When Science and Christianity Meet. Edited by David C. Lindberg and Ronald L. Numbers (Chicago, University of Chicago Press, 2003) 357 pp. $29.00

The history of the relationship between science and religion has been the focus of a growing number of books and collections in the almost twenty years that have elapsed since the publication of the present editors’ earlier volume, God and Nature.1 Like its predecessor, the book under review starts by distancing itself from the warfare metaphor used to characterize the relationship between science and religion in the nineteenth century and the first seven decades of the twentieth. More recent scholarship has tended to focus on the interaction—often positive—between the two areas rather than their conflict.

The well-written essays in this book cover material from the Middle Ages through the post-Darwinian debates, highlighting science in the medieval Church, the trial of Galileo, the mechanical philosophy of the seventeenth century, the history of the earth and the book of Genesis, various aspects of the debates about evolution, the Scopes trial, and secularization. Most of the essays are clear, and the excellent, annotated bibliography mentions many important readings.

The first five articles deal with the medieval, Renaissance, and early modern period. The remaining seven chapters deal with the nineteenth and twentieth centuries, focusing on the English-speaking world. In these respects, the present volume differs from its predecessor, which devoted a larger proportion of articles to the early periods and considered a wider range of linguistic venues. This difference mirrors changes in the history of science, a field in which the center of gravity has moved forward in time from the period of the scientific revolution to the nineteenth and twentieth centuries.

Although the essays in this book largely avoid the clichés of the warfare metaphor, they tend to treat science and religion as separate entities with a history of encounters. There is little consideration of the ways in which each area has penetrated the other and informed its concepts or ways of thinking. The volume would have been enriched by discussions of how the historical approaches of Charles Lyell’s geology and Charles Darwin’s theory of evolution reflect the influence of biblical narrative rather than the Greek emphasis on harmony and form or how nineteenth-century biblical scholarship reflects the application of scientific and empirical methods to all areas of intellectual life.

Nevertheless, Lindberg and Numbers have produced a useful collection, which does not replace their earlier volume but makes much of its content accessible to a wider, less specialized audience.

Margaret J. Osler
University of Calgary

Misa argues that there are distinct eras in the history of technology, emphasizing that technologies come from within particular societies rather than impacting them from the outside. He proceeds chronologically from an era of the courts (1450 to 1600) to one of commerce (1588 to 1740), and then to periods characterized by industry (1740 to 1851), empire (1840 to 1914), science and systems (1870 to 1930), modernism (1900 to 1950), military culture (1936 to 1990), and finally, global culture (1970 to 2001). These eras grow shorter and begin to overlap as the account moves into the twentieth century. The overlaps tend to undermine the general thesis of technological eras, but they bracket increasingly complex and satisfying accounts of the role of technology in society. Especially illuminating is Misa’s view of particular episodes, such as the creation of aniline dyes in the late nineteenth century or the development of nuclear power in the twentieth.

The earlier chapters of the book are weakest precisely because Misa attempts to apply his formula of technological eras too rigorously on the basis of too few sources, ignoring counterexamples and complicating factors. The so-called era of the court could just as well be called the era of the growth of commercial capitalism, and the subsequent era of commerce was profoundly influenced by courtly culture. Indeed, it is the complicated mix of commercial and courtly interests that contributes to the fascinating complexity of these earlier centuries.

As he moves closer to his own areas of expertise, Misa embraces a more varied group of primary and secondary sources, and the cultural complexity and nonlinearity of technological change come to the fore. For example, “Geographies of Industry” provides a masterful account of the different ways that industrialization developed in three English locations—London, Mansfield, and Sheffield. Misa characterizes London as a multidimensional urban network of numerous trades and industries, detailing in particular the production of beer. In contrast to London, Mansfield became the center of a single industry, cotton textile manufacture, featuring large factories. Sheffield, with its own organizational structures, produced high-quality steel products in an industry that was divided into numerous small-scale operations without factories. Misa’s comparative approach to industrialization demonstrates his thesis of the embeddedness of technological choices and developments within specific social and cultural matrices.

In the following chapter, Misa explores the ways in which technologies and imperialism reinforced and influenced each other. His recounting of how the telegraph and railroad contributed to the British domination of India is particularly compelling, showing his broad thesis to its best advantage. He suggests that imperialism did not simply continue the eras of commerce and industry but, to some extent, displaced industry by creating a captive overseas market.
Misa develops his points by choosing particular, usually already well-studied, episodes and creating absorbing accounts complicated by contextual analysis. He shows, for example, that technologies in the era of “science and systems” focused on incremental improvements and stabilization rather than on new inventions, even the electrical technologies that Thomas Edison furthered. To Misa, marketing, financing, and consumption had as much to do with the development of electric technologies as did particular inventions. His case studies, such as that of Italian futurism or the localizations of the global McDonalds, provide good starting points for thought and discussion.

Pamela O. Long
Washington, D.C.


The Neapolitan philosopher Giambattista Vico, who died in poverty and obscurity in 1744, could be the patron saint of Growing Explanations. Vico’s slogan, *verum et factum convertuntur*, “the true and the made are convertible,” announced that one could know what one could make and, conversely, that what one could make, one knew. Vico attached to this principle the consequence that history was the only true science since history, alone among subject matters, was entirely made by human beings. Beginning soon after Vico’s death, generations of seekers after the relations among truth, artifice, history, and science have adopted Vico’s dictum and applied it to their own devices. There are, after all, many ways to make a thing: by law, by language, by construction, by simulation. The authors of this rich and intrepid exploration are the latest to adapt Vico’s *verum factum*. They do so by deriving from the sciences of the last half-century a new meaning for “the made,” namely, “the grown.”

This most recent take on making gives Vico’s corollary a changed meaning. History remains the only true science, but for a new reason: Scientific explanations *are* historical explanations. According to the worldview described and keenly analyzed in Growing Explanations, to explain the complex features of the world—the structure of the cosmos, the functioning of natural and social systems, the nature of life, consciousness and selfhood—one must grow them. To know a thing, that is, we need somehow to enact or re-enact its history. Computer simulation is the paradigm method for growing phenomena described in the book. But other techniques, used both separately and in combination with computer simulation, also appear, including such mathematical systems as chaos theory (Amy Dahan Delmedico), catastrophe theory (David Aubin), and fuzzy logic (Claude Rosental). The book’s eleven essays also include suggestive examinations of “grown” explanations in string the-
ory (Peter Galison), finite-element analysis (Ann Johnson), cybernetics (Evelyn Fox Keller), immunology (Alfred I. Tauber and Ilana Löwy), and artificial life (Richard Doyle and Stefan Helmreich).

Growing differs from the older kinds of making—notably building—in a host of ways. It is irreversible: One can take a watch apart and put it back together as good as new, but one cannot do the same with a creature (or a river or the stock market). A grown thing, unlike a built thing, is irreducible to its parts. Growing diverges from building also in being driven and directed at least in part from within, not fully subject to the control of the grower. Growing, finally, is a contingent process with an unpredictable trajectory and outcome. At the heart of all these distinctions between growing and building lies the notion of emergence, which encompasses the gradual, contingent, irreversible, self-directed processes by which grown things happen.

But what precisely is emergence? This question lurks beneath all the discussion in Growing Explanations, coming to the surface in several of the essays and serving as the subject of the final chapter by Claus Emmeche. Emmeche writes that emergence is no longer “mysterious” (316), and he presents a technical definition that he translates as “the whole is greater than the sum of its parts” (318). But doesn’t this holism, no matter how plausible and compelling, retain a certain mystery at its core? Throughout Growing Explanations, the authors characterize emergent properties as surprising and unpredictable, making mystery seem actually definitive of emergence.

This important and provocative volume makes a persuasive case for the claim that complex phenomena require a particular kind of understanding, one that is historical in nature, and for the further proposal that the world will turn out to be complex all the way down. Less overt but equally present in Growing Explanations is the implication that this new, historical mode of explanation will be defined, in part, by its dramatic limits. Emergent phenomena—the objects of the new science charted in this book—seem to be those for which we cannot fully account even biographically. Unlike Vico’s model of constructed historical knowledge, distinguished by its completeness, grown histories include unfathomable moments of transformation.

Jessica Riskin
Stanford University


In recent decades, the deepening interest in the Holocaust and the spreading of civil and ethnic violence have led social scientists to turn to the study of mass killing, violence, and genocide. As students of international relations have shifted attention from interstate wars to intrastate conflict—which in the second half of the twentieth century became the
major source of violent deaths—theories of ethnic conflict have ranged from primordialist narratives of deep cleavages between peoples based on ancient antipathies to rationalist accounts of the instrumental employment by elites of ethnically inspired rhetoric to mobilize masses for murder. Reversing an older image of ethnic violence as emanating from the masses, strategic interactionist approaches have located initiative at the top, while provoking the question of why ethnic appeals have such powerful resonance below. In this mammoth comparative study of modern mass killing, Mann elaborates a compelling analysis of ethnic cleansings as applied to the Armenian Genocide, the Nazi Holocaust, and the murderous “cleansings” in Stalinist states, disintegrating Yugoslavia, and Rwanda.

In his model, Mann begins with eight theses about ethnic cleansing: Murderous cleansing is . . . the dark side of [modern] democracy” (2); “ethnic hostility rises where ethnicity trumps class as the main form of social stratification” (5); “the danger zone . . . is reached when (a) large movements claiming to represent two fairly old ethnic groups both lay claim to their own state over all or part of the same territory and (b) this claim seems to them to have substantial legitimacy and some plausible chance of being implemented” (6); “the brink of murderous cleansing is reached when [either] the less powerful side is bolstered to fight rather than to submit . . . by believing that aid will be forthcoming from outside [or the] stronger side believes . . . that it can force through its own cleansed state at little physical or moral risk to itself” (6); “murderous cleansing occurs where the state exercising sovereignty over the contested territory has been factionalized and radicalized amid an unstable geopolitical environment that usually leads to war” (7); “murderous cleansing is rarely the initial intent of perpetrators” (7); other plans fail before the most radical solution is adopted; “there are three main levels of perpetrator: (i) the radical elites running party-states; (b) bands of militants forming violent paramilitaries; and (c) core constituencies providing mass though not majority popular support” (8); rather than perpetrators being psychotic or uniformly sadistic, “ordinary people are brought by normal social structures into committing murderous ethnic cleansing” (9).

Mann argues that when ethnicity combines with economic inequality, the likelihood of ethnic conflict increases, but murderous cleansing is fundamentally related to rival claims to sovereignty in a given territory. Thus, such mass killing is a modern phenomenon, the consequence of the fusion of two understandings of “the people,” demos and ethnos. Citizenship is identified with a homogenous concept of organic ethnicity, rather than with liberal tolerance of diversity. Europeans and others thought of solving ethnic conflicts through population transfers, ethnic cleansing, and genocide. For example, settler democracies, like the early United States or Australia, cleansed their landscape of native peoples whose labor was not needed but whose territory was desired.

In the case of the Armenian Genocide, Mann compellingly rejects
the view that the Turkish governments had a consistent, long-term genocidal intent. He emphasizes the radicalization of Turkish policies from the “exemplary repression” of Sultan Abdul Hamid II to the forced application of “Turkification,” to politicide, and to the annihilation of the entire potential Armenian political and military leadership class, before the deportations (ethnic cleansing) and the mass killings of full-blown genocide became a reality.

Mann’s various histories are deeply researched and nuanced, with due recognition of anomalies and loose ends. His is historically sensitive social science rather than water-tight model building, more like the work of the great comparativists of classic sociology—Karl Marx and Max Weber, than the reductive current practices of many formal political scientists or sociologists. Critics both from the “hard” pole of social science and the “soft” pole of particularist historiography will undoubtedly find fault with Mann’s method and conclusions, but he has given us yet another study rich in insights, suggestive parallels, and powerful causal connections.

Ronald Grigor Suny
University of Chicago

The Specter of Genocide: Mass Murder in Historical Perspective. Edited by Robert Gellately and Ben Kiernan (New York, Cambridge University Press, 2003) 396 pp. $60.00 cloth $22.00 paper

In the last decades of the twentieth century, scholarship and interest in genocides and other mass crimes multiplied, reflecting the shocking range of killings from East Timor to Rwanda to the former Yugoslavia. This collection of original essays addresses key issues and ongoing debates in the emerging interdisciplinary field of Genocide Studies. Gellately and Kiernan discuss the complexities of definitions distinguishing genocide under the UN Genocide Convention, with its high legal-intent threshold and exclusion of political groups, and terms such as ethnic cleansing, which often includes territorial expulsion, and mass crimes (the subject of Jacques Semelin’s essay about the former Yugoslavia [1991–1999]). Why and how have so many civilians who are members of, or purported to be members of, specific groups been targeted for elimination from Asia and Africa to Europe and North America? Why and how some conflicts turn into mass murder is a recurrent theme in this collection.

Besides the editors’ introduction and conclusion, the book is divided into four parts: “Genocide and Modernity” (Kiernan, Eric D. Weitz, Omer Bartov, and Marie Fleming); “Indigenous Peoples and Colonial Issues” (Elazar Barkan, Isabel V. Hull, and John G. Taylor); “The Era of the Two World Wars (Jay Winter, Nicolas Werth, Gellately, and Gavan McCormack); and “Genocide and Mass Murder...
since 1945” (Edward Kissi, Robert Melson, Greg Grandin, and Semelin).

Certain essays focus on crimes in one country. Werth describes a murderous program of social engineering in “The Mechanism of a Mass Crime: The Great Terror in the Soviet Union 1937–38”; Gellately focuses on “The Third Reich, the Holocaust, and Visions of Serial Genocide”; and Barkan examines the conquest, abduction, and murder of indigenous peoples from the Americas to Australia in “Genocides of Indigenous Peoples: Rhetoric of Human Rights.” In “Reflections on Modern Japanese History in the Context of the Concept of Genocide,” McCormack asks whether Western demands (often unofficial) employed a double standard in demanding Japanese admission and apology for atrocities committed during World War II but excluding atrocities of major Western countries from accountability.

Kiernan in “Twentieth Century Genocides: Underlying Ideological Themes from Armenia to East Timor” provides a comparative analysis that describes how “racism, religious prejudice, expansionism and idealization of cultivation” have been transformed from a relatively harmless part of nationalism into a deadly combination (51). The essay traces the narrative of territorial decline and ideals of recovery in a series of diverse cases, such as the Ottoman Empire, Nazi Germany, Khmer Cambodia, and Mohamed Suharto’s Indonesia.

Weitz’s “The Modernity of Genocides: War, Race and Revolution in the Twentieth Century” emphasizes the aesthetic of death that emerged out of World War I and how rituals of killing play a crucial role in the drive for ideological purity linked to revolutions in modern states. Weitz points out that killing is a performative act and describes how “enormously inventive” human beings have been in carrying out mass murder. He draws up a list of human cruelties linked to creative rituals and explores the range of methods, from deportation to torture and random shootings to desecration of bodies, that help to explain why and how so many individuals take part in modern mass crimes.

The final section on post-1945 genocide is noteworthy in providing essays on Indonesia, Cambodia and Ethiopia, Rwanda, Guatemala, and the former Yugoslavia. The editors point out that gaps still remain—for example, scholarship about Bangladesh and events in Asia and Africa, especially from scholars in these regions. The section includes Kissi’s comparative essay on Cambodia and Ethiopia and how civil war became a vehicle for the Dergue to eliminate the Oromo and other ethnic groups, as well as Melson’s analysis of how revolution in Rwanda led to a total domestic genocide.

Grandin’s “History, Motive, Law, Intent: Combining Historical and Legal Methods in Understanding Guatemala’s 1981–83 Genocide” discusses the un-administered Historical Clarification Commission (CEH), its findings of state-sponsored genocide against the Mayan population, and its condemnation of, among others, the U.S. government support for Guatemalan security forces. The official U.S. version of the
Cold War battlefield excludes the targeted destruction of civilians and ongoing damage to individual lives and societies. Grandin’s essay compellingly analyzes the devastating human toll of Cold War foreign policy in Guatemala.

Spectre of Genocide provides an important contribution to the study of genocides and other mass crimes by including the histories of those targeted and often eliminated and countering the denial and distortion that all too often make up the official accountings of what took place.

Joyce Apsel
New York University


Globalization challenges not only policymaking institutions at the national and international levels, but academic analysts as well. This volume focuses on problems stemming from the increasing mobility and integration of international capital, and the resulting erosion of prestige and power for national currencies and monetary policies. Contributors include distinguished political scientists and economists, who analyze the evolution of Bretton Woods monetary institutions since 1945, advance the theorization of optimal and endogenous currency areas, and propose paths for future research in international political economy (IPE).

After the editors’ introduction, Robert Gilpin offers an admirably concise survey of how economics and political economy have defined their tasks over time, the current major theoretical schools, and the revival of interest in “political economy” by both political scientists and economists seeking to clarify the complex interactions between politics and markets.

Several essays review the developments in IPE theory and set agendas for developing a better theoretical grounding for analysis. Thomas D. Willett urges greater breadth in the use of public-choice theory, including “soft core” analysis in order to include psychology and human agency in studying policy determination. John S. Odell criticizes the unlimited rationality implicit in most rational-choice analysis, arguing that analysts should adopt principles of “bounded rationality” in order to take into account the limits of knowledge held by rational actors, the limits on their ability and willingness to calculate optimal solutions, and the instability and irrationality present in many individual preferences. Peter Kenen reviews the development of theory on optimal currency areas, in which he played a major role, from its origins in an era of low capital mobility to its current challenges in an era of high mobility, financial integration, and reduced monetary autonomy. Philip G. Cerny urges
greater attention to the development of new forms of self-regulation in markets as a result of global integration, and to the ways in which the role for the state is changing from economic management and wealth redistribution to the regulation of market norms to reduce instability. Given the high degree of uncertainty as to outcomes of current changes, these essays serve as timely reviews of how IPE theory has advanced to where it is today, and seek to set agendas for new research.

The essays with a historical approach place current trends in a longer-term perspective on changes in monetary governance. Kathleen R. McNamara examines the development of a single currency in the United States during the Civil War era, stressing the importance of war and pressures for market integration in the adoption of a single currency as a key element in building a national state. Her study provides an interesting contrast to current trends in the “deterritorialization” of money, marking how greatly the economic and political worlds have changed. Nation states are well established, currencies are less important as instruments of national identity, and expectations for national monetary policies have declined significantly since the mid-twentieth century.

Eric Helleiner’s analysis of the reasons for the degree to which territorial currencies are becoming “unpopular” adopts a strongly historical approach. He argues that saving on international transaction costs and skepticism regarding activist national monetary policies are the key factors promoting the move away from territorial currencies. State concerns for fiscal resources and the role of currency in establishing the identity and autonomy of nation states, both important in the “territorialization” of currencies in the nineteenth century, no longer weigh heavily in current concerns. Miles Kahler’s essay argues that the institutional character of the Bretton Woods system was a “striking innovation in international monetary governance,” based on U.S. ability to enforce its preferences and on a fundamental misreading of interwar monetary history.

The collection provides a snapshot of the state of the art in IPE at the turn of the millennium, with theory struggling to keep the reality of monetary union in Europe and currency deterritorialization within its grasp. The collection should be welcome to historians for its clear delineation of IPE theory and for its demonstration of the utility of attention to historical detail. As one author comments, “Political economy knowledge would be better empirically if we paid more attention to how economic policy decisions are actually made” (168).

Kenneth Mouré
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The objective of this collection is to find in economic history some guidance for dealing with contemporary economic issues. Almost all of the essays are by economic historians and rely heavily on the authors’ previously published work. The seventeen essays are grouped into three parts. Part one deals with the sources of long-term economic growth, Part two with changes in economic regimes and ideologies, and Part three with individual welfare and security.

The best essays show that economic history provides an important complement to economic theory. For example, the detailed analysis by Jan de Vries of what he calls the “industrious revolution” in northern Europe in the seventeenth and eighteenth centuries contains the material for a potential reorientation of modern growth theory. De Vries documents parallels between this earlier era and today’s world of proliferating gadgets and an overworked middle class, suggesting that changes in household attitudes have more to do with economic prosperity than contemporary theory allows.

The essay by Jane Humphries shows that the complementarity between history and theory works in both directions. She uses modern contract theory to analyze the role of the English apprenticeship system in the first industrial revolution. She points out that the system dealt effectively with what amounts to a “dual hold-up” problem familiar to contract theorists, according to which an apprentice could evade his obligations once trained, and the master could exploit the apprentice for cheap labor. By mitigating this hold-up problem, the system provided the well-trained and mobile labor force needed for England’s early transition from an agrarian society. Only someone equally familiar with archives and modern theoretical models could produce such an illuminating analysis.

Economic history reveals that historians do not just use first-rate theory; they sometimes even produce it, without the aid of mathematical models. Alexander Gerschenkron was such a historian/theorist, and the essay by Nicholas Crafts provides further evidence for Gerschenkron’s theory that late-industrializing countries can benefit from collective intervention of the sort that might have stifled innovation among earlier industrializers. Crafts examines the record of the East Asian “miracle” countries, pointing out the important role that state industrial policies played in the financing of their development. He also argues persuasively that these policies are unlikely to allow a complete catch-up to OECD income levels, because, in many cases, they provide a hierarchical solution to problems that at the highest level of development require a market solution. The issues that he addresses are on the current frontiers of growth theory, where theorists and historians have much to learn from each other.
One of the most pressing economic problems among industrial countries today is the provision of income security for the elderly. The essay by Paul Johnson and Mark Thomas shows that the problem was in many respects even more acute before the introduction of Social Security in the United States and the welfare state in Britain; in both countries, most people have never saved adequately for their old age. This essay is an excellent starting point for thinking about the issue.

The collection is certainly worth reading by any economist relatively unacquainted with economic history. Some of the essays might also serve as good introductions to contemporary economic problems. For non-economists, the book offers a useful demonstration of how modern economic theory is used by those who take it most seriously, not as a rigid set of formulas but as a guide to understanding the record of human experience.

Peter Howitt
Brown University


The volume under discussion is a sort of Festschrift for Paul David, of Stanford and Oxford. It rates at least an “above average” for a volume that originated as a set of conference papers. The short introduction by the editors does little more than summarize the papers. Also, although the title sounds like a broad manifesto, its message is aimed squarely at economists. Historians of other stripes will find it both obvious in general (of course, history matters!) and not infrequently impenetrable in its details. Demographers, however, will note several meaty chapters.

The essays are divided into four groups, the first two dealing with the issue of path dependence. The editors sum up the other two parts as “context matters” and “facts matter.” The third is a bit of a catch-all, with no obvious links among the four papers. The last should really be labeled “What is a fact?” Its papers grapple mainly with issues of measurement, including both the ambiguities that arise in aggregating data and the art of making statistical bricks with few actual data straws.

As to path dependence, the papers range widely, from technical exercises aimed at fitting the concept into standard theory to applications. On the methodological level, Melvin Reder’s paper starkly confronts the historical economics toward which David and others have been working with the neoclassical model that largely rules the profession.¹ This clear and vigorous essay will no doubt confirm some prejudices

against economists, since Reder comes perilously close to arguing for the “strong economics” model on the grounds that it makes the academic game of what to publish and who matters much more tractable than would an emerging and still amorphous historical economics.

Douglas Puffert’s contribution, though ostensibly a work in progress, goes farther than most in fleshing out the connections between path dependence, network effects and form, and technological change, which are at the heart of the putative new paradigm. And it does so with a laudable minimum of formalism. One promising notion is that of subnetworks with different standards or technologies joined by means of “gateways.” While limiting inefficiencies resulting from heterogeneity, such a compromise may improve the long-term performance of an entire network by preserving useful diversity during the—often drawn-out—process of competitive technological maturation. As Puffert says, “not only history matters in allocation, . . . time matters” (88, italics in original).

Paul M. Hohenberg
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We have waited too long for this fine book. It reports on a 1998 Stanford conference, and the delay in publication has led to an academic inversion: Saller’s cogent summary of the evidence on Roman economic growth has already been reprinted. The book contains pairs of essays on the Ancient New East, Greece, Egypt, and Rome, with comments by social scientists on five of the eight essays. As explained in the introduction, the aim of the book is explicitly interdisciplinary—to describe which disciplines and intellectual traditions have dominated the varieties of ancient economic history surveyed and to advocate the use of more social science in future writings on ancient economies. The great strength of this book is its comprehensive introduction to the main themes of several strands of ancient economic history. The drawback is the sparseness of new evidence; by and large, this is a work of secondary scholarship.

The ghosts that haunt these pages are those of Karl Polanyi and, to an even greater degree, Moses Finley. They are cited and quoted more often than other sources, and they are referred to in more essays than other authors. They are “ghosts” because they are cited in order to be

disputed, typically for being simplistic, linear, or static in their thinking. But even if they are now considered erroneous, their views still appear to dominate current writing—much as Hamlet’s ghost dominated his son’s actions. No alternate synthesizers have achieved anywhere near as much currency as these authors in the scholarship of ancient economies.

I concentrate primarily on the three essays that lack discussants in the volume, from the point of view of a social scientist—like the commentators on the other essays—to provide some sense of the content of the book. The first of these essays, by Morris, contains most of the book’s empirical evidence. Morris argues that from 800 to 300 B.C.E., house sizes increased rapidly in Greece. He defends this finding against several objections and buttresses it with data on house furnishings. In his rather startling words, “Fourth-century Greek houses were large and quite comfortable, even by the standards of developed countries in the early twenty-first century” (123). As Morris concludes, this finding raises important questions about the rate and extent of economic growth in the ancient world.

The other two essays that lack social-science discussants are the two on Egypt by Manning and Roger S. Bagnall. These authors each survey different contributions to the literature on the Egyptian economy, confronting one of the theses of the volume’s introduction: whether there was a difference between the economies of the eastern and western Mediterranean—that is, between the “ancient” and the “oriental” economies. They agree that the contrast is overdone. In the Hellenistic period, called the Ptolemaic period in Egypt, many connections were made between different regions. The result, they say, was that variations within regions were more important than variations between them.

That view is given force by the contrast between two graphs in this book. Most of the authors decry the absence of quantitative evidence, proclaiming the need to construct views from qualitative indications. There are however two quantitative graphs that provide sharply contrasting views of the ancient economy. The first reports Morris’ data on housing sizes in Greece during the Hellenistic period, used as a rough proxy for per-capita income (108). In the second (231), Saller shows an estimate of per-capita income in the Roman era inferred from data indicative of trade. Both graphs show economic growth; they deserve to be known widely. But the first shows an increasing rate of growth, whereas the second shows a decreasing rate.

Why should ancient historians be concerned about something as arcane as a different second derivative? Because these two graphs express sharply contrasting views of ancient economies. Morris’ graph shows accelerating growth. Since it did not continue, it must have been interrupted by some dramatic change. Saller’s graph shows decelerating growth that petered out gradually, without drama. Which view is correct? They might both be; they concern different places and times. But one of them might be wrong. For example, Robert Bruce Hitchner in
this volume presents a view of economic growth in Rome that is closer to Morris’ graph than to Saller’s. It is to be hoped that further interdisciplinary efforts will stimulate historians to investigate such questions. This book raises many questions of both method and substance that should be of interest to historians interested in long-run growth and the interaction of different historical disciplines.

Peter Temin
Massachusetts Institute of Technology


Chronologically, the ten essays in this collection span sixteen centuries from the early Roman Empire to the Baroque era. At issue is Rome’s technological legacy to Europe’s water systems, especially the intellectual impact of Sextus Iulius Frontinus’ De aquaeductu urbis Romae. Born before 35 A.D., Frontinus was curator aquarum under Nerva and a senator until his death in 103 or 104 A.D. In the sixteenth century, De aquaeductu was frequently printed with Vitruvius’ De architectura, but during the Middle Ages Frontinus’ influence was negligible.

Only two essays deal directly with Frontinus. In “The Literary Character of Frontinus’ De aquaeductu,” Saastamoinen examines the question of the work’s genre, whether it was a technical treatise or a general survey for the educated reader. In the end, he decides that it is neither, but rather a composite work of disparate parts left unrevised at Frontinus’ death. Bruun’s essay, “Frontinus and the ‘Nachleben’ of His De aquaeductu from Antiquity to the Baroque,” chronicles the work’s fate during the millennium and a half after its author’s death. Although the treatise survives in two medieval copies, it was only recovered and made famous after 1432 by Poggio Bracciolini and Flavio Biondo.

The remaining contributions mostly address practical issues of water supply in the post-Roman world. Robert Coates-Stephens examines “The Water Supply of Early Medieval Rome,” using detailed archaeological evidence to illuminate mundane matters largely absent from written texts. In “Classical Water Technology in the Early Islamic World,” Andrew Wilson shows how Arabs both adopted Roman hydraulic technology and introduced new techniques into Islamic Spain. In a similar vein, André Bazzana’s “Approvisionnements hydriques et maîtrise de l’eau dans al-Andalus du xer au xvier siècle” argues that the systematic application of minute technological improvements and the solidarity of local communities enhanced water provision in Muslim Spain. Klaus Grewe addresses the broader question of continuity between Rome and the Middle Ages in “Technologie-Transfer von der Antike in das
Mittelalter am Beispiel der Wasserversorgung,” concluding that Rome’s influence runs like a “red thread” through medieval hydraulic practice.

Two rather different essays focus on specific Italian towns. In “Orvieto e l’acqua nel Medioevo,” Lucio Riccetti chronicles Orvieto’s centuries-long struggle to provide clean water to a growing hill town. In “Tradizione e innovazione nel governo delle acque a Milano nel secolo XV,” Giuliana Fantoni looks at the administration of water under the Visconti and Sforza rulers of fifteenth-century Milan. Shifting the geographical focus to Rome, Meri Vuohu examines Renaissance hydraulic theory in “Water Supply as Part of Urban Hygiene in Fifteenth-Century Treatises on Architecture.” Finally, Leonardo Lombardi’s “L’ingegneria idraulica romana rivisitata in epoca rinascimentale e barocca” surveys the revival of large-scale water projects in Renaissance and Baroque Rome.

This well-produced volume is an ideal companion to Roberta Magnusson’s Water Technology in the Middle Ages (Baltimore, 2002).

Steven D. Sargent
Union College


The fallacy-inducing methodology of “reading history sideways” consists of taking contemporaneous evidence about other societies to infer past characteristics of our own. Typically, Thornton argues, social scientists of the eighteenth or nineteenth centuries (what he calls the 1700s and the 1800s) imagined that the past family structures of “modern” northwestern Europe could be investigated by looking at “traditional” societies outside of the region, for example in America, Africa, or Australia. Inversely, they thought that they could foretell the future state of “primitive” societies by assuming “development” would follow a predictable trajectory. Inferring temporal sequences from cross-sectional data assumed that “change is uniform, natural, necessary and directional”—what the author calls the developmental paradigm.

But this notion of steps on an inescapable ladder of development turned out to be a self-fulfilling prophesy, as it was vigorously promoted by powerful Western nations and widely adopted by societies that were seduced by the trappings of “westernization.” Thornton calls “development idealism” the preconception that societal development produces change from non-Western family systems to those of Northwest Europe. He restricts his analysis to changes in family life, and identifies four propositions of development idealism: Modern society is good and attainable; the modern family is good and attainable; the modern family is a cause as well as an effect of a modern society; and individuals have the right to be free and equal, with social relationship being based on con-
sent. These propositions—together with other forces such as mass edu-
cation, industrialization, urbanization, and the development of technol-
gy—have contributed to modifying family life, including marriage,
fertility, and patterns of residence.

The argument is provocative, systematic, and cogent. As one of the
citations on the blurb says, *Reading History Sideways* is an intellectual
feast. The author introduces occasional sophisms, however, to make the
argument more elegant. The book indicts many of the social theories
obtained from sideways reading in the textbooks of demography and
family sociology, and much of the conventional terminology of social
science. Take for instance an often–cited sentence defining the demo-
graphic transition, which the writer granted was “neither subtle nor pre-
cise,” but “described a central preoccupation of modern demography”:
“In traditional societies, fertility and mortality are high. In modern soci-
eties, fertility and mortality are low. In between, there is demographic
transition.”¹ Thornton would condemn the use of the words traditional,
modern, and transition, because they imply necessary stages in the devel-
opment of society; the word “transition,” in particular, suggests that
changes are irreversible. But one objection to his point is that “tradi-
tional” and “modern” are convenient shorthands that can be replaced
only by lengthy circumlocutions. He is correct in attacking the notion
that specific social changes are necessary. Belief in their irreversibility,
however, as in the case of fertility and mortality decline or technical
progress in general, is less controversial.

The method of reading history sideways may not have been as
dominant among the founding fathers of social science as the author
claims. The evidence that he presents is problematical, because the refer-
ces lack specificity. Footnotes cite the entire work of multiple authors
as authorities about an often trivial point, without reference to a particu-
lar passage. See, for example, a mention of polygyny that “drew wide-
spread attention and was judged by one observer to be so terrible that no
woman would enter it voluntarily” (56) The footnote cites nine authors,
including entire works of Friedrich Hegel, David Hume, Adam Smith,
Michel de Montaigne, and Montesquieu (the latter two authors, at least,
adopted nonjudgmental attitudes toward polygyny) without identifying
the said “one observer” or even specifying on which of the many thou-
sands of reference pages polygyny is mentioned. Many authors of the
past used foreign social systems not as images of their own societies in an
earlier time but as alternative models of society. Thomas Malthus or
Smith, for example, did not picture China at a lower level on the devel-
opment ladder, but presented it as a classless system in which marriage
did not function as a preventive check.

Thornton recognizes that reading history sideways has generated
important theories about social change. As a demographer, he might

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¹ Paul Demeny, “Early Fertility Decline in Austria-Hungary: A Lesson in Demographic
have acknowledged the importance of the historical record for devising models based on the recognition of empirically demonstrated regularities. History provides hypotheses subject to empirical verification, useful models from which to extrapolate and standards by which to measure actual developments. Such tools as model life tables or assumptions of natural fertility are useful in historical demography, and they shift the burden of the proof when evaluating dubious data. The synthetic cohort, which the author attacks as a presumption of future behavior (118), is merely a technique of measurement.

As for the presentation of developmental idealism as a powerful force for social change, the author does not give enough weight to the alternative hypothesis that “developmental realism” is also at work in the world. In other words, the search for effective solutions to locally perceived problems may have led to the adoption of proven technologies (printing, the automobile, the machine gun, whatever) with consequences on family life. Despite its controversial aspects, however, this book is a useful critical assessment of the comparative method, and is likely to be an important and influential contribution to family sociology.

Etienne van de Walle
University of Pennsylvania


Over the past decade, scholars of the European Middle Ages and postcolonial historians have formed exciting alliances devoted to questioning entrenched Eurocentric concepts of modernity. The story of Arthur, the once and future king, with its hints of other temporalities—ones that do not conventionally divide a “then” from the modern “now”—has provided rich subject matter. Finke and Shichtman examine Arthurian literature drawn from three periods of cultural crisis: the Norman colonization of England (eleventh through twelfth century), the War of the Roses (fifteenth century), and two books on Adolf Hitler and the Arthurian occult written in the 1970s. The volume under review is the fourth in a quartet of recent postcolonial publications on Arthur: Michelle R. Warren’s History on the Edge: Excalibur and the Borders of Britain 1100–1300 (Minneapolis, 2000); Patricia Clare Ingham, Sovereign Fantasies: Arthurian Romance and the Making of Britain (Philadelphia, 2001); Geraldine Heng, Empire of Magic, Medieval Romance and the Politics of Cultural Fantasy (New York, 2003). Even though Finke and Shichtman refer to these three companion studies, they do not adequately account for their implications. In this gap between acknowledgment and
dynamic critical transformation can be traced the challenges currently facing postcolonial medieval studies.

In this collaborative study, the authors analyze a selection of Arthurian texts as metropolitan productions circulated by court clerics and printers in competition for symbolic capital. Warren and Ingham have shown how medieval Arthurian chronicles and romances are riven with border writing (Welsh, Norman, Breton, and Scots). Taken together, these three studies suggest that we need more complex ways of imagining the contested narrative layerings of border and metropolis. Finke and Shichtman are also wary of Heng’s claim that Arthurian romance emerged out of the traumatic events during the First Crusade when Christians cannibalized dead Muslims during the extremes of siege warfare. In their critical desire to emphasize the provincialism of medieval European Christendom, the authors miss the opportunity to put their provincializing argument together with Heng’s to draw richer implications.

Arthurian narratives are striking for their turn to prophecy. Such recursive staging of vaticinatory traditions needs to bear on the discussion of Arthurian historicism, yet this issue too remains unaddressed. Ingham maintains that deeply ingrained evolutionary models of history (normalized versions of modernity) have blinded us to the cultural work of Arthurian prophecy. She shows how the study of these prophecies opens up another way of imagining the “unhistorical” in postmedieval medieval studies. She traces how the prophecies move dialectically between the prediction of devastating loss and the image of a communitarian future produced as a work of mourning. Merlin’s prophecies ask such questions as, Who gets to own periodization?

The hesitations of the authors are productive. They suggest that a commonly shared framework of postcolonial medieval studies is preventing a more thorough questioning of models of modernity that circulate in medieval studies. Like much other postcolonial medieval scholarship, the authors enfold their analysis within a normative historicist framework defined by the work of Anderson and Kantorowicz. They use Anderson’s modernization thesis to show how Arthur became modern. Finding a “modern” Arthur reinforces precisely the Eurocentric models of modernity that are supposedly in question. Postcolonial medievalists need to address more thoughtfully the compelling critique of Anderson’s thesis by postcolonial such scholars as Bhabha, Chatterjee, and Chakrabarty.

Finke and Schichtman adopt Kantorowicz’s notion of the king’s

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two bodies to identify the blind spot of a “modernizing” Arthurian narrative. They locate it in the violence that exceeds chivalric rules and strews the text with the broken bodies of a shattered body politic. They use this shattered body politic, called the “anamorphic blot” of the Arthurian imaginary, as a bridge to their readings on Hitler and the Arthurian occult circulated in the 1970s. The argument moves too fast at this juncture. It leaves unaddressed how political theology, developed and criticized by twentieth-century writers, as diverse as Schmitt, Kantorowicz, Blumenberg, Agamben, and Casarino, might be a model of modernity to be used with care.3

It is timely that postcolonial medievalists ask more critically what kind of model of modernity is political theology and what are its contested temporal stakes? Might it be that a Kantorowiczian political theology is an “anamorphic” spot in Arthur and the Myth of History that traps the argument in the “empty time” of modernity? Postcolonial medieval studies desires to rethink the temporal boundaries of its discipline. This provocative book points to the labor still required to take historicism to its limits to make its unworking visible.

Kathleen Biddick
Temple University

Predicting the Weather: Victorians and the Science of Meteorology. By Katharine Anderson (Chicago, University of Chicago Press, 2005) 331 pp. $45.00

The history of various fields and aspects of nineteenth-century scientific advances seems to have become a growth industry.1 Anderson extends this field into Victorian meteorology. But her book is not a recounting of the development of the advance of meteorology. Rather, it is a story of meteorology’s uncertain status in the nineteenth century. We could imagine meteorology located in a solid of three dimensions—natural science, laypersons’ judgment, and astrologers/almanac writers. It is not at all obvious that constant progress was made in the direction of the scientific plane. Natural scientists sought to establish fundamental properties of weather, but the public wanted weather forecasts and storm warnings. A key theme in Anderson’s study is the tension between


scientific truth and probabilistic, useful knowledge sought by the natural scientists and the public, as well as within the science community itself.

In some ways, Predicting the Weather is more ambitious than Fleming’s excellent Meteorology in America: 1800–1870, which limited itself to the development of meteorology in the United States up to the establishment of a government weather service with the Army Signal Service.\(^2\) Anderson takes on the more challenging goal of understanding how Victorian society perceived the weather science through her analysis of key scientists, administrators, publishers, and events. She succeeds to a reasonable degree. Highlights include the story of the establishment of a forecasting and storm-warning service in the United Kingdom and the suicide of its founder Robert Fitzroy in 1865, the scientific opportunities afforded by the British control of India (clear seasonal patterns and a large land mass with mountains and oceans as boundaries), and the role of weather prophets and almanacs.

Interestingly, Robert Fitzroy, director of the Board of Trade’s Meteorological Department, was the captain of the Beagle during Charles Darwin’s famous voyage. Fitzroy came to oppose Darwin’s theory from a religious perspective even as he was pushing to develop weather forecasts. Anderson discusses how scientists and theologians clashed over the call for divine intervention to alter the weather or protect a monarch’s health.

Anderson writes more about the tension between the goals of theoretical scientists and the public; more information about what scientists actually concluded would have been useful. There was vigorous debate about the role of lunar phases and sunspots on weather (that the moon affects tides seems to have been known). The overall story would be more enthralling if the (later) scientific consensus of these issues and barometric readings had been made clear. One chapter, entitled “Precision and a Science of Probabilities,” might well have been more specific about its subject. Since early forecasts and storm warnings of the Meteorological Department underwent heavy criticism, a quantitative measure of the accuracy of the forecasts and warnings might have been telling (the bias of an economic historian).

As Anderson notes, British forecasters were handicapped, relative to Americans, by the lack of a large Western land mass from which to gather data (storms generally travel west to east). British forecasters were humiliated in 1877 when New York Herald manager James Gordon Bennett, Jr., started cabling weather forecasts for England! With the exception of storm warnings, forecasts were discontinued after Fitzroy’s death, until 1879. Predicting the Weather achieves a good balance between giving us the facts and the excessively theoretical approach sometimes found in modern scholarship.

Erik D. Craft
University of Richmond

Imperial topics now constitute the area of greatest interest in the field of British history, with a particular emphasis on the integration of domestic British history with its former empire. Only a few scattered bits of land about the world still belong to Britain, but they can cause trouble out of proportion to their size, as in the cases of the Falklands and Gibraltar. Since granting independence to India in 1947, Britain has ceased to rule sixty-four countries and approximately 500 million people. Having lost an empire, Britain has provided historians with a role—studying the former empire. Certainly one of the more contentious areas within imperial studies is the question of how influential the empire was upon Britain itself. Some maintain that every aspect of British life was, and is, permeated by its empire. Others argue that most of the population, other than perhaps taking some pride in an empire on which the sun never set, was little affected by it.

Thompson’s extremely useful book takes a position halfway between these two points of view. He considers the various ways that the empire connects with Britain. His work is a summary one, a survey for scholars and students who want a penetrating overview of the subject. It is interdisciplinary in its approach, considering all aspects of the empire’s influence at home. He writes about architecture, museums, language, food, literature, and street names, among other topics.

Thompson also deals in fine fashion with the economic and political aspects of the empire. He concludes that it did not provide that much “outdoor” relief for the aristocracy. From the professional middle class came the civil servants who ran the empire. Thompson argues that the lower middle class was likely to be the most favorably disposed toward empire among the population. He does not pay much attention to the argument that the working class reconciled itself to its lot partly because it felt compensated emotionally and perhaps financially as inhabiting a country that ruled so much of the world. He discusses the role of emigration to the white colonies of settlement (although the United States as a former part of the empire was also a significant destination.) He has fascinating documentation on how much money emigrants sent back home. Not only those who emigrated but those born in the empire continued until recently to regard Britain itself as “home.” An excellent section discusses the interplay between domestic and imperial labor movements, such as the influence of Tom Mann’s experiences in Australia. The book proceeds by topics and not by chronology, limiting the sense of change and development.

A central question is whether the empire “paid.” Money was made; British investors were more interested in profits than necessarily in the empire per se. With exceptions, the government did not take actions that would particularly favor British businessmen and assure their profits.
But an aura of authority and power certainly hovered about the British as they moved in imperial and nonimperial space. (It is appropriately not part of the scope of this study to consider the role of the “informal” empire in terms of investment or in other ways.) Thompson concludes that the empire did not cause any fundamental change in the British economy and that it did not decisively influence politics at home. He gives little credence to those who have argued that the experience of empire made domestic politics more sympathetic to authoritarian trends.

Although this study primarily concentrates on the period before the Suez crisis of 1956, when Britain still acted as if it had an empire, it does not neglect the years since. The profound irony of the situation today, making the title of this work particularly vivid, is that when Britain was a great imperial power, few of its imperial subjects lived in the home country. Now that the empire has virtually disintegrated, once imperial subjects are a notable proportion of British citizens. The twin problems of their assimilation and the sensitive recognition of cultural differences present considerable challenges to British government and society today. Whatever the impact of the empire might have been on the home country before World War II, in those more hierarchical days, when the members of the imperial “family” were thousands of miles away, they presented far less of a challenge within Britain itself. In this intelligent study, Thompson reveals how the empire became one of the central components in the political, economic, and cultural life at home.

Peter Stansky
Stanford University


For at least twenty years, since the publication of William Beik’s Absolutism and Society in Seventeenth-Century France (New York, 1985), revisionist theories of absolutism have shaped our understanding of the old-regime French monarchy. Countering the traditional view of the French Crown’s tendency to assert its power over and against nobles and other privileged elites, the revisionist understanding of absolutism emphasizes the need for the Crown to negotiate with those powers. The Crown governed best, the revisionist argument goes, when it upheld the underlying interests of elite groups. Recently, Hurt has criticized this revisionist view for its exaggerated emphasis on interdependence, arguing that the Crown could—and did—undermine courts of law, or parlements, as independent political and judicial players.1 Into this fray Swann has

1 John J. Hurt, Louis XIV and the Parlements: The Assertion of Royal Power (Manchester, 2002).
stepped with his solidly researched history of provincial politics as centered upon the Estates General of Burgundy.

In his introduction, Swann adeptly wades through the historiography on absolutism, examining with an equally critical eye the assertions found in both the traditional and the revisionist views. Only recently have historians begun with any seriousness to consider the shortcomings of the revisionist views; it is notable that Swann contributes to this discourse: Revisionism, he argues, runs the danger of presenting the relationship between monarchy and ruling class as “both too static and oversimplified” (16). Further, there is a “danger that the revisionist argument more generally exaggerates the extent to which the monarchy was in thrall to vested interests,” in part because it rests on an overemphasis on pays d’États, or those uniquely privileged provinces that had their own estates (16). These are some important critiques, but Swann’s study has mixed success in addressing them.

Readers expecting strong statements in support of one position or another will be disappointed. Almost invariably, Swann charts a middle course. For instance, he writes, “Once war returned to darken the horizon after 1672, there was no revival of public resistance to the king’s fiscal policies,” but he does not conclude that “the Estates had been silenced or that they were simply content to bask in the rays of the Sun King” (192). Swann’s tendency to stake out the middle ground does not result in groundbreaking methodological or theoretical innovation; it does, however, represent solid and sensible history.

Swann’s scope—the intense focus on one institution over a relatively long term—is ideal for conveying an understanding of political change. Swann succeeds where the revisionist school comes up short. He presents political change as gradual and halting, though clearly in evidence over the long term. Yet Swann’s scope does not lend itself to commentary on the critique that the revisionist school’s views rest too heavily on research in pays d’États. A comparative approach would be necessary to gauge whether any accommodation between the Crown and local elites could take root in provinces without estates.

Swann presents an image of the Estates of Burgundy as a dynamic and responsive institution that could defend, to an extent, the interests of the local elites while undertaking some limited reforms in local administration. In short, it was an institution that worked well within the confines of the old-regime state. But it was also an institution that depended on the continuing notions of privilege and inequality for its authority, and herein lay its weaknesses, which became exposed as the political culture shifted distinctly away from those ideals before and during the Revolution. Swann has made an important contribution to our understanding of these political processes.

Mark Potter
University of Wyoming

By 1910, Arcachon/La Teste, a coastal fishing town southwest of Bordeaux, was France’s second biggest fishing port and one of its most popular beach resorts. It was not always so. Garner’s elegantly written study of the transformation of Arcachon/La Teste explores how the region came to be experienced as what Lefebvre, a social theorist, calls “lived space,” in terms of “people’s bodily experiences of a place undergoing transformation” (7). Garner is certainly not the first to examine the “modernization” of traditional communities or changes in coastal landscapes. Her work, however, is truly innovative with regard to her sources and her wide array of theoretical perspectives. The book draws on geography, spatial theory, maritime history, fine arts, literature, anthropology, tourism/leisure theory, architecture, urban planning, and the history of medicine. Like Corbin and Urbain, Garner explores the new emotional responses of a largely middle-class tourist population to coastal landscapes and how local communities came to be represented, romanticized, and exploited in the nineteenth and twentieth centuries. Finally, she analyzes the resistance of fishing families to their exoticization and displacement. Her work will particularly interest environmental historians and geographers for its insights about the changes in the coastal landscape itself.

Garner begins her study between the 1820s and 1840s, when the Compagnie des Landes and the Compagnie d’Arcachon set out to “colonize” and develop this hinterland of Bordeaux through engineering projects—including the construction of channels, the plantation of pines, and the stabilization of dunes. Although both companies ultimately went bankrupt, they opened up the region to further development, and they placed communal lands, particularly the area’s salt pastures, or prés salés, under assault, introducing the principle and practice of individual ownership where it had not existed before.

The 1850s and 1860s saw the building of railroads, which opened up the region to tourism. Arcachon gradually became a retreat for monied elites seeking solitude and the restorative fresh sea air. Urban spectators commented on their encounter with the landscape and its people in memoirs and letters that Garner carefully analyzes. She shows the way in which the region and its inhabitants were fashioned into objects of middle-class consumption. Garner tells the story of Arcachon’s gradual metamorphosis, the building of opulent villas, and the conflict between bathers, municipal authorities, and fishermen over access to the sea in eloquent and meticulously researched detail. Ultimately, this shore be-

1 Henri Lefebvre, La production de l’espace (Paris, 1974).
came a holiday resort, a site of consumption, leisure, and contemplation, and not of productive work.

In one of the book’s most interesting chapters, “Posing for Posterity,” Garner analyzes the visual representations of Arcachon and native fishermen and, especially, fisherwomen through posters, postcards, guidebooks, and illustrations, which helped to create an identity for the region. Arcachon was famous for its oysters and oyster farming, which is still an important business. Photographers depicted this occupation more than any other. The *benaize*, a form of head covering worn by female oyster diggers, came to symbolize regional tradition, even though oyster farming had developed only after the 1840s. Countless postcards show women, bent over, skirt or trousers hitched to their knees, bare-legged, suggesting an erotic pose. Garner indicates the ways in which these same women subverted the photographer’s gaze. Ultimately, however, by World War I, the viewer of such images is left in no doubt that the beach belonged to the visitors, as Arcachon’s original inhabitants and their boats were displaced one by one.

What makes *A Shifting Shore* unique is the way in which Garner not only captures the image of the people of Arcachon as presented by outsiders, but their experiences as well, even though she recognizes that these experiences are often mediated by the texts and photographs of others. In this sense, Garner’s book constitutes social and cultural history at its finest. She has delved deeply into the local archives and sheds light on the kinds of conflicts and anxieties that accompanied everyday life, including nineteenth-century fears of drowning and the national fascination with shipwrecks. In this masterful story of change, this talented historian was compelled to learn about the principles of engineering, map-drawing, oyster-farming, architecture, navigation, and medicine. In so doing, she has succeeded admirably in showing how Arcachon was transformed over the *longue durée*, even though many of the nineteenth-century tensions between locals and outsiders still clearly live on.

Caroline Ford
University of California, Los Angeles

*Bringing the Empire Back Home: France in the Global Age*. By Herman Lebovics (Durham, Duke University Press, 2004) 240 pp. $29.95

The reasons why France has made headlines in recent years in the United States may seem incomprehensible to many Americans. How could a sheep-farmer who destroyed a McDonalds become a national hero? Why, if not out of ungratefulness, would the French government lead an international cabal to undermine American foreign policy? How could a democratic state forbid its citizens from wearing religious displays, if this is what their faith demands? All of these apparently disjointed French peculiarities stem from the central question of Lebovics’
lively new book: What does it mean to be French today and what constitutes France’s shared national heritage?

The book proceeds to answer these questions by examining sequentially five ways in which the nature of French national identity has been challenged in recent decades. The interesting first chapter, “Gardarem lo Larzac!”, examines the birth of the antiglobalization movement in France *avant la lettre* by focusing on the struggle of ecologists, pacifists, and urban leftists in the early 1970s to promote regionalism in a highly centralized country and to link their regional struggle with the anti-imperialist decolonization movements of the 1960s. The second chapter delves more deeply into that link by exploring how France implemented an activist cultural policy in response to the erosion of its colonial empire. Through a fascinating focus on a Corsican civil servant named Emile Biasini, Lebovics recounts how successive governments used cultural instruments in order to pursue the country’s *mission civilisatrice* within the borders of continental France.

In the third chapter, the author develops the idea of *patrimoine*—a word difficult to translate with precision—conveying the sense of a shared national legacy, by focusing on internal conflicts and reforms within the field of ethnology. Chapter 4 examines the effect of multiculturalism on the traditional concepts of an indivisible republic and universal rights, and it questions whether France’s culture and polity have been updated to become pluralistic. The last chapter focuses on the recent “dance of the museums” to approach the question of what is distinct about France—what the French like to refer to as their national exceptionalism.

*Bringing the Empire Back Home* is, in a way, a tour de force. Through its lively narrative, it succeeds in painting a complex portrait of contemporary French identity and of the tools that socially and politically construct it. The book is particularly strong in showing how the current struggle to contest globalization arose from the interplay between French cultural policy and decolonization, and from the fact that the French centralized model manifests itself in all walks of life—from controlling academic curricula to deciding on the content of museums’ collections.

Yet Lebovics’ methodology, based on a juxtaposed narrative of five different challenges to French identity, leaves something to be desired. First, the focus of certain chapters on individuals and others on institutions seems to lack any underlying logic. Why not, for instance, pick an important actor in each of these five areas to illustrate the evolution of French identity? This consistency might facilitate the analytical leap from narrative to explanation, as well as to political implications, which introduces a second weakness of this book. Some of the paradoxical developments in contemporary French politics are not foreshadowed by the recent history as told by Lebovics: Why is the moderate right increasingly gaining the favor of the immigrants and the *beurs* (a variation of the French term, *arabe*)? Why is Jacobin republicanism a value of the
left, preventing the emergence of affirmative action and “positive discrimination”? Why has President Chirac, a conservative, become a national and international herald of anti-globalization, at least in rhetoric?

The strongest reservation about this otherwise interesting book is that it does not discuss the construction and constant reactive reconstruction of French national identity. What it means to be French is also partly defined by what it means not to be French. France’s exceptionalism emerges largely in opposition to the perceived flaws of other nations’ characteristics—the United States’, for one. On a related note, the author’s contention that the European Union does not enter French identity because it is not part of the French heritage is puzzling, to say the least. If true, he needs to explain why. The current lively debate on the European Constitution and the future of European integration certainly shows that this issue is far from settled. But is Lebovics correct? After all, France has abandoned much of its national sovereignty to Europe over the years, not the least its national currency, the Franc, a symbol until then of national identity. Whether the construction of French identity includes a European dimension is worth exploring further, perhaps in another book.

Sophie Meunier
Princeton University


Before Antwerp, Amsterdam, and London became, in Braudel’s view, anchors of the early-modern world economy, Bruges was the trade center of northern Europe, and the economic midwife to these heirs. For all its economic complexity and cultural richness, fourteenth-century Bruges is surprisingly understudied, especially outside of Belgium. Murray’s new book is a fresh, deeply researched social portrait of the city and its economic life from the late thirteenth to the late fourteenth century that goes a long way to fill this scholarly lacuna. The book bristles with important social data about Bruges’ financial world and its wider civic realm, offering a compelling, impressively researched case study of early capitalism in a mercantile society. For this reason alone, Murray’s study should be of signal importance to scholars who seek to understand better one of late-medieval Europe’s economic linchpins and how its nexus of moneychangers, brokers, hostellers, and merchants made Bruges an international “node and network” of commerce, finance, international trade, and textiles.

Murray’s book offers the fullest portrait hitherto of Bruges as an early center of capitalism. This city, after all, had the famous Bourse

square, site of the original hostel “Ter Beurse” (“Sign of the Purse”) that became a master symbol of late-medieval commerce. Murray is particularly fascinated with the role of money, credit, and exchange in Bruges’ public life, but less in their abstract, formal qualities than in their social dimensions. Money to Murray is a palpable commodity—“clinking money,” as Netherlanders called it. It is a thing of weight, fineness, and variety, signifier of Bruges’ commercial bustle, but the source of instability too, since its continuous debasement at the hands of Flemish counts rendered the city’s economy parlous. To Murray, pawnbrokers and moneylenders were the work-a-day agents who lubricated Bruges’ commercial world by their pragmatic ability to negotiate chronic shortages of ready money, sudden debasements of currency, and a wild variety of coinage.

Practitioners of a complex system of credit and debt that thrived despite official proscriptions against usury, pawnbrokers and moneychangers in Bruges offered a wide variety of financial services. Pawnbrokers took pledges and loaned money. Moneychangers—some of them licensed but many not—set up shop in public venues like Sint-Pieter’s bridge to examine and exchange coins and bullion, make payment orders for book transfers, dabble in real-estate investments, and even open accounts for deposit banking. But the two practices were not wholly discrete; pawnbrokers and moneychangers sometimes entered into business partnerships. Equally important were Bruges’ hostellers and brokers, who provided essential services in housing, warehousing, and deposits to the city’s swelling number of foreign merchants. They, too, were hardly isolated, offered a wide array of financial services, and could work in partnership with moneychanging operations.

Murray anchors his study in the concrete, material practices of Bruges’ citizenry. Murray’s portrait is not as cleanly demarcated as Raymond de Roover’s Money, Banking and Credit in Mediaeval Bruges (New York, 1948), which in many ways serves as Murray’s inspiration and foil. In de Roover’s classic study, moneylenders, moneychangers, and pawnbrokers catered to the local market, whereas Italian merchant bankers, equipped with a system of international credit transfer through bills of exchange, dominated the upper echelons of finance. Murray mines many of the same records that caught de Roover’s scholarly eye, particularly the ledgers of the moneychanging business of Willem Ruweel, whose license to operate came legally from his wife’s dower property, and the even richer records of Collard de Marke, who, unlike Ruweel, did not operate with an official permit. But Murray digs deeper and wider than did de Roover, analyzing Ruweel’s and de Marke’s transactions through the many layers of Bruges’ social world. The result is less de Roover’s multitiered hierarchy of financial practitioners than a “honeycombed” world of interdependence, in which moneychangers and pawnbrokers were as important as Italian banking firms.

Murray’s exhaustive research leaves no stone unturned in an effort to relate discrete financial practices to Bruges’ wider civic life. Indeed,
the first third of Murray’s book is dedicated to a social and political portrait of fourteenth-century Bruges, a broad yet finely detailed analysis of Bruges’ political importance in Flanders at the time. He charts Bruges’ built environment, its social pulse, its religious life—particularly its attraction to the Mendicant orders—and its social profile as a city of 36,000 to 45,000 people, though its commercial satellites, the Zwin river towns, added 13,000 more people to its general population. The result allows Murray to present a thorough-going portrait of Bruges’ financial word that is keyed to the pushes and pulls of its internal history, as well as the history of Flanders in the fourteenth century until the eve of its Burgundian era.

Murray’s expertise in social history inspires a picture of Bruges’ commercial world in all its variety, from the respectable hostellers and moneychangers to the red zone of taverns and houses of prostitution that flourished under the casual tolerance of the city fathers, who acknowledged their social usefulness, opted against a formal licensing system, but nevertheless drew a steady stream of revenue from regular fines. Murray also explores the remarkable presence of women in different realms of the marketplace—in guilds, in financial services, and in most aspects of public and financial life—a product, he argues, of the “relative openness” secured by Flemish property and marriage systems that guaranteed women equal rights to inheritance.

Murray’s book documents the halcyon days of late medieval Bruges at its commercial zenith, offering a story of its financial operations told in the vernacular, not from the elite summit of international banking but from the grit and grind of the daily operation of business, its social location, and its glorious variety. As valuable as this approach is, Murray has gone a step further by providing a total social portrait of late medieval Bruges. A product of exhausting research, Murray’s study deserves a wide audience. It should stand as the reference point for all further work on Bruges in this period, and interest all scholars of the late medieval urban world.

Peter Arnade
California State University, San Marcos

*The War against Catholicism: Liberalism and the Anti-Catholic Imagination in Nineteenth-Century Germany.* By Michael B. Gross (Ann Arbor, University of Michigan Press, 2004) 354 pp. $70.00

The *Kulturkampf*, the anti-Catholic campaign undertaken by Otto von Bismarck’s government in the 1870s, with the enthusiastic support of the liberal movement, remains a mysterious episode in modern German history. One of its most prominent mysteries is why Germany’s liberals, ostensibly committed to civil liberties and to opposing authoritarian state practices, would have been so enthusiastic about a public policy of reli-
religious persecution. In *The War against Catholicism*, Gross resolves the secret of liberal attitudes by reading these attitudes as cultural texts, which he deciphers and deconstructs. Investigating a wide variety of primary sources, ranging from public and parliamentary speeches to private correspondence, to pamphlets and popular periodicals, he uncovers a persistent cultural pattern of liberal anti-Catholicism. His work thus illuminates an entire little-studied dimension of the Kulturkampf, although its sometimes exaggerated interpretations demonstrate some of problems with the cultural deconstruction of historical sources.

Gross argues that German liberals developed anti-Catholic attitudes in an intellectual reorientation following the revolution of 1848. Having failed in their efforts to overturn Germany’s authoritarian regimes, post-1850 liberals increasingly perceived the Catholic Church as the great obstacle to the creation of a modern, progressive, scientific central Europe. Their ire was particularly concentrated on the Jesuits, who, in the two decades after mid-century, carried out a vigorous and successful campaign of reviving and renewing Catholic devotion. Gross also suggests that liberal attitudes toward Catholicism were gendered, involving a contrast between a manly, liberal Protestant world on the one hand, and a feminine, Catholic one, with its Marian devotion, rapidly growing numbers of nuns, and strong female piety, on the other. The Kulturkampf appears in his account as a gender struggle, and he asserts that liberal hostility to Catholicism was a form of displaced opposition to feminism.

These are all intriguing insights. Gross’ work is an important contribution to the history of the political imaginary in modern Germany as well as to the (often neglected) history of nineteenth-century anticlericalism. His book, however, also shows that reading texts as a cultural code requires a full grasp of contemporary culture, and careful attention to the chronology of its expression.

Gross’ account of liberal anti-Catholicism as a post-1848 phenomenon ignores the strong liberal hostility to the Catholic Church and the Jesuits before and during the revolution of 1848. Indeed, many of the liberal, anti-Catholic tropes that he investigates could be traced back to Martin Luther. On the other hand, one of his key pieces of evidence for a contrast between liberal modernity and Catholic backwardness, a number of articles in the popular weekly *Die Gartenlaube*, date from the years after 1866, that is, after the war between Prussia and Austria, widely perceived in Germany as a religious conflict between Protestantism and Catholicism. By interpreting a few pre-1866 articles, with pictures of ruins of old monasteries, as evidence of liberal relegation of Catholicism to medieval backwardness, Gross neglects the extent to which these images were a stock theme of a generally pro-Catholic Romantic art, exemplified, for instance, in the works of the celebrated German Romantic painter, Caspar David Friedrich.

Gross’ discussion of the Kulturkampf as liberal anti-feminism is particularly problematical. His assertion that the activities of female
Catholic religious were an example of feminism, to which liberals responded with hostility, involves an interpretation of nuns as feminists, devised by today’s historians, but hardly current to nineteenth-century contemporaries. Gross exaggerates the importance of a few letters by German liberals denouncing John Stuart Mill’s advocacy of woman suffrage; this issue was not even on the political radar screen in Bismarck’s Germany. German feminists of the time—they themselves largely Protestants and Jews, and no friends of the Catholic Church—did not call for it. Although Gross, following a number of other historians, points to the frequent use of the rhetoric of manhood in liberal discourse, he leaves open the question of whether this rhetoric was unique to liberals or was shared by other political groupings—by Germany’s Catholic politicians, for instance.

The War against Catholicism leaves a mixed impression. It successfully uses an innovative methodology to throw a good deal of light on political attitudes that are otherwise difficult to understand. The book also shows some of the weaknesses and problematical features of this approach, in particular the need to consider more fully the cultural context of the time when reading historical documents as cultural texts.

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**Hitler’s Police Battalions: Enforcing Racial War in the East.** By Edward Westermann (Lawrence, University Press of Kansas, 2005) 329 pp. $34.95

Westermann’s subject is the transformation of the traditional German police force (the Order or Uniformed Police) from protectors of domestic law and order to “political soldiers” dedicated to combating and killing perceived enemies in German-occupied regions. He carefully traces the persistence of right-wing feeling among police formations during the ill-fated Weimar Republic, and then their increasing militarization, along with the Nazification of their officer corps and mounting amalgamation with the ss in the early years of the Third Reich. These trends fostered a highly ideological organizational culture that demonized Bolsheviks and Jews and institutional norms that glorified hardness, obedience, and service to the “higher purpose” of defending national purity. By 1939, when the Nazi regime began deploying regular and reserve police units for “pacification” and population transfers in occupied Poland, the men enrolled had become, with few exceptions, effective, unquestioning, and brutal executioners. After the invasion of the Soviet Union in 1941, they meted out death to 1 million people, primarily Jews and other supposed “partisans,” but including prisoners of war as well.

The interpretive thrust of Westermann’s well-substantiated account is twofold. First, he seeks to debunk postwar legends about the role of
German police units on the eastern front by showing that they took part in both massacres and military operations on a massive scale; they did not return home in 1945 with clean hands. Second, he offers a subtly stated dissent from the reasons that Browning and Goldhagen each have advanced for the willingness of “ordinary” Germans to kill on such a scale day in and day out. Unlike Browning, Westermann does not think that peer pressure and group loyalty primarily explain what occurred. Even if these were the prevailing motives among the men of Police Battalion 101 who composed Browning’s Research sample, that unit’s reserve status and relatively high average age made it unusual among the murderous police formations on the eastern front. The great majority of these units comprised full-time career officers and volunteers drawn, according to the expressed wishes of the ss, from the most fervently Nazi age cohort—Germans born between the years 1900 and 1912—who were steadily inculcated during the 1930s with the doctrines and reflexes that the Nazi state’s “racial war” required. Yet, unlike Goldhagen, who also sees the policemen as driven by ruthless hatreds, Westermann defines these attitudes more broadly and depicts them as imparted by the Nazi regime—deliberately and gradually, through training and indoctrination—not embedded in German culture and merely expressed once Adolf Hitler ruled.

Although Westermann brings to bear insights derived from his own military experience and makes a brief foray into social psychology, via discussion of the pertinence of the famous Milgram experiment to understanding the behavior of German police units, this book is not really interdisciplinary in its methodology. Rather, the author blends the results of painstaking reading in vast archival and secondary sources into an analytical narrative to demonstrate anew the illuminating power of historical empiricism. But the central subject of the work—the mechanisms by which genocide operates—is currently a matter of intense interdisciplinary interest. Westermann’s fine book is of wide relevance.

Peter Hayes
Northwestern University

Vienna and the Fall of the Habsburg Empire: Total War and Everyday Life in World War I. By Maureen Healy (New York, Cambridge University Press, 2004) 333 pp. $75.00

In recent years, the historiography of World War I has moved from the trenches to the home front. Healy’s work is a noteworthy addition to the growing body of literature on the social and cultural impact of the war, and an equally valuable contribution to the long-standing debate

about the causes of the Habsburg Empire’s collapse. Healy traces the fall of the Empire during World War I from the perspective of everyday life in its capital city. She argues that the home front in Europe’s first total war, of which her book is a case study, was marked by civilian conflict in streets, shops, schools, and apartment buildings, chiefly over food and its fair distribution (9, 21). While Habsburg armies waged military campaigns on distant fronts, women, children and “left at home” men waged a protracted and socially devastating war against one another and state authorities that, by 1917, made Vienna nearly ungovernable. In this context, the study offers a penetrating look at the Austrian “idea of the state” (Staatsidee) and Habsburg citizenship by showing how ordinary men, women, and children, mostly German-speaking Viennese, conceived of Austria, as well as by showing who qualified as an Austrian citizen and who did not (Jews, Poles, and Slavs in general) in the Empire’s final years. In regard to the Jews, Marsha Rozenblit’s Reconstructing a National Identity: The Jews of Habsburg Austria during World War I (New York, 2001) complements Healy’s study in its elaboration of the development in Vienna of virulent antisemitism and blaming the Jews for acute food and housing shortages.

Healy’s “history from below” is innovative in two ways—its expanded definition of politics and its imaginative methodology. She maintains that the traditional view of “domestic” politics as the sphere of statesmen and politicians is much too narrow to grasp the social dynamic of daily life in war-time Vienna. Her approach to Austrian politics “centers on the feelings, emotions and actions of newly mobilized sectors of the population,” mainly women, who previously were excluded from participation in politics (19). The way in which these groups and individuals “negotiate, implement and enforce the competing claims that they make upon themselves and upon the whole” is central to her definition of politics (20). Viewed in that way, the state was no abstraction, but an arena “where civilians and state officials fought over resources, responsibility and the power to define the meaning of the war” (9).

Methodologically, the core of Healy’s source material consists of unpublished letters of ordinary people found in Vienna’s state and city archives. These letters to state authorities, to Emperor Franz Joseph, and to his successor Emperor Karl, as well as to the latter’s wife Empress Zita, and letters written between the military front and the home front discuss every aspect of the war experience. Numerous quotations from the letters illumine and enliven the text. The letters are augmented by newspapers, police reports on the mood of the people, other administrative accounts of the population’s words and actions and by published reports of various war-time agencies. The bibliography contains a complete list of primary and published sources, as well as a large number of contemporary published material and secondary works.

Healy draws two major conclusions from her analysis of the process of social disintegration in war-time Vienna. She comes down squarely on the side of domestic problems as the primary reason for the Habsburg
monarchy’s collapse. What she adds to that interpretation is a textured understanding of the home front. She maintains that “the state was discredited not only in the eyes of national minorities in other parts of Austria or in the minds of the weary troops at the front,” as other historians have shown, but also in the venues of everyday life noted above (300). Her interpretation is compelling but Healy should have paid a little more attention to the way in which developments on the military and home front interacted with, and reinforced, disintegrative tendencies.

The second conclusion is “that many of Vienna’s war-time conflicts were woven, unresolved, into the fabric of political life in the post-war period” (309). She suggests that historians rethink the place of the years from 1914 to 1918 in the periodization of Austrian history. It is not clear, however, where the chronological line would be drawn since she states that “preexisting [pre-1914] national and ethnic hatreds shaped Viennese reaction to material shortage” (301).

Quibbles aside, Healy’s study is meticulously researched, conceptually imaginative, and well written. It should be read by all those interested in the history of World War I and of the Habsburg Empire.

Solomon Wank
Franklin & Marshall College

Shifting Landmarks: Property, Proof, and Dispute in Catalonia around the Year 1000. By Jeffrey A. Bowman (Ithaca, Cornell University Press, 2004) 279 pp. $42.50

This book, intelligently and limpidly written, builds on several decades of scholarship on conflict resolution in early medieval Europe. It is a welcome harvest of the best insights on the topic, deployed in an analysis of judicial disputes in the Pyrenean province of Catalonia—the ecclesiastical province of Narbonne. As Bowman explains, this choice is warranted by a rare wealth of Catalan documents for the elsewhere usually document-poor tenth and eleventh centuries; it also allows him to enter the (mostly) French debate on the existence of a feudal revolution (henceforth FR), and convincingly so.

The FR thesis—which posits a violent transformation of society and institutions in most areas of Western Europe, with slightly different chronologies but all circa 1000 C.E.—has drawn much on Catalan evidence. In that region, it is asserted, an older order, based on the public power of counts and courts, that employed Romano-Visigothic law and favored the written proof broke down between 1020 and 1060. This period of anarchy saw a privatization of power, the demise of public courts, a recourse to the coercive appropriation of land, and a decline of legal adjudication in the favor of extrajudicial compromise brokered by informal bodies.

Bowman’s findings lead him to argue for some “gradual changes”
but in the main for “important continuities” without “dramatic rup-
tures.” Property disputes involved violence before and after 1020. Adju-
dications involved a written proof in 55 percent of recorded disputes be-
tween 980 and 1020; they did drop, but only to 40 percent, between
1020 and 1060. Counts, countesses, or viscounts presided adjudicating
bodies in 55 percent of the disputes from 980 to 1020, and 44 percent
between 1020 and 1060. Members of the professionalized class of judges
were present in both periods at about the same level, in 64 percent and
61 percent of cases, respectively. There was only a narrow increase in the
preference for compromise over unequivocal judgment. The major
change may be in the cohesiveness of the milieus involved: Tribunals
co-presided by a count (or countess) and a bishop, a hallmark of the ear-
lier era, became much rarer; so did the presence of a team of judges, as
opposed to a lone judge.

Bowman seldom draws explicitly on disciplines other than history,
possibly because the terms of the debate on the FR already owe much to
an older conceptual trunk shared by history and the social sciences that
has posited, since at least Coulanges, an antithesis between law and rit-
ual.1 Advocacy and dissent have thus run in a fairly predictable course
around the concept of “ritual”: In the FR model, the violent breakdown
of the old order and the weakening of Catalan public institutions were
correlated with a rise in “irrational” and ritual forms of proof. Con-
versely, critiques of the FR have drawn from structural-functionalist an-
thropology to argue that weak public institutions do not entail general-
ized violence, and to demonstrate how peace making works under such
conditions. Bowman has tried to show that liturgical penal clauses (“rit-
ual” maledictions and curses upon those who would go against the pro-
visions of a legal grant or settlement) constantly coexisted with legal pe-
nal clauses, and had comparable force.

The relationship between literacy and orality also constitutes an im-
portant theme in this book: Bowman demonstrates that throughout the
period, and the crisis from 1020 to 1060 in matters of law, oral and writ-
ten were inextricably intertwined. Disputes combined oral testimonies
and written charters. Institutions took care to prepare for potential chal-
lenges to their rights by periodically having their documents inspected
and read aloud to create human “aural” and visual memories that might
later be consulted if necessary. Adjudicating bodies had a slight prefer-
ence for the written proof, but by no means did it systematically trump
solid human witnessing. Written and oral testimonies were equally open
to challenges and interpretations. In the historiography of the European
Middle Ages, the issue of orality is often discussed with reference to
Vansina; Bowman chooses not to re-engage the Africanist ethnological
models.2 This decision is in conformity with his delicate, understated

1 Numas-Denys Fustel de Coulanges, La Cité antique. Étude sur le culte, le droit, les institutions
2 Jan Vansina, De la tradition orale. Essai de méthode historique (Teruven, 1961).
style. But it is this reviewer’s feeling that his excellent scholarship, and medieval history in general, can enrich sister disciplines and extra-European fields.

Philippe Buc
Stanford University

*Enrico Dandolo and the Rise of Venice.* By Thomas F. Madden (Baltimore, Johns Hopkins University Press, 2003) 298 pp. $49.95

Madden notes that the careers of Dandolo and his immediate ancestors parallel the trajectory of medieval Venice itself, and so, in the first half of the book, he can use the history of the family as a means to look at Venice’s rise to prominence. He also notes that Dandolo’s role in the Fourth Crusade has been surrounded by myth, surmise, and polemic, rather than hard study. The remainder of the book lays out the narrative, refutes past inaccuracies, and offers new readings.

Early members of the family followed a *cursus* typical of those destined for nobility. Their life events included such familiar phenomena as the rise of long-distance commerce, the ethics of state service and religious patronage, increasingly strained relations between Venetians and Byzantines, and construction projects that hastened the transformation of Venice “from an archipelago of independent family enclaves into a unified city” (6). The doge’s uncle, patriarch of Grado, was a major force for Gregorian reform. Father Alvise Dandolo’s career, in turn, illustrates the transformation of the state, with an incipient oligarchy seizing power from doge and popular assembly alike. None of this information is surprising, but Madden provides an unusually lucid and thorough account of both internal dynamics and the fearsomely complex interplay of Venetians with Byzantines, popes, Normans, Muslims, and Germans.

Dandolo’s early career took place amid further reform, most notably the election of the doge by less numerous panels of prominent men and the emergence of legislative councils. Once elected doge, Dandolo accelerated change: Legal reform, both in codification and reorganization, laid the “cornerstone” for later systems (108); overhaul of the coinage, especially the introduction of the *grosso*, took major steps toward establishing the prestige and stability of Venice’s money; and the Republic embarked on a vigorous foreign policy. Even had the Fourth Crusade never taken place, Dandolo’s would have been one of the most momentous reigns in Venetian history.

His place in the larger scheme of medieval history will always rest with his role in the diversion of the Fourth Crusade. Madden sets himself the task not of justifying Dandolo’s actions, but explaining them and putting them into context. He argues that the crusade turned disastrous because of bad decisions by many people, coupled with a generous amount of sheer misfortune. Wildly overoptimistic ambitions drove
some decisions; duplicity drove others. Many were simply the best choices among grim alternatives. Dandolo was, at most, one of many leaders—within Venice, subject to constitutional restraints, and, outside Venice, flanked by Frankish barons. Nowhere does Madden see a grand scheme, by Dandolo or anyone else, but rather a succession of expedient choices trying to make the best of an increasingly bad situation. In the end, no one—Venetians, Franks, prelates, or Byzantines—emerges unscathed, but at least Dandolo is spared a preponderant share of guilt.

Madden’s efforts to spread the blame will inevitably displease those whose favorites are now assigned a share of it. But even if they were inclined to do so, partisans would find the task of denying Madden’s interpretations difficult, given the massive documentation and careful analysis that he brings to bear.

James S. Grubb
University of Maryland, Baltimore County


Many attempts have been made to shed light on Vatican finances, but all come up against the paucity of crucial documents in the Vatican archives. At the time of this book’s writing, those archives were not available for documents more recent than 1921, and, even for the earlier period covered by this book, which focuses on the 100 years from the mid-nineteenth to mid-twentieth century, Vatican archives notably lack records of key financial dealings.

Pollard has done a commendable job in tracking down available sources from state archives—from Italy to Britain to the United States—from various Italian bank archives, and from the archives of an American and a British archdiocese to shed new light on Vatican finance. He also makes good use of the diary of Bernardino Nogara, financial advisor to Pius XI, in illuminating Vatican finance in the contentious 1930s. The author’s ambition is great; his intention is not simply to trace such evolving financial arrangements but to make the case that the changing sources of Vatican income had a significant effect on papal and Church religious and political policy.

The story that the book tells is of the evolution of a Holy See supported largely by a semifeudal system of landholding in the Papal States into a Vatican that has vast financial holdings throughout the world. Thus it is the story of how the Church came to terms with modern capitalism, a fraught relationship not without its elements of contradiction. Pollard argues that the post-1870 (the year the papacy lost Rome to Italian forces) rapprochement of the Church with the new Italian state—which it would not recognize until 1929—was aided greatly by the Vati-
can’s involvement in the Banco di Roma, which brought the Church elite into close working relationships with the patriotic Italian elites. More controversially, Pollard argues that it was Benito Mussolini’s offer to rescue the Banco di Roma from a financial crisis that led Pope Pius XI to abandon the Catholic Popular Party, and its leader, Don Sturzo, and deliver Italy to the full force of the fascist dictatorship. Likewise, he argues that one of the major reasons for Vatican support of Mussolini through the 1920s and 1930s was the pope’s view that the Italian dictator would best protect the Vatican’s financial interests. These are not implausible arguments, but Pollard does not produce the evidence that would convince a skeptic of their veracity.

There are a handful of small errors in the book: La Civiltà Cattolica published its first number in 1850 not 1854 (8); Pius IX died in February, not January 1878 (53); the police chief of the zone around the Vatican in the late nineteenth century was Giuseppe, not Guido, Manfroni. But Pollard’s scholarship is impressive. He tackles material that most Italian historians regard as important but, because of its sometimes arcane economic and business nature, most of them try to avoid.

Those seeking to understand the transformation of the Roman Catholic Church during this period—when the pope became an object of popular adulation and established absolute power over the far-flung national churches—will benefit greatly from reading this book. The story of a center of moral authority that became one of the world’s largest financial holding companies offers more than one lesson.

David I. Kertzer
Brown University


In scope and content, this work is a case study of relations between the Russian “center” and non-Russian “periphery” of the Russian Empire (including its Soviet and post-Soviet incarnations) from the 1890s to the 1990s. It looks particularly for continuities and novelties in the two-way relationship between “state culture,” centered in the European capital cities of St. Petersburg and Moscow, and the regional culture of the Evenki ethnic minority, scattered among the remote forests along the Podkamennaya Tunguska River—a tributary of the Yenisei River. The book examines the center’s objectives in trying to impose its values and institutions on the Evenki during the reign of three governments (Tsarist, Soviet, and Russian Federation), and it also examines the ways in which the Evenki adapted to various aspects of “state culture.” Much of the book explains how Soviet state policies transformed Katonga—an uninhabited storage point in 1917—into a town by organizing the population into a “primitive production unit” (1930–1938), a
“collective farm” (1938–1967), and a “state collective farm” (1967–present). The Soviet government achieved its objective of creating a stable populated town from which it could rule and through which it could distribute and collect information and material. But, while the regime sought to homogenize the Evenki, at least superficially, into “the new Soviet man,” the regime’s practice of using the ethnicity of a regional population as the major means of determining its relationship with the center (including establishing the boundaries and name of the administrative subdivisions of the Soviet empire) further strengthened some of the characteristics of Evenki traditional culture.

In the post-Soviet period an infusion of multinational, non-Evenki “newcomers,” especially former prisoners in concentration camps and workers fleeing the resource-extracting industries that once functioned in inhospitable northern Siberia, increased the size and ethnic complexity of Katonga.

The study is based on extensive field work among the Evenki. In 1988/89, as a researcher at the Moscow Institute of Ethnography, the author lived among the Evenki with a family of fur hunters and reindeer herders. He considered the Evenki “an enclave of ‘traditional lifestyles’ surviving at the forest margins of the socialist state” that were “less affected by the Soviet policies of forced collectivization of the 1930s and the villagization of the 1960s” (16–17). He returned to the same area in 1993–1995, living with another family of fur hunters and reindeer herders related to his previous hosts.

The author places his observations against a vast literature that ranges from the classics in social theory through sociology, anthropology, and ethnography, some of it in specialized journals published by Russian research institutions. Particularly interesting is his use of comparative studies of center-periphery relations in other ethnically diverse states, such as Indonesia.

Although the book has its fair share of terms with specific meanings in sociology, social theory, ethnography, and anthropology, its main ideas will be clear to specialists in other branches of the social sciences. It complements other works in the rapidly expanding literature in English on Soviet policy towards Siberia. For examples, this study complements the two-volume study by James Hughes—_Stalin, Siberia and the Crisis of the New Economic Policy_ (New York, 1991) and _Stalinism in a Russian Province: A Study of Collectivization and Dekulakization in Siberia_ (New York, 1996)—as well as Fiona Hill and Clifford Gaddy, _The Siberian Curse: How Communist Planners Left Russia Out in the Cold_ (Brookings, 2003).

Charles E. Timberlake
University of Missouri, Columbia
Those seeking to study the Soviet experiment during most of its existence had access to only a very limited supply of what could be conventionally described as primary sources. Information from a handful of defectors, and the occasional significant government document published abroad or in samizdat (the underground Soviet press) by such dissident scholars as Roy and Zhores Medvedev and Alexander Solzhenitsyn, constituted the basic raw material for our understanding of the often surreal world of Joseph Stalin and his successors. There was, for all meaningful purposes, no such thing as “archival access.”

With the advent of Mikhail Gorbachev’s policy of “openness” (glasnost), the situation began to change, for a while at an almost dizzying pace. Russian and FSU (former Soviet Union) scholars and groups like Memorial—an organization built around former dissidents and relatives of the victims of Soviet, and especially Stalinist, “repression”—took to the archives with a zeal rare among their “free world” colleagues, who soon tried to join the “gold rush” to the new wealth of sources. The results were—and continue to be—astounding. Yet, by the end of Boris Yeltsin’s presidency, the Old Ways began to reassert themselves, and the archives starting shutting down again. This trend greatly accelerated under Vladimir Putin, and today the plight of would-be researchers into things Soviet is not dissimilar to that of their counterparts not so long ago.

Yet a vast outpouring of literature remains from that brief flowering of the archives, including books like Murphy’s *What Stalin Knew*, which constitutes an important effort to make accessible to a much wider audience a wealth of archivally derived material produced by Russian researchers formerly unavailable in English. (Murphy could have made his debt to the research of others more explicit.)

Therein lies the primary value of the book, since it does not offer any major new interpretations. Murphy’s basic argument is already familiar: A plethora of intelligence starting at least during the summer of 1940 provided the Soviet leadership with increasingly accurate and complete information about “Operation Barbarossa,” the planned German invasion of the Soviet Union, though Stalin dismissed it as an “English provocation.” Murphy’s greatest contribution is to complete this picture with unprecedented and systematic detail. He thoroughly mines the published Russian, and occasionally Western, literature to describe intelligence provided by such diverse sources as the Red Army’s Intelligence Directorate, the NKVD/NKGB secret police and foreign intelligence sections, diplomatic sources, railroad reconnaissance troops, and foreign governments, including Great Britain and the United States.

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1 Murphy’s most important source seems to be Aleksandr N. Yakovlev (ed.), *1941 god* (Moscow 1998), a multi-volume collection of archival documents pertaining to the start of Operation Barbarossa.
With respect to the larger issue of the historiography on Operation Barbarossa, however, *What Stalin Knew* is less impressive. The central questions today are who exactly had access to this information (recent research, supported to some extent by Murphy’s evidence, seems to indicate that this elect group may have been significantly larger than previously thought, perhaps even including the celebrated Georgi Zhukov), and, more importantly, why did Stalin refuse to believe this overwhelming mass of information? Murphy, although he provides the occasional valuable tweak that only a trained ex-intelligence officer could, especially in his analysis of the operation of espionage networks and of German disinformation efforts, does not shed much new light on either of these questions.

The problem for historians with respect to Stalin’s behavior in the spring and summer of 1941 is that they are ultimately trying to solve an essentially psychological puzzle. Either Stalin was simply irrational (which seems to be Murphy’s interpretation), or the ever-scheming but deluded Stalin thought that he ultimately could make German intentions irrelevant through a preemptory attack (the so-called “Icebreaker” thesis) or a crushing counter offensive, either of which would spread Communism to the rest of Europe. The important point is that this question is at the heart of the historical debate about Barbarossa now, not whether the Soviets should have seen it coming.

Given the limitations described above, however, Murphy’s book may stand as the definitive one-volume treatment of the subject of “what Stalin knew.” Despite a few quirks (using a novel as a historical source, for example), and a narrative that may be confusing for readers not familiar with intelligence operations, a book that fully accomplishes what it promises to do on the cover must be judged a success.2

Raymond W. Leonard
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*Encyclopedia of the Great Plains*. Edited by David J. Wishart (Lincoln, University of Nebraska Press, 2004) 919 pp. $75.00

The Center for Great Plains Studies has produced a broadly conceived and wide-ranging reference work that brings together a multitude of disciplinary perspectives on a single region. Wishart has assembled twenty-seven topical chapters, from “Agriculture” and “Architecture” to “Literary Traditions,” “Music,” and “Water.” Chapters open with introductory essays of five to seven pages by an expert in the field and then

2 The novel in question is Igor Bunich, *Groza: Piatosotchnaia Voina v Rossy* (Moscow 1997). Murphy observes that it is a “historical novel” and that “much of this work is based on verifiable events and documents. Other elements are products of the author’s vivid imagination” (287, n. 12). Unfortunately, nowhere does Murphy explain what those “verifiable documents” are; nor does he indicate which parts are “products of the author’s vivid imagination.”
offer alphabetical encyclopedia entries, each signed by its author. Most chapters include forty to sixty entries. The boundaries of the Great Plains in this context follow the front range of the Rockies, from Roswell, New Mexico, through Denver and Cheyenne, and swing west to include Billings, Montana, and Calgary and Edmonton, Alberta. The eastern boundary, always more problematic, captures Winnipeg before dropping southward along the eastern border of the Dakotas, Nebraska, and Kansas and then swinging west again through Tulsa, Oklahoma City, and Fort Worth. Omitted from the formal Great Plains are tallgrass prairie portions of Minnesota, Iowa, Missouri, and eastern Oklahoma. The editorial staff has done an excellent job of addressing the multinational aspects of the region, with full integration of both Canadian and U.S. portions of the Great Plains and consistent inclusion of Native Americans.

The core value of this encyclopedia lies in the quality of the more than 1,300 individual entries by authors who are often well published on the topics that they summarize. Great Plains scholars will recognize Thomas Isern, Dan Flores, John Hudson, John Opie, R. Douglas Hurt, Leslie Hewes, and Allan Bogue, to name a few historians. Younger scholars, such as Pekka Hamalainen and Theodore Binnema, are represented as well. The able entries identify the two or three best publications on their topics. Though generally authoritative, entries are not always easy to find. The topical chapter organization does away with a conventional encyclopedia’s A–Z arrangement, making it cumbersome to locate some entries. Where is the entry for *Brown v. The Board of Education of Topeka*? Is it in “African Americans,” “Law,” or “Protest and Dissent”? Solving this problem requires a search through the volume’s index. Cross-references within chapters help, but two searches are often necessary to find one entry.

According to the work’s introduction, the thematic organization brings an interpretive function lacking in most encyclopedias. This ambition is best realized in Michael Conzen’s stellar essay introducing the “Cities and Towns” chapter and in Bret Wallach’s nuanced, interpretive, and remarkably readable summary of the “Physical Environment.” The editorial effort to guide Great Plains scholarship is most evident in the five chapters focused on racial and ethnic culture groups (“African Americans,” “Asian Americans,” “European Americans,” “Hispanic Americans,” and “Native Americans”) and in the chapter entitled “Gender.” Wishart says, “In doing this, we wanted to emphasize the contributions of these peoples—contributions that have often been overlooked—to the shaping of the Great Plains” (xi). The effort is only partially successful. The introductory essay by Malcolm Yeung and Evelyn Hu-DeHart is not likely to convince skeptics that Asian Americans merit a chapter of their own. It reports that Chinese populations peaked in 1890, when 224 Chinese lived in Nebraska. The 2000 Census map shows only 11 of some 500 U.S. plains counties with more than 3 percent Asian Americans, and the chapter includes only 21 entries.
The chapter on “European Americans” suffers from another problem. Aidan McQuillan provides a strong and useful introductory essay about European immigrants to the Great Plains and their lasting cultural imprints. The chapter contains entries about Czechs, Danes, Germans, Icelanders, etc., who moved into the region during the late nineteenth and early twentieth centuries. What is omitted is any significant discussion of Euro-American immigrants from the East. The hundreds of thousands of families who moved into the plains from eastern states and provinces are completely neglected. The few hundred African Americans who migrated from the Old South to Kansas after the Civil War receive careful treatment, but not the multitudes of white southerners who populated Texas, Oklahoma, and southern Kansas; they don’t fit into any of the five cultural categories. The chapter “Gender” is well justified, as Deborah Fink’s useful historiographical essay makes clear, but it is misnamed. Despite Fink’s comment that “Gender doesn’t mean women,” the list of entries is almost exclusively about women (322). The chapter does not, however, correct the assertion in Yeung and Hu-DeHart’s essay that the Chinese in the Great Plains “did the vast majority of the region’s laundry” (135)!

Some choices for thematic chapters seem odd. Many entries in the “Film” chapter are devoted to actors who happen to have been born in the Great Plains (like Demi Moore and Lon Chaney), but who have no other apparent connection to the region. (This problem of “a celebrity was born here” shows up in other chapters too; Malcolm X gets a long entry and photo in “Protest and Dissent,” although his connection to the plains lasted for only the first year of his life.)

The high quality of the introductory essays compensates for obstacles created by the book’s thematic approach. This interdisciplinary and authoritative encyclopedia will be of considerable value to scholars and to the broad public. It presents the current state of scholarship in a dozen disciplines on this region at the heart of North America.

Geoff Cunfer
University of Saskatchewan

The “Conquest” of Acadia, 1710: Imperial, Colonial, and Aboriginal Constructions. By John G. Reid, Maurice Basque, Elizabeth Mancke, Barry Moody, Geoffrey Plank, and William Wicken (Toronto, University of Toronto Press, 2004) 297 pp. $60.00 cloth $29.95 paper

Less an interdisciplinary work than a multiperspectival one, The “Conquest” of Acadia adopts a Braudelian approach to the relatively little-known, little-studied event at its center. Most immediately, the book recounts the event itself; second, it explores the conquest as a conjunctural phenomenon that revealed a larger transition in Acadian experience; and third, it seeks to place the conquest of Acadia within the framework of
the *longue durée* of imperial enterprise and intercultural contact in the Americas. Thus, the authors argue that “there is no single valid narrative” of the events of 1710, and the volume is made up of a series of linked essays exploring them from a variety of perspectives (xi).

This is an ambitious undertaking that succeeds in its aims, though it suffers from some of the difficulties characteristic of both multiple-author works and multilayered analysis. Most importantly, Reid and his co-authors offer rich explorations of Acadian social and political history that place the conquest of 1710 in illuminating contexts. Mancke and Reid use the episode as a point of departure for a wide-ranging comparison of French and British approaches to empire and argue that the French hold on Acadia was tenuous because of the relative weakness of the French creole elite. Basque contends that Acadian residents responded to English conquest in complex ways, conditioned in part by the close ties that many local elites had to New England’s maritime economy. Plank illuminates this relationship from the opposite direction by showing the strong interest that many New Englanders had in Acadia. Wicken argues that although the Mi’kmaq initially did not show much concern about the conquest of 1710, they gradually learned that British sovereignty brought greater pressures to bear on their independence. Moody traces the largely failed attempt to create British settlements that might have dominated, or at least blended with, the region’s French population. The authors conclude that the conquest of Acadia created a “fragile equilibrium” among the local populations that is characteristic of much North American history, and indeed of many early modern empires (207).

This bare summary hardly does justice to the book’s many rich insights: The nature of early modern empires, the comparison between French and British approaches to colonization, and the particular challenges of administering Acadia/Nova Scotia are all treated insightfully. Yet it also illustrates the principal shortcoming of the volume. Despite the authors’ extensive collaboration (“no mere collection of essays” but “a coordinated effort to portray a multilayered reality”), the book has a tendency toward fragmentation that is inevitable in any work with six authors (xi). Moreover, the authors’ approach produces another kind of fragmentation, a product of their debt to the *Annales* school. Although each level of analysis offered makes sense on its own terms, the levels remain discrete; for the most part, the insights achieved on one level do not substantially shape the approaches or findings of the other levels. Nonetheless, the book is obviously the definitive work on the conquest of 1710; more generally, anyone interested in the history of early modern empires will benefit enormously from its analysis and insight.

Eric Hinderaker
University of Utah

To paraphrase the ancient sage, of the making of books about the Library of Congress, there is no end. Although many such volumes have been exhaustively descriptive, lavishly illustrated, anniversary-inspired celebratory works, comparatively few have examined the institution in its socioeconomic, political, and cultural contexts. Ostrowski has breathed life and context into a narrative that brings his subject into the heart of American history—and, more broadly, American studies—as well as the smaller, but robust and related, sub-disciplines of library history and book history.

In six substantive chapters, in addition to a succinct introduction and conclusion, the author traces the personalities, prevailing attitudes, scholarly and literary moods, and national ambitions that shaped the degree of support and the direction that Congressional leaders gave the Library. A thorough scholar, Ostrowski has mined the primary manuscript and printed archival sources, contemporary periodical literature, and the widest spectrum of secondary literature—material that others have missed in their exclusive focus on providing institutional chronicles. Whether he is discussing the various librarians and their roles or Congressional committee members, he provides carefully argued and plausible explanations for important decisions regarding collection emphases and financial support. These reasons frequently enhance commonly accepted notions.

Several themes recur throughout the monograph, including the status of the Library in relationship to other libraries in the capital and the nation, the vision of influential members of Congress about the Library, and the qualities that individual librarians brought to the institution. The conflicting ideological directions of early republican vision, Whig mentality, Jacksonian democracy, and the tensions of the pre-Civil War years all reveal forces that shaped the Library. The tenure of Librarian John Meehan (1829–1861), the longest of those under study, was a period of limited scope and function compared to the practices of those who came before and after him. His term also witnessed the struggle for definition that engulfed the Smithsonian Institution and its library in relation to the Library of Congress. A national library befitted a new nation, but the Smithsonian library, as Charles Coffin Jewett envisioned it, would not be it if Joseph Henry were to have his way, and he did. Neither did James Alfred Pearce, Congressional Joint Library Committee member and Smithsonian regent, allow the Library of Congress to move toward that status. In fact, the evolving goals that Pearce, a Southerner, had for these libraries were critical for both institutions. Only with the end of the Civil War and the appointment of Ainsworth R. Spofford did changes begin to occur.

Two examples demonstrate the multidisciplinary approach em-
ployed by the author. An illuminating analysis of four Library catalogs that appeared in this period portrays the changing collection’s strength and balance of subjects. A study of accession and circulation records indicates the actual use made of the Library during the 1840s and 1850s. These examples represent a wedding of methodologies fruitfully employed by other scholars.

This study is a challenging example, showing that the history of libraries is not antiquarianism but a fertile field for testing cultural hypotheses or themes that are multidisciplinary in nature. It is a model well worth studying and emulating.

Donald G. Davis, Jr.
University of Texas, Austin

The Confederacy on Trial: The Piracy and Sequestration Cases of 1861. By Mark A. Weitz (Lawrence, University Press of Kansas, 2005) 219 pp. $29.95

Because of the difficulty of learning law and history, both highly specialized disciplines, the legal and constitutional history of the Civil War era have not flourished since Harold M. Hyman paved the way for the subjects more than thirty years ago in *A More Perfect Union: The Impact of the Civil War and Reconstruction on the Constitution* (New York, 1969). Therefore Weitz’s engagingly written book makes a particularly welcome addition to the crowded Civil War shelf.

Weitz discusses, on the one hand, two trials of Confederate sailors for piracy held in the North in 1861 and, on the other hand, the attempts of South Carolina lawyers in the same year to challenge the requirements of the Confederacy’s Sequestration Act, which allowed the Confederate government to seize the property of enemy aliens. Weitz has brought these trials together because they hinged on the status of the Confederacy as a nation-state with a central government of coercive power. Weitz’s method is to rely on rare printed and manuscript sources for the trials and to describe the arguments in them at considerable length, offering analysis along the way. He gives us flesh-and-blood lawyers and judges as well as clear explanations of legal and constitutional issues. “People and their stories are what distinguish constitutional history from the study of constitutional law,” Weitz says.

In the North the trials dealt with the legal status of Confederate sailors who captured Union commerce at sea: Were they pirates or prisoners of war? In the end, the decisions of the courts did not matter, and the Lincoln administration finally decided to treat the sailors as prisoners of war. The sequestration cases tried in Charleston dealt with difficult issues, involving the status of debt and lawyer–client privilege. The Confederate judge, Andrew G. Magrath, sided uncompromisingly with the Confederate government.
Weitz recognizes the importance of political-party affiliation in the mid-nineteenth century, noting the role of politics in the Northern cases (the Confederacy had no political parties), a reality too often slighted in constitutional and legal history. But he might have placed even more emphasis on partisan motivation. The lawyers seem less the heroes of civil liberty when seen as partisans. George Wharton, for example, a Philadelphia lawyer and Democrat who defended some of the Confederate sailors in 1861, a year later became a leading critic of the Lincoln administration’s suspension of the privilege of the writ of habeas corpus. But civil liberty really was not on trial in the piracy cases, which asked whether the Confederacy could be considered a nation in international law. What tied the issues together was Wharton’s Democratic hatred for the Republican administration, which tried (at first) to hang the pirates in 1861 and sweepingly suspended the writ of habeas corpus in 1862.

Attention to neglected legal and constitutional matters should not obscure the large policy questions involved. In a sense, the decision not to hang the sailors as pirates (coupled with the decision to blockade the expansive Confederate coast) allowed Confederate commerce raiders to drive the Union’s merchant marine from the seas. But these are minor criticisms of a solid book that is bound to make others look into the legal and constitutional history of the Civil War era.

Mark E. Neely, Jr.
Pennsylvania State University

From Property to Person: Slavery and the Coniscation Acts, 1861–1862. By Silvana R. Siddali (Baton Rouge, Louisiana State University Press, 2005) 298 pp. $44.95

In April 1861, Abraham Lincoln solemnly denied any intent to attack the institution of chattel slavery. His administration insisted that its sole objective was the preservation of the Union, and most northerners concurred. Yet from the outset, hundreds of slaves were used in direct support of the Confederate military effort. Millions more propped up the Confederate economy. Unsurprisingly, northerners became increasingly willing to confiscate slaves and other property as a means to cripple the rebellion and punish traitors. Between August 1861 and July 1862, Congress passed two bills ostensibly designed for that very purpose, but they were singularly ineffective. “They did not bring southern wealth into the Treasury, they did not materially aid the war effort, and they did not lead to the punishment of the southern rebels” (249). Nevertheless, Siddali argues, they were the vehicle for a useful debate in which politicians, opinion makers, and military men wrestled with some of the most critical legal/constitutional questions of the era. Through exhaustive research in public documents, private papers, and newspapers, Siddali meticulously recreates the ebb and flow of these discussions.
Because northern statesmen denied that the Confederate nation had any legal existence, they were obliged to treat the rebellion, in constitutional terms, as treason on a massive scale. By this logic, the rebels remained American citizens and were thus entitled to due process of law. One difficulty—never really resolved—was how to make court action a realistic system for the confiscation of property. Another, also unresolved, had to do with the status of confiscated slave property. A confiscated horse remained a horse. It simply went from one owner to the next. But a slave? Although a few conservatives contemplated government ownership of slaves, most moderates and all radicals understood that slaves were also human beings. It followed, then, that they were really confiscating not the slaves per se but rather their labor. The new owners of that labor were the former slaves themselves.

Logically, this interpretation might have introduced the question of civil and political rights for slaves freed through operation of the confiscation acts. But on this point, policymakers remained deliberately obtuse. “Few northerners credited black people with much enterprise or intelligence,” notes Siddali; “they also insisted that the confiscated slaves had to remain in the South, under some form of military or government control” (186). Many northerners, including Lincoln, favored the idea of colonizing freedmen to South America or the Caribbean. The radical senator James Lane of Kansas wanted the two races separated with “an ocean rolling between them” (112). It was no accident that the Second Confiscation Act authorized $500,000 to support a colonization program.

Large-scale colonization did not materialize, and the confiscation acts had scant practical impact. An 1867 Treasury department solicitor’s report showed that less than $130,000 was paid into the Treasury from confiscated rebel property. But that is beside the point of Siddali’s study. The debate itself, she maintains, is what mattered. By showing the clumsiness of congressional action as a weapon against enemy property, it reconciled northerners to the need for the major extension of presidential war powers embodied in Lincoln’s Emancipation Proclamation. By showing the hopeless tangle of tensions and contradictions within the old constitutional stance toward property and slavery, it laid the groundwork for the subsequent Reconstruction amendments. Moreover, by underscoring the extreme reluctance with which northern policymakers made provision for a future life for freed African Americans in the United States, the debate revealed the strength of racial prejudice in mid-nineteenth century America.

This book is as sure-footed and careful a study of the Confiscation Acts as we are likely to get. Along with Michael Vorenberg’s Final Freedom: The Civil War, the Abolition of Slavery, and the Thirteenth Amendment (New York, 2001), it is one of the most significant political and legal/constitutional works on the Civil War to appear in several years.

Mark Grimsley
Ohio State University
Edmund Morgan’s *The Puritan Dilemma* (Boston, 1958) posed the question that all people of principle must face, “the question of what responsibility a righteous man owes to society.” The answers, Morgan observed of seventeenth-century New England, were diverse. On the one hand were the come-outers (called “Pilgrims” back then), who would not compromise with “the world” and who remained pure—and ineffectual. On the other hand were the “Puritans,” who adopted a more realistic pose of “being in the world without being of the world.” They would transform Anglo-American culture into a messianic “City Upon a Hill.”

Curran’s comprehensive *Soldiers of Peace* is a nineteenth- and twentieth-century reprise of Morgan’s *Puritan Dilemma*. This time, the protagonists are not Calvinist Pilgrims and Puritans but “perfectionist” and “restorationist” northern pacifists who emerged during the Civil War, and received notoriety for their implacable opposition to it and to the draft that sustained it. As North and South moved ever-closer to total war, the perfectionist pacifists divided from the vast majority of their countrymen by asserting that war was a sin and not a “sacred crusade.”

Curran traces the roots of perfectionist pacifism to a radical “restorationist” tradition that drew a sharp distinction between the ends of Christianity as they interpreted it and the ends of American civil government. This tradition included some representation from such peace denominations as the Quakers and Mennonites, as well as the Shakers, but it was not itself a denomination. Nor were its members automatically draft-exempt as “conscientious objectors.” Many perfectionists shared their ideas through William Lloyd Garrison’s *Liberator*, but when Garrison came out in favor of a war for abolition (together with the nation’s largest peace organization, the American Peace Society), they broke ranks. Instead of looking forward to a millennial age, perfectionist pacifists looked backward for their model, arguing that the millennium had already appeared and that an individual committed to God’s laws of love and non-violence automatically became a citizen of God’s millennial kingdom.

In consummate detail, Curran traces how, after the Civil War, the perfectionist pacifists evolved into a formal reform organization, the Universal Peace Union (*UPU*), with the indomitable Alfred Love as their president for the next forty-seven years. Although focused on issues surrounding war, the *UPU* soon expanded to include other social ills involving labor and the inequality of women that stood between their present and a perfected future where all would live in peace.

Without Love’s energetic, even inspired, leadership and his fearless resistance to both war and conscription, the movement would have never grown. Indeed, to a significant degree, this history of “soldiers of peace” is a biography of Love and his single-minded campaign to stand...
for non-violence, no matter what the consequences. That his peace movement never really drew significant numbers during the war is less a reflection of passive leadership than the consequence of a civil war that was so total and all-encompassing that virtually no Americans wanted to derail the bloodbath.

The book is divided almost exactly into two parts—radical pacifists in the Civil War and radical pacifists in the postwar period until their demise early in the twentieth century. After the war, Love was able to increase the ranks of the upu from the hundreds to the low thousands, not a large number but a definite influence. With the support and participation of other peace and women’s activists, including Lucretia Mott, Julia Ward Howe, Joshua Blanchard, and Francis Willard, the movement found a voice under Love’s leadership.

The upu’s greatest post–Civil War issue was Native American rights and the reform of violent solutions. But by agreeing with the government that Indians had to become Christian if they were to be civilized, they fell into the same condescending trap as the more militant army officers who preferred to deal with the same problem through extermination. Again the conflation of restoration and millennialism limited widespread support. Ultimately, the movement could not outlive its leader. When Love retired and soon died in 1913, the movement foundered—a classic example of Max Weber’s notion that followers of “charismatic” leaders cannot sustain a movement once the leader has passed.

One ongoing frustration with this book is the relative absence of numbers. It is hard to know at any given time exactly how widespread support for the movement was or, for that matter, how many young male believers signed up to be conscientious objectors. Furthermore, the fact that the book’s voices belong almost exclusively to the author and Love raises an obvious question, although these soldiers of peace obviously had a radical take on the meaning of America, and the wars that it relentlessly pursued, what difference did they make? The surprise answer appears only at the end, not much. The movement was never strong, and after the passing of the founding generation, it had little ability to recruit. Unlike so many success stories in the historiography of social reform, the upu’s is a story of a failed idea.

The reason for its failure, Curran concludes, is the reformers’ embrace of too many social issues and, more important, their exclusion of all but like-minded believers: “The perfectionist pacifists fell victim to the very perfectionism that drove them” (202). Clearly the Puritan Dilemma lives in and for those who choose retreat rather than selective engagement; the result is always the same. Whether seventeenth-century Pilgrim or nineteenth-century Perfectionist, purity at the expense of power is a route to political irrelevance. But what is inevitably lost in the political realm is not always lost in the moral. An uncompromised voice of peace continues to stand outside politics and for that, Love deserves to be remembered.

Harry S. Stout
Yale University

In Reforging the White Republic, Blum assembles an impressive array of material to rewrite the role of religion in the late nineteenth century. Rather than emphasize the work of the Social Gospel movement or immigrant diversification, Blum puts a spotlight on Protestant evangelicals, who formed the majority of American religious believers during this epoch. He argues persuasively that these Christians played a critical role in sectional reconciliation following the Civil War. However, this appeasement came at a cost: According to Blum, northern and southern Christians bonded over their racial self-categorization, using the language of white nationalism to ameliorate sectional discord.

Blum organizes his study chronologically, providing a timeline of ascendant whiteness. He begins with Abraham Lincoln’s dream of post-war racial harmony, and concludes with William E. B. DuBois’ gloomy appraisals of race relations. In between, Blum flits among intellectual and social-historical studies, evaluating key texts and movements, organizations and events. The effect is impressive. Blum’s text brings the northern landscape that dominates his Gilded Era survey to life. On one corner stands revivalist Dwight Moody, commanding Christian men to abandon social work and join the national fraternity of stout-hearted, manly, compatriots to Christ. On another corner is a newsboy, shouting Horace Greeley’s headlines goading citizens toward sectional peace. France Willard, national president of the Woman’s Christian Temperance Union (wctu), endorses the Christian virtues of white southern womanhood and calls for African-American repatriation to Africa. In bookstores, Harriet Beecher Stowe’s Palmetto Leaves (1872), Henry Ward Beecher’s Norwood (1868), and Josiah Strong’s Our Country (1885) describe a nation divided by race and Christian commitment, not by North and South. Perhaps most intriguing is Blum’s chapter on the yellow fever epidemic of 1878, which portrays northern and southern whites finding national community through their common prayers and shared avoidance of African-American suffering.

By the close of the century, Blum observes a wide range of white American Protestant tropes used in defense of white nationalism. From the novels of Thomas Dixon, Jr., to the imperial dreams of Theodore Roosevelt, white America had constructed an Anglo-Saxon identity for its emergent status as a reigning world power. Not coincidentally, this era overlaps with the greatest expansion in foreign missions; the intersection between American imperialism and American missionary work are the subject of Blum’s final chapter. Whiteness, godliness, and American nationalism were fused in an impenetrable shield, one which Blum believes has survived to the present day.

Imaginative, provocative, and expansively researched, Reforging the White Republic offers an important twist to surveys of Reconstruction-
era America. Rather than imagine religious belief as subservient to politics or industrial growth, Blum centers the Christian imagination as critical to the genesis of a national self-image. Like any book attempting such a large-scale revision of the American imagination, this one has a few documentary gaps. For example, although the WCTU clearly propagated a message of Christian righteousness alongside a vision of national cohesion, evidence that Willard and her compatriots summarily endorsed an exclusionary white identity is scarce. Likewise, Blum’s chapter on the yellow fever epidemic provides ample proof of the sectional reconciliation accomplished during that tragedy; however, the materials supporting a thesis of triumphal whiteness are thinner. In short, perfect connections between Christian cohesion and white nationalism are not often available. When Blum describes “the vast majority of white Protestants” as “largely anti-Catholic, anti-Spanish, racist, commercially minded, and militant,” he invites quick criticism and important conversation (226). The strongest virtue of Reforging the White Republic is its argumentative audacity despite occasional lapses in proof.

Kathryn Lofton
Reed College


This book is a welcomed addition to the growing literature on the history of U.S. financial markets and institutions. Previous studies of the thrift industry largely concentrated on the Savings and Loan crisis of the 1980s while ignoring or oversimplifying the industry’s earlier history. Mason’s account gives short shrift to thrifts’ nineteenth-century roots (one of ten chapters), but it carefully details the industry’s entire twentieth-century history.

Mason’s story is important because few scholars yet appreciate the crucial role that finance played in the development of the U.S. economy and way of life. By pooling the savings of numerous small investors and making home mortgages, thrifts provided vital intermediation services that allowed a large percentage of American families to purchase homes. Besides helping to stabilize the country politically, the easy availability of mortgage credit helped to increase the efficiency of the construction industry, a crucial component of the economy. Sadly, to this day, people in many countries, including Brazil and Russia, cannot easily borrow on mortgage. They build their houses over the course of many years, one inefficient brick at a time.1

The book is also a joy to read. A former banker, Mason effortlessly explains complex financial concepts, including the theory of asymmetric

information and asset-liability matching, without resorting to the use of technical jargon. Though written for the generalist rather than the specialist, the book is so thoroughly researched that even financial historians will learn much from it. To create his narrative, Mason deeply mined government documents, including materials in five presidential libraries. He also conducted ten interviews and thoroughly scoured thrift-industry publications.

A wide range of scholars should find the book useful. Political scientists and public-choice economists, for example, will appreciate Mason’s careful treatment of the changing role of government regulation in the development of the industry. Thrifts clearly “captured” regulators. Except in the aftermath of crises in the 1890s, 1930s, and 1980s, the industry’s lobbyists effectively blocked legislation that they considered harmful. As a result, thrifts long enjoyed preferential tax treatment and less stringent regulations than commercial banks did. Importantly, Mason’s account is well-balanced ideologically. He calls each episode as he sees it rather than pandering to Harvard’s statists or Chicago’s libertarians.²

No book is flawless. Mason’s treatment of America’s early nineteenth-century financial system is misleading, though, to be fair, my works on the subject were published only recently or are still forthcoming.³ More importantly, Mason avoids direct discussion of a topic of major interest, the role of the mutual form in the financial-services sector. Mutuals—corporations owned by their clients rather than by a separate class of stockholders—traditionally dominated the building-and-loan, savings-bank, and life-insurance industries. Though the dominance of mutuals waned in the 1980s and 1990s, many analysts still believe that mutuality provides companies a distinct advantage in certain realms of finance, including community banking and whole life insurance.⁴

Despite those shortcomings, Mason’s well-researched, well-written book will interest anyone, lay or specialist, interested in the development of the U.S. financial or regulatory systems.

Robert E. Wright
New York University

⁴ For a recent treatment of the importance of mutuality, see Wright and George D. Smith, Mutually Beneficial: The Guardian and Life Insurance in America (New York, 2004).
In this insightful and gracefully written study of white American men’s and women’s fascination with western mountain ranges, Schrepfer uses letters, memoirs, and diaries to establish differences between male and female tropes regarding the outdoors. She especially features the Sierra Club and California peaks, with an occasional reference to Colorado and the Pacific Northwest. Beginning in the 1860s and ending in 1964, Schrepfer shows how gendered views of western wilderness, which she calls the masculine and feminine sublimes, eventually shaped environmental policy.

Although Schrepfer argues that during the nineteenth century, “botanical studies shaped feminine visions of wild nature, whereas geology, in particular, underscored masculine perceptions,” this discovery is not new (6). Historians of the westward trail, as well as environmental historians, have long observed this difference. Far more interesting is Schrepfer’s assertion that men surged into western landscapes less as a reaction to the growing feminization of family and work than from their own confidence in carrying white—and increasingly feminized—civilization into mountainous wilds. One example of this motivation was men’s descriptions of mountains in terms of the female body. Others were men’s penchants to name lakes and mountains after women and their willingness to include women in even the most dangerous expeditions.

Schrepfer’s study is perhaps most valuable regarding the twentieth century. She demonstrates that male and female ethics changed due to such factors as world war and industrialization. Social norms urged men to be manly and aggressive, whereas women were to dedicate themselves to “self-sacrifice, motherhood, and service” (182). Accordingly, men seized leadership positions in the environmental movement, whereas women worked largely in such grassroots organizations as garden and women’s clubs.

Schrepfer focuses primarily on white, middle-class, mostly eastern men and women. She offers no discussion of why or how they came to dominate the environmental movement. Schrepfer mentions that environmentalism was a form of colonialism in that it displaced sheep-herders, but has far less to say about the dislocation of native peoples. An application of postcolonial theory in this context would add nuance and depth to her understanding of gender.

From the viewpoint of a western historian, Schrepfer also needs to develop a sense of regionalism. For one thing, the tension between easterners who reshaped western lands and land policy needs exploration in relation to gender. Another ignored area is the deviation between eastern and western conceptions of masculine and feminine behavior. And what of the South? What role did southern men and women and south-
ern constructions of gender play in the growing environmental movement? Certainly, a rich literature exists on regionalism and on gender in the West, East, and South.

Despite these suggestions, Schrepfer’s book offers a wealth of information and analysis regarding historical and contemporary environmentalism. Although not conceived as an interdisciplinary work, the study smoothly combines such approaches as women’s history, environmental history, and male studies. As a result, it confirms the growing awareness that understanding the role of gender is crucial to understanding any historical event or movement.

Glenda Riley
Ball State University

*The Slaughterhouse Cases: Regulation, Reconstruction, and the Fourteenth Amendment.* By Ronald M. Labbe and Jonathan Lurie (Lawrence, University of Kansas Press, 2003) 331 pp. $45.00

The Supreme Court’s decision regarding the *Slaughterhouse Cases* in 1873 has been controversial ever since. Later cases relied on *Slaughterhouse’s* constricted reading of the Fourteenth Amendment’s Privileges or Immunities Clause was to cripple the plan of the Amendment’s leading framers—to require states to obey the Bill of Rights.¹ Most of the rights that the *Slaughterhouse* Court said were guaranteed to American citizens by the Privileges or Immunities Clause are bizarre—the right of citizens to protection on the high seas and in foreign lands, for example.

Blacks and Republicans were facing serious problems from 1866 to 1868, but not on the high seas or in foreign lands. Because *Slaughterhouse* helped to make the anti-Klan civil rights acts of the 1870s a dead letter, progressive scholars often see the *Slaughterhouse Cases* as a serious setback for civil and constitutional rights. Since the case approved a legislatively created monopoly slaughterhouse, conservative scholars fault the decision for its failure to protect economic rights.

This fine book by Labbe and Lurie reminds us about the other faces of the *Slaughterhouse Cases*. As Labbe and Lurie show in admirable detail, New Orleans slaughterhouses had been a public health and environmental disaster. Their book collects and analyzes contemporary medical thinking about the dangers of slaughterhouses in general and those in New Orleans in particular. It also demonstrates that efforts at more narrowly tailored health regulations had been a failure—in part because of the considerable political clout of New Orleans butchers.

Labbe and Lurie carefully trace the legislative struggle to provide for a single privately operated slaughterhouse, the nineteenth-century legal

environment that often (but not always) supported such an approach, the confusing welter of lawsuits attacking the “monopoly,” the court decisions, and finally the arguments and the decision in the *Slaughterhouse Cases* themselves. The book is a masterful examination of the intersection of law, politics, public health, and medical knowledge at the time.

The authors note the claim that in some respects the legislatively created monopoly slaughterhouse increased competition. The new slaughterhouse provided a venue where the less affluent butchers (black as well as white) could ply their trade without a large capital investment.2

Like all historical explorations, this book presents only a part of a multifaceted reality. The authors portray Samuel Miller, who wrote the *Slaughterhouse* opinion, as influenced by his medical background and as committed to preserving state power over ordinary economic questions. In contrast, Richard Aynes argues that Miller was fundamentally unenthusiastic about the constitutional changes—such as requiring states to obey the Bill of Rights—that many supporters of the Fourteenth Amendment envisioned.3

Labbie and Lurie see the *Slaughterhouse* opinion as consistent with footnote 4 of the *Carolene Products* case—leaving ordinary commercial matters to the political process.4 To them, *Slaughterhouse* supported broad governmental authority to regulate economic activity in order to protect public health, and so it did.

In stark contrast to *Slaughterhouse*’s constricted reading of the privileges or immunities clause, footnote 4 also suggested heightened scrutiny for those guarantees of the Bill of Rights that the Court had *finally* applied to the states under the Fourteenth Amendment’s due process clause. But in the years following *Slaughterhouse*, the Court again and again held that Bill of Rights guarantees did not apply to the states, citing *Slaughterhouse* as authority.5 That doctrine, combined with the Court’s subsequent embrace of the idea that rights secured by the Fourteenth Amendment limited only the states and not private actors (such as Ku Klux Klan members), robbed the federal government of power to protect the most basic constitutional rights of blacks in the South.6

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happily, this sort of “healthy respect for federalism,” which the authors see in *Slaughterhouse* (16), was not consistent with vigorous protection of basic rights for black citizens in the South. As the authors sometimes suggest, the problem with the decision was not the narrow holding, but the sweeping rationale for it.

Michael Kent Curtis
Wake Forest University


Lynching and capital punishment are linked. Both practices end in death, and reflect the public’s desire for retributive justice. Pfeifer takes this connection even further and argues that the rise of the death penalty in the United States allowed respectable middle-class Americans to assert a bureaucratic due-process rule of law over a rural and working-class sense of rough justice. This process began in the Northeast, where the rise of industrial capitalism and the triumph of the middle class early in the nineteenth century preempted any effort to establish lynching. Next, lynching ended in the upper Midwest, where many northeasterners settled and brought with them their bourgeois notions of law and order. By the late nineteenth and early twentieth centuries, the rest of the Midwest and the Mountain West had joined the ranks of respectability, followed by the Far West of the Pacific Coast. Finally and belatedly, the South abandoned lynching, which was most often used as a means of racial oppression, as middle-class modernists sought to lure capital investment and “to erect a new legal order that would replace communal retribution with efficient, regularized, and racialized legal executions” (139). In short, Americans substituted lethal justice for rough justice.

Pfeifer does what few other scholars have attempted. He brings together an understanding of the lynching of the South with the lynching of the West and Midwest. He analyzes lynching in seven states: Louisiana, Iowa, Wisconsin, Wyoming, Washington, California, and New York. Despite providing a comprehensive thesis that covers lynching in such disparate locations, Pfeifer does not fall into the trap of oversimplification. In his discussion of Louisiana, which has by far the greatest number of lynchings, he carefully demonstrates significant differences in how rough justice was used in different corners of the state. He demonstrates similar sophistication in his approach by dividing Iowans use of lynching between the northern and southern part of the state, and showing the regionalization of lynching in California. How many more local explanations for lynching might have been added had Pfeifer covered more states? Asking that he provide a neighborhood by neighborhood breakdown of every state would have been too much.

7 For example, Curtis “Bingham,” 652–655.
However, adding one or two more southern states to the database, especially an upper-South state, might have offered a more complete picture. Why study Iowa and Wisconsin and not add Virginia and Georgia? (That Wisconsin had fewer incidents than any southern state would have made it relatively easy to add, whereas a southern state would have meant at least a year more of research). Pfeifer only included New York, for which he described one lynching, as a northeastern counterpart to the rest of the country and a place that moved quickly and extensively to a reliance on the bureaucratic, scientific, and antiseptic death penalty (the electric chair).

Ultimately this book is more than a social scientific explanation of the relationship between lynching and the death penalty. It is also an indictment of capital punishment. By tarring (to use a mob metaphor) both lynching and the death penalty with the same brush of retributive justice, Pfeifer provides a powerful argument against having the state murder criminals. Pfeifer convincingly shows the correlation between the rise in executions in any given state and the decline in the number of lynchings. Further, he shows how, in many cases, states used the death penalty disproportionately against minorities in an effort to intimidate the lower class and placate the anxieties of the middle class. His epilogue briefly traces the recent constitutional history of capital punishment to suggest that even in the beginning of the twentieth-first century, we wield lethal justice the same way mobs once turned to rough justice.

Paul A. Gilje
University of Oklahoma


This book usefully reminds us that Americans have made lynching a metaphor for racism. The most famous example came in 1991 when Clarence Thomas, a conservative Republican and African American, accused Senate Democrats of trying to “lynch” him during his confirmation hearings for a seat on the Supreme Court. Prior to 1991, most scholars offered only case studies or statistical analyses of lynching, glossing the problem of defining the term. Into this void stepped Thomas. No scholarship existed to contradict his assertion. Academics have rushed to catch up ever since, including Markovitz, who begins his introduction with Thomas, devotes a chapter to Thomas, and concludes with Thomas. In some ways, Markovitz makes the Thomas accusation—lynching can be a metonym for all racial misconduct—his own. Markovitz despises Thomas and his conservatism, but his manipulation of lynching rhetoric inspired this book.
Chapter 1 interestingly looks at the fight between opponents and apologists of lynching about its meaning. Chapter 2 examines lynching, or the violence that the author thinks comparable to lynching, in movies. Chapter 3 critiques four big news stories involving race: Bernhard Goetz gunned down four African Americans on a New York subway. Charles Stuart and Susan Smith both fictionalized black assailants to cover their own crimes. Markovitz is not sure whether Tawana Brawley, a black woman, faked her victimhood. She claimed to have been beaten and covered with feces by rogue white cops. The last chapter revisits Thomas.

The author sometimes prefers commentary over vigorous research. The movie chapter, for example, covers, with one exception (Within Our Gates [1919]), movies released in 1989 (Do the Right Thing), 1995 (Just Cause), 1996 (A Time to Kill and Ghosts of Mississippi), and 1997 (Rosewood). It is tempting to conclude that Markovitz chose these mostly pedestrian movies because they happened to be playing as he pursued his graduate studies. In any case, he undercuts their value as evidence by indulging in film criticism, calling the lynching message in Do the Right Thing, for example, “blunted” (49). If so, then it offers only “blunted” evidence for the author’s argument. A wider selection of more significant movies drawn from the entire twentieth century would have strengthened the argument.

As a first-time author, Markovitz should not be blamed for all of his book’s shortcomings. His publisher might have warned him away from so much quoting. In one instance, the author puts six block quotes in the space of just two pages (16–17). All too often, pages contain more lines of quoted material than original text (12, 13, 14, 16, 17, 19, 58, 78, 117). Even when he is not quoting directly, the author routinely relies on the findings of other scholars as a substitute for his own research and thinking. This brief book makes an important and useful point, but it could easily have been a lot better.

Christopher Waldrep
San Francisco State University


Dorsey’s study analyzes the Atlanta black community’s development and evolution during the post-Reconstruction era. She emphasizes the importance of the slave experience in the process, suggesting that the first post-slavery generation had a sense of racial solidarity and group decision-making that carried over into late nineteenth-century community building. In 1860, Atlanta had fewer than 10,000 people, one-fifth of
them slaves and only a small number of free blacks. Mostly destroyed during the Civil War, the rebuilt postwar city attracted rural freedmen in considerable numbers. Clustered in several separate enclaves, Atlanta’s black population reached about 35,000 by 1900.

In separate chapters, Dorsey explores the building of black community through the formation of churches, black colleges, social and fraternal organizations, and entrepreneurial activities. Collective action expressed through these institutions provided educational and economic opportunity and, despite limited resources, underwrote upward mobility. Blacks also organized politically to achieve community goals, especially adequate public schools for black children and the appointment of black policemen, as well as to protest white violence. Nevertheless, black voting power was contested and severely circumscribed by whites throughout this period. Over time, social stratification divided the black community; the division was based not so much on class as on status. A small black elite of professionals, such as preachers, editors, teachers, and businessmen, promoted a “politics of respectability” and a gospel of education, prohibition, and moral reform. Black professional women played a major role in this moral-uplift campaign. All of these efforts at community building, however, were hampered by persistent white efforts to regulate, control, threaten, and exploit Atlanta’s blacks.

Dorsey’s work suggests that white supremacy shaped the African-American community as much as black agency. Adhering to the concept of black inferiority, whites reacted negatively to black activism and resorted to such methods of social control as the convict lease system and segregation ordinances for streetcars. Curbing black voting rights became central to white supremacists by the turn of the century. As the black community grew in population, intensified competition for living space antagonized whites.

State politics played a role in worsening race relations as well, especially after southern populists Tom Watson and Hoke Smith turned away from an earlier coalition with black voters in Georgia. Rising tensions, stimulated by fear-mongering newspaper campaigns, led to the race riot of 1906, in which rampaging whites burned black businesses and killed a dozen or more black citizens. An interracial effort involving the black elite restored calm, but the riot, Dorsey writes, “had a transforming effect on the black community” (166). Black housing was pushed into more spatially separated areas, segregation practices were legalized, and blacks were disfranchised. Atlanta’s black elite pulled back from political and social activism, focusing instead on internal development of the black community rather than openly challenging white supremacy. Oddly, the author devotes comparatively little attention to the Atlanta riot, the culminating event of thirty years of post-Reconstruction race relations.

This book is extremely well written. It is based on research in almost two dozen manuscript collections and a large number of published
sources. The data are historical, but the conceptualization cuts across the social sciences. As a community study, the book contributes in important ways to our understanding of late nineteenth-century southern urban history and race relations.

Raymond A. Mohl
University of Alabama, Birmingham


Drawing on social, legal, and architectural histories; personal memoirs; housing brochures; courthouse deeds; and the proceedings of real-estate associations, Fogelson has found, in the history of restrictive covenants, a largely unexplored niche in the mostly recent but still rich history of suburban development. The author means his title, *Bourgeois Nightmares*, to offset the *Bourgeois Utopias* that Robert Fishman sketched nearly twenty years ago, using private land-use regulations as an index to the specific fears that drove Americans who bought or sold lots in new developments between the mid-nineteenth century and the Great Depression.¹

However neglected in the past, these covenants, which flourished well before Los Angeles introduced municipal zoning in 1915, prove to be an interesting subject. Pioneering subdividers had to conquer deep-seated American traditions about absolute property rights, not to mention real-estate speculation, many of them ambivalent themselves. Why, Fogelson asks repeatedly, did they do it? The answer is always the same: The “restrictions”—better-named “protections”—sold lots, as the citizens of a rapidly changing nation, famously on the move, yearned for stability and permanence, and wanted assurances that whatever attractions had led them to buy a specific plot would be preserved as long as possible. As they pressed onward, sellers managed to move past the usual common-law “nuisances,” hogs and slaughterhouses, eventually to bar rabbits and poultry, grocery stores, fences, even professional offices. Rare before the 1890s, some form of these restraints had become the norm by the 1920s. Not until Arthur Levitt, after World War II, did developers build houses on their own; their responsibilities came to an end as soon as they sold their lots. Long-term enforcement devolved to homeowners’ associations, which worked out the standards for architectural specifications and additions, approving styles, colors, and dimensions.

The most famous, or infamous, of restrictions were racial and ethnic, though Fogelson contends that they, while important, were less of a constraint than the others. The white working class, which resisted most

other limitations—for example, on keeping poultry, renting, and informal building—were most insistent on these exclusions. But developers were not always as enthusiastic about them. For one thing, racist rules were legally shaky, even before the United States Supreme Court, in 1948, decisively declared them unenforceable. They also posed the problem of determining precisely, in certain cases, who was “white” or “Caucasian. Finally, in a competitive real-estate market, turning down Jews and others with money was difficult to do. Middle-class buyers were most afraid of what their own neighbors might do in the future if not restrained, to bring down property values.

Despite the author’s own predilections (he is proud to inform us that he has never lived in a suburb himself, although some of his best friends have), his tone is dispassionate rather than polemic, more apt to switch into low-key humor, as when discussing bans on cats and dogs, than anachronistic indignation. The book has no real heroes, although Fogelson clearly prefers the approach of the great landscaper Frederick Law Olmsted, Sr., who thought in terms of how future buyers might misuse their property, rather than that of his son and stepson, big developers, who worried about who they might be. Fogelson reminds us that, given government zoning’s inability to demand many things desirable to homeowners, such as minimum purchase prices and (until recently, in a few places), a uniform architectural style, restrictive covenants are still in force.

The book is framed by the story of Palos Verdes Estates, an exclusive, highly expensive, development located on a spectacular peninsula southwest of Los Angeles. First developed early in the last century, early in this one it still fulfills most of its promise to the original buyers: Less than 15 percent of the owners are now of Asian descent, 2 percent Hispanic, and 1 percent black. The place itself is still beautiful, its property values as secure as any in the Golden State

Roger Lane
Haverford College


Volumes have been written about the Spanish-American War of 1898. Is there room for another? Much will depend on the perspective that the author brings to the familiar story. In this book, Schoonover seeks to put the war in the context of international history, especially the history of globalization. Arguing that the 1898 war was a culmination of the United States’ long quest for establishing its economic hegemony over the Caribbean and for opening the markets of Asia, the author views the conflict with Spain as the catalyst that ensured the success of these ef-
forts, thereby catapulting the nation to the front rank of forces promoting globalization, turning it into a “metropole state” (59).

The book’s overarching framework is global. Rather than viewing the Spanish-American war as a response to the Cuban revolt—which somehow came to entail fighting in the Philippines, as most accounts note, thereby stressing the accidental nature of the conflict and the subsequence emergence of the United States as a colonial power—Schoonover sees an inexorable march of history in which the nation was a primary mover. Since early in the Republic, he contends, Americans had been anxious to expand, commercially if not territorially, southward and eastward, thereby establishing a connection with both the Caribbean and East Asia. The United States would be at the center of this huge Pacific–West Indies region, integrating it into the world economy and in the process accumulating its own wealth and power.

This is a rather sweeping generalization, and a book consisting of only 122 pages of text could not ordinarily be expected to provide detailed data to amplify it. But the treatment works well in Schoonover’s hands. A specialist in the history of U.S.-Latin American affairs who is also conversant with European history, he has now added an East Asian dimension to his body of knowledge. Admittedly, virtually all of it comes from secondary sources, but he amplifies his account by utilizing U.S. and European archival material.

One serious reservation concerns the relationship between imperialism and globalization. This book is based on the assumption that they were interchangeable phenomena, that the United States sought to promote globalization through imperial expansion. Domestically, the author stresses social imperialism, contending, as others have done, that social and economic ills were such that the nation’s leaders sought to alleviate the problem by undertaking expansionist programs overseas. Since the economy benefited from its integration into the globalizing world, it follows that imperialism and globalization were two sides of the same coin. Such a generalization may be contested, however. Imperialism was not the only pervasive phenomenon in international affairs during the last decades of the nineteenth century and the early twentieth century. Internationalism, too, was spreading in Europe, the Americas, and Asia. Such internationalist projects as economic interdependence through free trade, cooperation among nations in solving disputes, and cultural and educational exchanges that were being promoted with vigor at the time were just as important a development, if not more so, than imperialism.

Akira Iriye
Harvard University

This book is an inquiry into the relationship between “dominant constructions of American individuality” and their influence on federal policies for American Indians (15). The author further pursues his interest in the cultural and literary history of subjectivity by analyzing an impressive array of sources: official documents, photographs, political writings, autobiographical texts, and fiction. Each of the book’s two parts is divided into two chapters that “chart the uneven and often contradictory ideological passage from the nineteenth century’s industrial-producer-sentimental culture and its styles of individuality (which value character, the work ethos, self-control, respectability) to the twentieth century’s corporate-therapeutic culture and its styles of individuality (which value personality, the psychological self, impression management, sex appeal)” (14).

The first part focuses on the Carlisle Industrial School for Indians, which aimed at erasing their students’ Native cultural heritage and producing “civilized” individuals. The second part traces the “connections between the early-twentieth-century Taos White bohemians and [John] Collier’s protomulticultural Indian New Deal” (13). Pfister opts for the case study rather than the survey approach because it enables a more nuanced and complex account of “the historical and ideological multidimensionality” of his sources (13). He views “individuality as a social form of subjectivity production” and follows the history of individualizing that influenced federal policies on American Indians (11). The book’s title draws on Williams’ concept of “incorporation” developed to explicate how hegemonic power structures control political and cultural discourse.1 That American Indians have, historically, been successful at incorporating White culture—as evident in much of their art—is just one of the ironies that Pfister’s study highlights.

The main goal of the policies in both case studies was full participation of American Indians in mainstream U.S. life. Pfister investigates the paternalist views driven by capitalist interests, from nineteenth-century Indian hating to twentieth-century glamorizing of the sanitized “primitive.” In his historical analysis of how forms of individuality have been produced, Pfister closely follows the main individual figures within American and American Indian intellectual history who were responsible for re-writing “Indianness” from a White perspective and determining policies. Moreover, he carefully considers the more ambivalent position of American Indian collaborators who were successful products of the system, if only for a while (such as Charles Eastman, a key factor at Wounded Knee, who became disenchanted with the failure of assimilationism). Pfister closely evaluates the historical circumstances from which these social constructions of individuality emerged and the political associations that they produced.

1 Raymond Williams, Marxism and Literature (New York, 1977), 114.
The study of the forms of individuality that shaped federal policies, though helpful in illuminating how different federal policies emerged, does not explicate why, despite the shifts in those forms of individuality, the invariable results were further confiscation of Indian lands and denial of Indian rights, including freedom of religion.

Maria Orban
Fayetteville State University


We know that the progressive era incubated the modern American state, but we are at odds over explanations for how progressivism shaped that development. Opening with a penetrating survey of competing interpretations of progressivism and American state development, Harrison observes that one of the few points of agreement among scholars is that Congress was “reactive, residual, even epiphenomenal” (10). Yet, new administrative institutions and novel regulatory powers required new laws for their establishment. “In an important sense the new American state was a congressional creation.” (10) Harrison’s major aims in this excellent book are twofold. First, he analyzes congressional action on progressive reform legislation. Second, and consequently, he examines Congress’ imprint on early twentieth-century state development.

Harrison ably uses both empirical and theoretical literatures in history and political science to frame his research. The study’s empirical focus is congressional action within three areas—railroad regulation, labor disputes, and the regulation of urban life in the District of Columbia. Each issue arena was prominent for progressive reform. Simultaneously, each is sufficiently distinct to test whether congressional progressives supported a cross-issue reform agenda or whether they were issue-specific reformers. Using legislative roll-call data, Harrison employs Guttmann scaling to discern the relative propensity of legislators to vote for reform in an issue arena, identifying the spectrum of reform voting from weak to strong. Then calculating Yule’s Q for his roll-call data, Harrison identifies how legislators’ votes cluster, according to their propensity to vote for or against reforms in multiple issue arenas. He finds that reformist legislators clustered around specific issue arenas and did not unify behind a progressive agenda. Proponents of strengthened railroad regulation were not necessarily supporters of labor reforms or “model city” legislation for the District of Columbia.

Harrison demonstrates that legislators’ propensities to support reform bills were related to both constituency and party interests. Democrats from the agricultural and extractive constituencies of the West and South supported railroad regulation but not reforms for the District of Columbia. But constituency economic interests were not the sole determinant of reform voting; Harrison’s analysis differs importantly from
Sander’s argument that the agricultural periphery sent progressive reformers to Congress and created progressivism. He argues that congressional reform voting was influenced by the political calculations of legislators as well as by constituency economic interests. Democrats, even those who were hostile to populism, had partisan organizational incentives to oppose the positions of the dominant, conservative Republican congressional leadership. Moreover, stymied by their party’s political machine, many Midwestern Republicans broke with the conservatives to support some reform legislation.

Finally, Harrison asks how Congress’ fragmented progressive reform influenced the formation of a more unified, expanded, and bureaucratized state. He looks to the executive branch for an answer. Policy leadership by progressive-era presidents, and their entrepreneurial officials, established a multi-issue agenda of reform and muted the congressional reformers’ preferences for clear statutory standards in favor of administrative regulatory discretion. The tug of war between congressional institutional characteristics and a modernizing executive created “the unevenness of state making in early twentieth-century America” (275). Harrison’s insights into the contributions of institutional politics to American state formation are rich. This important contribution deserves attention from historians, political scientists, and sociologists seeking to understand American political development.

Peri E. Arnold
University of Notre Dame


Like any government agency, the U.S. intelligence community, comprised today of fifteen agencies, has had its ups and downs. When the intelligence agencies err, however, the consequences can be far-reaching. Examples of mistakes by the intelligence agencies since the emergence of the United States as a world power include, most notably, the failure to anticipate the Japanese attack on Pearl Harbor in 1941, the Bay of Pigs covert-action fiasco in 1961, the domestic spy scandal revealed in 1974 (Operation Chaos), the Iran–contra affair of 1987, the 9/11 terrorist attacks of 2001, and the faulty estimate about the existence of Iraqi weapons of mass destruction (WMDs) in 2002. These events represent large stains on the record of American intelligence. But the agencies have also

had successes, such as the destruction of Colombia’s Cali drug cartel and a close monitoring of the declining Soviet economy in the final years of the Cold War.²

The subject of *U.S. Intelligence and the Nazis* falls squarely into the category of U.S. intelligence blunders—arguably one of the most wrong-minded and embarrassing. The four co-authors have devoted an enormous amount of time and effort studying recently declassified U.S. government records in compliance with the Nazi War Crimes Disclosure Act of 1998. The fifteen essays in this book cover significant topics illuminated by the archival research, including examinations of what U.S. intelligence knew about the holocaust in its early stages, the degree to which the Federal Bureau of Investigation (FBI) monitored Nazi collaborators in the United States, and the association of the Central Intelligence Agency (CIA) with former Nazi spymasters in an attempt to enlist allies in a new Cold War against the Soviet Union.

With respect to the holocaust, the book concludes that America’s key intelligence agency during World War II, the Office of Strategic Services (OSS), “does not seem to have taken much detailed interest in German camps as they concerned the extermination of Jews . . . The OSS seems to have undertaken no general study concerning the German extermination of its Jewish prisoners” (37). Such was the terrible failure of U.S. intelligence to come to grips with one of the most monstrous events in history. It was also a failure of policy, since the Roosevelt Administration displayed little interest in knowing more about the death camps in Germany, Poland, and Austria. In response to the lack of pressing concern in the White House, the OSS and other U.S. intelligence agencies never placed much of a priority on collecting intelligence related to holocaust rumors.

As for ties to former Nazi war criminals, the CIA and other U.S. intelligence agencies forged relationships with some of the most sordid characters of the Third Reich, such as Otto Albrecht von Bolschwing and Erich Rajakowitsch. In a fine essay, Tim Naftali notes that “the CIA and its representatives consciously chose to fight the Cold War in an amoral environment where recruitment decisions rested primarily on the perceived operational utility of an agent” (341). Past records were whitewashed to secure the assistance of erstwhile Nazi experts on Soviet intelligence operations. The results of this Faustian bargain? According to Naftali, “the CIA got very little” (365). In return, it gave away America’s good reputation and the moral high ground.

From the point of view of interdisciplinary methodology, this outstanding archival research done for this book might have been supplemented by interviews with key intelligence, military, and political figures from the era to place the findings into a broader context. Atten-

tion might also have been paid to related articles published by scholars in the top intelligence journals, *Intelligence and National Security* and the *International Journal of Intelligence and Counterintelligence*. The four co-authors, nevertheless, have performed great service with their laborious examination of 8 million pages of government documents and their insights into a sad history from which the nation can only hope to learn.

Loch K. Johnson  
University of Georgia

*Treasonable Doubt: The Harry Dexter White Spy Case*. By R. Bruce Craig (Lawrence, University Press of Kansas, 2004) 436 pp. $34.95

In terms of American historiography, the Cold War has replaced Reconstruction as a dark and bloody ground. With great effort and often much passion, historians have staked out the subject and covered its many dimensions. Not only have the key players been studied; so too has the impact of McCarthyism on Hollywood, labor unions, foreign policy, political parties, civil liberties, and universities, among many other topics. The intensity of interest and feeling has yet to subside after fifty years. For many inside and outside of the academic world, the immediate postwar years were a time of struggle for the moral high ground. The issues and personalities of that period became mobilizing symbols and myths that still animate the memories of both left- and right-wing America.

Craig’s *Treasonable Doubt* enters the debate about the loyalties of Harry Dexter White, who rose in his twelve years of government service (1934 to 1946) to become Assistant Secretary of the Treasury and played a major role in the founding of the World Bank and International Monetary Fund. Craig is painstaking in his intensive review of the sources, which include White’s personal papers, government records, FBI files, federal grand-jury testimony, House Committee on Un-American Activities Committee materials, interviews with key surviving players, and the Verona File—the intercepted and decrypted cable transmission from Soviet agents in the United States to their superiors in Moscow. Playing the historian as detective rather than defense attorney, he evaluates the charges against White and hopes to offer insights about how the American political and judicial system operates in times of crisis.

Craig makes a convincing argument that White did not subvert American foreign policy to Russian ends. If involved in decision making, White followed the lead of his American superiors with regard to the Morgenthau Plan for Germany, loans to the Soviet Union, the occupation currency-issue in Germany, and aid to Nationalist China. Nor did he help create the International Monetary Fund to aid and abet the economic interests of Moscow.

Yet, as Craig indicates, White had multiple loyalties. He was an
American who sympathized with communism and the interests of the Soviet Union. To realize his “utopian vision of world peace,” he worked to ensure continued American–Soviet cooperation (275). Unlike many who shared his vision, White pursued this task through espionage, conveying government secrets to the Soviet underground in the United States for an extended period of time. When confronted, White lied about his activities and the hidden allegiances of his communist friends and fellow workers.

Carefully argued and well researched, Treasonable Doubt is an important addition to the literature. Yet, the book has shortcomings. The author’s discussion of White’s motivation is too brief and superficial. Surely there were other Americans strongly attracted to progressive politics and New Deal activism who did not step over the line into treason. Craig’s definition of terms causes confusion. Although he maintains that White did not subvert policy to pro-Soviet positions, he repeatedly describes his subject as an “agent of influence,” as an individual engaged in policy subversion. Only toward the end of the book does Craig lose his balance. He shifts the argument in White’s favor by denying that the assistant secretary played a “major role” in the Soviet underground (269). On the last page, Craig’s measured tone becomes special pleading and even raises the specter of an apologist. Despite Joseph Stalin’s contention about breaking eggs to make an omelette, the end does not justify the means.

Robert Alan Goldberg
University of Utah


Have partisanship and scandal mania become so intense from Watergate to Whitewater that politics itself has become criminalized? Zelizer thinks so. He argues that the nation’s current predicament is the result of well-intended efforts to reform the United States Congress by destroying the committee system. Because that seniority-dominated system allowed conservative and sometimes blatantly racist southern Democrats to wield disproportionate power, liberal reformers coalesced around civil rights from 1948 to the mid-1960s by proposing a series of institutional reforms to attack what they viewed as the decidedly undemocratic nature of congressional decision making. If the reformers were largely unsuccessful and civil rights itself lost its grip as a unifying national issue after the race riots of 1965 (Watts) and 1967 (Newark and Detroit), Vietnam and Watergate emerged to provide a new crusading impetus. The difference is that the movement to restrain the power exercised by southern Democrats through the committee system gave way to a
movement to restrain all political power through a wave of institutional changes. Proposals focused on, among other things, the power of committee chairs, campaign financing, ethics, budget processes, legislative war powers, filibuster rules, party caucuses, and open government (for example, authorizing television coverage of Congress).

Given the dynamics of the civil rights movement, urban race rioting, Vietnam, and President Nixon’s Plumbers, Zelizer argues that the nation faced a watershed worthy of any pondered by Henry Adams. Those with the reform impulse moved from their assaults on specific problems (centered on the fundamental question of who had the right to vote and who did not) “to alleged pathologies of the entire political process” (10). By 1979 the contemporary era had arrived, with its peculiar mix of centralized and decentralized authority, ubiquitous rules, brutal partisanship, and a scandal politics in which the government’s foot soldiers included FBI agents, grand jurors, special prosecutors, and independent counsels. One of the ironies is that those southern Democrats who unintentionally inspired the institutional reformers in the first place went over to the Republicans, and the GOP itself came to rule over the new system, dominating cable TV news, the campaign contribution system, the redistricting process, the procedural minutiae of party caucuses, and the scandal apparat.

Zelizer has written an unusual book in that it is both sweeping and dense. Regardless, the product of those contradictions is impressive enough to border on numbing—in regard to both what Zelizer describes and the intellectual achievement of writing such a fine book. Some readers might object to the relatively episodic effort to hook money in politics to the institutional changes described, but that seems more a topic for another book. Thus, it barely rates as a quibble given what the chapters discuss—the southern revolt against the New Deal; the rise and fall of the civil rights era; the redistricting wars of the 1960s; the birth of scandal politics in Congress during that same decade (with emphasis on lobbyist Bobby Baker and Representative Adam Clayton Powell and Senator Thomas Dodd); the disintegration of the New Deal order (symbolized by Richard Nixon’s victory in 1968); Watergate as an largely ineffective impetus for campaign finance reform; Congress under the influence of “Watergate babies”; scandal without reform, especially pertaining to Representative Wayne Hayes, Koreagate, and Abscam and culminating in the defeat of campaign finance reform in 1978; Congress in the age of cable TV; and what might aptly be called the Gingrich era (in honor of Speaker of the House Newt Gingrich).

Kenneth O’Reilly
University of Alaska, Anchorage

Reel History is written by someone whom I like as a person and admire as an active publicist, but whose scholarship I have always found lacking in depth. Toplin has been tireless in promoting the subfield of history and film, in trying to show that historians cannot simply reject dramatic film as a medium for telling the past. To this end he has sponsored panels at academic meetings and conferences; served as the media editor for the American Historical Association’s monthly bulletin, Perspectives; edited two volumes, one devoted to the films of Ken Burns and the other to those of Oliver Stone (to which I was a contributor); written numerous essays; and produced two books entirely on his own.

The burden of Reel History lies in the title of Chapter 1, “Cinematic History as a Genre.” Drawing to some extent on the theory of genres as developed by scholars in cinema studies, he in this chapter proposes nine rules (or practices) that mark “cinematic history” (Toplin’s term) as a genre. I can forgive the fact that at least five of his categories seem to derive directly from an essay that I wrote on historical film in 1988 in a forum of the American Historical Review, to which he was also a contributor, because I am well acknowledged elsewhere in the book.1 What is less forgivable is that even described at length, these categories do not advance our understanding of what film might contribute to our knowledge of the past.

The underlying problem seems to be with the notion of genre. Even if historical film were given the status of a genre, what would be gained? Unlike other genres (the Western, the Musical, the Gangster Film), the historical film ultimately refers to a discourse outside the frame. Scholars in cinema studies can study genres to analyze different patterns of meaning produced in Hollywood in various eras. Scholars could do the same with historical films, without answering the larger question that Toplin does not really address—how do such films relate to the more traditional history that we read and write? That is, how do they relate to (reflect? impact? undermine? supplement? contest?) the larger discourse of history to which they must inevitably refer? Rather, Toplin tends to back and fill, saying that a particular film is right in some particulars but wrong in others, or that a particular filmmaker got something right and other things wrong. But never does he fully face the broader theoretical issue of how historical films communicate about the past. My own answer would be that they do so not literally, but symbolically and metaphorically, but Toplin is too narrowly focused on the literal to make any such assessment.

The categories that mark the historical genre for Toplin are not sharply defined: For example, Cinematic History (CH) simplifies histori-

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cal evidence and excludes many details. \textit{CH} offers partisan views of the past, clearly identifying heroes and villains. \textit{CH} simplifies plots by featuring only a few representative characters. \textit{CH} frequently injects romance into its stories. \textit{CH} communicates a feeling for the past through attention to details of an earlier age. \textit{CH} often communicates as powerfully through images and sounds as in words.

None of these characteristics is, exactly, wrong. But two decades after historians have begun to explore the meaning of film, they seem so obvious as hardly to need repeating. This is a problem that marks the whole volume, including later chapters on judging and studying \textit{CH}. Toplin not only tends to belabor the obvious; he does so repetitively. It might not matter were there a clear and cogent proposal in the book about how historians should think about historical film, or how such film sits with regard to our current knowledge of history. Instead Toplin provides a great deal of information about, and a certain amount of insight into, the production and reception of such films as \textit{Amistad}, \textit{The Hurricane}, \textit{JFK}, and \textit{Patton}. Interesting as this material can be, it is never terribly illuminating. Nor does it ever add up to a coherent or forceful argument.

\textit{Reel History} is for those who have never thought much about historical film. From it, they would obtain a certain introduction to a subfield in which a number of us have labored (part time) for a couple of decades. But the book does no more than skim the surface of what seems to be an increasingly important topic—how to think about the visual media, which seem to be redefining our notions of reality, past and present. Given the fact that people are exposed to more history in the visual media than in books (as Toplin often reminds us), historians need to think long and hard about this issue. \textit{Reel History} might get you started, but it will not take you very far in the realm of this interesting and important subfield.

Robert A. Rosenstone  
California Institute of Technology

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Drawing on traditional methods and straightforward approaches—government documents mostly, and previously published studies and memoirs—Vinovskis fails to add anything new to the story of the origins of Head Start despite constant and irritating assertions to the contrary: “Most studies . . . begin . . .” (12); “previous studies focus almost exclusively on . . .” (71); “most studies . . . overlook . . .” (80); “this analysis . . . provides a rather different perspective than do most earlier studies”
The book is a solipsistic, performative stutter worthy of a credulous doctoral student, not an established senior scholar.

Vinovskis frames his story inside the received wisdom of those times, repeating cliché after cliché from the official narrative: “the belated discovery of poverty” (5), “the ignorance of young, disadvantaged children” (180), “extreme cultural and intellectual deprivation and isolation” (88). We get the gloss of tragic assassinations and a political convention “marred by antiwar activists” (138), as well as the predictable bemoaning of “political expediency” that would both allow the “controversial” involvement of poor people in decisions that affect their lives and rush Head Start into being on a large scale, “ignoring the advice of experts” (154).

Vinovskis’ neat and linear prescription for social change is to develop experiments and pilot programs run by academics first, and then to let the government scale them up in an orderly and generously funded second step. There’s no room for the “maximum feasible participation” of the poor and no need for any social movements that simply distract, mislead, or muddy things up.

Intent on highlighting a specific flaw in Head Start, Vinovskis resorts to internally contradictory assertions like, “early evaluations indicated that the programs did not significantly help poor children make long-term intellectual gains” (87; italics added). The current debate about Head Start—still wildly popular, still massively underfunded—is about whether to maintain and possibly even strengthen its comprehensive approach to child development, or to focus exclusively on a narrow academic agenda. Although the early childhood community is decisively on the side of broad development, Vinovskis is consciously siding with the crowd advocating “the need to teach Head Start children their alphabet” (180). Nothing new.

William Ayers
University of Illinois, Chicago

Griswold v. Connecticut: Birth Control and the Constitutional Right of Privacy.
By John W. Johnson (Lawrence, University Press of Kansas, 2005)
266 pp. $15.95

This book comes out of an extremely admirable and valuable series called Landmark Law Cases and American Society. The series is especially useful because it is generally committed to the idea of rescuing important cases in American history from the dead hand of legal scholarship. The goal is to bring them back into the real world—that is, the social, political, economic, and cultural context that gave them birth. The books in this series also modify and enrich the legal context. Treatises and case books tend to look at Supreme Court cases as if they somehow sprang full-blown from the heads of the nine justices. But they all
have significant pre-histories: They all struggle up the pyramid of courts, and they are all shaped, influenced, and molded by briefs, oral arguments, memoranda circulating in chambers, discussions between clerks and Justices, and so on.

_Griswold v. Connecticut_ struck down a stubborn Connecticut law that, in effect, outlawed contraception and contraceptive advice. At the time (1965), no other state had so restrictive a law on the subject. Johnson recounts, in some detail, the futile attempts to persuade the legislature of Connecticut to repeal the law. Repeated failure led to a judicial strategy—trying to convince the judges, state and federal, to step into the breach. For the most part, the judges were extremely reluctant to get involved. There were all sorts of detours along the way; but finally, in _Griswold_, the majority of the justices swept the Connecticut law into the dustbin of history.

The fate of one archaic law in one small state would ordinarily be of only marginal historical interest. But _Griswold_ is the case that, in the standard account, invented the constitutional right of privacy, and of course in a sense it did. But Johnson points out how the concept of privacy, and the language of privacy, had popped up here and there, almost incidentally, in cases turning on other questions of constitutional law. He also shows the role that the concept played in the briefs and arguments, and in the debate among the justices inside the Supreme Court. The narrative account does not end with _Griswold_. Johnson also follows, succinctly, the dramatic career of this new constitutional right, through _Roe v. Wade_ to _Lawrence v. Texas_. The _Griswold_ case, as Johnson tells us in his concluding paragraph, “unleashed” the constitutional right of privacy, which has “transformed the legal landscape”; its “repercussions have touched the most intimate aspects of human life” (234). The key word in this last sentence is “intimate.” As Johnson points out, the tort of invasion of privacy, launched in the late nineteenth century, was never successfully applied against law enforcement officers (62). It is an interesting fact that the dramatic increase in protection of “intimate” choices coexists with the Patriot Act, and a scary expansion of the government’s right to snoop into private affairs in other than these “intimate” aspects of life. This development does not downsize the importance of the right of privacy in those regions of intimacy.

_Griswold’s_ legacy has been deeply controversial in at least two areas, abortion rights and gay rights. Yet _Griswold_ itself seems serenely unassailable. Nobody calls for overruling it, even though scholars are critical of the actual opinions in the case (rather than the result). In particular, scholars sneer at what Justice William O. Douglas wrote, his “sloppiness and facile reasoning,” and his invocation of such weird ideas as “emanations” from, and “penumbras” surrounding, the Bill of Rights (224). Somehow, the result in _Griswold_ suited the temper of the times. Contraception was on its way to becoming a non-issue, for the most part, and, though there is dispute about boundary lines, personal privacy and rights
of intimate choice are also in essence wildly popular. Many new constitutions, all over the world, make explicit mention of rights of privacy, or something equivalent. In the United States, we have a creaky old Constitution, drafted in the eighteenth century, and exceedingly hard to amend. No matter. The nine justices try to make sure that master trends of the contemporary world get slotted into the text. Not everybody agrees about these trends, and some people strongly disapprove of how the justices seem to be rewriting the constitution. Nonetheless, the Supreme Court goes on with what it sees as its job.

This is a well-written book on an important case and an important subject. There are some inaccuracies scattered throughout the book, but none of them is particularly serious or damaging to the main line of the narrative. The writing is clean and supple. Johnson has an interesting story to tell; and he does a good job of telling it.

Lawrence M. Friedman
Stanford University

Borderland Religion: The Emergence of an English-Canadian Identity, 1792–1852. By J. I. Little (Toronto, University of Toronto Press, 2004) 386 pp. $75.00

Borderland Religion convincingly demonstrates that religion was a powerful force in reshaping an American identity into a British one. The importance of this volume lies, first, in its exhaustive, detailed research of a hitherto unexplored religious terrain. Little has deftly utilized the Eastern Townships as a vast case study by which to test and revise persuasively the overemphasis upon Upper Canada as the template for discussions of English-Canadian identity. Second, Little brilliantly dislodges one of the most enduring canons of Canadian religious historiography—Clark’s argument that the variety of smaller Protestant sects, which included Methodism, constituted the only vital and expansive form of religion in early nineteenth-century Canada. Clark portrayed the Anglican church as an outmoded vestige of Europe, out of touch with the democratic values of North America. Although Little does not dissent from Clark’s theoretical dichotomy of radical American religion versus British conservative religiosity, he effectively demonstrates the extent to which, especially following the Rebellions of 1837, American-based churches—including Baptist, Methodist, Quaker, and Congregationalist—slowly waned with the advance of the more stably funded British Wesleyan and Anglican denominations. Of equal significance, the volume gives due attention to such smaller sects as the Universalists and Millerites, which have been little explored within the

1 S. D. Clark, Church and Sect in Canada (Toronto, 1998).
Canadian context, thus further enriching the framework of religious pluralism as a central characteristic of English Canada’s formative years.

The book’s organization around denominations allows Little to penetrate beneath the macro-level of census statistics and to explore the growth, internal dynamics, finances, and, most importantly, the life histories of many hitherto unknown clergyman through their correspondence and discourse about popular religious practices. In focusing upon the success and failure of specific churches, the author connects the pattern of religious revival to specific socioeconomic factors in a manner that will be familiar to American historians who have emphasized revivalism as the central lever of religious progress. His attention to micro-history well demonstrates the degree to which localism was a central feature of religious culture during this period, and to which the rise and fall of particular religious groups depended on the personalities of specific clergymen. Little’s vast knowledge of the financial structures of each denomination helps to explain the patterns of religious adherence within and between denominations. Indeed, Little is one of the first historians of religion in Canada to focus upon monetary considerations as a primary dynamic of religious growth—hence, the ultimate success of British-funded denominations, most notably, the Anglican Church.

The cumulative effect of these denominationally based chapters is a portrait of extreme religious instability and fluidity in a pluralistic environment where ordinary parishioners exercised a high degree of religious choice. In this regard, this book affirms for the Eastern Townships Lane’s conclusions about St. Stephen, New Brunswick, another borderland community. Although Little did not have access to Lane’s extensive church-membership lists, his extensive analysis of the discourse of various clergymen and their constant complaints about religious indifference illustrates the frustrated attempts of the upper clergy to define strict denominational identities for ordinary people. Like the church adherents of New Brunswick, Little’s American and British settlers did not frequently become church members, often changing religious affiliations between evangelical and high Anglican conformity. Little’s focus upon attitudes to baptism, churching, and burial reveals that ordinary people contested the authority of, and in turn shaped, the Anglican Church.

In the final chapters, Little palpably revises the stereotype of Anglican ministers as effete failures. He also shows that High Church practices were effective and probably a direct response to the ostensible American loyalties of this area, thus rendering the Eastern Townships unique insofar as the Anglican Church in Upper Canada was largely controlled by evangelical clergy. How much of a role did the French-Canadian factor play in this imperial project? If the Presbyterians, who were British and

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often politically radical, had entered the mix, would they have recast the dichotomy between radical American religious values and conservative British church traditions? As Little himself stated, this volume anticipates further explorations of religious expression within particular communities at the micro-level and he has accomplished this task impressively.

Nancy Christie
Trent University

A Pueblo Divided: Business, Property, and Community in Papantla, Mexico. By Emilio Kourí (Stanford, Stanford University Press, 2004) 389 pp. $60.00

“This was the world that vanilla had made” (283). The last in line Kourí’s meticulously woven story of the experience of the cantón of Papantla, Veracruz, in the nineteenth century encapsulates the book’s approach, even as it obscures the complexity and sophistication of Kourí’s analysis. This is economic and business history at its best, committed not only to rigorous probing of the explanatory power of external factors like world commodity markets, but also to exposing and exploring the places where those factors lose their power to explain.

Kourí begins the story with two relaxed, almost leisurely background chapters: Chapter 1 traces the scientific and commercial history of vanilla and its relevance to Mexico, and Chapter 2 concerns the geography, demography, and economic organization of the Tecolutla river basin. The relationship between product, land, and society is so carefully established that when Kourí later explains how land tenure could change without major alterations in land use patterns and even access to land, this important distinction is completely clear. Chapter 3 deals with vanilla exploitation in Papantla from 1760 to 1870, with emphasis on the post-1830 period, during which the trade expanded greatly. Vanilla profits initially had the effect of strengthening the traditional core of the region’s economic activity, milpa agriculture. But new actors (including a number of Italian immigrants), new monied groups, new frictions within the Indian community concerning taxation and the control of the vanilla trade, new sites of power (as ayuntamientos replaced the In- dian governments of the colonial period, and powerful rancherías emerged as the political competitors of the ayuntamientos), all had the potential to be unsettling—perhaps even revolutionary—sources of social and economic change, even if two other factors had not come into play.

But they did. The first was the dramatic expansion of the U.S. market for vanilla after 1870, as improved freezer technology made ice cream available to the masses. The second was the ideological imperative to privatize land in the wake of the liberal triumph in 1867. The intertwined effects of these factors are treated in chapters 4 and 5. Crucially,
because of the earlier success of vanilla in Papantla and the labor-intensive nature of vanilla cultivation, the debate about land reform had to involve the holders of the communal properties, who controlled the best vanilla lands. (By contrast, in most of Mexico, Indian peoples did not have the economic power to exert their will on the national project to privatize communal land.) In a particularly subtle discussion, Kourí shows how both potential winners and potential losers in the vanilla economy could see privatization as potentially advantageous; thus, the debate was not so much about whether to privatize, but how. Totonac leaders were not willing to accept any reparto that might further empower merchants and townspeople, and they were able to demand a compromise—brilliantly analyzed—known as condueñazgo, a system of privatized but not individualized ownership in which communal lands were divided into grandes lotes in which the “primitive” owners were given shares.

But even though the vanilla-driven economy had given the Totonac producers a say in how the lands would be privatized, it had also created a class of merchants and townspeople for whom the central ideological message of liberalism, individual property ownership, was increasingly attractive. They could secure a piece of the vanilla action by duplicity, trickery, corruption, malicious rumor mongering, and assassination (all depressingly well-documented), but the best way to consolidate their gains was through an individual property-rights regime. The condueñazgos proved not to be viable in the long term because they were neither private enough to satisfy those committed to parcelization nor fair enough to satisfy those who received shares in distant or less desirable lands, or were denied access to milpa lands, burdened with unexpected taxes, disinherited, or duped out of their shares. As the perhaps too lengthy Chapter 6 details, the difficulty of undermining the condueñazgo did not stop local, state, and even national figures from trying to do so, spurred by the combination of continuing demand for vanilla in the 1890s but narrowing profit margins. Various rebellions and schemes to derail or appropriate parcelization, mounted by young Totonacs and other disfranchised elements who resented the increasingly interventionist merchants and their allies among the more powerful Totonacs, were the result.

None of the well-drawn characters, with the possible exception of jefe político Lucido Cambas, a believer in the Lerdo-era rhetoric about the benefits of small, privately held property but also a mediator and reluctant protector of the condueñazgos, plays an admirable role in the painful decade-long process of parcelization. In the end, more than half of Totonac households were left propertyless, and the best lands passed into the hands of the top merchants and the Totonac elite.

Yet two-thirds of the old communal territory remained in the hands of Totonac family farmers. It is unclear whether Kourí attributes this circumstance to their resistance to a process that more pervasively demolished peasant landownership elsewhere in Mexico, or to the na-
ture of vanilla cultivation itself, which guaranteed that access to land would not be universally denied even to those who lost title to it. Such is the book’s subtlety. It is neither (despite the concluding sentence) a story of vanilla as a kind of unitary driving force a la staple theory, nor a story about peasant “agency.” Instead, elements of both of these recent and not-so-recent theoretical frameworks are blended together in a skillful, original, and convincing narrative in which structural factors and business practices are assumed, without embarrassment, to be important to an understanding of politics and social change, and vice-versa.

Margaret Chowning
University of California, Berkeley

Yaxcabá and the Caste War of Yucatán: An Archaeological Perspective. By Rani T. Alexander (Albuquerque, University of New Mexico Press, 2004) 207 pp. $49.95

Alexander uses a blend of historical documentation and archaeological fieldwork to address the causes and consequences of the nineteenth-century Caste War, when the Yucatec Maya rose up against the Creoles who had dominated political and economic life in the decades following Mexican Independence. Her specific focus is the parish of Yaxcabá, some 100 km southeast of Mérida. Prior to the Caste War, the parish had a peak population of more than 11,000 distributed among 30 settlements, the largest of which was the cabecera (administrative seat) of Yaxcabá itself (with a population exceeding 3,000). Creoles controlled local politics and owned a growing number of ranches dedicated to the raising of livestock. The parish experienced its first attack by Maya rebels in December 1847. The cabecera itself was occupied by rebels in February 1848 but was retaken by a Creole army six months later. Over the next twenty-two years, the parish was attacked eight more times, resulting in severe infrastructural destruction and a population decline of more than 90 percent. Even today, population in the area has not returned to its prewar peak. As Alexander points out, the historical record has been interpreted as supporting the hypothesis that the Caste War was caused by an agrarian crisis, itself the product of population pressure on arable land and an economic system that favored Creole cattle ranching over indigenous agriculture.

To explore the agrarian crisis hypothesis further, Alexander draws upon historical archaeology. In 1988/89, she conducted a settlement-pattern survey in the Yaxcabá region, followed by more intensive fieldwork at selected sites. During her survey, she sought to link every archaeological site with a historically known occupation. After defining four settlement types using archaeological criteria, she discusses the points of disagreement with the historical record, the most notable of which is that many of her category III sites (livestock establishments)
show much less architectural elaboration than might be expected from the documents—indicating to her a lack of success for these haciendas and ranchos. She also uses the documentary record to organize her archaeological data into seven phases of occupation in the region, from 1547 to 1900. Intensive mapping and surface collecting were conducted at three archaeological sites. Her analyses of data on site size, house-lot size, garden size, artifacts, and such ancillary features as animal pens reveal considerable variability among these sites, which appear to reflect differences in the adaptive strategies pursued by their inhabitants.

The agrarian-crisis hypothesis requires some modification in view of Alexander’s study. She sees little support for an overarching condition of population pressure, though she does find evidence of a growing imbalance between land allocated to cattle raising (which was largely unsuccessful) and subsistence agriculture. Indigenous peoples responded to this imbalance by altering their productive activities (more house-lot gardening and pig raising, for example), especially in settlements that were less subject to Creole control. Such responses, she argues, were part of a long-standing pattern of “resistance adaptation” in the region. Noting similarities between the aftermath of the Caste War and the late Colonial period (1600 to 1750), Alexander suggests that the demographic and economic declines seen in both periods resulted from unsuccessful attempts by outsiders to impose a commercialized regime upon the Yucatecan hinterland.

Charles S. Spencer
American Museum of Natural History


This is a brilliant contribution to Islamic studies. The first third of the book explains the origin of the various Muslim sects, religiopolitical factions, and parties and movements as they evolved in response to the historical circumstances of the first two Muslim centuries. Much of it is based on the author’s previous contributions. The political, religious, and philosophical positions of the Shiites, Mutazilites, Abbasids, Zaydis, Imamis, and the Hadith party are explained with incisive clarity and rich detail.

The second part is a review of the Muslim political literatures in the Persian and Greek traditions, and Ismaili and Sunni theory, of the period from the ninth to the thirteenth centuries. Though less revealing, this part sets the stage for the final third of the book, which deals with the nature and functions of government, the concepts of freedom and society, and the relations between Muslims and non-Muslims. Set out logi-
cally and thematically, these chapters explain the positions of the major schools and sects on each of the issues.

The chapter on the nature of government—which deals with the questions of why people live in societies, why they must have law, why law must be God-given, and why God-given law necessitates a monarchical and absolutist regime—is the most powerful and revealing chapter in the book. An equally interesting chapter on Muslims and non-Muslims lays out the concept of holy war, explicates the justifications for Muslim religious imperialisms, the debates about the treatment of non-Muslims, and Muslim attitudes toward conversions. An unusual added dimension is the discussion of Muslim views on the treatment of dissident or nonconforming Muslims and the law of war in such cases. These thematic reviews will be the most useful to scholars and students outside of the field, but because of the way in which the views of the various schools are fragmented by topic, it will not be easy to get an overview of the individual positions.

This important book, however, suffers from some of the limitations inherent in Islamic scholarship. The book is concerned with literary discourses, which are only one dimension of political thinking. Real-world political culture—including individual loyalties and ambitions, family and tribal commitments, patronage and clientage, and struggles for power and wealth—is not considered.

Despite its forceful clarity and strong logical orderliness, the book will not be easy to read for people outside of the field. The historical examples are selected far and wide in the period from the seventh to the thirteenth centuries in the region from Central Asia to the Atlantic. The high level of detail and the endless references to historical persons and events that will not be familiar to uninitiated readers are a barrier to a wider audience. So too is the absence, apart from sporadic passing comments, of any comparative reference to Western or other political theory. That Crone is conventionally historical and literary in her approach and does not break new methodological ground is not a criticism of her work, but a comment on the isolation of Islamic studies from other fields, and a caution that the subject has become so self-referential that even someone so brilliant and learned does not readily communicate with scholars outside of her field.

Ira M. Lapidus
University of California, Berkeley


This well-researched book paints a colorful tapestry of street life in a major metropolis in southwestern China. It also chronicles, after Gramsci
and Geertz, the shifting alliances, struggles, and resistance among the state, local elites, and the subaltern within popular culture.¹

This contest was waged on the streets of Chengdu, a city of over 300,000. Urban elites had “always sought opportunities to shape the values and habits of the lower class” (131), but the book analyzes the radical changes and continuities of the modern period in four stages. Part One reconstructs “traditional” public space—the streets, neighborhoods, markets, fairs, and the vibrant commercial and urban culture of opera, teahouse, ritual, and popular religion. Relatively free from official supervision at a time when there was no municipal government, local elites sought to control the uncouth and uneducated. In this exercise of cultural hegemony and informal governance, they often had the blessing of local officials, a symbiosis that furnished the basis of urban autonomy, something that had once been thought lacking in China.

Part Two focuses on the critical period of the early twentieth century, when China was condemned to modernize under the pressure of foreign imperialism. In the first stage, the reigning Manchu dynasty sought to strengthen itself through reforms, creating institutions that gave local elites the authority to refashion Chengdu through “modern” policing institutions of public order, discipline, and hygiene using “Western” science and medicine. The commoners resisted this uncritical imposition of “modernity” with what Scott characterized as “weapons of the weak”—from gossip to flauting to daily resistance.² Little had been achieved by these reforms when the confrontation between local elites and commoners entered a suspended stage during the 1911 Revolution and its aftermath. Both became united against what they saw as a callous Manchu state infringing upon their lives, using popular culture to mobilize the masses.

Part Three chronicles the fourth stage in the ever-shifting alliance between the state, local elites, and the commoners during the Republican period as the chaos of warlordism and civil war engulfed the country. While paying a staggering price for “modernity” and “revolution,” popular culture on the streets of Chengdu continued.

Wang is at home with a wide range of Chinese sources, oral tradition, and the latest theoretical developments in history, anthropology, and sociology. With the precedent of this book, the field can move to more sophisticated comparative studies on popular culture in China and elsewhere.

Kwan Man Bun
University of Cincinnati


In this book, Bian thoroughly explores many hitherto untapped official archives of ordnance enterprises operating under Nationalist Party (Guomindang) auspices during the war of resistance against Japan (1937–1945). Based upon this rich material and an analysis of both business history (notably the concept of “path dependence”) and cognitive theory (the concept of “mental models” and institutional change associated with North), he delivers some fundamentally important findings.¹ His argument that “the basic institutional arrangement of China’s state-owned enterprises—the bureaucratic governance structure, the distinctive management and incentive mechanisms, and the provision of social services and welfare—took shape in China during the Sino-Japanese War” boldly challenges the idea that modern China’s industrial state enterprise system was variously derived from the pre-1949 experiences of the Chinese labor movement, the history of the Chinese Communist Party (CCP), or the Soviet development model that the CCP emulated in the 1950s (213). This conclusion is controversial, especially with regard to the historical parentage of the once ubiquitous danwei or urban “work units,” which are steadily being phased out in China’s increasingly marketized economy.

Bian begins by tracing the vicissitudes of China’s ordnance industry from its origins in the 1860s to the Japanese invasion in the early 1930s. That crisis impelled the Nationalists to centralize and rationalize the arms industry in line with their comprehensive economic-mobilization plans for the heavy industrial sector as a whole. While the industry’s traditional bureaucratic organization persisted, the novel use of technocratic management, modern cost-accounting methods, incentive mechanisms in the form of work-emulation campaigns, and the provision of welfare and social services in ordnance enterprises evinced the pragmatic importation of foreign models. Formal institutionalization of these enterprises as danwei soon followed, buttressed by frequent Nationalist ideological justification of the developmental state and state socialism as the future path for China’s industrial growth.

Bian’s fine study reinforces an evolving body of scholarship suggesting continuity between the Nationalist and Communist eras, an evolution rather than a revolution of both institutions and ideas. Despite instances of overly repetitive invocation of theory, and an occasionally formulaic organization and presentation, this well-written work makes an important contribution to the history of economic organization in modern China.

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In the early modern, or Tokugawa, era (1600–1868), many professional and serious amateur artists from all walks of life joined societies dedicated to practicing traditional Japanese art forms in the company of others through collaborative interaction. These voluntary associations cut across the government’s rigidly assigned gender roles and hereditary class divisions and violated edicts prohibiting the formation of horizontal alliances among individuals. In Bonds of Civility, Ikegami argues that such “sites of aesthetic appreciation” in the otherwise highly stratified and segmented Tokugawa society became “enclaves of free socialization” in which artists “could temporarily suspend the application of feudal norms” and identities (4). With their voluntary mix of men and women, commoners and samurai, and, especially—in the case of haikai poetry groups—with their encouragement of subversive humor, networks of artistic associations formed a kind of “aesthetic public” functionally similar to civic networks in the West and potentially capable of slyly subverting the antiquated Tokugawa order.

This development, the author suggests, contributed to post-Tokugawa Japan’s formation of a modern nation-state during the second half of the nineteenth century because it forged a psychological sense, at least among participants in aesthetic circles, of being “part of a universal fellowship rather than prisoners of a hierarchically segmented political structure” (368). Artistic networks, abetted by other developments in the realm of popular culture and the market economy—such as the spread of literacy, the rise of commercial publishing, the increase in domestic travel, and the growing audiences for theatrical performances (and the transgressive fantasies that they depicted)—created opportunities for temporary escape from formal feudal status into areas where the self was not defined and constrained by hereditary social roles. Consequently, individuals could recognize that social boundaries set by the Tokugawa order represented merely one of many possible “modes of sociability.”

Cultural activities, however, with few exceptions, did not lead to oppositional political movements or even political discourse. The political implications of aesthetic life lie in the “sense of commonality” and civic space generated by freely chosen participation in artistic activities and further fueled by the growing popularity of other arenas for the expression of beauty in everyday life, such as fashion, manners, deportment, and food presentation. In other words, shared aesthetic values and cultural idioms, together with aesthetically pleasing and elaborate codes of etiquette (some of them reinforcing, rather than violating, observance of hierarchical relationships), produced a collective identity in uniquely aesthetic terms even before the rise of the modern nation-state.

There is much to admire in this richly detailed sociological exami-
nation of early modern cultural life. On the conceptual level, however, a more explicit differentiation of gender might have been helpful. Also, inasmuch as the cultural identity forged around aesthetics did not feed into post-Tokugawa, emperor-centered nationalist ideology, and Meiji leaders themselves quickly dismantled divisive Tokugawa laws, customs, and institutions in their efforts to build modern citizenship, the overall political significance of aesthetic networks for the modern Japanese nation-state requires further elaboration, hopefully in a follow-up volume.

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