, allowing the radical values of the 1650s—along with some of the old radical leaders—to re-emerge. In particular, the radicals resumed an old campaign against the autocratic authority of the Crown, the Lord Mayor, and the Court of Aldermen, on behalf of relatively egalitarian civic institutions like Common Hall. In the 1670s, this civic opposition formed bonds with

the “country” opposition to the court and its chief minister, the Earl of Danby.

By the time of the Popish Plot of 1679, London politics was already structured around divisions that stretched back into the Civil War period, giving rise to recognizable parties that fought each other for control through the press and civic demonstrations, as well as through elections. A distinctive political and religious geography that had already emerged in the early Restoration continued to be visible in this partisan contest. Some wards were oriented toward Anglican and Tory values; others were strongly Whig or fiercely contested. But the Whigs remained stronger among the London electorate, even after the court and Tories prevailed at the national level. From 1681 to 1683, the Crown and its Tory allies within the City tried to reassert control, while London Whigs fought back. A late chapter provides a subtle analysis of the blend of legal and violent strategies employed in this resistance.

De Krey does not go out of his way to display interdisciplinary methods: He avoids long discussions of theory and methodology, simply performing the traditional task of exploring his subject with whatever tools come to hand. Although this study could not have been written before historians absorbed methodologies from disciplines like quantitative sociology and urban geography, it does not consciously attempt to stretch the disciplinary envelope. Its primary use for scholars wanting to pioneer interdisciplinary approaches will probably be to provide a solid foundation for investigations of more specific problems, ranging from the evolution of urban political rituals to the relationship between politics and print culture. But every scholar of the Restoration will need to take this important work seriously.

Malcolm Smuts
University of Massachusetts, Boston


Agar has written a substantial history of “the mechanization of government work in the United Kingdom, with a focus on the changing capacities of government” in the nineteenth and twentieth centuries (2–3). Agar’s goal is to bridge the scholarly histories of science and technology and of public policy and public administration, thus integrating the history of the machines used in government with the history of the civil service. Central to his task is to employ and critically analyze the metaphor of the “government as a machine” (15).

The British government worker before 1800 was a “well-connected gentleman . . . who regarded his job . . . as a sinecure” (46). Agar tells the story of the explosion in the number of government positions (from under 20,000 in the late eighteenth century to 40,000 early in the nine-
teenth century and to more than 500,000 at the end of the twentieth century). He describes the transformation of these positions as the civil service was restructured into a complex “mechanical” hierarchy. Some positions were professional and required university education, whereas others were routine and required rationalization of the division of labor. He also traces the changes in the work of particular branches of the government, including the statistical offices, the Treasury, and the war agencies of World Wars I and II and identifies the main theorists who debated the proper organization of government work—Walter Bagehot, Charles Edward Trevelyan, Sir Stafford Northcote, Lord Haldane, and I. James Pitman. Some of the “machines” that flourished in particular offices were, among many others, the typewriter, which, Agar shows, allowed women “typists” to replace the male clerical position of copyist; the Hollerith tabulator used on the census; radar and air-traffic control; and the “electronic stored-program computer” (264).

This wide-ranging book is more intent on examining episodes that illustrate Agar’s arguments than on providing a systematic history of civil service, computerization, military organization, and government. Agar’s detailed chronicle of administrative and technological development runs the gamut from nineteenth-century copyist rooms in Whitehall, to census processing, to proposals for population registration, to the code breaking of Bletchley Park, to the “organization and methods” innovations of the mid-twentieth century. He explores the challenges to the legitimacy and propriety of new methods and structures, such as the emergence of concerns about “privacy as a political issue” in the 1960s, and the attack on government itself—what he calls the “hollowed-out state” (367).

The book is full of suggestive insights and proposals for new conceptualizations. Agar notes, for example, that in the United States, the word “machine” in connection with government refers to political parties, not the civil service. He coins the phrase, “discreet modernism,” to signify the movement within the civil service from World War I to the 1960s, which espoused “technocratic government and . . . a society deferential to the expert” (427). Throughout, he engages larger literatures about state making, governmentality, technological development, and modernization. His insights and arguments sometimes tumble onto the page with overwhelming rapidity and detail. Agar admits that his treatment is not sufficient to serve as a “comprehensive story” or coherent synthesis of the issues involved (3). Nevertheless, it should inspire additional work on these matters in both the history of technology and the history of the state.

Margo Anderson
University of Wisconsin, Milwaukee

In this provocative study, Cook, making use of fertility-rate data, sex manuals, sex surveys, and personal accounts, argues that English women’s desire to limit births led them, in the absence of contraceptive knowledge, to resist sex. This resistance, in turn, played a major role in the creation (contra Foucault) of a repressive Victorian sexuality. Only in the second half of the twentieth century, with the advent of the pill as a reliable contraceptive, did English women experience a sexual revolution.

Central to Cook’s argument is the assertion that contraception was unavailable to English women and men until well into the twentieth century. In opposition to scholars who have emphasized early, if imperfect, contraceptive techniques and technologies, Cook argues that even withdrawal was little used in England and that other forms were virtually unknown. The only option for women who wanted to control their fertility was abstinence. Given the high rates of fertility around the beginning of the nineteenth century and the physical toll that multiple births took on women’s bodies, English women, Cook argues, had a strong desire to limit births. Abstinence required not only women’s control of men but also women’s disciplining of their own bodies. Voilá, women’s presumed passionlessness emerged as the ideological product of women’s agency in the marital bed.

Cook relies heavily on fertility rates in constructing her argument, insisting that historians of sexuality have ignored this crucial evidence. But in order to explain the changes, she turns to qualitative evidence—in the case of the absence of contraceptive techniques, lack of knowledge on the part of worried husbands, or adulterous men who desperately wanted to prevent births. Thus, she argues that neither withdrawal nor abortion was widely used. What had to suffice was sexual restraint, which women practiced while men made use of prostitutes when they could. Cook’s point is that women’s lack of desire (which other historians have contested) made sense and that we must understand the Victorian sexual system from that perspective.

Throughout the book, Cook makes brief comparisons to other cultures, emphasizing the uniqueness of the English sexual system. In the United States, historians have argued that contraceptive knowledge was more widespread and more accepted; Cook explains that contention by both citing different conditions and traditions and questioning the evidence. She pays brief attention to the fact that certain forms of heterosexual sex do not lead to reproduction, but she insists that oral and anal

sex were unacceptable. But why? The logic of Cook’s argument—that women made difficult choices based on their strong desires to limit births—might lead as well to other outcomes. Why did abstinence within marriage, “a course of desperation that could be sustained only by imposition of a repressive sexual and emotional culture, initially by individuals of their own accord” (161), become the only option?

Cook is convincing that ability or inability to control reproduction played a central role in shaping the English sexual system for more than 200 years, and her insistence on women’s agency in the realm of sexuality is laudable. But, in the end, we still might not understand why England should have followed this path when women in other cultures found other ways to save their bodies from incessant childbearing. Nevertheless, Cook has provided a wealth of evidence and a thought-provoking argument about the complex linkages among contraception, sexual practices, sexual attitudes, and birth rates during the “long sexual revolution” of the nineteenth and twentieth centuries.

Leila J. Rupp
University of California, Santa Barbara

*Bombay to Bloomsbury: A Biography of the Strachey Family.* By Barbara Caine (New York, Oxford University Press, 2005) 488 pp. $45.00

Caine’s richly textured biography offers a thoughtful portrait of one of the most prominent families of the Victorian and Edwardian eras. Starting with the marriage of Richard and Jane Strachey in 1859, the narrative follows the lives of the Strachey family and the extended families of their ten children who survived into adulthood. Her interdisciplinary approach to writing history weaves together a set of stories that span almost 100 years and chronicle the Strachey family’s encounter with a rapidly changing world. This rather sizable fourteen chapter collective biography started as a study of the Strachey women. It grew to include the men of the family as the story began to intersect with the histories of feminism, imperialism, and British intellectual life. The experiences of the Strachey women, however, remain firmly at the center of the narrative.

The story of the Strachey family starts in India. Caine’s engagement with postcolonial scholarship in Chapter 1, “An Anglo-Indian Family,” demonstrates the centrality of the British Empire to this biography. Continued connections between the colony and imperial metropole influenced both the female and male members of the family, albeit for different reasons. The early Strachey men understood India as a place for career advancement and participation in the larger British imperial project. In this way, they were a reflection of a liberal Victorian idea of empire that understood Britain as engaged in a “civilizing mission.” Empire
proved more complicated for the later Strachey men, who resisted their father when he pushed them toward a career in the colonial service.

Empire also shaped the lives of some of the Strachey women. In this context, Caine follows scholars such as Burton and Strobel in understanding the Empire as a place of freedom for white women to take positions of authority as representatives of British civilization.\(^1\) India gave Jane and her daughter, Pippa, the opportunity to hone management skills that they took back to England and utilized in the suffrage campaign.

Subsequent chapters place imperialism in dialogue with the histories of feminism and the British literary establishment. Caine adeptly alternates between the stories of individual members of the Strachey family. Each time that Caine reintroduces a character in a chapter, however, she repeats personal details to remind the reader of his or her importance to the argument. For example, she uses the same quotation to describe the influence of Millicent Fawcett on Ray Strachey in Chapter 11, “A Feminist Family” (317), and in Chapter 13, “A Literary Family” (376).

True to her claim to have written a family history, Caine devotes a good deal of attention to sibling relationships and their extended families. Chapter 8, “Sibling ties,” uses methods from sociology and psychology to uncover connections between the Strachey children. A duty to Empire binds together the eldest Strachey children, who came of age during the height of British imperial power. This connection does not continue for the younger Stracheys, however. Rather, the figure of Pippa Strachey provides the glue that binds the family together through her capable sensibility and appealing personality. Sibling relationships in large families, Caine concludes, prove the most effective means of shaping the lives and patterns of each member. Caine demonstrates in Chapter 6, “Modern Marriages,” however, that multigenerational bonds also prove significant. Jane’s continued pregnancies meant that she and her eldest daughter shared the bond of motherhood in a direct way, creating a closeness between the two that overshadowed Elinor’s relationship with her own siblings.

Collective biography provides Caine an opportunity to make a convincing argument about the continuities between Victorian and Edwardian suffragism. This multigenerational approach, popular with early biographies of the feminist movement, demonstrates the different challenges faced by mother and daughter in terms of private commitments and public life. The Strachey women, for example, demonstrate how middle-class women negotiated the rapid changes to their status caused by legislation such as the Married Women’s Property Acts. Although only Chapter 11 deals directly with feminism, the feminism movement seems to have touched each of the Stracheys in some way.

Caine uses gender analysis to discuss both the Strachey women and men. Chapter 9, “Work, Income, and Changing Career Patterns,” interrogates the importance of paid work for the Strachey women, many of whom excelled in their professional lives, unlike several of their brothers. Employing recent work on masculinity, Caine argues that the Strachey men’s own sense of themselves came from sources that included imperial service and sexual experimentation. The transgression of gender boundaries discussed in Chapter 10, “Gender Transformations and the Question of Sexuality,” ends with a discussion of James’ and Alix’s engagement with psychoanalysis particularly in relation to the question of sexual identity.

One of the strengths of this collective approach to biography is the ability to see how changing times affected the attitudes and values of one emblematic family. Caine sums up her assessment of the Strachey family in Chapter 12, “Continuity and Change,” in which she restates her initial claim that the Strachey family remains worthy of study because of its ability to look toward a new world while keeping firmly grounded in a Victorian past. The relationship of the Strachey family to modernity thus provides a key tension in the narrative.

What then defines modern? Caine is equivocal on this point because, for her, the Stracheys represent both the past and the future. Richard, for example, had a “modern” sensibility about “his profession and his life” (329). Lytton and James, though using the term modern to describe themselves, had a distinctive Victorian sensibility that allowed them “to retain old values and assumptions while engaging in the world of modern art, society and politics” (347).

By the 1930s, this long family line had almost come to an end. In the final chapter, “Old Age and death,” Caine brings the Empire home as it slowly fades away from the Strachey family experience and the larger British political landscape. The specter of empire haunts this book, as it did the Strachey family. The end of the empire also marked the end of first-wave feminism. Caine suggests that the convergence of these two histories is somehow connected within a larger crisis of modernity, though she does not explicitly discuss it. Future scholars would do well to follow Caine’s lead further to interrogate the connections between these two distinctive historiographical traditions.

Michelle E. Tuson
University of Nevada, Las Vegas


In a time when elections are being organized in new nations around the world, it is instructive to look back at the role of elections during that great turning point of modern democracy, the French Revolution. In
this regional study of elections before, during, and after the French Revolution in the province of Champagne, Horn makes a good case for the centrality of elections as the key to legitimacy and the exercise of power in the democratizing nation of France. Rejecting an emphasis on discourse as the way to understand the revolutionary dynamic, Horn argues that elections were crucial in determining who was able to gain and retain legitimate power during the revolution’s democratizing process.

Because manipulation of votes and absenteeism at the polls were common during the Revolution and Empire, elections might not appear to have signified much. Horn’s evidence suggests otherwise. Just because the Revolution rejected divine right, custom, and inheritance as the basis for rule, elections became one of the most important means for conferring power, and perhaps the only way to legitimize political authority. For this very reason, authorities were intent on designing electoral procedures with predictable outcomes. Ultimately, the restricted, complicated system of voting installed first in 1790 and then under the Consulate and Empire created a stable foundation of wealthy, agriculturally based notables whose power underpinned the centralized French state and whose presence lasted well into the nineteenth century.

Horn’s analysis begins in 1765 when the royal government began experimenting with municipal and provincial elections as a way to create closer ties between the government and its subjects in the hopes of extracting more financial resources. Until then, elections were generally systems of cooptation that largely validated the prestige of groups already in power. After the debacle of the Seven Years’ War, however, royal reformers were intent on establishing representative assemblies to generate support for royal policies. Even though the government limited these assemblies to the local level, never establishing a national body that could check royal sovereignty, this experiment helped to spread the idea that the public should participate in government. The rejection of the vote by status groups, that is by “order,” in the Estates General cleared the way to a fluid, utilitarian system of voting by head and direct taxation.

The electoral system during the Revolution was designed to slight the importance of such volatile urban centers as Troyes, which had swept away the leadership from the old regime, as well as the institutional basis of power, in a violent “municipal revolution.” As a result, electoral politics created an ongoing division between urban and rural areas, in which well-to-do rural inhabitants controlled the newly created departmental functions, and citizens in Troyes used their urban power base to push forward revolutionary ideals. The Jacobin society in Troyes arose largely as a way to influence elections in this unfavorable situation.

Horn’s account shows that electoral politics by itself could not bring stability to a region plagued by food shortages, ideological division, and mobilization for war. During the Terror, the central government’s representatives purged many local officials. Alexandre Rousselin, in particular, was so violent in Troyes that he destroyed whatever trust was left between the locality and the center. Because the Directory found itself
unable to bring order or legitimacy back to France, it continually sponsored elections, but annulled results that displeased them.

The genius of Napoleon was to overcome this factionalism and disorder by reducing the electoral procedures necessary for legitimacy to the bare minimum through plebiscites and through the appointment of more officials from the center. Wealthy local elites were willing to trade some of their independence in return for political and economic stability. The complex, restrictive, franchise devised by Napoleon for elected officials brought the department of the Aube under the hold of rich, rural notables, who maintained their position for decades through similarly contrived elections. Notables who came to power in 1790 bore a striking similarity to those who reappeared under Napoleon. Thus, Horn’s analysis supports a Tocquevillian interpretation emphasizing the strengthening and continuity of the state during the Revolution, rather than a Marxist one that argues for the rise of a capitalist bourgeoisie.

Although reliable statistics are not always available, Horn uses a variety of archival resources to uncover the professions, wealth, geographical origins, and careers of electors and officials. When possible, he places local statistics into comparative context with other regions of France. Horn’s work provides a fine case study of regional electoral politics. It offers insights into the process of the French Revolution and, more broadly, why elections are critical to modern democratic legitimacy and power.

Gail Bossenga
College of William and Mary

_The Family on Trial in Revolutionary France_. By Suzanne Desan (Berkeley, University of California Press, 2004) 456 pp. $50.00 cloth $24.95 paper

An event that initiated profound political, social, and cultural change, the French Revolution is one of the most studied, and debated, movements in world history. One debate revolves around the Revolution’s impact on women and gender norms. For twenty years, the scholarly consensus has been that the Revolution helped to usher in nineteenth-century domesticity by explicitly banning women from political involvement and by emphasizing women’s nurturing roles in the family. In this interpretation, the exclusion of women from politics was intrinsic to republican ideology. Desan’s examination of familial legislation and resulting court cases from 1789 to the creation of the Civil Code in 1804 rescues republicanism from this accusation, placing the blame instead on the post-Thermidorian quest for social stability cemented into law under Napoleon.

A deeply researched and ambitious book, _The Family on Trial in Revolutionary France_ blends legal history, the social history of the family, and the cultural history of politics. It also combines a local study of legal
cases in one part of Normandy with analysis of the pamphlets, petitions, and legislative debates that emerged around familial issues on a national scale. The result is a convincing and rich exploration of the changing legal terrain of the period, as well as of the various ways people interpreted and exploited the new laws to accomplish their goals—from divorce, to the recognition of an illegitimate child, to inheritance matters.

The book is organized into eight thematic chapters. It begins with an examination of pamphlets, petitions, and speeches critiquing marriage between 1789 and 1792. The second chapter focuses on how Revolutionary legislators viewed marriage as a potent tool for remaking French society and citizens. The next four chapters deal with specific familial issues and their treatment in court cases and legislation: divorce (made legal in 1792), inheritance, illegitimacy and unwed motherhood, and paternity. The final two chapters show how the backlash against the social and political experimentation of the Revolution’s early years led to a more conservative outlook on the family. Chapter 7, on the period from 1795 to 1799, focuses on a rhetorical shift from an emphasis on the natural rights of the individual (as had been the case from 1789 to 1793) toward an emphasis on the importance of maintaining social, familial, and sexual order. This rhetorical shift was then codified and put into practice under Napoleon. The creation of his Civil Code is the subject of Desan’s final chapter.

The most impressive aspect of this insightful and important study is its ability to highlight how practices shaped national legislation. Desan is able to accomplish this feat thanks to her various methods and sources. Her four chapters based on judicial records from Normandy provide rich information about how ordinary people interpreted Revolutionary legislation on the family, and her examination of legislative debates shows how court cases, petitions, pamphlets, and speeches influenced legislators. The give and take between national politics and local practices and solutions comes across with great clarity. The other important contribution of the book is its emphasis on the family, not simply as an institution that was touched by the Revolution but as “a crucial arena for working out the practical meaning of the most fundamental revolutionary goals” (311). This book proves beyond any doubt that remaking the family lay at the very heart of the revolutionary project.

Denise Z. Davidson
Georgia State University

Harnessing the Holocaust: The Politics of Memory in France. By Joan B. Wolf (Stanford, Stanford University Press, 2004) 249 pp. $50.00

The passing of François Mitterrand in 1996 marked the end of a definitive phase in the memory of the Holocaust, and of late twentieth-century France in general. Wolf’s Harnessing the Holocaust explores the
postwar era ably, looking at the discursive uses to which the memory of Jewish genocide during World War II has been put in France from the 1960s to the 1990s. In exploring this history, Wolf advances a complex and intriguing thesis. Starting with the Six Day war of June 1967, she argues that French Jews formulated a reading of the Holocaust as trauma, making it key to an emergent sense of Jewish identity in contemporary France. She also explores how non-Jews increasingly came to use the Holocaust as a template upon which to inscribe their own feelings of suffering and victimization. Although this universalization of the Holocaust plays an important role in the book, Wolf is not primarily concerned with representations of Jews as the “Other.” Rather, she foregrounds the interplay between Jewish views of both the Holocaust and its appropriation by other sectors of French society. For Wolf, the key aspect of this interplay is its articulation of a larger phenomenon, the conflict between Jewish and French identity, that she sees as rooted in the history of Jewish assimilation in France and tragically underscored by the Holocaust itself.

Wolf traces this history from the assertion of French-Jewish identity and Holocaust trauma in 1967 through the various debates about antisemitism and the memory of the Occupation ending with the trial of Maurice Papon. The story of France’s struggle to come to grips with the legacy of World War II, in particular the trials of Klaus Barbie, Paul Touvier, and Papon, has been covered in depth by a number of scholars and popular writers over the last few years. The originality of Wolf’s contribution lies in her focus on the reaction of France’s Jews to these events, and to their implications for both Jewish life there and for French identity as a whole. In considering this perspective, she intelligently criticizes the recent controversial thesis of Henry Rousso, according to which public discourse about Vichy has overemphasized Jewish suffering and the Holocaust. Though not disagreeing with the idea that the Shoah has occupied a central role in public reevaluations of the war years, Wolf argues that this emphasis spoke to the dissonance between the Jewish experience of the Holocaust as trauma and the universalist model of French citizenship. Differences between Jews and non-Jews about how to interpret the relationship between Holocaust and Resistance, for example, illustrate the contradictions of assimilation for both sides. In a particularly intriguing section, Wolf argues that the rise of a more assertive young Jewish generation in France has tended to heal the trauma by rendering the assimilationist paradigm less relevant, paradoxically promoting a different kind of integration into the nation.

Harnessing the Holocaust does an excellent job of analyzing Jewish perceptions of the legacy of the Holocaust in France. It is less successful in making the broader case that many other groups in French society embraced the Holocaust as a metaphor for their own victimization. The fact that such comparisons have at times occurred in particularly inflammatory circumstances, such as the conflict between Israel and Palestine or in Jacques Vergès’ defense of Klaus Barbie, does not necessarily
make them ubiquitous. For example, many have written about the massacre of October 17, 1961, ever since the declassification of the official state documents about it in 1999. Yet few have compared it with the Holocaust, preferring to consider it in the context of France’s colonial past and its troubled relations with its Muslim minority today. This plurality of perspectives, in fact, supports Wolf’s claims about the declining significance of traumatic interpretations of the nation’s Holocaust legacy.

Wolf has written a fine study of memory, ethnicity, and assimilation in postwar France. She makes a valuable contribution not only to recent French history but also to Holocaust studies and the history of memory.

Tyler Stovall
University of California, Berkeley

Bárbaros: Spaniards and Their Savages in the Age of Enlightenment. By David Weber (New Haven, Yale University Press, 2005) 466 pp. $35.00

Weber’s new book is a powerful reminder that at the end of the eighteenth century, more than half of the landmass of what today is Latin America remained under the effective dominion of independent Native American polities, which were, nevertheless, profoundly changed by their contact with Europeans. Since the study of these colonial frontiers makes special demands on providing evidence, Weber avails himself of the methodologies of various disciplines, including cultural anthropology and geography; social, political, and intellectual history; and literary criticism and theory. The inevitable result of his interdisciplinary approach, as well as of the vast geographical scope of his comparative study of borderlands in both North and South America during the eighteenth century, is an enormous apparatus of notes and bibliography that accounts for no less than one-third of his book.

The first chapter establishes the European intellectual context, particularly that of the “Spanish American Enlightenment,” for eighteenth-century Spanish interactions with Native Americans. Whereas the Spanish Habsburgs during the sixteenth and seventeenth centuries had tended to turn to scholastic philosophy and ecclesiastical authorities for guidance in questions relating to Native Americans, the new Bourbon dynasty followed the conservative rationalism that Spanish Enlightenment thinkers (sometimes erroneously) imagined to be exemplified in the French and British approach to Empire. Thus, eighteenth-century Spanish ethnographies, such as the travel diaries kept by Malaspina during his journey from Acapulco up the Pacific coast to Alaska, manifested a “new Spanish sensibility” that viewed the Native American “savage” primarily as a “specimen” in the Enlightenment’s scientific quest to theorize the origin of human society. It also emphasized trade and cultural

integration over conquest and cultural separatism and Spanish de facto occupancy of territory over de jure territorial claims.

The translation of these new sensibilities and policies into actual interactions with Native Americans on the imperial frontiers was, as Weber shows, highly variable—mitigated by and dependent on local geographical, social, and political situations. Among those Europeans who lost influence under the Bourbons’ “new method” of conversion and Hispanization of Native Americans were the regular clergy, whose strong former authority over neophytes was transferred to the secular clergy, and private colonists (the Jesuits were even expelled from the Americas in the 1760s). The beneficiaries were rich private landholders, who were able to buy the secularized mission lands on the old frontiers and to profit from increasingly available cheap Indian labor to work on large haciendas. Weber argues that in Spanish interactions with “unpacified” Indians on the new frontiers, power, though not the power of ideas, typically determined the outcome.

In both North and South America alike, the introduction of European technologies, especially horses, tended to transform loose alliances of smaller independent Native American tribal, ethnic, or political groups into larger and more hierarchical polities. In fact, some of the groups who were most successful in resisting Spanish encroachments—such as the Comanches in North America, as well as the Araucanians, Pampas, or Guaycuruans in South America—were the very products of social, cultural, and demographic transformations that had occurred partially in response to contact with Europeans and their technologies in previous centuries. Especially effective were Native American groups with access to trade goods from rival European powers. They could play off one power against another in shrewd diplomatic maneuvers. Although some native groups had no choice but to submit to Spanish rule, others were actually able to exact tribute from their Spanish neighbors—who typically preferred to view these tributes as “gifts.” Nevertheless, they were forced to recognize the limits of their dominion.

Of special importance in these negotiated border relations were “peoples in between” (256)—cultural brokers, such as captives, traders, and deserters, both Spanish and Indian—whose cultural knowledge was valued by pragmatists on both sides but who were also frequently viewed as a scandale in Spanish ideology for exposing the permeability of the borderline between “civilized” Spaniards and “savage” Indians. Ironically, the “savage” unconquered Indians, such as the indomitable Araucanian warriors, assumed a new ideological significance in early nineteenth-century Spanish American revolutionary propaganda as symbols of American independence. Nonetheless, many of the actual “unpacified” Indians sided with the Crown—perhaps suspecting that their interests were better guarded under an imperial, rather than a national, order.

In its vast geographical scope, covering both North and South American borderlands, Barbaros necessarily relies heavily on a large body of specialized scholarship. Its command and use of the secondary litera-
ture is impressive and responsible, bringing together various disciplines and their methodologies. Thus does it represent a magisterial synthesis of knowledge about eighteenth-century Spanish borderlands and a model of interdisciplinary history.

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*The Culture of Profession in Late Renaissance Italy.* By George W. McClure
(Toronto, University of Toronto Press, 2004) 373 pp. $65.00

Tomaso Garzoni’s fascinating work, *The Universal Piazza of All the Professions of the World*, like a pebble tossed into a pond, provides the central core that moves this study of the culture of the professions in late Renaissance Italy. Republished twenty-nine times in Venice alone between 1585 and 1683, Garzoni’s proto-encyclopedic examination of more than 150 professional categories is the focus of the central chapters of the book (3 and 4), and the other chapters ripple out from them, discussing a rich and understudied group of literary texts that address the meanings of work and the professions. Anticipatory ripples are provided by early chapters, which discuss works that McClure sees leading up to, and influencing, Garzoni and post-ripples are provided in later chapters that examine texts that flesh out a sixteenth-century flourishing of interest in the topic.

Apparently providing a revisionary perspective on the received wisdom that a progressive “aristocratization” of culture and society took place in Renaissance Italy, McClure argues that the literature of the period actually saw a deepening interest in, and understanding of, the professions and their culture. A quick overview of humanist and theological backgrounds opens the discussion, followed by a richer look at the way in which jokes, carnival songs, and parlor games reveal, often in ironic and playful ways, this growing interest. The central Garzoni chapters follow, examining Garzoni from the perspective of authorial intention, but breaking from tradition by not proclaiming a consistent voice. McClure notes, for example, that Garzoni slipped easily from lauding the professions to condemning them and that he often used irony to meld the high and low even as he analytically separated them. Two points are central, however: that, in the end, Garzoni’s work had a leveling effect on the professions (it “self-consciously sought to disrupt traditional hierarchies”) and that it signaled “a fuller discovery and glorification of the lower arts” (209).

The work concludes with chapters that examine late Renaissance texts about dress, ritual, and death for their attitudes toward the professions—most notably, Cesare Vecellio’s *On the Ancient and Modern Dress of Diverse Parts of the World* (1598) and Fabio Glissenti’s revealing, *Moral Discourse Against the Displeasure of Dying* (1596), which, for
McClure, confirm that “the silent and inglorious arts . . . [had] found their voice” (215).

This excellent book provides a challenging new perspective on the meaning and place of the professions in Renaissance culture. But like all works that range widely and make big claims, it will elicit controversy. If McClure had melded his literary studies with the rich material on the topic in Italian archives and the secondary literature based on them, his book and his conclusions might well have been different. A closer analysis of the relationship in practice between nobility and the higher professions might have also been useful, given that Venetian nobles were merchants by tradition and that many of the great families, like the Medici in Florence, were bankers or merchants with strong economic ties to the trades. Finally, the increased interest in the professions at the end of the Renaissance might have coincided with an increased emphasis on social hierarchy not with the result of conferring more respect on the lower professions, as McClure claims, but of keeping them in their proper places.

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_Taming the Wild Field: Colonization and Empire on the Russian Steppe_. By Willard Sunderland (Ithaca, Cornell University Press, 2004) 239 pp. $35.00

“The history of Russia,” wrote Kliuchevskii, “is the history of a country that colonizes itself.”1 By the time readers of this fine history of Russia’s colonization of the steppe frontier get to this well-known line, they will be in a position to understand a body of literature about the edges of the expanding empire that included a cast of characters far beyond Kliuchevskii (209–210). In contrast to Kliuchevskii and the many others who shared an inclination to depict Russia’s expansion as sometimes spontaneous, natural, and organic, Sunderland emphasizes that these views and attitudes disguised the imperial nature of Russia’s contiguous settler colonialism.

Sunderland belongs to a cohort of historians of Russia, perhaps initially inspired by the role of the nationalities and interethnic conflict in the unraveling of the Soviet Union, that has devoted extensive attention to the question of empire in Russian history. Scholars have recently studied the experience of the settlers, cultural borrowing and trade between settlers and natives, the state’s regulation of Islam and other faiths on the frontier, Orientalism and various visions of eastern peoples and regions, and native responses to empire. Sunderland’s focus is the conception of the steppe itself, or what he regularly refers to as its “symbolic appropriation,” which means many things: the land, its peoples, the ex-

1 V. O. Kliuchevskii, _Sochineniia v deviatii tomakh_ (Moscow, 1987), I, 50.
perience of settlement, and especially its meaning for “Russia.” The steppe frontier serves Sunderland as a location perpetually useful for the exploration of the central characteristics of a Russian identity that is always changing over time.

These changes are evident in the evolving preoccupations of ecclesiastics, administrators, military officials, travelers, ethnographers, and geographers who addressed the nature and meaning of the frontier through Russian history. Sunderland’s focus on the steppe is narrow but his geographical and chronological scope is vast. The ever-receding frontier means excursions to southern Ukraine, the North Caucasus, the Middle Volga, Siberia, and Central Asia. The ambitious chronological narrative traces a series of important changes over time, shaped by the unfolding issues of religion, science, and empire. Thirteenth-century chroniclers and churchmen simply noted “unknown people” (Mongols) on the steppe, or “evil beasts and godless creatures” (15–16). The evolution of late Muscovite society, however, included the emergence of chanceries increasingly dedicated to territorial description, cartography, and even the “faith and customs” of the nomad inhabitants of the steppe (34).

The concerns of faith were eventually eclipsed by the “self-consciously Westernizing domain” of the eighteenth century, when the new notions pertinent to the exploration, description, and treatment of the steppe were “utility” and “science” (36). “The new meanings did not come about because the steppe changed but because Muscovy did” (35). Russia’s modern turn to the West had important implications for its civilizing mission in the East and the terms of assimilation of the inhabitants of the steppe. The nineteenth-century witnessed an extraordinary amount of learned speculation about the nature of Russia’s national identity and destiny (narodnost’), which was closely related to the imperial civilizing mission.

Sunderland does not ignore the actual experience of frontier settlement. State policy toward peasants and agricultural practices and conditions are at the heart of his narrative. He provides stories of settler poverty and starvation, problems with supplies, water, and timber, the contraction of malaria and plague, encounters with native inhabitants, and the impact of the absence of women upon settler communities (90–93). He describes the efforts of bureaucrats to alleviate these problems and provide for order on the frontier. He uncovers a world of ethnic diversity, the practices of an empire that was rossiiskaia (“all-Russian”) rather than russkaia (ethnic Russian). Colonists were not just Russian and Ukrainian but often Czech, Bulgarian, Jewish, and Greek. Officials especially sought out German Mennonites (114–122). Only later in the century did officials show a preference for the “Russian element,” although Sunderland finds no evidence, even in the decades of “Russification,” of an empire-wide decree declaring discrimination in favor of Russian settlers (187–188).

Readers of this journal will appreciate Sunderland’s many brief but
learned references to other zones of cultural contact, exploration, and conquest, such as the history of Spain in the New World, American expansion and settlement, and even the expanding Chinese empire in its frontier areas under the Qing dynasty. Still, his interests and source base could have been even broader. That he tends to shy away from literary works is a shame, since both the leading and lesser lights of the Russian literary tradition from the early nineteenth-century were actively involved in the exploration of the frontier and its meaning for Russia. Moreover, he might have continued his story beyond the imperial era. The conquest of the steppe was largely complete by 1900, but the many debates over the past two decades about the definition and boundaries of “Russia” suggest that Sunderland would find plenty to analyze. Sunderland successfully addresses numerous and fascinating issues pertinent to the comparative and interdisciplinary study of colonialism, settlement, empires, and cultural identity.

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Solidarity and Contention: Networks of Polish Opposition. By Maryjane Osa (Minneapolis, University of Minnesota Press, 2003) 240 pp. $64.95 cloth $21.95 paper

This book deals with the issue of how collective action is possible in nondemocratic societies. As the author puts it, “How is it possible for the seeds of democracy to germinate in the unyielding ground of authoritarianism?” (169). Since the authoritarian state is highly capable of repression, and social groups are not allowed the civic space to pursue the protest paths available in democratic societies (such as freely organizing a demonstration, lobbying elected representatives, or electing one’s own), how can challengers bring about genuine political change?

Osa addresses these questions through a study of contentious politics in Poland from 1956 to 1980. She looks closely at three waves of protest—1956 to 1958, 1968 to 1970, and 1976 to 1980—and asks why the latest one succeeded whereas the earlier ones did not. Her explanation is based on a clever application of three basic elements of recent social-movement literature—political opportunity, organizational networks, and cultural framing—to an empirical record that she produces by combining historical accounts with quantitative studies both of protest events and of the personal networks binding individuals from different protest groups. The theoretical model is dense, and the early going is slow, but the book largely succeeds. Her network analysis helps to explain why the protests of 1980, specifically in Gdansk, turned into the massive and unstoppable movement that the previous protest waves did not. Many international factors were also at work, but favorable moments must be seized. For Osa, the international developments are part
of the “opportunity structure” that 1980 activists were able to exploit because of the interconnectedness of their networks, which itself was possible because of the dominance of an inclusive “us versus them” master frame. Osa touches all of the bases. In the end, just as in democratic societies, all three factors must be present for a protest wave to succeed.

The most controversial claim of the book is Osa’s insistence that the protest cycle culminating in the rise of Solidarity in 1980 had an essentially religious master frame. Osa acknowledges that Solidarity was a civic movement, not a religious one, but she claims that only a religious worldview could have united groups as diverse as farmers and workers. That notion seems wrong. Poland’s private farmers often doubled as industrial workers, and they had so many restrictions placed on them that they often thought like workers. A civic and class-based framework (taking into account communism’s peculiar ways of making diverse social groups identify as workers) could unite them just as well as a religious-nationalist framework could.

Osa is not primarily interested in adding to the historical record. She is not interested in discovering what the different protest movements accomplished but in explaining how they were able to get off the ground or why they could not. Although she builds on an established body of social-movement literature, her real contribution is her empirical methodology. She mines existing sources to distinguish the various groups participating in a given protest cycle and to trace the personal linkages between activists in these groups. This is no easy task, particularly when it concerns movements in the past that were illegal and repressed. Her approach is to go through police archives (now that they have opened up), as well as primary and secondary documents, and to compare the names of those social-movement activists who did not hide their identities. Osa’s innovation is to combine rigorous investigative historiographical work with sociological models of network analysis, under an overarching theoretical framework derived from the resource-mobilization school of recent social-movement theory.

How generalizable is Osa’s approach? Poland, with a strong Catholic Church and a government that always shied away from the worst Stalinist abuses, is far from a model authoritarian country. Her claim to provide a study of protest-movement dynamics in nondemocratic societies is thus open to easy criticism. Yet her model lends itself to good comparative research. It could probably be used fruitfully to study both successful and failed protest cycles in other nondemocratic societies, and even to predict which protest movements have a chance of succeeding through analysis of the density and linkages of the opposition groups and the operative master frame. Protest waves with dense network ties based on an inclusive ideological frame have a better chance of succeeding; those with competing movements are likely to fade (though, as Osa rightly points out, not in democratic societies, where protest waves are often advanced by the competition between radical groups). In the end,
although Osa does not emphasize the comparative possibilities of her work, students of social movements in a variety of contexts will find many useful ideas here.

David Ost
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British Atlantic, American Frontier: Spaces of Power in Early Modern British America. By Stephen J. Hornsby (Hanover, University Press of New England, 2005) 307 pp. $60.00 cloth $29.95 paper

Drawing upon an impressive array of historical sources, Hornsby provides a thoughtful synthetical overview of the evolution of Atlantic-facing British America. In *British Atlantic, American Frontier*, he offers “a geographical reconceptualization of early modern North America” before 1480 and 1583 (1). He focuses on the period from the founding of Jamestown to Independence and on Anglo-America.

A brief introduction frames six substantive chapters. Hornsby emphasizes the merits and demerits of the two opposing interpretations—Turner’s frontier thesis and Innis’ staples thesis.1 In an interpretative twist, Hornsby combines the two approaches, arguing that “early modern British America was divided into two principal kinds of spaces”: (1) “an oceanically oriented periphery or marine empire comprised of Newfoundland, the West Indies, and Hudson Bay,” which he confusingly calls “the British Atlantic,” and (2) “a territorially oriented periphery or settler empire that extended along the eastern seaboard of North America from Maine to Georgia,” to which he refers as “the American Frontier.” He also adds an intermediate space comprising “the continental staples and port towns along the eastern seaboard that had links to both the continental interior and the world of Atlantic trade” (5). This space provided “connection, articulation, and friction between the larger oceanic and continental spaces” (6).

Chapter 1 summarizes the slow, often costly expansion of the English into the increasingly important Atlantic marketplace and the eventual establishment of a viable commercial fishery in Newfoundland, a plantation colony in Virginia, and a religious community in Massachusetts. The settlement and development processes at work, Hornsby avers, “would define the spaces of English America” for the succeeding 175 years. Chapters 2, 3, and 4 examine the three key regions—Atlantic staple regions producing fish, sugar, and skins; continental staple regions producing tobacco and rice; and the intermediary “agricultural frontiers.” The British Atlantic region, for instance, “formed islands of settle-

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ment . . . on the western edge” of the ocean. Within their littoral spaces, they had much in common. “A particularly attenuated pattern of settlement emerged” (69); “settlements were functional, utilitarian work places . . . marked by relatively small, racially distinct, and unbalanced populations” (69, 70). In contrast to the North American agricultural empire he sketches in later chapters, “this mercantile empire of the Atlantic was capital intensive, hierarchical, and familiar” and “the oceanic frontier was the preserve of mercantile capital” (71). These “were spaces dominated by metropolitan authority” (72).

In contrast, the “[a]lmost limitless land, immense agricultural opportunities, and scarcity of labor” of the continental staple regions of New England, the Chesapeake, and South Carolina frustrated “tight metropolitan control over staple production” of cod, tobacco, and rice. According to Chapter 3, “[a] continental staple region gradually formed occupying an intermediate position between the trade circuits of the Atlantic and the resources of the continental interior” (73). Chapter 4 argues that the agricultural frontiers of New England, the Mid-Atlantic, and the southern backcountry, “offering immense opportunity to immigrant Europeans and generations of Americans, would help turn the seaboard colonies away from the Atlantic toward the interior of North America.” Ultimately, the westward turn would “set up clashes first with the native peoples and then with the French and the British, and contribute powerfully to the breakup of colonial British America” (126).

This reconceptualization is intriguing, yet its explanatory power is unclear. Does it explain why the thirteen colonies rebelled whereas the other American dependencies remained loyal? Not exactly, for certain similarities among the different regions would have worked against divergence. Does it illuminate the uncomfortable stance that men of the early republic took vis-à-vis the rest of the world? Perhaps so. It certainly confirms the highly integrated and porous nature of the world before 1815, and not just that of coastal communities but those deep inland as well.

This work is a grand summary. No specialist will be surprised at the synthesis that Hornsby provides; it is about as good as one could hope for, given the vast literature in the past fifty years of early American scholarship. Land, labor, and capital receive intensive scrutiny, as does space and its evolving configuration. Unlike most scholars writing today, Hornsby gives more than a nod to Canada and the West Indies. Obvious questions may not have been answered, but few will be unhappy with the attention that he has lavished on geography, agriculture, immigration, and architecture, and with his ability to weave the familiar with the not so familiar.

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American Military Tribunals & Presidential Power: American Revolution to the War on Terror. By Louis Fisher (Lawrence, University Press of Kansas, 2005) 279 pp. $35.00 cloth $16.95 paper

When President George W. Bush issued, on November 12, 2001, a “military order” authorizing the use of military tribunals to try non-citizens who provided assistance to terrorists, hardly anyone knew what he was talking about. Even officers in the Army’s Judge Advocate General’s Corps had to read an obscure twenty-year-old article by this reviewer in order to learn something about a type of court that this country had not seen for half a century. No longer unknown, military tribunals have sparked scholarly debate and public controversy. The Supreme Court even granted certiorari in a case, Hamdan v. Rumsfeld, challenging their legality.

For anyone wanting to understand the current controversy, Fisher’s Military Tribunals and Presidential Power is must reading. Fisher is the author of Nazi Saboteurs on Trial (Lawrence, 2003), an excellent short history of Ex parte Quirin, the 1942 case in which the Supreme Court handed down by far the most important decision on this subject. As Fisher and others have established, Quirin is a thoroughly disreputable precedent, the product of a judicial process hopelessly corrupted by wartime emotions. Yet it remains the controlling precedent in this area of the law.

Fisher devotes one long chapter of Military Tribunals and Presidential Power to summarizing his earlier assault on Quirin. This newer book, however, also offers a complete history of American military tribunals from colonial times to the disputes that followed the issuance of Bush’s military order. It is by far the best available survey of the whole subject.

If that were all it was, this would be a terrific book. Fisher, however, has a broader purpose than simply chronicling the evolution of American military tribunals. “[M]y principal concern,” he writes, “is the breadth of presidential power in time of war, at the cost of legislative and judicial control” (xii). Hence, he extends his coverage to other actions by presidents for which military justifications have been offered to augment political power, including the imposition of martial law in Hawaii and the forcible removal of the Japanese Americans from the West coast during World War II. Fisher also offers lengthy discussions of the post-9/11 cases of Zacarias Moussaoui, Yaser Esam Hamdi, Jose Padilla, and the Guantanamo Bay detainees, none of which involved military tribunals. These other topics are interesting, and Fisher, whose scholarship on military tribunals is outstanding, has done an impressive job of researching them as well. Unfortunately, what he has written reads more like two books pasted together than like one coherent monograph.

Fisher has sacrificed coherence to hammer home a point: The Constitution empowers Congress, not the president, to be the dominant branch so far as the law governing the military is concerned. Military tribunals are obnoxious to him because they “represent an unwise and ill-
conceived concentration of power in the executive branch” (255). Bush’s efforts to keep the judiciary from intervening in the Hamdi, Padilla, and Guantánamo detainee cases represent the same impropriety. Fisher’s militant endorsement of legislative supremacy is hardly surprising; he is, after all, an employee of the Legislative Reference Service of the Library of Congress. Whatever his motivation, Fisher makes a damning case against military tribunals. Any judge familiar with this book could only have voted in *Hamdan v. Rumsfeld* to condemn them.

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Johnson offers a strongly nationalist interpretation of the original constitutional design, of a sort not seen since Crosskey’s massive and eccentric work in the 1950s.¹ Crosskey placed congressional power to regulate commerce among the states of the heart of his nationalist interpretation. Johnson substitutes the national government’s power to tax and spend for the general welfare. According to Johnson, James Madison, Alexander Hamilton, and their Federalist allies were outraged at the states’ failures to supply the national government with the fiscal resources that it needed, particularly because the Federalists foresaw the need to raise money quickly in the likely event of military attack on the new nation. They proposed a constitution that authorized the national government to tax citizens directly, depriving the states of the fiscal control that is the essence of sovereignty. As the book’s title suggests, Johnson emphasizes the moralistic tenor of Federalist advocacy: They used the language of vice and virtue, of shame and duty, to demonstrate the need for a new kind of government.

Johnson’s nationalist interpretation is a useful corrective to several decades in which the scholarship of the Republican Revival has strongly affected originalist constitutional interpretation. For some time, it has seemed, oddly, that we should take the views of the anti-Federalists not merely seriously (they were serious people, after all) but as good guides to interpreting the Constitution. But, as Johnson correctly says, since the Federalists won the battle over replacing the feeble Articles of Confederation, the Constitution should be interpreted as the instrument of the Federalist victory. Today’s Supreme Court majority pursues what is ironically called the “Federalism Revolution” to protect some residuum of state sovereignty against an overreaching national government. But,

in Johnson’s view, the Federalists left no residuum whatever. Anything arguably responsive to anti-Federalist concerns was a “sop” (9), with no significant impact on the national government’s power.

Johnson tells his story with great verve. Along the way, he explains why the Constitution was *not* importantly shaped by the Federalists’ concessions about adding a bill of rights, or by slavery. He acknowledges that some support for the Constitution flowed from concern that the national government have sufficient power to regulate interstate commerce and to ensure that creditors would have adequate protection, but argues that these matters were decidedly secondary to the interest in creating a government able to serve the people by taxing them directly. Johnson also acknowledges that Madison suffered some “partial losses” (100)—such as equal state representation in the Senate and no general national veto over state legislation—but treats them as modest institutional modifications that did not significantly impair the fundamental nationalist plan of the Constitution.

Johnson’s treatment of aspects of the founding that do not fit neatly into his nationalist story shows that he is as much a lawyer building an originalist case for present-day nationalist constitutional interpretation as he is a historian retrieving the past’s complexity. Still, he has documented the central place of nationalism in the Constitution’s framing, and thereby shifted the ground of discussion. Proponents of the Federalist Revolution will have to contend with the large body of evidence that Johnson has compiled demonstrating that nationalism, not state sovereignty, prevailed in the Constitution.

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*Creeks and Southerners: Biculturalism on the Early American Frontier.* By Andrew K. Frank (Lincoln, University of Nebraska Press, 2005) 192 pp. $49.95

*Creeks and Southerners* explores the lives of bicultural Creeks—people with Creek mothers and European fathers—who lived in the American Southeast in the eighteenth and early nineteenth centuries. Seven thematic chapters cover the Creek practice of kinship adoption, the influence of European men and Creek women on their children, the role of bicultural Creeks as intercultural negotiators, and the divided loyalties of some Creeks of European and Indian descent. Throughout, Frank argues that Creeks defined themselves in nonracial, matrilineal terms. Creek Indians, he concludes, had a “nonracial worldview” well into the early nineteenth century (129).

Frank draws impressively on a multitude of sources to craft numerous fascinating portraits of bicultural Creeks. These individuals, drawing
on skills learned from their European or American fathers, frequently served as interpreters, diplomats, and economic innovators. Though colonists used racial terms such as “half-breed” and “mestizo” to describe them, Frank argues that Creeks, who determined clan membership and identity matrilineally, saw them solely as Creek (129). “For most of the pre-removal period,” Frank writes, “paternity and race had little effect on identity” (8).

Yet Creeks and Southerners never considers the possibility that Creeks could have identified themselves by both kinship and race at one and the same time. By drawing more deeply on anthropological, sociological, and historical literature, Frank might have clarified the relationship between clan and racial identity. In particular, Brubaker and Cooper’s work on identity might have brought some precision to a term that appears throughout the book.1 Moreover, scholarship by Hodes, Holt, and others might have offered useful approaches to thinking about the role of race in the early modern Atlantic world.2

Given the thoroughness of Frank’s research, it is puzzling that he excludes Africans and Afro-Creeks from his discussion of biculturalism. The omission of people of African descent from this investigation of identity in a slave-holding society calls into question the book’s overall argument that matrilineality trumped race. In fact, at times, this conclusion seems forced, as in the epilogue. Frank invokes a Creek origin story to show that Creeks still adhered to a nonracial worldview in the early nineteenth century. Yet he cites a narrative that was first recorded in 1842 in Indian Territory, after the Creeks had been removed from the Southeast at gunpoint. The story, about the creation of “white people,” “Indians,” and “negroes,” suggests that Creeks believed that “people were essentially the same,” writes Frank, but it could be just as convincingly read to suggest the opposite (129). Frank then introduces a different origin story to illustrate the “increased importance of race” by the twentieth century (131). This time he cites a narrative first recorded in 1823. On the occasion of its telling, the speaker said that it had been handed down to him from his forefathers.3

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1 Rogers Brubaker and Frederick Cooper, “Beyond’ Identity,'” Theory and Society, XXIX (2000), 1–47.
This book makes every effort to present a “detached analysis” that avoids the old “Christophilic Triumphantist” view associated with Franciscan histories and celebrants of the missions, or the “Christophobic Nihilist” view that condemns the missionaries’ efforts and results and that the author claims to have dominated California mission historiography. This approach is problematical for several reasons. One is that such a “balanced view” is likely impossible given the current level of religious proselytism in certain circles. A second is that avoidance of judgment almost always eschews moral teaching and insight, and a third is that scholars of the missions, at least the top ones in such organizations as the California Mission Studies Association, have abandoned such binary oppositions as the “triumphantist” versus “nihilist.”

Sandos concludes, “From a scholarly perspective . . . Indians and Franciscans together created a mission culture in a complex interplay in which the identification of heroes and villains is as difficult as it is irrelevant” (184). Many will be sympathetic to Sandos’ nonjudgmental position, but others will want to know what, besides disease, caused the calamitous end of so many mission Indians. As an example of how such a putatively balanced view can miss significant points for analysis, the author maintains, “The Spanish and Franciscan ignorance that resulted in the unintentional diminishment of the California Indian population is not comparable to the deliberate Nazi practice in Europe” (180). Aside from the straw man in this comparison, Sandos’ point elides an explicit part of the Spanish program, namely, the genocide of native peoples’ indigenous cultures. The Franciscans’ “conquista espiritual” may have intended conversion but not death, but to ignore the full implications of such conquest is to avoid what ultimately happened to the Indians.

The most original parts of the book lie in the author’s authoritative discussions of music’s role in the conversion of the Indians and in the use of sophisticated, contemporary writings on epidemiology to explain how venereal disease was able to race through the Indian populations so quickly and lethally. For Sandos, the meaning of Christianization encompasses more than a simple discussion of whether or not baptism and communion had much meaning for the Indians. His analysis of the “emotive state” that the competent Indian choristers likely achieved helps to humanize the experience of the missionized Indians. That venereal disease “constitutes one of the previously underappreciated factors contributing to the process of precipitous native population decline” ignores what many have written about the disease, but Sandos’ analysis is the most thorough (111).

Sandos’ research is impressive and his use of theology, epidemiology, and music history admirable. However, the book will provoke more disputes than it settles in this prominent and contentious field of
study. That “priests taught Indians patriarchy and, in the process, lowered the status of women within Indian culture” assumes that Indian women had strong status before the Spaniards arrived and that the Indian cultures of California are susceptible to such generalization (166). Some scholars will challenge the static analysis of the missions and note that the dynamics in their first decades were different from those in the 1830s, when the missions were “secularized.” Consensus on these matters will not be forthcoming, but such interdisciplinary approaches will lead to new ways of thinking about the California missions.

Douglas Monroy
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This work contributes to a recent stream of interest in studies about the history of the book, examining the interaction between religion and print in American culture. Along with such works as Peter Wosh’s Spreading the Word (Ithaca, 1994), Paul Gutjahr’s An American Bible (Stanford, 1999), and David Paul Nord’s Faith in Reading (New York, 2004), Brown studies the complex and often competing relationships between religion, publishing, material culture, commerce, and value formation in nineteenth-century America. In distinction from these works, however, this book’s scope of genres is ambitiously broad. As such, its singular accomplishment is its insight into how a multiplicity of publishing forums worked in concert to produce a national community of religious readers. Thoroughly interdisciplinary in nature, this work draws upon, and contributes to, a wide variety of fields, including American studies and religious history, the history of the book, social history, anthropology, and literary theory.

Evangelical Protestantism was certainly the largest and most dynamic American religious movement in the nineteenth century. It was motivated by what Brown terms dual, and often paradoxical, desires, purity and presence. That is, evangelicals placed a high value on sanctification, which more often than not implied some degree of separation from culture (purity). At the same time, they also sought to have a redemptive or transformative effect on society, which necessitated immersion in the culture (presence). Fulfilling these goals of segregation and integration was by no means a simple task. Nonetheless, evangelicals saw the newly emerging world of inexpensive mass publishing as an ideal means to accomplish both ends simultaneously.

The study is divided into two parts. The first treats the institutional, commercial, and material structures created to transmit the values of evangelical religion to what Brown terms the textual community. She
examines who published (denominations, religious societies, and commercial publishers), what they published (creating an “evangelical canon” of approved works), and how they reinforced the reading habits of their audience (by publishing advice on what and how to read, as well as evidence of reader response).

The second part of the work examines the practical uses to which this publishing universe was put. In particular, Brown explores the ways in which evangelicals deployed two primary forms of publishing, the religious periodical and the hymnal, to reinforce beliefs and engender piety and devotion. The relative accessibility of these forms of print allowed otherwise marginalized segments of society (laypersons, women, and African Americans) to join in the literate world of American religion. The result was a national community of readers who, as they shared timely access to the same information, could participate in a common religious discourse that addressed anxieties about purity and presence.

The most impressive feature of Brown’s methodology is the wide range of historical resources and literary formats that she has investigated: sermons, doctrinal and historical works, diaries, biography, fiction, anthologies, periodicals, and, most provocatively, hymnals—as published by denominations, tract societies, and commercial publishers. The methodological and interpretive challenges were surely daunting. These sources and genres are almost too vast to comprehend, and they contain large lacunae with regard to specific data on the writing and reading habits of the period. Nonetheless, Brown provides a virtual taxonomy of religious publishing in nineteenth-century America, revealing its significance for comprehending the social history and material culture of the era. In particular, she demonstrates the importance of denominational publications for any contemporary inquiry into the intersection of religion and culture in American life.

Robert E. Brown
College of Wooster


In this brief and overreaching book, the authors maintain that the absence of government in the nineteenth-century American West permitted the quick, efficient development of natural-resource economies. Their underlying goal is to instruct the future by illuminating the past of the American West, which holds clear lessons for those attempting to organize and develop the values of nascent resource-rich environments. With some fanfare, the discussion begins with the long-standing
controversy between the Old and New Western History. The Old Western History holds to the smug triumphalism of Manifest Destiny in the expansion of the United States across the continent in the nineteenth century. The westward movement in Turner’s terms is a part of an American creation myth filled with nostalgia and romanticism that portrays the triumph of right and might over wrong and progress and democracy over the backwardness of wilderness, Indians, and foreign governments.\footnote{See Frederick Jackson Turner, \textit{The Frontier in American History} (New York, 1921).} The New Western History eschews with embarrassment these triumphant notes of American “conquest,” highlighting instead violence, injustice, racism, and environmental exploitation. Forever marred, the history of the West stands akin to the history of the South with its stigma of slavery. As unbridgeable and irreconcilable as the gulf between these two camps appears, the authors step to the fore and brashly promise, “We fill the gap.” Let the reconciliation begin (4).

They fill the gap by employing what they call “the new institutional economics” to analyze, and evaluate, the outcome of western settlement in the distribution of resources—land, water, minerals, wild life, and even scenic places. The result is not necessarily a bridge over a deep chasm but an explanation of why Americans should generally take pride in the efficient and rapid production of wealth from the rich treasure house of the American West. The bridge quickly becomes an affirmation of the Old Western History minus the romanticism of Hollywood and the dime novel. The “new institutional economics” celebrates the Old West because it presumably permitted the free market to work. Where barriers occurred that stifled the willing buyer/willing seller relationship, in the form of cultural traditions or governmental rules, violence inevitably ensued. But these were the exceptions, not the rule.

The Wild West was not so wild after all. Resource ownership fell peaceably into private hands, where true “rents” could be realized. The authors’ implication that rational individuals made rational choices according to market needs inevitably creates a West in the Panglossian image of the best of all possible worlds. From this standpoint, the conclusions that may be drawn about the continued bureaucratic presence of the federal government on the public lands of the West into the twenty-first century should be obvious.

That the history written in \textit{The Not So Wild, Wild West} is not always correct seems clear. Individuals influenced by different convictions in the light of contemporary issues can come to far different conclusions about “what history teaches” than do the authors of this book.

William D. Rowley
University of Nevada, Reno
The title of this book is misleading, because it is not about the interstate slave trade in the United States but about southern white attitudes toward that trade. The only transformation being considered is the shift in attitudes among whites, which is not directly tied to the trade itself but rather to U.S. politics alone. The book draws on an impressive array of source materials, from a long list of archives, libraries, newspapers, and primary publications. Gudmestad cannot be faulted on the extent of his research, but he can be questioned on the relevance of this research to many of the most important issues relating to the history of the slave trade and its relationship to the institution of slavery.

The problems with this book raise issues concerning the value of comparative history. This book makes no reference to the influential historiography that has emerged during the last twenty years about the scale and direction of the slave trade. Instead, Gudmestad’s analysis demonstrates a need to counter narrow views of U.S. history that fail to recognize the wider context of the history of slavery elsewhere in the Americas and beyond. Although the book is about the slave trade, it has no discussion of the trans-Atlantic slave trade during the same period, c. 1810 to 1865, nor even of the illegal importation of slaves into the South after abolition in 1807 or American business involvement in the Cuban and Brazilian slave trades. Even the attitudes of Southern whites on these subjects are averted.

Moreover, Gudmestad makes no attempt at a demographic analysis of who moved, how many, or when, other than in a brief appendix. He discusses neither the scale, the direction and flow, nor the relative importance of overland migration versus movement by sea. In short, the broader context for this study is entirely missing. Without demographic analysis no conclusions relating to age or gender of the displaced population are possible. The considerable discussion of divided families and the attempts to avoid them is purely impressionistic, tied to what whites were thinking not to what the enslaved population was experiencing. Furthermore, the book has nothing to say about the impact of the interstate slave trade on the scale of the flight of blacks, both free and enslaved, to Canada and elsewhere.

Moreover, Gudmestad undermines his own case about the ideologically inspired debate among white Southerners by insisting on calling the slave trade little more than “speculation,” in effect adopting the discourse, uncritically, of his principal subjects. He states, in contradiction, however, that the trade was “the most blatant form of capitalist exploitation in the South” (184). What he means by that statement is not clear, because his study is not business or economic history. He has much fascinating information on such businessmen as Isaac Franklin and John
Armfield, but their activities are not compared with the business activities of other slave traders, about which much information and analysis is available. Furthermore, it is difficult to discuss the business activities of such men without constructing price series, examining the economics of credit mechanisms, and otherwise assessing the accounts of such men. Hence, the description of this book as about commerce and trade is misleading.

To Gudmestad’s credit, he offers numerous, fascinating quotations and references from the wealth of archival and other primary material that he examined. Unfortunately, he did not try to establish some way to analyze what surely is an enormous body of information, which is certainly substantial enough to warrant a database or two. The material could have been used to elucidate the voices of those who were being traded, and not just focus on the white elite. Without such analysis, the book misses the context in which the attitudes of that elite were shaped. In the end, the book provides a myopic view of the interstate slave trade that ignores the important historical debates that are currently shaping the reconstruction of the history of the African diaspora, including the history of African Americans.

Paul E. Lovejoy
York University


This extensively researched book draws ambitious conclusions about the sugar industry and slavery in pre-bellum Louisiana. It relies heavily on traditional historical sources—correspondence and plantation records left by large planters, travelers’ accounts, newspapers, slave narratives, and extensive secondary sources about slave systems in other places in the United States and in the Caribbean. It emphasizes the capitalist nature of this market-driven sugar industry, which operated with a partial cash wage system for its slaves. It presents the Louisiana sugar masters as highly efficient and innovative. It touches on demographic history—age, gender, and birthing patterns among slaves on estates, as well as among slaves that the masters purchased. Its greatest strength is in its impressively rich, descriptive details from the points of view of both masters and slaves. Like many good, serious scholarly works, it raises questions and arrives at answers that call for further exploration. But it touches much too lightly upon the slave system in Louisiana before 1820, including early sugar estates.

The book’s greatest weakness as an interdisciplinary study is, surprisingly, its economic and demographic research of Louisiana’s sugar
plantations, arriving at conclusions that are speculative rather than based upon solid evidence. For example, Follett understates the masters’ problems in controlling their slaves. The motivation for masters’ purchase of young women could have been to keep the male slaves on the estates with their wives and children. On most sugar plantations in the Americas, however, it was much cheaper to buy than to breed; this issue calls for a cost-benefit analysis. The unhealthy climate of Louisiana should not be ignored. Moreover, Follett fails to consider masters’ sexual exploitation of nubile females as a motive. The extent of wage labor among slaves no doubt stemmed from the traditional customary demands of Louisiana slaves.

As Follett points out, the Louisiana sugar system was unique in that the grinding season lasted six to eight weeks to avoid frost and freezing. In the Caribbean and Latin America, the grinding season lasted six to eight months. The Louisiana system was protected by tariff walls and could not have survived without them. Certainly the Cuban system was technologically competitive with the Louisiana system. Steam engines and railroads were introduced early. Fraginals pointed out that the Cuban sugar system could not have survived without Norbert Rillieux’s closed vacuum-pan system.1 The assumption that Louisiana sugar masters were exceptionally and extraordinarily efficient is based upon their own self-images rather than economic studies.

If Follett had paid sufficient attention to the Louisiana slave system before 1820, he would have avoided some serious mistakes. He claims that the masters taught the slaves whatever skills they knew. In fact, African slaves brought many skills with them from Africa, especially from Senegambia and West Central Africa—most notably, blacksmithing and metallurgy. Remarkably, he claims that the sugar belt was far removed from New Orleans and its slave-operated markets, and that slaves remained isolated on plantations. In fact, commercial sugar was first produced in rural sections of Orleans Parish and then spread to the nearby German Coast. The broad network of rivers and bayous and the presence of runaway-slave communities in the cypress swamps greatly facilitated communications and marketing by slaves.

Follett’s brief concluding discussion of culture ignores the 100 years of developing Afro-Creole culture in Louisiana before 1820. In the sugar belt, the Louisiana Creole language was widely spoken until World War II. Nevertheless, Follett writes of “large Louisiana slave communities where Afro-Haitian traditions fused with those of Virginia and Carolina slaves” (229–230). Although Haitian influence was strong in urban New Orleans, few Haitian slaves arrived in rural areas.

Gwendolyn Midlo Hall
Rutgers University

More than thirty years ago, Ackerknecht called upon his colleagues in the history of medicine to shift their emphasis from the published accounts of the medical elite to the more intimate records of daily practice found in diaries, daybooks, case records, correspondence, and physicians’ ledgers—in other words, to close the gap “between the medicine preached and the medicine actually practiced.”¹ This he called a “behaviorist” approach to medical history. Fulfilling this worthy suggestion imposed a daunting task for historians. To follow this behaviorist model the readily available journal, proceedings, and textbook literature could no longer serve the purpose; rather, archives containing elusive scraps of personal memorabilia squirreled away in out-of-the-way places would become the prize of the historical prospector. Given the magnitude of the behaviorist project, few have been willing to embark upon this difficult and time-consuming mining expedition.

Stowe, however, has accepted the Ackerknecht challenge. Through his meticulous digging in archival collections throughout the South, he has shown himself adept in ferreting out the gems of everyday practice as it really was in the nineteenth century. This prodigious research is augmented by a well-written narrative that takes readers to southern medical-school classrooms, to doctors’ offices, and to the bedsides of the neighbors that they sought to mend and heal. “The achievement of country orthodoxy,” writes Stowe, “was to affirm that all the experience a man needed to be a good doctor was available to him in his community” (270). Indeed this is a book about the bonds and boundaries of country medicine delivered in a caring environment that placed emphasis less on science than on the strong attachments of region and religion. One rather surprising conclusion drawn from Stowe’s book is that, except for race, the practices of doctors in the South probably varied little from those of their northern colleagues; in both cases, the close relationship of physicians with their community shaped and molded their everyday professional affairs. In a nation that was predominantly rural, the seasonal rhythms of agrarian life above and below the Mason-Dixon Line conferred continuity and commonality on physicians’ practices, distinguished only by the sharp realities of the South’s herrenvolk social order. Stowe’s book is a tale written large upon the American landscape.

Two relatively minor points need clarification. First, Stowe asserts that by mid-century, clinical work and experience with the sick was “widespread and taken seriously” (52). Although he may be correct, he needs to marshal more evidence than mere assertion and a few sketchy

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examples. Countering Stowe is the South Carolinian J. Marion Sims who received his medical degree in 1835 after attending colleges in Charleston and Philadelphia. Sims insisted that he embarked upon his medical career with "no clinical advantages, no hospital experience, and had seen nothing at all of sickness."\(^2\) Similarly, when Simon Baruch earned his degree from the Medical College of Virginia in 1862 he did so without "ever treating a sick person or even having lanced a boil."\(^3\)

The second point of contention is Stowe’s persistent casting of the southern physician in terms of orthodoxy. "As with other contexts of orthodox practice we have seen," writes Stowe, "the fact that each man’s personal style enlarged rather than transgressed orthodoxy is the key to understanding how orthodoxy retained is powerful continuity on the rural, domestic scene" (176). But mid-nineteenth century also saw a smaller but nonetheless widespread and important assortment of alternative practitioners on the American scene, North and South. Surely the connections between botanics and homeopaths—all of whom sported degrees from sectarian colleges throughout the young nation—and the communities that they served mirrored in numerous ways those of their regular counterparts.

Typical were the recollections of George Taylor concerning his eclectic physician/father who practiced as an integral part of his community in Graves County, Kentucky, from 1889 to 1915: "He waged a never ceasing war against many diseases, morbid humors and other ills . . . getting little financial reward."\(^4\) The close bonds forged between doctor, patient, and community were as much in evidence with this eclectic practitioner as with any of Stowe’s physicians who were taking the “broad, social meanings from their work” and making “their personal experience in medicine essentially, irreducibly moral” (227). These doctors were not forging orthodoxy, they were meting out empathy and care. One is persuaded that in rural America such services knew no label other than “Doc.”

Indeed, not until medicine defined itself increasingly as a “science” rather than an “art,” a science that validated itself within a context of research-based, clinical instruction delivered in an institutional rather than a domestic setting, did the debate between regular and irregular move beyond rhetorical point–counterpoint to expose increasingly marked distinctions between the two.

Rather than diminishing Stowe’s contribution, however, this picture, which may shed a broader light on nineteenth-century practice than intended, only supplements the value of Stowe’s work. His master-

\(^2\) J. Marion Sims, *The Story of My Life* (New York, 1884), 139.


ful achievement should become a model for Ackerknecht’s behaviorist approach to medical history. But it is also a work that tells an important story on many levels—religious, economic, regional, and social.

Michael A. Flannery
University of Alabama, Birmingham

Red Earth: Race and Agriculture in Oklahoma Territory. By Bonnie Lynn Sherow (Lawrence, University Press of Kansas, 2004) 178 pp. $29.95

Farming on the southern Great Plains has been a risky endeavor since the white settlement of the region. In Oklahoma, agriculture probably produced more failures than successes during the territorial period. This book is the story of white, black, and Indian farmers who struggled against the environment, as well as against government policy, to coax a living from the soil. It is not the story of inevitable triumph and prosperity. Rather, it is a sobering account of the effects of racism and culture in Oklahoma’s agricultural history.

Lynn-Sherow has written an overview of three distinct county settlements that serve as case studies for the ecological and social transformation of agriculture in the Oklahoma Territory. Significantly, she focused on the agricultural settlement of African Americans in Logan County, white settlers in Blaine County, and the Kiowas in Caddo County, all in relation to agricultural development between 1889 and 1906.

Lynn-Sherow is particularly interested in ecological changes resulting from the agricultural activities of these groups. She observes that although white and African-American settlers considered the Oklahoma Territory a virtual wilderness, the Indians, particularly the Cheyennes, Arapahos, and Kiowas, had used that area for a long time before white settlement. Moreover, the Native Americans traditionally used the land far differently than federal policy now required. Not surprisingly she found that racism and government policy prevented African-American and Indian farmers from succeeding in a market economy. Federal policy also jeopardized the traditional Indian economy through the allotment of lands authorized by the Dawes Act.

Lynn-Sherow makes the important observation that racism forced black farmers to travel from the towns where they lived, and received community support, to work the land, often as sharecroppers rather than landowners. She also notes that Indian farmers failed partly because agriculture in the white tradition destroyed traditional community life and culture. Racism also prevented government policy from providing the agricultural education and technical aid that African-American and Indian farmers needed to advance beyond subsistence to commercial agriculture. In contrast, white farmers increasingly borrowed capital and invested in land and machinery to participate in the market economy.
based on wheat and cattle rather than corn and cotton. Moreover, whites relegated black and Indian farmers to marginal lands where agricultural success became increasingly problematical. By the early twentieth century, few African Americans or Indians farmed.

Overall, Lynn-Sherow has provided a clearly written synthesis of the major factors affecting African-American, Indian, and white agriculture in the Oklahoma Territory. This study primarily is based on secondary historical literature, local newspapers, and government documents. Although it is not interdisciplinary, it merits the attention of anyone interested in the history of Oklahoma’s agricultural frontier.

R. Douglas Hurt
Purdue University


For liberal, university-based historians looking for evidence to use in refuting the right-wing mantra of the culture wars—that the nation’s campuses are dominated by left-wing thought-police eagerly indoctrinating students with political correctness, multiculturalism, and hate-America-first ideas—Historians in Public is not the answer. The book, however, does confront the lesser included offense—that academic historians know more and more about less and less; that they write in jargon decipherable only by other members of the sect; and that they are lost in poststructural and postmodern theory rather than doing their job, which is to illuminate the narrative of the human experience in ways that will enrich and inform citizens. On this charge, Tyrrell and his evidence pronounce the profession “not guilty.”

Perhaps it would be more accurate to say that Tyrrell documents the struggle of professional historians to find ways to share their knowledge with the public. By examining the practice of history from 1890 to 1970, from the onset of self-conscious professionalism to the shattering of the profession during the unrest of the 1960s, Tyrrell is able to tell the story of early and continuing efforts to lure the public into an engagement with history; to make history useful in public-policy discussions; and to link academia to state, local, and public history organizations.

Tyrrell deduces from the official records of historical societies, contemporaneous newspapers and journals, and the papers of prominent figures that “the threat to history is a recurrent, exaggerated, and often misunderstood one and that history has adapted to and influenced its changing publics more than the profession is given credit for, though not evenly and not always in ways that are readily apparent” (2). Rather than being a full history of History, Tyrrell follows the thread of the relationship between practicing historians of the United States and the
American public, whether the public appears in the guise of a mass audience or a group organized by state, local, genealogical, or patriotic institutions.

Of particular interest is Tyrrell’s treatment of the profession’s attempt to ensure that the teaching of history in schools is well structured and appropriate. This goal involves finding a place for history as part of “civic education,” in which it must compete with the social sciences for scarce space in the social-studies curriculum. Tyrrell insists that the attacks on history in the schools have been poorly informed, wrong in assuming that the problem stems from history’s professionalization and specialization, and generally overly simplistic in evaluating the politics of the school curriculum. Although never more than a minority of academic historians was ever interested in promoting history in schools, and although their success tended to ebb and flow, the effort of the profession has been constant.

Through the avalanche of information, there is a discernible narrative line in *Historians in Public*. “Scientific History was challenged by Progressives, who sought with some effect to reconnect with wider audiences; combat excessive specialization; apply history to the pressing social problems after 1900 through government agencies and policies; and make the teaching of history more relevant to the lives of the more democratized schools after 1920. Attempts were also made to revive state and local history and put it on a more professional and yet more ‘popular’ basis” (252). The result is a picture of a profession constantly debating not only the content of the history being propagated but the purposes of that history as well.

The jeremiad that Tyrrell effectively undercuts has grown more insistent since 1970, especially that aspect of it that alleges political bias. With the debate about “who we are as a nation” made more urgent by the war in Iraq, any attempt to bring Tyrrell’s story forward from 1970 will need to grapple frontally with the ideological dimension. That attempt will have sound footing, provided by *Historians in Public*.

Sheldon Hackney
University of Pennsylvania

*From Jim Crow to Civil Rights: The Supreme Court and the Struggle for Racial Equality.* By Michael J. Klarman (New York, Oxford University Press, 2004) 655 pp. $35.00 cloth $19.95 paper

In our public culture, the civil-rights movement (really an interlocking set of many different movements happening in different places and in different ways) has become embodied in the triumphant moment when Martin Luther King, Jr., stepped to the microphone in August 1963 and told the country that he had a dream of a glorious future in which character mattered more than color. The actual history of resistant local, state, and federal governments has turned into a story of American
exceptionalism: “America” recognized, without complication or caveat, the eloquence and truth of King’s dream and passed a series of laws that changed the way the country operated and preserved the United States’ place as a beacon of hope to all disfranchised people around the world. But mythologizing the events of the civil-rights movement into a national narrative that ignored the extremely complicated and contested nature of the movement was also an act of denying meaning to all of those citizens who could not enjoy their full rights and even today still struggle for their full realization.

Klarman’s From Jim Crow to Civil Rights does not participate in this myth-making endeavor; it makes an important contribution to the body of work that looks at the civil-rights movement with an exacting gaze. Scholars are well beyond the hagiographies of the great movement leaders and have learned much from the social histories of places like Mississippi in the 1960s. Now, many scholars are moving back in time to the 1930s or earlier in their efforts to understand the civil-rights movement. Klarman makes his most valuable contribution in this context. His work examines the legal, political, and sociocultural histories of race and America from Plessy v. Ferguson (and other, less well-known cases) in 1896 through Brown v. Board of Education in Topeka in 1954 (with an extended chapter on the effects of Brown on the rising civil-rights movement). Some might consider all but the final chapter a pre-history of the civil-rights movement, but it is more accurate to consider it a legal history of the underpinnings of the movement—one that became coherent as a movement at least as early as 1941.

Klarman is a legal historian and it shows. He pays less attention to presenting a unifying narrative than to presenting his arguments in a highly disciplined and well-organized fashion. Each chapter contains a core thesis about the most important race/civil rights cases of the respective era and an explanation of the various judges’ decisions and how and why they either tended to the literal interpretation of constitutional law or wrote decisions that reflected a sensitivity to the shifting cultural and social zeitgeist.

There are no easy answers, but Klarman does a fine job of demonstrating why legal fights over peonage, housing discrimination, and school desegregation evolved as they did. What may be of most value to American historians is that Klarman casts a wide net in his study but resists the temptation to gloss over complications. His history is sweeping yet detailed, argumentative but not self-righteous. Klarman has a tendency—as he admits in the book’s introduction—to deploy technical legal jargon, and he relies upon a counterfactual mode of engagement from time to time that is less effective outside the context of a law school lecture hall. But these are not so much defects as they are a reflection of his attempt to talk to two sets of disciplines in a language that both will understand. Overwhelmingly he succeeds.

Jonathan Scott Holloway
Yale University

What lies behind our common adage that “boys will be boys”? A complex and evolving blend of ideas, writes Kidd in his literary and historical analysis of how American professionals have defined boy culture over the past century and a half. Utilizing insightful research that blends sources ranging from YMCA leader Henry W. Gibson’s handbook to the ideology of Boy Scouts to Huck Finn to Rudyard Kipling’s *Jungle Book* to Sigmund Freud and “Oedipality,” Kidd weaves a broad but detailed account of two basic concepts—“boyology” and the “feral tale.” Gibson coined the term *boyology* in his 1916 book *Boyology or Boy Analysis*, which appeared at a time when child study and “child saving” had become widespread. His goal, shared by a variety of youth workers and organizations, was to inculcate “character” and harness the mischievous spirit of the so-called Bad Boy so as to prepare him (meaning always the white, middle-class youth) to become a powerful and manly yet respectable adult.

Sharing the cultural landscape with boyology was the “feral tale,” the image—confirmed by widespread folklore, testimony, and fictional characters such as Mowgli and Tarzan—of the child raised in the forest by wolves. The feral tale, says Kidd, played an important role in boy culture because it “embodied both the normative self and the cultural other” (6). This concept actually preceded and “set the stage” for boyology, and it remains present currently, represented in examples as diverse as Maurice Sendak’s picture-book *Where the Wild Things Are* (New York, 1964) and Robert Bly’s *Iron John: A Book about Men* (New York, 1990). As well, the image of the “wild child” has influenced “theories about race, gender, the psyche, and culture more broadly” (7).

*Making American Boys* serves as a useful work of cultural history. Kidd provides a context for understanding a wide variety of boy-related institutions and attitudes. His analysis is particularly strong on issues involving homosexuality and popular culture. A chapter on Father Edward Joseph Flanagan and Boys Town, including the popular 1939 motion picture, is particularly noteworthy, though Kidd seems not to have realized that the institution’s expansion since the 1980s resulted from an exposé concerning the hoarding of donations solicited from a sentimental public. Kidd’s occasional attempt to impart academic gravity by inventing and/or applying poststructural terms such as “hypercanonization,” “bibliotherapeutic,” and “minoritizing” seems unnecessary, but he presents a thesis that is thoughtful and provocative.

Howard P. Chudacoff
Brown University
In *The Southern Diaspora*, Gregory sets forth the important but often overlooked theme that “migration matters” (327). Whether the large-scale migration of Southern, Central, and Eastern Europeans to the United States or the involuntary migration of Africans to North America, migration has consequences not only for the individuals involved but also for the places of departure and arrival. The Great Migration, according to Gregory, is no less important: Gregory’s account moves the study of internal migration beyond economic or demographic statistics, or even historical reports of individual stories, and places the Great Migration in a more comprehensive context. In particular, Gregory makes wonderful linkages among migration, race, class, and social change.

In the first few chapters, Gregory sets the stage for his argument that the migration of southern-born persons throughout much of the twentieth century changed the South as well as other regions. Using the invaluable Integrated Public Use Microdata Series (*ipums*), Gregory presents a statistical picture of the Great Migration by comparing blacks and whites over time. He shows, for example, that although the proportion of southern-born blacks living outside the South was larger than the proportion of southern-born whites in each decade after World War I, the absolute number of whites was always larger.

The greatest proportion, and strongest part, of this book, though, follows the descriptive, statistical components. Gregory analyzes the social and cultural impacts of black and white migrants throughout the United States. However, the consequences are not the same. For blacks, Gregory is dissatisfied with the lack of explanation presented in the literature about their impacts; he aims to provide more. For instance, in a key chapter on civil rights, Gregory shows how “the Black Metropolises provided the base for a sequence of extremely important political developments that were not just prelude but precondition to the southern civil rights breakthroughs” (238; italics added). For whites, though the literature is smaller, Gregory still questions many of the assumptions held by scholars and the media. He writes, for example, “It is easy to overlook the white liberals and radicals who moved north but a mistake to do so, for they [Robert Penn Warren and Woody Guthrie, to name two] sometimes found themselves in influential position” (320).

Gregory skillfully combines a number of issues that are often kept separate—in part because of disciplinary boundaries—in the Great Migration literature. His examination of such matters as the influence of media outlets like *The Chicago Defender*, the increased popularity of country music across the United States, or the role that (white) Protestantism has had in the development of political conservatism helps to forge new histories of black and white migrants. However, his analysis of how the Great Migration forced changes in the American racial land-
scape does not overstate its influence. For instance, Gregory recognizes the ways in which southern whites fueled northern-style racism, but he does not forget that racism existed in the North before the southerners arrived. Finally, race itself remains central to Gregory’s story: “Racial privilege granted white southern migrants significant economic advantages over their black counterparts and also spatial advantages: the choice of where and how and with whom they settled” (325). This book is essential reading for anyone interested in migration but also for those with an eye on race, class, and sociocultural change in twentieth-century America.

Robert M. Adelman
State University of New York, Buffalo


Trost is correct: Most of the many studies about the creation and workings of the juvenile court concern the major urban centers of Progressivism—Boston, New York, Chicago, and Los Angeles. Historians have not mined other regions of the United States in their quest to explain the juvenile justice system that emerged throughout the nation during the first decades of the twentieth century. In Gateway to Justice, Trost provides a corrective to this regional centrism.

On the one hand, Trost shows in this history of child welfare in Memphis, Tennessee, during the 1910s and 1920s that Progressives in the South were tied into national child-welfare networks and that their solutions to delinquency and dependency reflected national norms. Women formed the core of child-welfare advocacy in Memphis. According to Trost, their efforts paralleled the Progressive maternalism that Muncy, Gordon, and others have identified as a central part of urban progressivism. The women of Memphis modeled their juvenile justice institutions on those established in other cities. They created a separate juvenile court, relied on probation and family intervention with institutionalization as a last resort, and tried to establish a child-guidance clinic (succeeding only briefly for two years in the 1920s).

On the other hand, Trost shows that child welfare in Memphis had several unique characteristics. In this southern city, Progressives forged a special relationship between reliance on government-sponsored institutions and dependence on private charitable organizations. The juvenile court, Trost argues, became a “gateway” to the network of private agencies and institutions that served delinquent and dependent children in

Memphis. Families and welfare agents used the court to address child-rearing problems. At the same time, southern racism created a problem for Progressives in Memphis that differentiated the city’s work with children from the programs in other regions. The court reinforced a segregated welfare system, even though the private services available for African-American youths were far less extensive than those for white children, and government-financed services (such as probation officers) were unevenly distributed. The South was different, Trost concludes, but not completely different from other urban centers of Progressive child welfare.

Trost’s study makes use of the records of the Memphis juvenile court, and she situates her research at the junction of legal and social history. Based on her sampling of the court data, she discovers that the court had two functions—handling cases of dependency and neglect and cases of delinquency—and that the race, class, and gender of those appearing before the court shaped the outcome of both types of case. She finds that the court was not an “instrument of systematic racial discrimination,” although it “invested fewer personnel, less money, and less time” in black children than in its white clients (154–155).

Gateway to Justice points to the value of local studies of Progressivism that test models based on evidence from major urban centers. Localism certainly helped historians redefine the nature of political progressivism. Trost’s book suggests that localism is also an important factor in the history of child welfare.

Kathleen W. Jones
Virginia Tech

Good Americans: Italian and Jewish Immigrants During the First World War. By Christopher Sterba (New York, Oxford University Press, 2003) 271 pp. $24.95 paper

The recent congressional debates and street protests regarding the legal treatment of immigrants come in the midst of the U.S. war in Iraq. Given that the first soldier killed in the war was not an American citizen, Sterba’s book on immigrants’ military service is particularly timely and important. Good Americans is a well-written and thoughtfully researched comparative study of Italian and Jewish immigrants in the eastern United States during the World War I era. Sterba challenges immigration historians both to center national events in the making of local communities and to return to older questions that focused on how European ethnic groups were integrated into the political and cultural landscape of America’s past. Sterba’s study is significant because it reflects an attempt to blend two generations of historiography that have often been in conflict.

Good Americans is organized chronologically around three major thematic issues: (1) How did the nation contend with diverse ethnic
populations that supported and criticized the war for distinct reasons? (2)
In what ways were Italian and Jewish responses to World War I shaped by national and international circumstances that defined their situations in New Haven and New York, respectively? (3) What were the long-term consequences of America’s short combat involvement in the war, and how does an assessment of the postwar period reshape our understanding of new immigration. Sterba is at his best when he articulates gaps in scholarship that allow only a myopic view of immigrant life. His point that analysis of national events is crucial to understanding the meaning of ethnic experience is well taken. The book’s comparative framework lends itself well to addressing the first two themes that he highlights. But, because the author devotes only a short epilogue to the postwar period, he is incapable of providing much meaningful contribution on the third theme.

Sterba is meticulous in his depiction of New Haven’s Italian and New York City’s Jewish populations. He shows that, despite all of the differences, these immigrants had some things in common. Comprising the largest ethnic group in their respective locales, they had relative autonomy, as well as political and economic heterogeneity. Although they were able to remain relatively isolated during the Progressive period, World War I forced a confrontation between nation and enclave. The federal government’s nationalization of the armed forces and its demand for loyalty required direct responses. In both cases, this nationalization was, in fact, a transnational phenomenon. The appeal to serve resonated with dual (not hyphenated yet) identities as Italians/Jews and Americans. As the U.S. was pulled into the war, distinctions between the two communities became more apparent. New Haven Italians embraced the war. They were recruited into the Italian army as early as 1915 if they had not yet declared their intention to become U.S. citizens, raised money to support the war before American involvement, and joined ethnically segregated units of the national Guard before 1917. New York City Jews were more divided, waiting for conscription and resisting the draft more consistently than their Italian counterparts. In the end, Sterba argues, regardless of how the Jews and Italians entered the military, the war created unity out of diversity.

Sterba’s point that military service created unity (President Theodore Roosevelt made the same point at the time) is well documented, but the author’s claim that immigrants on the homefront underwent a similar transformation is less convincing. The main problem stems from Sterba’s failure to engage fully primary (especially foreign-language) and secondary sources that focus on dissent.

Caroline Waldron Merithew
University of Dayton
FDR and the Soviet Union: The President’s Battles over Foreign Policy. By Mary E. Glantz (Lawrence, University Press of Kansas, 2005) 264 pp. $34.95

Presidents routinely complain about the difficulty of implementing their policies in the face of an intractable federal bureaucracy. Glantz, a foreign service officer in the State Department, helps to explain the sources of presidential frustration in a fascinating and important account of the conflict between Franklin D. Roosevelt and the bureaucrats in his own government about U.S. policy toward the Soviet Union. According to Glantz, Roosevelt came into office believing good relations with the Soviet Union were critical to America’s national security and strategic interests. As early as his first year in office, he saw in the Soviet Union a potential ally against Germany and Japan. But so suspicious of the Soviets were most of the career officers in the State and War Departments, including the Russian experts, that they resisted Roosevelt’s overtures to the Soviets at almost every step of the way. In 1933, they opposed diplomatic recognition of the Soviet Union; in 1941, following Germany’s invasion of the Soviet Union, they opposed unconditional military aid to the Soviets; and throughout the war, they opposed Roosevelt’s plans to include the U.S.S.R. as a full partner in the postwar world.

In typical fashion, Roosevelt usually tried to maneuver around his internal critics rather than to fire or reassign them. During World War II, instead of dealing with Soviet Premier Joseph Stalin through the American ambassador in Moscow and his embassy staff, he dealt with the Soviet premier personally or through trusted advisers, such as Harry Hopkins. In 1942, Roosevelt even authorized a private meeting between Stalin and Wendell Willkie, the man that he had defeated for the presidency two years earlier—a meeting to which the U.S. Ambassador William H. Standley was not invited. In the ongoing battle with his subordinates, Roosevelt usually prevailed, although not without difficulty. In the end, however, as the author maintains, those same subordinates found, in Roosevelt’s successor, Harry S Truman, a sympathetic ear.

Glantz clearly sides with Roosevelt rather than her professional forebears in the foreign service. In her view, he was more foresighted and realistic than they, and his determination to cultivate good relations with the Soviets stemmed not from naiveté or ignorance but from a clear-eyed understanding of the benefits of cooperation for both countries. By contrast, most of his diplomatic and military personnel not only allowed hostility to the Soviet Union to cloud their professional judgments but also to distort the intelligence that they provided the White House. The most serious example of faulty intelligence was the nearly unanimous prediction from U.S. diplomatic and military officials that the Soviets could not hold out against the German invasion in 1941 and that therefore U.S. military aid to them would be a waste of resources.

Agreement with Glantz’s depictions of a Roosevelt and his bureaucratic antagonists is not a prerequisite for finding this book rewarding.
To this reviewer at least, despite Glantz’s impressive research in American, British, and published Russian sources, her Roosevelt is more prescient and realistic—more of a strategic thinker—and his critics more bullheaded and biased that the evidence can support. But the chief value of this book is not its interpretation but its attention to the second and third-tier bureaucrats and diplomats who, even if they did not get their way during Roosevelt’s administration, were an essential part of the diplomatic and military history of his administration. As such, Glantz’s study is a welcome addition to a literature that remains, in both domestic and foreign policy, top-heavy with works on Roosevelt to the exclusion of other key players and institutions.

Patrick J. Maney
University of South Carolina


Visions of Belonging argues that from the late 1930s through World War II and into the early Cold War era, popular family stories produced in every medium articulated what it meant to be an “ordinary” American. Smith demonstrates how a pluralistic vision of the late 1930s that began to include white ethnics and, to a certain extent, people of color among the “ordinary” expanded to some extent during the war years to reinforce the need for unity to fight the war. After the war, the potential for further inclusion came to a halt, in part because of the increasing chill of the Cold War that re-marginalized workers, radicals, and people of color. The postwar theme of assimilation into the mainstream solidified the color line, as white ethnics increasingly became unmarked ordinary Americans while African Americans remained on the outside.

Smith uses major works in American radio, theater, film and television to trace the complex and subtle transformations in ideas about those who belong, those who do not, and why. She examines several major works of art that appeared between 1940 and 1960 and traces how they transformed over time, from the stage to the screen to television, in an ongoing construction and reflection of typical American families. She uses family stories to capture the meaning of American citizenship, reflecting the centrality of family to the concept of national identity. Each chapter is filled with insights gleaned from such popular works as Marty (1953), A Tree Grows in Brooklyn (1943), I Remember Mama (1944), Strange Fruit (1944), Gentleman’s Agreement (1949), Death of a Salesman (1949), and A Raisin in the Sun (1959). Smith examines how creators and audiences endeavored to come to grips with the powerful push toward assimilation, the primacy of the nuclear family, and a panethnic white identity.
The book makes a powerful and significant contribution to our understanding of the popular arts during the middle decades of the twentieth century in the context of shifting political and historical circumstances. Challenging notions of ongoing progress, the book demonstrates how the 1930s expanded the American mainstream, only to have the boundaries constrict in the wake of World War II and the advent of the Cold War. Smith’s subtle examination of shifting ideas about race in the postwar era reveals dramatic changes in attitudes and perceptions. No other book examines the cultural aspect of changing ideas about race and ethnicity with such insight and depth.

*Visions of Belonging* is a monumental work of cultural history. Through her clear, detailed, and meticulous examination of major works of popular art, Smith has challenged the common wisdom about ethnic and racial assimilation and made a powerful contribution to our understanding of the profound changes that took place in American society from the Depression through the Cold War.

Elaine Tyler May
University of Minnesota

*The Zapruder Film: Reframing JFK’s Assassination.* By David R. Wrone (Lawrence, University Press of Kansas, 2003) 251 pp. $29.95

Did Lee Harvey Oswald act alone to kill President Kennedy while he rode through Dallas on November 22, 1963, as the official Warren Commission report concluded? Or, was the murder actually carried out by a vast conspiracy that might have included the Central Intelligence Agency and organized crime? Did Oswald even fire a shot? Was movie maker Oliver Stone right?

David Wrone addresses these questions in the first book-length examination of the 26 seconds, 486 frames, and six feet of film taken by Abraham Zapruder on the day of the assassination in Dealey Plaza. He concludes that at least two shooters were responsible for Kennedy’s death, that Oswald was likely not one of them, and that there were more than three shots fired. Wrone’s message is clear: The Zapruder film proves that the Warren Commission was wrong.

The most important part of the book is Wrone’s frame-by-frame analysis of the film. The book’s method is not so much scientific as it is meticulous in its detailed evaluation of each frame against the proposition that Oswald alone killed the president. Wrone aids his case by wisely bringing other photographic evidence to bear, notably the stills taken by Phil Willis and Ike Altgens, that help to document what the Zapruder film purportedly shows.

What this approach yields, however, is inconclusive. The exercise, as was true in dealing with the Rodney King videotape, is a vivid reminder that what you see on film is not necessarily what happened. Take
for example, the critical frames in Zapruder’s film and Wrone’s analysis of them (337–338). Wrone insists that these frames show clearly that Kennedy was struck by bullets fired in front of the limousine rather than, as the Warren Commission reported, behind it. Yet the photographic evidence provided in the book and the corroborating evidence that Wrone provides are subject to a different interpretation. In the end, the photographic record, on which Wrone pins so much of his argument, is necessary but not nearly so sufficient as Wrone believes. These frames, Wrone insists, “clearly show the back of President Kennedy’s head undamaged, with his clothing intact and unbloodyied. It proves that the shot that caused the president’s massive head wound came from the front and that there was no shot in the back of the head, thus affirming that two or more assassins conspired to kill him” (181). Wrone asserts that the Warren Commission had “misstated” the fact that the large wound in the front of his head was an exit wound, when it should have concluded that it was an entrance wound (183). Wrone then couples this finding with the movement of Kennedy’s head backward, which suggests a shot from the front, with no evidence in either frame of damage to the back of Kennedy’s head, which would have suggested a shot from the rear. Other scholars and technical experts using considerably more sophisticated techniques than Wrone employs have reached just the opposite conclusion. Such techniques led ABC News on the fortieth anniversary to conclude that the Warren Commission got it right.

The book treats what might be thought the perfect piece of evidence with far too much authority. What the Zapruder footage shows is that a man was killed before our eyes, but Wrone’s efforts to determine the origin of the shots is, in the end, frustrating and ultimately problematical.

Kermit L. Hall
University of Albany

Looking Back at LBJ. Edited by Mitchell B. Lerner (Lawrence, University Press of Kansas, 2005) 303 pp. $35.00

In the preface, Lerner describes this book as “the step-child” of the well-known three-volume series, Robert A. Divine (ed.), The Johnson Years (Lawrence, 1987–1993) (vii). Like Divine, Learner, has edited a collection of essays based on materials at the Lyndon B. Johnson Library in Austin, including the hundreds of hours of President Johnson’s taped phone conversations and White House meetings made available to researchers since the last of Divine’s volumes appeared in 1993. Using these tapes, other records at the Library, and recent scholarship, the authors employ the traditional methods of the historian to study discrete and unrelated aspects of the Johnson presidency. Readers will have to look hard to find approaches that might be described as interdisciplinary.
The focus is on Johnson, and the purpose is to contribute to the ongoing assessment of his presidency.

The ten essays in *Looking Back at LBJ* do not provide materials for broad generalizations about this administration. For one thing, none of them addresses the crucial topics of civil rights or the war on poverty. For another, some of them discuss issues of only marginal importance to this administration, in particular, Indian affairs and women’s issues. Nonetheless, taken together, they add useful detail to the immense literature on Johnson’s policies and character. Depending on the essay, his achievements are flawed, he lacks a moral center, or he is overwhelmed by events beyond his control.

The four essays devoted to foreign policy show the limitations of Johnson’s leadership or his inability to control events. The best essay, Mark Atwood Lawrence’s study of the Panama crisis in the winter of 1964, effectively uses the tapes to reveal a president determined to look tough and motivated in large measure by domestic political calculation. Johnson got what he wanted in this episode, first by forcing the Panamanian president to back down and then by offering cosmetic concessions designed to forestall Panamanian radicals and to maintain in power a man who was “in most respects an authoritarian figure of the type that had long served U.S. interests in Panama” (35).

In the other foreign policies considered in this volume, Johnson fared less well. Jeremy Suri argues that social upheaval at home damaged Johnson’s leadership abroad. Distracted by unrest, the president was unable to pursue an opportunity in 1968 to improve relations with the Peoples Republic of China. Weakened by unrest, he unwisely sought a summit in Moscow that year “for purposes of public image rather than policy substance” (66). Peter Hahn offers a good summary of Johnson’s diplomacy before and after the Six Day War, showing that the decline of American power doomed his efforts to prevent an Arab–Israeli clash and to fashion a lasting peace after the war was over. David L. Anderson asks whether Johnson’s failure to resist the Cold War inertia that led him ever deeper into the quagmire of Vietnam constituted a failure of moral courage. His answer is “yes.”

Would an interdisciplinary approach have benefited the essays in this book? The authors are historians asking the kinds of questions that historians often ask. Each essay rises or falls on how good the answers are. With the exception of David Shreve’s essay on economic policy, the historians in this volume did not resort to other disciplines because they would not have helped. This is not to deny the enormous contributions that other disciplines have made to historians who have asked other kinds of questions. It is merely to say that not every worthy academic enterprise must be interdisciplinary.

Allen J. Matusow
Rice University
The publication of Benedict Anderson’s *Imagined Communities* (New York, 1983) inspired a generation of scholars whose explorations of the poetics and politics of “imagining” greatly enriched our understanding of that enduring historical artifact, the nation. Latin Americanists have taken up Anderson’s evocative, if notoriously slippery, claims with both a critical eye and an enthusiastic embrace, as they have scrutinized and refined many of his arguments in order to analyze the discourses and practices of nationhood more effectively. It has become commonplace to describe national formations as processes; to look to museums, novels, maps, and censuses as evidence; and to disagree on the extent of inclusiveness or contradiction in nationalist discourses.

Hence, little is surprising about Benavides’ attempt to use the archeological site of Cochasquí and the various narratives that explicate it to explore twentieth-century Ecuadorian nationalist discourses. He argues that the site is at the center of a number of historical narratives that inform popular understandings of Ecuadorian nationhood. Several entities, including the state and indigenous groups, offer their own renditions to legitimize their power or their authenticity (and thus their claim to power). Benavides draws principally from textbooks, tour-guide accounts, and texts used by CONAIE (*Confederación Nacional de Indígenas del Ecuador*), Ecuador’s most powerful indigenous group. Written in the wake of successful large-scale indigenous mobilizations of 1997 and 2000, this book argues that much is at stake in the relationships among history, archaeology, and politics.

Unfortunately, serious drawbacks greatly limit its utility for scholars. Benavides’ use of the concept of hegemony lacks dynamism. Despite his invocation of Roseberry’s injunctions to understand hegemony as a contested process, the author treats hegemony as a static object, speculating at one point about “the Indian movement’s encounter with hegemony” (160). With such statements as “official discourse operates as if it has a life of its own” (86), his other key concept, discourse, comes across as disembodied and free-floating rather than constituent of, and constituted by, specific actors and entities.

Most troubling is the author’s failure to apply his critical insights to his own work. By omitting any discussion of his methodology or primary sources, he hides the process by which his own narrative was constructed. Given his interest in the transparency (or lack thereof) of authoritative discourses, the absence of precise references and of a list of interviews is puzzling. Moreover, his insistence on rendering Indigenous-European interaction as 500 years of oppression and discrimination

greatly oversimplifies a complex history. By taking this as his point of
departure, he naturalizes a narrative that has been contested by many
parties, including indigenous movements themselves. In its favor, the
book admirably refuses to romanticize Ecuadorian indigenous move-
ments and contributes new information to a relatively understudied
field. Yet the methodological and theoretical foundations lead to con-
cussions that are less than illuminating.

Alejandra Bronfman
University of British Columbia

Contentious Republicans: Popular Politics, Race, and Class in Nineteenth-
Century Colombia. By James E. Sanders (Durham, Duke University
Press, 2004) 258 pp. $79.95 cloth $22.95 paper

Since the late nineteenth century, Colombian history—or outsiders’
perception of it—has been dominated by violent protest, assassination,
regional rebellion, and full-blown civil war, driven by coffee, cocaine,
and charisma. Sanders focuses on an earlier Colombia, the fledgling Re-
public of New Granada that emerged between Simon Bolívar’s demise
(1830) and coffee’s rise (c. 1880). The author argues that Colombia’s
early experiments with republicanism compare favorably with those of
the United States, Italy, and other “Atlantic” nations in transition during
the same period. In the Colombian case, elite stifling of popular partici-
pation near the turn of the last century obscured the novel first phase.
Indeed, it has been all but forgotten.

Sanders’ specialty is southwestern Colombia, the once-vast Prov-
ince of Cauca. This region produced many of Colombia’s most promi-
nent nineteenth-century political figures. It was also arguably the cruci-
ble in which Colombia’s (still) dominant political parties, the Liberals
and Conservatives, forged their respective notions of participatory re-
publicanism. The result, Sanders argues, was not quite democracy, but
neither was it the shame politics and bald oligarchic rule so long as-
sumed.

The region’s “subalterns,” according to Sanders, were divided into
three major groups in the mid-nineteenth century—liberal Afro-Co-
lombians, conservative Amerindians, and conservative white/mestizo
homesteaders. After establishing this typology, cleverly linked to the re-
publican refrain of “liberty, fraternity, and equality,” Sanders chronicles
the shifting allegiances of each group. The Liberal Party’s early ascen-
dancy is attributed to its elite leadership’s push to include Afro-Colom-
bians. Liberal-driven abolition in 1851 swelled party ranks, much to the
dismay of Conservatives, backed by most land-holding and goldmine-
owning elites. Despite abuse by land speculators, northern homestead-
ers’ aspirations were mostly in line with Conservative notions of reli-
gion, family, and property. Native groups, meanwhile, feared the Lib-
erals’ anticorporatist desire to break up communal land reserves.

The scene shifted, however, as popular groups seized upon their
newfound power to press for change. Racism, never absent, flared. Pre-
dictably, blacks were blamed for all manner of ills, including contraband
alcohol and tobacco sales (an issue dating to colonial times). By 1870,
Afro-Colombian participation in electoral politics, as well as in riots and
chronic civil wars, came to be regarded as disruptive of the social order
by not only Conservatives but also Liberals.

Sanders offers a fine-grained analysis of this painful process of elite/
plebeian embrace and separation, based almost entirely on primary re-
search in regional and national archives. This is an admirable feat. As the
title suggests, the author was inspired by the work of Tilly on European
politics, but it is subaltern studies that most inflect his conclusions. In-
deed, an alternative title might have been “Contentious Subalterns.”

*Contentious Republicans* is a good book on an important subject.
Sanders’ links to broader Atlantic trends in the conclusion should be re-
quired reading for all nineteenth-century historians and political analysts
interested in modern democracy. Colombia’s experiment deserves to be
remembered.

Kris Lane
College of William and Mary

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*To Die in Cuba: Suicide and Society.* By Louis A. Pérez, Jr. (Chapel Hill,
University of North Carolina Press, 2005) 504 pp. $39.95

We all live on our way to the cemetery, as famous contemporary Cuban
songwriter Silvio Rodríguez states, but many Cubans have shortened
this journey by accepting suicide as a reasonable response to life. The
rates of suicide in the island have been among the highest in Latin Amer-
ica and the world since the nineteenth century, a trend that several de-
cades of socialism and revolution have not changed. Why?

This is the question that Pérez asks in his provocative and path-
breaking book. Using a vast array of primary sources, including large
numbers of little-known literary products, cartoons, and lyrics, as well as
more traditional sources such as government statistics, Pérez seeks to un-
derstand the cultural and material circumstances that have made suicide a
plausible alternative for many Cubans. He argues that suicide was one of
many possible behaviors that “can reasonably be included among the at-
tributes of being Cuban” (11). His purpose is to identify and analyze the
cultural frame of reference that made suicide a logical and acceptable al-
ternative and the contexts in which aspirations that could not be realized
formed. Thus derives his need “to examine the history of the future”

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1 See, for example, Charles Tilly, *Contention and Democracy in Europe, 1650–2000* (New
York, 2003).
In a cultural formation such as Cuba’s, characterized by frequent transitions and marked by the uncertainties of the sugar export economy, the future was the last, and often the only, hope for a better life. When even this possibility faltered, suicide became one possible response.

But suicide was not always an expression of despair and hopelessness. Death and suicide figure prominently in the Cuban imagination. Since the nineteenth century, the very possibility of patria, of a sovereign and independent republic, was associated with armed struggle—what José Martí called the necessary war. The anticolonial wars were little more than a call to immolation and sacrifice, in which poorly prepared nationalist forces did battle against a powerful and well-supplied European army. Despite their destructive nature, these wars were celebrated as journeys to nationhood and life. “To die for the patria is to live,” proclaims “La Bayamesa” (1868), Cuba’s national anthem.

In this context, death and suicide became an expression of agency and self-assertion, not a deed of defeat and desolation. In an association that was hardly creative, Nationalist intellectuals equated the colonial condition with slavery and proclaimed that death was preferable to life in servitude. Suicides had been part of the behavioral repertoire of slaves for decades, as Pérez explains in an excellent chapter on suicide in the plantations. In some cases, it was literally used as a gateway to life; some Africans believing that they would return home after death to join their friends and relatives. In other cases, the possibility of suicide was used to negotiate better living conditions. In all cases, suicides challenged the power of the master and were a “means of agency of last resort” (47).

Several factors contributed to making suicide an unremarkable element of everyday life during the republican years. The daily press published sensationalist accounts about successful and unsuccessful attempts at suicide. Cartoons that portrayed suicides humorously and lightly “appeared with remarkable frequency in all the principal newspapers and magazines” (264). Some of these cartoons are reproduced in the book, to the publisher’s credit. Public figures who chose self-inflicted deaths added to this sense of normalcy. So did the prominence of the theme in literature. Even some advertisements relied on suicides to make a case for the desirability of their products.

Given the sense of relative commonality that was attached to suicide, it is not altogether surprising that the revolutionary triumph of 1959 had a limited impact on self-inflicted deaths. “Men and women in all age categories appear to have continued to kill themselves at a rate more or less comparable to pre-revolutionary trends,” notes Pérez (352). Although suicides were decried as a behavior unworthy of revolutionaries, the image of death as a reasonable alternative was reinforced in public discourse by the most popular slogans in revolutionary parlance: “patria or death” and the most recent “socialism or death.” What is perhaps surprising is that the crisis of the 1990s, which had a devastating impact on living standards and offered little other than uncertainty when it
came to the future, did not result in an increase of suicides. Is this an indication of deeper social and cultural changes?

It is impossible to answer such a question at this point. Doing research on postrevolutionary Cuba is a difficult enterprise, and the author is to be commended for extending his study beyond the usual divide of 1959. Needless to say, much additional work will have to be done on this period in the future. *To Die in Cuba* offers a first and valuable comprehensive survey of this understudied aspect of life in Cuba and in Latin America more generally. To those who may think this book depressing, the book is much more about life and the living than about death and the dead. It is a fascinating journey into culture and everyday life in Cuba that readers from various disciplines and geographical areas will find illuminating.

Alejandro de la Fuente
University of Pittsburgh


This volume addresses an important question: Why, given the political instability in Mexico during the years in question, and particularly during the revolutionary decade 1910–1920, did certain key sectors of the economy continue to grow and attract foreign investment? The authors’ focus is on the issue of property rights, which would have seemed to be imperiled by the political climate. Unfortunately, as they move across sectors, their answers are not uniformly successful. At times, their analyses seem idiosyncratic. Consider their discussions of the petroleum industry and of mining.

In the successful chapter on mining, the authors point out that in the prerevolutionary period, mining interests had been able to persuade the government of Porfirio Díaz to amend the legal codes in ways that favored them. They correct the impression that these codes provided fee-simple subsoil rights; rather, rights to a claim could be re-taken by the government and reassigned to another miner without compensation if it were not worked. However, with the exception of petroleum, water, and materials suitable for building, the Mexican government became the residual claimant to the subsoil, which, in effect, gave miners the rights to access minerals through expropriation of surface owners. Furthermore, they not only gained rights to prospect and mine but also to establish rights of way through adjoining properties. Despite attempts by the Díaz government to require American companies to incorporate as Mexican companies, and the later implementation of such policies, the skill and capital requirements for successful exploitation of these re-

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sources simply did not exist in Mexico. Continued exploration and operation required the involvement of foreign interests, and investment persisted when prices of metals were favorable because the companies believed that they could mitigate any government efforts to limit their property rights.

The authors’ discussion of petroleum is less successful. They argue that revolutionary and postrevolutionary attempts to secure additional revenues and controls over foreign oil companies were insignificant in the decline of investments during the 1920s and that the real (to them, the only) factor leading to this drop was geological (192). Although geological factors were not unimportant, oil production and investment stayed high through the revolution and during the early 1920s so long as the Mexican government was perceived as too economically weak to do without oil revenue, too politically unstable to withstand outside pressures, and too lacking in capital and know-how to produce the oil themselves. Only when the Mexican government began to make clear that new arrangements would not be beneficial to the companies, and other opportunities opened elsewhere, did investment decline. The authors’ own tables show that the fixed assets of the major foreign oil companies in Mexico began to decline c. 1925 and 1926, just as the Mexican government’s new resolve, along with its greater stability and control, made it less likely that the companies could expect its extraordinarily favorable circumstances to continue.

In fact, during the revolution, the enormous value of the Mexican oil resources, discovered at a time when petroleum demand was high, and the convenient geographical location of Mexican oil fields on the Gulf Coast made export easy regardless of the violence throughout the country. At the same time, the various Mexican factions had little ability to enforce any limits on the companies. Political stability, when it came, led to significantly less maneuvering room for the companies, which had been looking for resources in a better political climate for some time. But, as the authors show, exploration continued for a while longer. Oilmen still might have hoped for large strikes to change the stakes and alter the political situation.

Although the authors’ data do not show whether the drilling was done by large or small companies, large-company investment is known to have fallen, as noted above. Mexico may have been attractive, despite the political risk, for smaller investors who had long hoped to profit from the bonanza; they might have remained in higher proportions than their stronger competitors. The authors’ data showing that the fixed investments of the major companies were declining would support such a possibility. Perhaps in an unfortunate desire for parsimony in analysis, the authors have taken a well-known and carefully documented case and provided a too-easy explanation.

It is unfortunate as well that the authors have not chosen to test their conclusions against comparative cases. They claim that, though desirable, such studies are difficult to undertake because of the lack of his-
torical literature. Indeed, it would have been difficult to find a well-studied case that matches many of the variables involved in all of the economic areas that they target. Nevertheless, the analysis could have been enhanced by available comparative material. Russia might have provided a particularly interesting comparison, undergoing revolution at the same time and apparently containing enormous oil resources. Venezuela, the area to which much of the petroleum investment seems to have migrated, was in a similar situation, politically and otherwise. These two cases might have added a global dimension to the analysis.

The authors’ basic argument is that in a situation of political instability, investments can continue to take place given appropriate property rights and credible threats by investors. This argument is entirely reasonable for Mexico, although the authors do not always make it in the most nuanced fashion. Many other factors can be significant over time in changing the nature of these relationships and can make a substantial difference in the willingness of companies to invest. In the case of petroleum, the authors ignore a complicated and interesting set of factors for the sake of an easy answer.

Idiosyncracies aside, the volume stands as an important look at what seems to be a paradox—that economic growth can persist in a period of great political instability. Perhaps the answer has to do with a number of factors, including property rights but also the value of the resources involved and the ease of exploiting them despite the instability. As a complement to other sources, this book is extremely valuable, but if read in isolation, it may provide a misleading picture.

Linda Hall
University of New Mexico

*White Creole Culture, Politics and Identity during the Age of Abolition.* By David Lambert (New York, Cambridge University Press, 2005) 245 pp. $75.00

Lambert works at the intersection of historical geography and cultural studies in order to examine social and cultural changes in Barbados between 1780 and the 1830s. This time span being known as the “age of abolition” and the “age of revolution,” he chose an important period of transition. But is the tiny colony of Barbados an important place in which to examine the culture, identity, and politics of “whiteness”? The answer, as Lambert shows superbly, is emphatically positive. Barbados, small as it is, was one of the first sugar colonies in the Caribbean in which slavery became pervasive, and the Caribbean was for two centuries the “principal fulcrum” of the emerging Atlantic World and world-system (23). Barbados, far from being peripheral, was at the center of modern colonialism and slavery. The discourses and debates concerning
slavery and whiteness that occurred there in the period that Lambert considers are crucial to our understanding of the modern world.

Lambert correctly insists that whiteness is not a racial identity so much as a political one that involves claims and assumptions of superiority that bolster a structure of power relations. Understanding the “problem of slavery,” or the process of “enslaving,” as it was attacked and defended by free and enslaved people of various complexions and locations, is therefore crucial for understanding the place of whiteness in our world. The struggle over racial representation and identity is about power and privilege, and part of the privileging of whiteness “is its apparent invisibility to critical inquiry” (16). Making its origins visible is an important contribution not only to Caribbean studies but also to intellectual history.

Lambert focuses his inquiry on selected key people and episodes, including Joshua Steele, an English planter who criticized slavery after he arrived in Barbados in 1780; John Poyer, a middle-class Barbadian and author of the History of Barbados (1808); the slave rebellion of 1816; the anti-Methodist agitation in the 1820s; and the struggle in the 1830s to shape a “free” society after legal emancipation. In each episode and period, Lambert shows how “political and cultural practice are . . . bound up with the fashioning of a new form of white colonial identity on the threshold of emancipation that was more racially supremacist in outlook” (9). He demonstrates that in many contexts, the “articulation of white colonial identities was . . . contested” as part of the broader process of cultural creolization, a process that involved Blacks and Whites as well as their offspring (211).

Lambert’s use of documents and his interpretation of events are imaginative, and his arguments are coherent and clearly expressed. The only problem with this excellent book concerns Lambert’s conceptualization of the “geography” of slavery in Chapter 1. Lambert, following Davis and others, conceives of the “slave world” and the “free world” as a dichotomy, which not only diminishes the roles of free people in the former and enslaved people in the latter but also implies that they are separate worlds, when, in fact, they are interconnected parts of the same world.1 The “spatialisation” (his word) of slavery and colonialism is misleading because even when some parts are spatially separated they are always socially, economically, politically, and culturally interconnected. The unity and persistence of social relationships that existed in far-reaching networks are crucial to understanding the nature and consequences of slavery and colonialism, and anti-slavery and anti-colonialism. To some extent, Lambert’s view of slavery as “a geographical problem,” though reflecting the view of many protagonists at the time, may hinder our understanding (11). Despite this issue of conceptualization, however, Lambert’s data and interpretations frequently reinforce the in-

1 See, for example, David Brion Davis, Slavery and Human Progress (New York, 1984).
terrelatedness of these historical networks, revealing some of the key social origins of our modern racialized culture. His book is important and highly recommended.

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A mantra in European history is that the middle class is always rising. Middle Eastern historians now have their own mantra: The artisanal class is never disappearing. Until recently, African, Asian, and Middle Eastern historians have espoused the view that the integration of the third world into the world economy in the nineteenth and twentieth centuries spelled the end of independent third-world craftsmen and women and small-scale industrialists. Yet, anyone who has ever spent any time in the developing world and observed the vitality of small-scale crafts persons knows how inaccurate this view is. What social scientists now call the informal sector is alive and well. If your car needs repairs or you require home maintenance, mechanics, often possessing uncanny skills, instantaneously appear. And anyone who has dug into the social and economic records of third-world countries from the nineteenth and early twentieth centuries surely is aware of the continuing size and dynamism of this earlier informal sector, populated at this time by sweaty and hard-working tailors, seamstresses, weavers, and hanson drivers. It would seem that the third-world informal sector has been around for a long time. It has a deep genealogy.

What makes Chalcraft’s new book on Egypt’s late nineteenth-century artisan class so exciting is not merely its treatment of Egyptian craftsmen and guilds in late nineteenth-century Egypt, although its excellence in this area is palpable. Far more important is its skillful handling of Egyptian archival materials. _The Striking Cabbies of Cairo_ is one of the few books in English to make full use of the Egyptian National Archives, the _Dar al-Watha’iq al-Qawmiyya_, and thus to indicate that these records are now being made freely available to scholars.

The book has many virtues, only a few of which can be noted in a short review. Its primary theme, that craft groups and guilds did not shrink into nothingness when European manufactures arrived in Egyptian markets, like violets at the first blast of cold weather, has already made its way into Middle Eastern historiography, though mainly through the work of Ottoman historians. Chalcraft provides his readers with a nuanced view of the guilds and the guildsmen as they tried to keep the modernizing and increasingly powerful Egyptian state at bay.
and to move their economic activities out of areas in which European imports threatened to dominate the local market into areas where they had comparative advantages. For politically minded historians, who still tend to dominate this field, the later chapters on craft persons and the ‘Urabi revolt and the nationalist movement are treasure troves of facts and generalizations. The author argues that the increasingly intrusive state had torn aside the protections that guild shaykhs provided to guild members and left the artisans face to face with government bureaucrats. Although this new arrangement exposed workers to heavy taxation, it also led the crafts persons to fashion new relations with the state. Working-class Egyptians, like their middle-class counterparts, began to establish citizenship ties to the state during the reign of Khedive Isma’il (1863–1879) and the ‘Urabi revolt (1881–1882), only to see these promising constitutional innovations rudely interrupted by an autocratic colonial power.

Equally stimulating, though, in this case, much less well documented, is the connection of the urban artisans to the nationalist movement of the Egyptian National Party, led by Mustapha Kamil and Muhammad Farid. Historians have seen Egyptian anticolonial nationalism of the late nineteenth and early twentieth centuries almost entirely through the eyes of intellectuals and the middle class. There was, however, a working-class component, which Muhammad Farid is known to have cultivated. Chalcraft offers copious data on rank-and-file nationalist protest—by cab drivers, carters, and the like, who accomplished a general strike in 1907, briefly crippling the transportation system of Cairo and forcing the government to make significant concessions. Although he does not prove his case for the “making of an alliance” between middle-class nationalists and urban workers (and he is honest about having only circumstantial evidence), his evidence and arguments are persuasive. This effective book will become essential reading for historians of modern Egypt.

Robert L. Tignor
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A History of Modern Palestine: One Land, Two Peoples. By Ilan Pappé (New York, Cambridge University Press, 2004) 352 pp. $60.00 cloth $22.00 paper

This is a book-length interpretive essay that addresses the modern history of Palestine as a region of the Ottoman Empire, as a British Mandate, as Israel with leftover Arab fragments in the West Bank and Gaza, and as Israel with the occupied territories. The history of this small, beguiling and tragic country is much-covered terrain. This book’s distinctive contributions flow from the author’s assumptions (1) The country
(Palestine) can be treated as a unit of history despite the political vicissitudes of the last 150 years; therefore histories of Israel (alone) or the Palestinians (alone) after 1948 are incomplete. (2) The history of Palestine is not coterminous with the history of its nationalisms and national movements (Arab or Jewish). (3) The history of Palestine needs to be addressed from the perspective of its non-elites and non-politicians to understand its populations’ worldviews, cultures, and formative experiences. (4) The history of the country includes (after 1948) refugees living outside of it, who nevertheless continue to make Palestine (or its memory) a focus of their identities. Pappé writes with unabashed and undisguised sympathy for those who have been marginalized or excluded by nationalist-centered modernity. Although critical of both the Israeli Jewish and Palestinian Arab national narratives, Pappé more often than not interprets and presents events in terms that will resonate more easily with sympathizers of mainstream Palestinian rather than mainstream Israeli perspectives.

Because the author works consciously outside of the modernization-inspired nationalist framework—citing his intellectual debt to subaltern studies instead—he raises issues about the standard narratives and poses problems that are rarely addressed in the standard general literature: the differential effects of Ottoman state-building and Eurocentric modernity on Arab peasants, merchants, and landowners; the persistence of cooperation and shared lives across ethnic and religious boundaries in Palestine; the relationship of Israeli and Palestinian nationalist elites to subaltern groups within their respective national communities; and suppressed or alternative narratives to the hegemonic nationalist ones.

The last point illustrates the author’s activist agenda in writing this book. It arose, he relates, from his experiences of teaching history at Haifa University, where he struggled to identify themes that his Arab and Jewish students could absorb and consider as they faced the challenge of understanding their shared country and the prospects for building a more humane future out of the tensions and wreckage of the present. The point is less to suppress or deny national(-ist) narratives than to indicate that other frameworks for considering the past, present, and future also exist.

Readers may wish that the book had more footnotes to permit certain contentions or interpretations to be checked more closely, or to provide clarifications. The book assumes a basic familiarity with standard historical narratives, but those familiar with them—regardless of their political sympathies—will find much to ponder in this work.

James A. Reilly
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Fratricide in the Holy Land: A Psychoanalytic View of the Arab-Israeli Conflict. By Avner Falk (Madison, University of Wisconsin Press, 2004) 300 pp. $35.00

The visitor to Sigmund Freud’s home in London will be struck by the apparently historical manner in which he laid out his books. At the beginning stand archaeology and ancient history (with John Locke and Immanuel Kant mixed with the archaeology, since Freud believed that the forms of mind arose in prehistory). Next, after an enormous gap of post-ancient history, modern authors such as Honoré de Balzac appear. Finally comes the present: the natural sciences, psychology, and psychoanalysis. Freud’s library illustrates, in strikingly graphic form, the problem with the psychoanalytic approach to history. It concentrates heavily on a few, critical upheavals of early history (the murder of the primal father, the murder of Moses, the invention of religion), a compelling interest in the present, and nothing in between: no medieval civilization, no rise of Islam, no Renaissance, no Reformation, no French Revolution, no industrialization, no China or India—none of the usual signposts by which historians orient themselves as they grope their way through the past.

As Falk’s well-meaning work argues, profound irrational elements are certainly at play in the Arab–Israeli conflict. On the one hand, the Arab reluctance, and even refusal, since 1948 to recognize Israel as a Middle Eastern nation and the continuous dwelling on the *naqba*, the catastrophe of Israel’s founding, flies in the face of self-interest, except perhaps for a very few. On the other hand, Israel’s compulsive use of overwhelming force to bully, intimidate, and cripple a neighbor, the undermining of the very United Nations that created the Israeli state in the first place, and racist policies instituted by a people who themselves have been the victims of racism, certainly calls out for an approach that highlights irrationality and unconscious motivation.

Insofar as there is a unifying theme to Falk’s disorganized work, it is the psychical insecurity that underlies Arabs and Israelis. To illuminate this insecurity, Falk discusses the role of trauma in constituting Israeli, Arab, and Palestinian identity; the wounded and paranoid character structure of the two great leaders in this opposition (Ariel Sharon and Yasser Arafat at the time that Falk wrote); the idea of nationalism as a “group self,” which, like an individual self can have aporia, defects, vulnerabilities, trigger mechanisms, and the like; the significance of land, place, and home as a kind of “mother,” infused with childhood images and memories; sibling rivalry (Jacob vs. Ishmael); the death wish, and the like. Since Falk has chosen the already well-refuted path of Freudian reductionism, he has no way to address the obvious question, Is psychical insecurity the cause of the conflict, or is it the result?

Ultimately, Falk has not availed himself of the enormous resources of the historian’s craft: putting things in some sort of chronological order; contextualization; establishing a coherent narrative; distinguishing
such levels of explication as deep historical structures, persistent myths, ideologies and assumptions, contingent events, and personalities. Above all, historians try to relate their work to the existing historical matrix. The case of the Arab–Israeli conflict would implicate the historical Christian/Islamic conflict, the rise of modern nationalism, imperialism and colonialism, the Cold War and its end, globalization, contemporary economic and political conflicts, the role of the United States et al. When Falk touches on any of these questions, he does so to illustrate psychoanalytic ideas; he does not use psychoanalysis to illuminate history. For this reason, the book cannot be recommended.

Eli Zaretsky
New School for Social Research


Lu devotes this study to Chinese beggars to move away from “the powerful, the well-known, or the privileged” and bring “the obscure and disadvantaged to record” (xi). This is not an easy task. Due to the murky conditions of beggars and vague written records about them, Lu has to abandon the regional approach that has become familiar to many China scholars since Skinner published his landmark studies on China’s macroregions in the late 1970s. Instead, Lu can only follow the footsteps of those mendicant groups in written records and enter whatever historical periods these records indicate. He encounters beggars not only in big urban centers like Shanghai, Beijing, and Tianjin but also in cities and towns in peripheral Inner Mongolia, Manchurian frontier, Tibetan Plateau, and northwest and southwest regions. Not enough archival materials are available to warrant a book-length study based on a micro- or anthropological investigation of particular groups of floating beggars living in one region during a specific historical period.

To resolve this dilemma, Lu skillfully turns to the “cultural homogeneity” of Chinese beggars across the country, focusing his study on their “shared characteristics” (2). This cultural approach enables him to exploit a rich variety of materials from different historical periods. In this regard, Lu shows the influence of Chinese scholars who are particularly good at writing “a total history,” such as Cen Dali and Qu Yanbin, each of whom wrote A History of Chinese Beggars. Indeed, while treating and reconstructing Chinese beggars as ahistorical social groups, or as an “ideal type” of social and cultural existence to be handled by the public


and the state in almost every recorded historical period (except in the Mao years when beggars’ records were suppressed), Lu wisely utilizes such source materials as the political-economics writings of Guan Zong, Sima Qian’s Shiji, and other official histories; ancient legends, Ming fictions, the biji (anecdotes) of Qing scholars; and modern popular stories revealing the “secrecies of jianghu (the unofficial and unorthodox world); and accounts of foreign travelers and observers, local gazetteers, social surveys, and contemporary wenshi ziliao (personally experienced cultural and historical accounts). Lu thus creates a cultural encyclopedia about Chinese beggars.

Lu engages in extensive discussions on the relationship between beggars and disasters, famines, and poverty (Chapter 1), public sympathy and antipathy toward beggars (Chapter 2), the state’s management and control of beggars and other vagabonds (Chapter 4), operations and internal rules of beggars’ own “guilds” (chapters 4–5), and wisdom and various strategies of mendicancy (chapters 6–7). Another important contribution that he makes (Chapter 3 and Chapter 8) is to show Western readers the strong cultural and romantic connections established in Chinese history between the “subaltern” and mainstream worlds. In terms of social mobility, mendicancy in China was (and is) not necessarily a permanent status mark for any particular individual. In fact, both Chinese popular and elite cultures place great value on stories about those who overcame their earlier misfortunes to become heroic figures. For example, Zhu Yuanzhang, a beggar who later founded the Ming dynasty, was revered by many mendicant guilds as their “forefather.” Lu also reminds his readers that both Confucius and Chairman Mao had endured embarrassed moments as wanderers.

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**Fragmented Memories: Struggling to Be Tai-Ahom in India.** By Yasmin Saikia (Durham, Duke University Press, 2004) 345 pp. $84.95 cloth, $23.95 paper

Saikia has selected a fascinating problem—the highly politicized construction of the ethnic identity Tai-Ahom in India’s far northeast. From 1981 to 1997, various groups in upper Assam created this ethnicity to express their resentment against dominant groups in both lower Assam and distant India. Saikia uses complex historical and literary sources and methodologies to represent the feelings and aspirations of these self-proclaimed Tai-Ahom, to reveal what the indigenous histories on which they base their claims literally say, and to show how the British colonial state and independent Indian nation alienated them. Throughout, Saikia discusses her own location as a Muslim woman from an elite family long prominent in the region, who is also a American-based academic expected by many Tai-Ahom to publicize their cause internationally.
Saikia thoughtfully considers “the processes and agents that organize the production of memory-history” as used by the Tai-Ahom (15). To legitimate what were to these peoples real memories of their once glorious origins, the Tai-Ahom accepted British colonial historiography about a people who had migrated from northern Thailand and established their rule over Assam in the thirteenth century. Yet British ethnographers also identified these people as savages outside of history, thus making them “dead” (254). Similarly, the Indian nationalist narrative—which Saikia argues centered on north Indian high-caste Hindus—ignored the Tai-Ahom, thereby failing to integrate them into the new Indian nation. To recover their self-esteem and garner power, their cultural and political movement articulated a rich Tai-Ahom history, including creating symbolic clothing, international conferences with similarly inclined ethnic activists from Thailand, and explicitly anti-Hindu rituals like beef eating and rice-beer drinking.

By actually studying the surviving indigenous local histories, buranjis, Saikia discovers the Ahom were an “honored clique” of king’s men (officials, officers, and courtiers) from diverse origins (130). This finding explains why so many peoples could identify themselves with the fluid and changeable Ahom identity. Thus, although “Tai-Ahom history . . . has very little to do with factuality but rather is a charged emotional discourse,” Saikia argues that these people believed it “valid” (226).

Many ethnic separatist movements vie for recognition in far northeast India; some have obtained their own new provinces there. Saikia traces how the people calling themselves Tai-Ahom were marginalized economically, culturally, and politically by the British colonial administration, by the Indian government, and even by the Assamese ethnic movement based in lower Assam. Yet, she concludes that the Tai-Ahom movement “became a failed political rhetoric” after Assam’s Chief Minister Hiteshwar Saikia, its powerful patron, died in 1996 (250).

Saikia’s book contrasts with the other major recent book on this region, Sanjib Baruah’s India Against Itself: Assam and the Politics of Nationality (Philadelphia, 1999). Baruah concentrates on other ethnic movements, only briefly touching on the Tai-Ahom movement in an endnote (217, n. 1). Baruah’s political-science orientation leads him to conclude that a federated system giving more autonomy to Assam would alleviate the region’s “sub-national” movements.

Saikia, stressing cultural constructions, argues instead that formal recognition by the Indian government of the erased histories of these peoples would alleviate their alienation. Her impressive array of methodologies and diversity of sources in several languages makes Saikia’s Fragmented Memories an absorbing evocation and analysis of the Tai-Ahom movement.

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