Reviews


This broad and provocative collection of essays has an ambitious goal—to systematize the history of past human interactions with the environment, to understand our environmental future better. It is hard to imagine a larger task. The thirty-four contributors seek to craft a “fully integrated history of humans and the rest of nature” (4). These essays are the first steps of the larger project, called the Integrated History and Future of People on Earth (IHOPE).

Their is not a purely academic exercise. The contributors hope to produce a practical model (or a series of practical models). The data include knowledge of such events as the rise and fall of the Roman and Mayan Empires, the historical impacts of El Niño, twentieth-century geopolitics, and so on. The resulting models will enable predictions about such diverse phenomena as land-use patterns, ecosystem dynamics, and climate shifts. Should accurate predictions appear too tough a proposition, the volume editors are comfortable instead with the expectation that “IHOPE can use a deeper understanding of the past to help us create a better future, rather than to predict the future” (14).

The volume is divided into five parts. The first introduces the volume and discusses methodology. The following parts attempt to integrate history and nature within four time frames—the millennial (up to 10,000 years ago), the centennial (up to 1,000 years ago), the decadal (up to 100 years ago), and the future. Each of the last four parts contains three-to-five topically focused essays, as well as a longer “group report” that draws larger conclusions and relates the lessons of each time scale to the IHOPE project.

This project reveals interdisciplinarity at its best and worst. The authors come from a wide variety of fields—economics, anthropology, climatology, resource management, and a host of others. Almost all of the contributors take approaches derived from the social and natural sciences; only two or three humanistic perspectives are represented. One of the greatest challenges facing the IHOPE project is the difficulty of bridging disciplinary divides, especially in the assessment of data. How might someone assess—for modeling purposes—the information drawn from pollen sediments, archeological sites, and written documents? More generally, how can human social processes like land-use patterns or information flows be integrated with such quantifiable natural processes as soil erosion or greenhouse-gas concentration? Costanza suggests a system for “grading” data as a place to start. The volume makes its greatest contribution to interdisciplinary history in answering these types of questions.

Some of the essays are dense, jargon-laden, and hard to follow. But the authors are asking important, hard questions, and their answers to these questions are nearly always provocative. IHOPE is certainly a wor-
thy, if tremendously ambitious, project. If the goal is to understand every-thing that has ever happened to guide future responses to environ-mental problems, asking eminent scholars from a variety of disciplines to tackle hard questions is the place to start.

James Feldman
University of Wisconsin, Oshkosh

Looking at Animals in Human History. By Linda Kalof (Chicago, University of Chicago Press, 2007) 222 pp. $45.00

Within the last decade or so, animals have emerged as a focus of research across the humanities and social sciences. The resulting scholarship is sometimes grouped under the rubric of “animal studies,” although its authors share no common methodology or core expertise, and diverge still more widely in their cultural, political, and ethical commitments. Even the meaning of the term animal is open to debate. On a more practical level, this disciplinary range has meant that publications are scattered throughout a wide range of academic journals. In her ambitious survey, Kalof has attempted to digest and organize the work of this burgeoning field.

As the title indicates, the main organizing principle of Looking at Animals in Human History is historical. The book is divided into six chronological chapters, conventionally labeled “Prehistory,” “Antiquity,” “The Middle Ages,” “The Renaissance,” “The Enlightenment,” and “Modernity.” As the periodization suggests, and as Kalof apologetically notes in the preface, the “human history” of the title refers only to “the western world” (viii). In fact, her geographical scope is even narrower; the book focuses almost exclusively on northwestern Europe, especially on Great Britain. Even when so restricted, however, “human history” remains an unwieldy span. Hence, as her title more reliably indicates, Kalof has chosen to analyze the role played by animals primarily through their representation in visual and plastic art.

Such representations constitute a rich and suggestive source of evidence. Kalof’s account emphasizes such famous works as the Paleolithic cave paintings of Spain and southern France, Albrecht Dürer’s rhinocero,s, and William Hogarth’s “Four Stages of Cruelty.” She also discusses less well-known and more popular animal images—mosaics from ancient homes, prints illustrating eighteenth- and nineteenth-century animal shows, and photographs of hunting trophies. Her interpretations are supported by numerous well-selected and nicely reproduced illustrations, a few in impressive color.

This book is not intended as a history of animal art, however, but as a history of the relations between humans and animals as presented through art. The unavoidable question is whether such representations provide an adequate basis for historical generalization. Kalof implicitly acknowledges this issue by attempting to place her discussions of indi-
vidual representations in context. But, given the extended chronological scope of the book and its brief length in pages, most of her background summaries seem potted. In addition, her individual chapters convey little sense of historical change, even though the last three cover two centuries each and the first three cover much longer periods. Conversely, some topics—for example, the emergence of humane sentiment and activism—recur in several succeeding chapters, without any acknowledgment of historical continuity.

Other problems result at least in part from Kalof’s exclusive reliance on secondary sources. She emphasizes the topics that other scholars have studied; what they have neglected remains in the dark. Hence, her book does not contain any surprises for historians with an established interest in its subject, but it may offer others a useful, if partial, introduction to the field.

Harriet Ritvo
Massachusetts Institute of Technology

Blood and Soil: A World History of Genocide and Extermination from Sparta to Darfur. By Ben Kiernan (New Haven, Yale University Press, 2007) 724 pp. $40.00

Kiernan’s mammoth chronicle of mass killing will divide readers into two categories. Those who appreciate well-written sourcebooks containing narratives of vicious violence unencumbered by analysis will applaud the one-stop emporium of cases that the book provides. Those who (like me) expect a big new book on genocide and extermination to advance beyond existing knowledge by challenging old explanations and/or proposing new ones will leave its pages disappointed. Blood and Soil applies the criteria of the United Nations’ (1948) convention on genocide—acts committed with the intent to destroy, in whole or in part, a national, racial, ethnical, or religious group as such—retroactively, searching through history for cases that conform to the UN criterion.

Kiernan musters his many cases in support of the claim that four themes recur in mass killing across human history: cults of antiquity, fetishes in favor of agriculture (therefore in opposition to nonagriculturalists), ethnic enmity (which the book expands to include racial and religious hostility), and territorial expansion by the perpetrators. This is universalizing history. Kiernan presents his cases under three main headings. “Imperial Expansion” covers genocide (especially Rome’s annihilation of Carthage) in the ancient world, Spanish conquest in the Americas, European genocides in East Asia from 1400 to 1600, and comparable massacres in early modern Southeast Asia. “Settler Colonialism” gives us the English in sixteenth-century Ireland, colonial North America, and nineteenth-century Australia; genocide in the postcolonial United States; and settler genocides in Africa. The section on twentieth-century
genocide reviews massacres of Armenians in the Ottoman Empire, German and Nazi genocide, Japanese atrocities in East Asia, Soviet terror, Maoism in China, and mass killing in Cambodia and Rwanda. An epilogue slips in the cases of Bangladesh, East Timor, Guatemala, Iraq, Bosnia, the Sudan, and al-Qaeda.

Kiernan closes by reasserting the generality of his four themes in mass killing, which after 600 pages have mutated into “race, antiquity, agriculture, and expansion” (605). He leaves the logical status of the four themes unclear. Are they necessary conditions for mass killing, jointly sufficient conditions for mass killing, separate elements that independently increase the probability of mass killing, or simply characteristics that frequently accompany mass killing? The book neither identifies systematic variations in genocide from time to time or place to place that require explanation nor proposes explanations for change, variation, and continuity in genocide. Its exclusive concentration on genocidal attempts that produced massive deaths deprives us of the opportunity to learn under what conditions popular resistance or third-party intervention prevented or mitigated massacres. Its descriptions, furthermore, concentrate overwhelmingly on ideologies and actions of perpetrators rather than on analyzing victims’ responses or interactions between perpetrators and victims. Kiernan’s forty-three-page treatment of English killing in Ireland between 1565 and 1603, for example, portrays Shane O’Neill as a rebel against English rule but neither as a military contender for the earldom of Tyrone nor as the Irish lord who accepted Queen Elizabeth’s support in his bid to head the O’Neill clan. Such are the perils of universalizing history.

Charles Tilly
Columbia University


This engaging small book is a scholarly popular book on financial crises. This apparent oxymoron is, in fact, a new genre of books started by Kindleberger and Krugman, in which economists attempt to communicate with the general public—that is, non-economists—about current and past affairs.1 The book under review appears to be a broad survey based on a more scholarly book that the authors wrote about eighteenth-century French finance.2

Their account of financial crises is engaging but difficult to follow if you have not been a student of historical finance. The authors range over continents and centuries with the confidence that readers will follow their intellectual travels. They tend to focus on France and the United States, but their accounts range from the seventeenth to the twenty-first century, requiring knowledge and imagination from their readers. This is not to say that anyone is barred from understanding their argument, but this kind of historical survey cannot provide the background for any particular story that most historians would think necessary.

The book is organized around two “models” that are used for various purposes in the narratives—not formal models of behavioral relations among quantifiable variables but lists of factors or causes and generalizations of a simpler sort. The first model appears in Chapter 1 to help predict financial crises, and in the last chapter to predict the likelihood of good reforms. The three factors in this model are sensible government finance, as identified by not too much government debt; abundant information about financial affairs; and a large middle class. The corollary to it is that conditions that make financial crises more likely make constructive reforms less likely. Unruly governments are too predatory to avoid crises or to fix the financial system. The middle class loses the most from financial crises, but since it needs personal finance the most, it supports reform. Asymmetric information is the bane of all financial systems and needs to be minimized for financial health.

The second financial model is Marxist in origin—another oxymoron? It appears in the middle of the book to inquire more closely into the actions of governments. The three factors (in Marxist terms, classes) of this model are the poor (proletariat), the middle class (bourgeoisie), and the rich (capitalists). As in Karl Marx’s work, the dynamics of the model come when any two factors/classes get together. Depending on which two enforce their will on the third, the outcome is better or worse for the poor and middle class. As before, the existence of a large and powerful middle class or bourgeoisie makes for better outcomes. The paradox of most analyses of progress is that the middle class is both the engine of economic stability and growth and the beneficiary of economic stability and growth.

This book was written after the fiasco of Long Term Capital Management’s melt-down and the collapse of the dot-com bubble, but before the subprime mortgage crisis. How well does history inform the likely progress of this last financial crisis? The first thing to note is that the United States lacks two of the three factors making financial crises less likely and reforms more effective. The government today is more indebted than ever, and the information—apparently even to professionals—about the complex securities that were traded in the mortgage market in the last few years is scarce. Hence, this book would advise us not to be surprised if a financial crisis occurs or to be hopeful that effective and sober reforms will follow it.
This book will appeal to those who like financial affairs. The history is generally sound and the arguments sensible. The authors make a good case for the importance of history, and the lessons from this brief book are clearer than most.

Peter Temin
Massachusetts Institute of Technology


*Appeasing Bankers* refers to bankers’ acute anti-war bias as well as to politicians’ predilection to bend to bankers’ dovish desires. “Because of the [negative] macroeconomic consequences of war,” Kirshner argues, “financial communities within countries will be among the most cautious elements when it comes to waging war or supporting foreign policies that risk war” (9). That hypothesis, he claims, “approaches a lawlike statement” because it holds over large swaths of time and place and is not contradicted by the only type of evidence that could falsify it, “instances where finance was leading the charge for war or was at least near the head of the pack” (211, n.12).

The book’s methodology, however, precludes finding any such falsifying instances. *Appeasing Bankers* situates itself solidly in the government/political science/international relations literature and never budges. Rather than formally testing his hypothesis in the way that a good cliometrician would, by statistically analyzing time series and/or panel data, Kirshner argues that war is too complex to predict because of the intricate interdependence of causal variables. War is a difficult subject to be sure, but the adroit use of statistics can certainly help to illuminate the contours of its causes.

The dearth of hard analysis aside, Kirshner examines a disappointingly small and narrow number of cases. The first case concerns the Spanish-American War, the second interwar Japan, the third interwar France, the fourth the early Cold War and the Korean conflict, and the final case the Falklands War. Kirshner also briefly discusses other episodes, including—most importantly but unsatisfyingly—the coming of World War I. He explains that he chose cases in which he had no previous knowledge of the financial sector’s stand. Nevertheless, even if the five case studies were impeccable, the hypothesis would still be disputable for the twentieth century and would be a far cry from establishing the lawlike status claimed for it. From Adam Smith to Niall Ferguson, for example, scholars have convincingly shown that British financiers benefited immensely from the numerous wars of the eighteenth century.

The case studies proper are problematical because Kirshner fails to account for the diversity of views and interests *within* the financial sector. “Finance wants macroeconomic stability,” he asserts (1). In fact, some
financiers—public officials, the “in” group, bond bulls, defense stock shorts, dealers reliant on bid-ask spreads, the highly leveraged, and banks with negative interest-rate gaps and high war-related credit risks—generally seek peaceful stability. However, other financiers—private ones with no ties to government, the “out” group, bond bears, defense stock longs, commissioned brokers, those rolling in cash, and banks with positive interest-rate gaps and low war-related credit risks—are more likely to crave war-related instability. For example, as William Silber explains in When Washington Shut Down Wall Street (Princeton, 2007), the outbreak of World War I hurt some financial interests, including the New York Stock Exchange, while aiding others, including foreign-exchange brokers and broker-dealers involved in the curb exchange that filled the vacuum left by the closure of the NYSE. Furthermore, anti-war financiers would be expected to be more vociferous than their pro-war colleagues, who would be fearful of giving away their trading positions and balance-sheet conditions by clamoring for conflict. Had Kirshner read more widely in financial history and archival sources, he undoubtedly would have told a more nuanced story.

Appeasing Bankers is a clearly written book proffering an interesting and potentially important hypothesis. A monolithic conception of the financial sector and a dubious methodology, however, will likely limit its audience.

Robert E. Wright
Stern School of Business, New York University


There is something unsettling about this book. The author is famous; the analysis is cogent and stimulating. But the general thrust of the book is unsatisfying. The disappointing result stems from the work’s carping and overly critical character. The book is savage in its criticism of numerous European scholars. Only V. Gordon Childe (not a historian) wins Goody’s wholehearted approval. He set Western scholarship on the right path with his book, What Happened in History (New York, 1942), only to have his broad Eurasian-wide perspectives hijacked by powerful Eurocentric historians of antiquity (Finley, among others), Chinese science (Needham), the world of the Mediterranean and capitalism (Braudel), and the emotions and the civilizing process (Elias).1

Goody’s argument is important, though by now more widely ac-

cepted by the historical profession than the author allows. He claims that Western scholars have had an irresistible tendency to see the European experience as exceptional and superior, the model by which all other cultures should be measured. In his view, Europeanists assert a unique genealogy for Europe, alone inheritors of the legacy of the ancient Greeks and Romans, and of a profoundly influential and singular feudal past, all of which led to European capitalism; science; the Enlightenment; the nation-state; and European world political, economic, and cultural dominance. The author demonstrates conclusively, if tediously in places, that many of the preeminent historians of the twentieth century find countless reasons to assert the uniqueness and superiority of the European experience. What these authors failed to recognize, in Goody’s opinion, is how similar the Asian experiences of science, capitalism, and notions of civilized behavior were to those that Europe had. Indeed, in his view, most Europeanists overlook that in some instances, Europe borrowed shamelessly from Asia. For Goody, scholars need to set the history of Europe in its proper Eurasian framework, much as Childe did in his study of ancient civilizations. Eurasia is, in fact, Goody’s favored politico-cultural-geographical and analytical term.

Methinks, however, that the gentleman doth protest too much. Elias’ *The Civilizing Process* appeared in 1939. Braudel’s major works on the Mediterranean and capitalism came out in French just after World War II, and most of Needham’s work on Chinese science took place in the 1950s, 1960s, and 1970s. His 1994 essay, “Conclusions and Reflections,” reflects his lifetime of work on China and attempts to answer the question why Europe, not China, had a scientific revolution, but the work is far stronger on China than on Europe. Lots of water has poured under the historical bridge since these books appeared. Many of the textbooks in European history today try to guard against Eurocentrism; many books in world history actually subvert Eurocentrism.

Thirty years ago Edward Said attacked the field of Orientalism in his controversial and path-breaking book, *Orientalism* (New York, 1978). Goody seems to want to do the same for the historical profession in this study. But thirty years is a long period in historical writing; what seemed fresh and exciting in the late 1970s now seems well known, even passé.

Goody’s charge that Western historians have stolen many of Asia’s historical achievements should not be taken lightly. It is, therefore, disappointing to find Goody guilty of the same offense. He promotes Eurasia as his new analytical unit. But what about Africa? Even according to his own evidence, parts of Africa, notably Egypt and North Africa, have significant historical roles to play. So why does he not employ the term Afro-Eurasia or at least Eurasia and North Africa. For a man so sensitive to ignoring the contributions of others to world history, this seems a significant oversight.

Robert L. Tignor
Princeton University
Historical Knowledge, Historical Error: A Contemporary Guide to Practice. By Allan Megill (Chicago, University of Chicago Press, 2007) 288 pp. $60.00 cloth $25.00 paper

The charge is often made that most historians fail to think very deeply about epistemology or to think clearly enough when they do. That criticism is difficult to refute, and Megill seeks to rectify the discipline’s bad habits by posing clear, straightforward questions about the nature of historical knowledge, questions that address topics—memory, identity, narrative, objectivity—that invoke terms currently much in vogue. Most of the book’s ten chapters were previously published over a couple of decades; in this volume, they are usefully grouped into four sections. The many references from one chapter to another, although somewhat disorienting, demonstrate intellectual coherence. Together, they treat all of these complicated matters and more, offering by way of examples extended criticism of specific historians.

Demanding intellectual rigor, Megill re-defines familiar terms and interjects his own firm opinions, leading readers along a circuitous path through the brambles of his own impressive learning. Readers can only trail along after this insightful and candid guide as he provocatively cites noted philosophers and historians, chastising them for solecisms commonly encountered in the press and classroom. While his often idiosyncratic use of learned references hastily expands the wide relevance of his analysis, it leads in many directions at once and becomes frustratingly idiosyncratic; for Megill rarely takes time for the fuller argument that might compel agreement. Firm questions posed in philosophical terms launch discussions marked by a (sometimes dense) search for intellectual precision and then quickly spill over his carefully constructed conceptual dykes. To read his essays is like auditing a bracing seminar conducted by a stimulating, opinionated teacher.

Megill attacks current fashion that privileges memory as personal and significant in its own right. Using examples from Herodotus to the Holocaust and the Vietnam memorial, he rejects the conflation of memory with history, which must require evidence. He warns against the political purpose of historical accounts that seek to reinforce identity. Conceding that in the postmodern world, historians’ grand narratives (of Christianity or progress) no longer carry the conviction that gave them power, he makes a case for the hermeneutic importance of historical narrative, despite its limitations. These points are all part of a larger schema. Narrative leads to “The Four Tasks of History-Writing.” Preoccupation with whether history can have a coherent meaning runs throughout the volume, to be taxonomically pulled together in “Four Ideal Typical Attitudes toward the Overall Coherence of History,” followed by “Four Postulates.”

The Annales school in particular comes in for sustained criticism, but Megill finds much to warn against in many other contemporary approaches to the past, from neopositivist imitations of science to cultural studies. He uncovers underlying biases in methodological preferences

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for problem-oriented history and for seeking underlying causes, and he
distrusts syntheses that imply causation without proving it. Not surpris-
ingly, his conclusion is titled “Against Current Fashion.” The surprise
lies rather in the traditional moderation of his recommendations for the
proper conduct of historical inquiry. Historians should avoid propagan-
dizing or any claim to exclusive truth; they should cross disciplinary lines
while respecting disciplinary boundaries; and they must attend to evi-
dence. He hopes “that the conceptual devices offered . . . in this book
will be taken seriously both by those who are attempting to read works
of history in a critical frame of mind and by those engaged in the difficult
task of writing history in an epistemologically responsible mode . . .
[without being] paralyzed by the uncertainties of historical knowledge”
(167). Taking these recommendations seriously does not make that chal-
lenge easier.

Raymond Grew
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To Have and to Hold: Marrying and Its Documentation in Western Christen-
dom, 400–1600. Edited by Philip L. Reynolds and John Witte, Jr. (New
York, Cambridge University Press, 2007) 536 pp. $75.00

This useful volume explores the documentation, definitions, and prac-
tices of marriage in the premodern, Christian West. In thirteen articles,
an international assembly of distinguished scholars addresses “how, why,
and when pre-modern Europeans documented their marriages . . . both
the function of documentation in the process of marrying and what the
surviving documents say about pre-modern marriage and about how
people in their day understood it” (ix). They consider how marriage was
defined and recorded in different regions for 1,200 years in both secular
and religious contexts. The volume reveals that even though contracting
a legal marriage for the purpose of ensuring the legitimate devolution of
property remained a principal concern, the ways to express it could be
subject to alteration or interpretation. Liturgical practice and charter for-
numiae might reflect differing secular and sacred marital traditions, and the
sometimes-competing religious, political, and personal interests.

Reynolds begins with a learned explication of often misunderstood
premodern marriage terminology, followed by a discussion of issues re-
garding marital consent. In a later chapter, he discusses Frankish dotal
charters based on documents from Karl Zeumer’s Formulae Merovingici et
Karolini Aevi (Hannover, 1886). Judith Evans Grubbs looks at written
contracts of later Roman marriage law across a broad area of the Empire,
and David G. Hunter concentrates on the Tabulae Nuptiales for Roman
North Africa. Laurent Morelle examines five dower charters from Laon
and Soissons for the later twelfth century, specifically addressing lineage
strategies in marriage. Cynthia Johnson also treats twelfth-century
France in her analysis of seven Occitanian marriage agreements. The emphasis on property devolution is unmistakable in her examples.

The British Isles are represented first by R. H. Helmholz’s analysis of marriage contracts in medieval England; next by Frederick Pederson’s examination of consistory court evidence from York; and finally by Art Cosgrove’s discussion of medieval Irish depositions. Agnes S. Arnórdsdóttir explores marriage documents for medieval Iceland. Thomas Kuehn puts a novel spin on the controversial and (in)famous case of Giovanni and Lusanna in Renaissance Florence. Martha C. Howell compares northern and southern marriage practices in her examination of late medieval Douai. John Witte, Jr., concludes the volume with a study of Reformation Geneva, demonstrating the persistence of property concerns against the backdrop of a changing theology that did not seem to affect marriage contracts.

This volume makes three especially noteworthy contributions to scholarship and education: Reynolds’ aforementioned introductory essay; the authors’ translations of the (mostly complete) documents that support their articles and their explanations of the methodologies that they employed to unearth clues about marriage practices; and a snapshot of the current state of scholarship for this type of social and legal history. A comprehensive bibliography, organized geographically, would have been helpful, but the book is nonetheless a marvelous contribution to our understanding of medieval marriage traditions based on an analysis of the documents that preserve them.

Joanna H. Drell
University of Richmond


Almost two decades ago, Durliat issued a challenge to other historians working in the late antique, early medieval period. He doubted that the lurid, frequently quoted descriptions of pestilence were more than literary hyperbole. This collection of essays provides the response to those who agree that big epidemics can produce significant historical effects. All of the contributions are by historians, and most of them circle back to the texts that insistently prompt re-examination of the origins, path, duration, responses, and long-term effect of recurrent plagues—works by Procopius and Bishop John of Ephesus (541–543), Gregory of Tours, Bede, Paul the Deacon (writing later), and a few other vivid ones not cited so often.

The two essays relegated to the end of the volume, by Robert

Sallares and Michael McCormick, pursue a multidisciplinary perspective, exploring what new, nontraditional investigations can be envisioned. These contributions show that recent developments in archaeology and molecular biology lend weight to a claim that texts alone can merely make suggestive—that *Yersinia pestis* was responsible for the more dramatic features displayed by victims of recurrent pestilence throughout this two-century interval. Epidemics driven or enhanced by this particular pathogen cause catastrophic mortality, and contribute to broad social, economic, demographic, and cultural changes.

The volume shows that a synthetic assemblage of plague events across great geographical, temporal, linguistic, and cultural divides is possible with collaborative research. The twelve chapters in the volume are grouped into four geographical and linguistic bundles and two bracketing overview sections. Alain Stoclet’s chapter within the section devoted to “The Latin West”—a trimmed, updated, and translated article originally published in 1988—comes closest to the theme of a culturally distinctive early medieval Christian society emerging in response to catastrophe. In Christian Gaul, bishops departed from their Byzantine counterparts by using processions, litanies, relics, and other material manipulations of the sacred to confront epidemics. Already in the sixth century, Latin Christendom was distinguished from the still-classical eastern Mediterranean by its particular religious responses to plague.

John Maddicott’s review of plague in seventh-century England from the same section—also an earlier publication—acquires new force within this context, accentuating the explanatory problem that plague presented within the newly vibrant Church in England. The archaeological and historical evidence singles out monasteries not only as sites where plague was especially devastating but also as sites from which it tended to disseminate. Apostasy among recently Christianized kingdoms and the creation of a new English saint (Cuthbert) to “spin” interpretation of what had occurred were urgent matters for the Church.

Michael Kulikowski’s contribution on the plague in Visigothic Spain summarizes chronicle accounts before wresting new evidence from a Toledo homiliary (four sermons on plague), providing the texts in an appendix. Kulikowski also connects recent archaeological findings to larger changes in burial practices, showing how plague may have propelled social change.

Ann Dooley meticulously assembled Irish evidence of a more textual and linguistic orientation ultimately to postulate a connection between plagues and thaumaturgic monarchs. Little’s introduction to the volume adds to this section’s general emphasis on the religious changes linked to plague in Latin Christendom, discussing the early medieval selection of new saintly protectors, especially Saint Sebastian.

A number of the essays summarize more expansive publications about the plague in Constantinople and Syria, where the spiritual issues and the archaeological evidence cast a vastly different light on the effects.
of recurrent plague. The scholars contributing to the large section on “The Near East” and “The Byzantine Empire” (Michael Morony, Hugh N. Kennedy, Dionysios Stathakopoulou, and Peter Sarris) spend relatively more time on the textual and archaeological challenges of researching the plague centuries. These four contributions are more ponderous, but they make important evidentiary claims about the recurrences of a specific epidemic disease distinguishable from viral infections (such as smallpox) and acute immiseration due to war or harvest failures. Sarris’ article bears a different title from his 2002 article in Continuity and Change, but it is essentially a republication. Nevertheless, Sarris’ essay is the only one to wrestle with evidence for the economic and demographic impacts of recurrent plague, though he does so largely through coinage and the fiscal destabilization of the Byzantine Empire.

Little’s useful introduction makes the surprising point that this volume is the first collection on the topic since the nineteenth century, and the collective bibliography at the end certainly offers a nearly complete update. Jo Hays’ overview of the general comparative aspects of human social responses to epidemics draws primarily from modern, comparative studies of epidemics, but it is the only contribution to examine the broad, exogenous environmental and climatological changes affecting Europe, the Mediterranean, and western Asia—though mainly from a social and cultural perspective. Only in the bibliography are works mentioned that articulate the demographic and environmental aspects of recurrent, highly virulent epidemic disease in global history. The volume would have benefited further had Lawrence I. Conrad, once a participant in this project, summarized his previous work on early Islamic sources about plague.

The final section—“The Challenge of Epidemiology and Molecular Biology”—carries the agenda for interdisciplinary, comparative research. Both Sallares and McCormick appreciate how heated an issue the retrospective diagnosis of late medieval plagues has become within the last decade or so. Both explain why and how modern scientific tools have been, and should be, used to look at the evidence anew and why understanding the past solely in its own terms is not a sufficient scholarly response to great ecological, demographic calamities. They exactingly explain why bubonic plague, caused by the emergent pathogen Yersinia pestis, is still the best provisional diagnosis for the distinctive and recurring pestilences. That diagnosis provides some measure of comfort that human knowledge, understanding, and capacity to mitigate devastating pestilence will continue to progress.

Ann G. Carmichael
Indiana University
This book is one of four volumes in a series edited by Robert Muchem-bled and William Monter. The overarching goal of the series is to inves-
tigate the cultural roots of Modern Europe, specifically concerning the
“origins of the European identity [that] has become a fundamental issue
at the dawn of the twenty-first century” (xxi). The seventeen chapters
that comprise this volume take a “cross-national, interdisciplinary and
comparative approach” (xxv). Each article considers the city as a site of
cultural exchange, examining, from a variety of perspectives and meth-
odologies, how and why cities absorb or simply juxtapose various cul-
tures.

Greeks, Jews, and Turks populate these pages as much as the more
well-known European transients (merchants, students, craftsmen, and
scholars, for example), but key to the issue of assimilation or segregation
that is central to all of these essays is the role of the foreigner. This high-
light becomes all the more interesting in light of the fact that townsmen
across Europe reached little consensus about which people actually
qualified as foreigners, and how desirable their presence in a native city
might be. City magistrates in host cities addressed questions of integra-
tion and segregation in various ways, expressed in institutional (legal, re-
ligious, and economic) and spatial (ghettos, fondaci) ways. Some cities
were more “closed” (for example, Lubeck) than others, and some more
“open” (like Hamburg), each with its own historical reasons. Foreigners
played decisive roles, sometimes favoring exclusion to strengthen or pre-
serve cultural identity (often accomplished through religious institutions
like confraternities and liturgy), and sometimes seeking assimilation by
intermarrying with the native population.

As expected in a book about cities, economic exchange is a recur-
rent theme in many of the chapters, but especially welcome to readers of
this journal will be its treatment from the perspective of “Structures and
Spaces of Cultural Exchange,” the focus of Part III. An especially fruitful
way to consider the dynamics of exchange is to locate the “liminal areas”
in the city where the “local [mixed] with the foreign”(204), such as fairs,
markets, squares, certain streets, mercantile exchanges, merchants’ lodg-
ings, and even princely residences, and to examine how people used
these spaces and invested them with meaning.

The omission of any sustained attention to the role of language in
the formation, conservation, or transmission of cultural identity is sur-
prising, given the long-recognized place of language in cultural identity,
and regrettable, given the polyglot nature of Europe’s cities. Perhaps lan-
guage is treated in another volume of this series. But the question of
how the channels of effective communication actually worked in these
urban towers of Babel remains. Aside from this minor quibble, the inclusion of such diverse places and peoples in a single volume with clear thematic objectives is a laudable accomplishment.

James R. Farr
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_The American Discovery of Europe_. By Jack D. Forbes (Urbana, University of Illinois Press, 2007) 272 pp. $34.95

In this volume, Forbes takes a new perspective on an old subject; the early presence and influence of Americans in Europe. Forbes argues that Americans arrived in Europe long before Columbus’ voyage to the New World, citing data from oceanography, genetics, botany, and archaeology, as well as from numerous documentary sources. Forbes argues plausibly for a significant American presence in Scandinavia and Europe beginning in the ninth and tenth centuries, but he is most convincing in his discussion of the presence of North and South Americans, Inuits, and American “captives,” some of whom were enslaved Africans or their descendants, in Europe from the fifteenth through the sixteenth century. His principle goal is to celebrate “the skill, courage, and adventurousness of these Americans” (4).

Forbes begins with a chapter outlining what is known of Columbus’ prior knowledge of the New World. He makes much of the marginal notes in Columbus’ copy of Pope Pius II’s _Historia rerum_ concerning two people of “Catayo” (Cathay?) who were said to have washed ashore in Galway, Ireland, some time prior to the 1470s. Forbes believes that these people were American Indians and that Columbus, then a seaman, may have spent time in Galway in 1476, where stories of these people provided him with convincing evidence for the existence of a continent to the west of Ireland, albeit one that he believed to be India.

Forbes argues that ocean-going vessels were common in maritime regions of eastern North and Central America and in the Caribbean, for at least the past 6,000 years, and that the Gulf Stream made the inadvertent or deliberate passage from the Caribbean or from North Eastern North America to Europe not only likely but certain. In further support of this view, he cites botanical evidence that shows many American plant species present on the Atlantic shores of Ireland, Scandinavia, and Spain in contexts that suggest that they arrived as flotsam, and that Caribbean maritime turtles are regularly sighted along Britain’s western shores.

Contrary to the idea that most of the indigenous inhabitants of the Americas were from a genetically homogeneous group that arrived by land bridge, Forbes states that at least some of these people were likely to have arrived via rafts, canoes, and other vessels. This perspective allows
him to argue for an “American” population that was the result of multiple ancient “inter-ethnic” relations. Marshalling a great deal of genetic and historical evidence, he suggests that theories of genetic uniformity among Native Americans are both historically misinformed and premised on antiquated “racial” categories.

In subsequent chapters, Forbes addresses theories concerning the possibility that some Americans were known to the ancient world as well. He also outlines his theories concerning the presence of Inuits from Newfoundland, Greenland, and Iceland in Europe, and suggests that mixed Norse-Inuit populations may have returned to Scandinavia prior to Columbus’ voyages. Forbes maintains that Americans (broadly defined) had an influence, both genetic and cultural, in Europe long before the seventeenth century, and that evidence for this influence can be found in folklore and material culture as well as in DNA.

Forbes’ suggestion that voyages to Europe by Americans, especially Inuits, are more likely than those from Europe to the New World prior to the fourteenth century is plausible. However, his presentation of the supportive data lacks coherence. Forbes ranges in time from 3,000 B.C. to the seventeenth century, sometimes in the same chapter. A heavy reliance on premodern historians, as well as on data from a variety of studies of varying quality and relevance, weakens his position as well. Although Forbes is critical of certain archaeologists for what he considers to be hidebound thinking, he himself uses archaeological data incautiously. For example, his discussion of the possibility that objects found on some Central American sites may be of “European, Semitic, [or]African” origin is highly speculative (108), and he casts doubt on the much-better supported notion that such artifacts demonstrate Asian influence in the Americas from an early period. Similarly, Forbes’ conclusions based on the appearance of purportedly “American” people and plants in Roman frescos would not pass muster among knowledgeable classicists. Nevertheless, readers will find much to ponder in this volume, and the extensive bibliography is a valuable guide to further study.

Kathleen Bragdon
College of William and Mary


Epstein’s _Purity Lost_ is about cultural boundaries in the Middle Ages and the people who crossed them. The setting for this stimulating and provocative study is the eastern Mediterranean world between 1000 and 1400. Epstein has drawn on his considerable interdisciplinary skills to investigate the “peoples who were usually separated in the Middle Ages by various constructed boundaries” (xii). Those barriers included color, re-
ligion, language, ethnicity, political affiliation, class, gender, and status (slave or free). Epstein has deftly brought together social, political, economic, and cultural history to examine “how people defied, overlooked, or transcended boundaries in order to establish relationships with those different from themselves” (xii).

While many historians continue to explore how and why medieval elites constructed social categories based on skin color, religion, and status, Epstein convincingly demonstrates that “mixed relationships” among those living in the eastern Mediterranean subverted those classifications (xiii). Self-interest, especially in its economic form, often trumped the barriers of color prejudice or religious affiliation. These bonds were so strong, the author argues, that “the skills developed in these relationships were one of the major engines of historical change in the eastern Mediterranean region” (xii). (How these social networks actually constituted a “world system” [xiii, 205], as the author argues, is however left unclear.) Throughout his study, Epstein emphasizes the persistence and “durability” of the classical cultural boundaries (especially color prejudice) (206). Nonetheless, the evidence shows that these “boundary makers” had limited influence on those living in this diverse part of the world (5). Real life did not always operate in ways that ancient and medieval guardians of culture would have liked or approved. Notions of honor, purity, and hierarchy may indeed have pervaded Western society, but those living on the periphery of Europe routinely created bonds according to their own interests. In the author’s words, “Purist values ran up against the realities of daily life” (8).

The book consists of five carefully crafted chapters, framed by an introduction and a brief, succinct conclusion. Chapter 1 (“The Perception of Difference”) examines the ancient roots of medieval hierarchies of ethnicity and color. Chapter 2 (“Mixed Relationships in the Archipelago”) relies primarily on notarial records from places like Crete and Chios to demonstrate how the marriages and business partnerships of “ordinary people” constituted “survival strategies in a challenging world” (52, 95). Even masters and slaves crossed boundaries. Chapter 3 (“Treaties and Diplomacy”) shows how the commercial and political tendencies of the Byzantines, the Venetians, the Tartars, and the Genoese could create “common ground” “across major barriers of faith and language” (135). In Chapters 4 and 5 (“Renegades and Opportunists” and “Human and Angelic Faces”), Epstein explores the renegades and opportunists who broke social barriers while the ancient and early medieval concepts of physiognomy and angelology continued to sustain the ideas of purity and inequality.

In his conclusion, Epstein suggests that the social ties examined in the book might have played a role “in eroding the consensus on boundaries and purity, drop by drop, slowly dissolving rigid thinking” (207). Indeed, at least in the eastern Mediterranean, merchants, slave owners, diplomats, and missionaries were routinely bending the “so-called rules” as they went about their lives (207). Ancient notions of hierarchy might
still have been persuasive in certain circles, but in others they were mere concepts that did not speak to daily existence. The eastern Mediterranean, as Epstein has elegantly portrayed it, was a special place.

George Dameron
Saint Michael’s College

*Heredity Produced: At the Crossroads of Biology, Politics, and Culture, 1500–1870.* Edited by Staffan Müller-Wille and Hans-Jörg Rheinberger (Cambridge, Mass., The MIT Press, 2007) 496 pp. $50.00

Müller-Wille and Rheinberger’s collection of historical essays about the ways in which European physicians and natural scientists thought about hereditary phenomena for four centuries before the modern era is an immensely rich tapestry of scientific, professional, cultural, and political cross-currents. When scientists (and most lay people) speak of heredity today, they take for granted a strictly biological process of inheritance that depends on hereditary elements (we call them genes) residing inside the body, transmitted during conception and completely insulated from an organism’s subsequent experience. We further assume that these elements determine our basic physical and mental constitution. If nothing else, the essays in this volume provide a welcome (and perhaps even timely) reminder of how particular, and how recent, all of these presumptive axioms of biological inheritance are.

But in fact, these essays do a great deal more. Collectively, they offer a broad narrative of the emergence of a modern science of heredity (and indeed, of the practice, new in the nineteenth century, of referring to heredity in nominal form). Against that narrative, however, they also provide a wealth of nuance and detail that repeatedly challenges the possibility of any simple account of this exceedingly complex (at times, almost fractal) history. As the individual authors meticulously demonstrate, scientific perceptions and conceptions of inheritance throughout this period were filtered (as indeed they always are) through a myriad of other concerns (about class, caste, race, property, disease, kinship, generation, evolution, religion, etc.). The net result does not easily lend itself to a straightforward history either of ideas or of discipline. Hence, the editors instead aim to depict “the emergence of a knowledge regime” by focusing on “the emergence of specific practices; the shaping of standards, taxonomies, and arguments; the evolution of architectures of hereditary knowledge; and the conjunctions of these elements in a variety of social arenas” (13).

With the help of an assemblage of conspicuously talented (and mostly young) historians (too numerous to name), the editors have succeeded in their goal; certainly, they have produced a handsome and emi-
nently readable book that should be of interest to historians and philosophers of biology for a long time to come.

Evelyn Fox Keller
Massachusetts Institute of Technology

Inventing the Indigenous: Local Knowledge and Natural History in Early Modern Europe. By Alix Cooper (New York, Cambridge University Press, 2007) 232 pp. $80.00

As Europeans in the sixteenth and seventeenth centuries encountered the realm of the “exotic,” some of them, especially those living in German-speaking lands, also took stock of natural objects in their local areas. The result was a form of “local knowledge” that augmented the general understanding of nature and revised the identity of Europeans themselves as inhabitants of specific indigenous domains. In inventing, or imagining, the indigenous within a European milieu, no one stands out so much as the Swiss German physician Paracelsus. Cooper perceptively identifies his advocacy of domestic German medicines (in contrast to exotic imports) as part of a medical polemic that called attention to local places, illnesses, remedies, and economies and that helped to create a discourse juxtaposing indigenous and exotic parts of nature. Others soon followed suit, sometimes compiling extensive tracts describing the natural worlds of specific territories and defending local medicines. Cooper notes especially Jan van Beverwyck’s Autarkeia Bataviae (Leiden, 1644) and Lambert Bidloo’s Dissertatio de re herbaria (Amsterdam, 1683) as attempts to reestablish a sense of self-sufficiency in a Holland overwhelmed by alien influences.

For medical students, knowledge of native plants was especially important. Early modern attempts to supply that information led to an entire genre of botanical texts ultimately described as “local floras”—inventories of plants growing within precise areas, usually no more than three to five miles around a town. Most often, the town in question possessed a university or botanical garden. In one example, Ludwig Jungermann and Caspar Hofmann, two medical students at the university town of Altdorf, blended into their list of plants growing near Nuremberg a description of the local landscape, emphasizing its fertility and celebrating the civic identity of those who lived there. Other texts emphasized the productivity of certain regions, spawning yet another genre of local natural history, the regional mineralogy, which proclaimed the latent political and economic power of subterranean natural wealth.

In contrast to “local floras,” larger natural histories included reference to entire territories and took account of living (including animals) and nonliving parts of local nature. Yet in these works, different visions of natural history, reflecting dissimilar concepts of “nature” and “terri-
tory” when tied to economic strategies and political ambitions, began to take shape. By the eighteenth century, another problem related to local knowledge—whether knowledge based upon local informants could properly be considered “scientific”—began also to divide those making room for indigenous parts of nature within larger discussions of natural phenomena. Cooper’s focus in documenting this quarrel is upon the bibliographies of natural history that were published by Johann Jakob Scheuchzer, a Swiss entrepreneur and polymath, and Carl Linnaeus, the more famous Swedish naturalist. Whereas Scheuchzer listed such works as local floras and popular pamphlets recording marvelous events like earthquakes and insect showers, Linnaeus tended to devalue the contributions of local natural history.

Cooper’s study is valuable, well informed, and, in making a case for the role of German territories and of learned local physicians in the pursuit of natural history, imaginative and challenging in its focus. It brings to light important sources that would otherwise remain obscure and makes a convincing case for their relevance among the practices of natural knowledge in the early modern era.

Bruce T. Moran
University of Nevada, Reno


This volume is a dense, resourceful, empirical, and revisionist exploration of localism and belonging in nineteenth- and early twentieth-century rural England. Snell is concerned particularly with how the political culture of parishes and townships impeded the full implementation of the Poor Law Reform Act of 1834, as well as with the loss of community that accompanied the forward momentum of modernization, and now globalization. His work, although much less friendly toward modernization, bears comparison with Eugen Weber, Peasants into Frenchmen: The Modernization of Rural France, 1870–1914 (London, 1979). Snell’s chapter-length treatments of local identity, settlement, rural marriage patterns, the clash of the old and new poor laws, the sustaining role of parish overseers, and the multiplication of parishes are imposing monographs in their own right.

The author’s first challenge is to show that the onset of industrialization and modernization, well underway by the 1840s, initially did not dislodge the embedded localism of the rural English. Loyalty to place was often fierce in its xenophobic resistance to newcomers and violent rivalries among proximate villages. A high percentage of endogamous marriages characterized village life well into the last years of the nineteenth century, and the sense of belonging to the parish and its traditions
was deep. On these issues, Snell’s work is pointedly anthropological, but his anthropology is heavily data-driven and descriptive, not theoretical. His conclusions on local marriage patterns, for instance, are informed by reports of 18,442 marriages from 69 parishes across 8 counties.

Snell is convinced that historians, in failing to appreciate “the main instincts and counter-alignments affecting rural societies” (73), have overestimated the extent of the new poor law’s influence and its deleterious impact on local attachment. Thus, two commonplaces have emerged. First, the new poor law’s intention—to confine poor relief as much as possible to residency in union workhouses where paupers’ livings would be “less eligible” than they would be through self-support on the outside—was largely achieved throughout the realm. Second, the new policy diminished the separate parishes’ control over settlement (place of entitlement) and their ability to dispense the outdoor relief authorized by the Elizabethan, or old, poor law.

A disincentive to building or expanding workhouses and providing indoor relief was the costliness of the effort. More telling, though, was that the workhouse idea was unable to prevail against the closely guarded rural tradition of parish-centered outdoor assistance. Villagers who “felt themselves to belong in their own parishes” resisted the intrusion and centralization of the new policy (211). Thus, measured by financial outlay, Victorian social welfare was not workhouse-centered: From 1840 to 1939, outdoor relief averaged 79 percent of all welfare costs in England and Wales. Outlays for workhouse-centered relief achieved parity with outdoor expenses in the Northwest by 1874/75, but only in London were they notably higher. Snell finds in the “participatory community spirit” of the overseers of the poor the best representation of the parishes’ strength (364); by comparison, the current pursuit of virtual communities is a callow substitute. Snell urges historians to adopt a revised approach to Victorian localism, however coarse it was in some aspects. The lessons learned about parish or local belonging may yet stimulate current policymakers.

Paul A. Fideler
Lesley University

Indian Ink: Script and Print in the Making of the English East India Company. By Miles Ogborn (Chicago, University of Chicago Press, 2007) 288 pp. $ 40.00

In Indian Ink, Ogborn examines how the print culture of the English East India Company compressed distant relations between Asia and Europe to create Britain’s eighteenth-century Empire. Trading companies played an integral role in the European competition for ocean-trade monopolies that began during the late sixteenth century. Among them, the English East India Company and the Dutch East India Company
Vereenigde Oost-Indische Compagnie or voc) secured the circuits and sinews of trade and political control through a combination of military action, economic aggression, diplomatic negotiation, and skullduggery that proved essential to the consolidation of European imperialism in Asia.

By the mid-eighteenth century, the English East India Company was using armed trade and assertions of corporate sovereignty to create a seaborne empire bounded by fortified coastal settlements. By the 1800s, the Company had edged out rivals from the subcontinent’s coast, turning its forts into the nucleus of its fast-growing territorial domain in India. Territorial control marked a shift in Company priorities in India from sea to land and a complex power struggle in Britain as the Company became embroiled in public debates about imperial aspirations. The struggle concerning ideologies of imperialism ultimately led to the Company’s mid-nineteenth-century demise, clearing the way for Victorian imperialism. Not surprisingly, the Company’s peculiar institutional form and ideological claims to sovereignty have attracted the attention of both historians of European expansion and scholars of modern India seeking to understand the onset of colonial rule in the region. Against this historiographical background, Ogborn represents a new voice—as a historical geographer interested in written artifacts.

By arguing that the interrelationship of geography and writing was essential to networks of trade and the establishment of political domination, Ogborn offers fresh perspectives on a literature preoccupied with the Company’s involvement in bullion and opium and embroiled in debates about whether the Company’s imperial ambition was preordained or whether it marked a break from earlier political systems in India. Much of this debate, which is conducted from the vantage point of eighteenth-century materials, makes Ogborne’s interest in folding the seventeenth century into his analysis a welcome intervention. His narration of Company history is an attempt “to take seriously forms of writing in order to understand how the changing relationships of knowledge and power shaped the encounter between Europe and Asia between the seventeenth and eighteenth centuries” (xxii). The main claim of the book is that “the Company and those who stood to profit from it depended on forms of writing to construct a new global geography of empire” (22).

Ogborn makes a persuasive case for viewing the geography of writing—the mobility of written objects and circuits of information—as a technique of spatial compression that enabled the making of empire during the eighteenth century. His chronologically arranged chapters lay out the Company’s history according to its writing practices, each chapter centering on a historical geography of a particular documentary form. Chapter 2 presents the exchange of Royal letters as crucial to securing initial trading relations in India. Chapter 3, the most interesting in the book, describes how correspondence and systematic accounts written in Company factories created an ordered office and organized...
the trade between Asia and Europe, to be read ultimately by governing committees in London. The focus on Madras in this chapter widens the conventional Bengal-centered view of Company activities.

Chapters 4 and 5 show how a veritable avalanche of printed pamphlets, lists of stock prices, and regulations influenced views about the Company’s credibility in late eighteenth-century Britain. In the final chapter, Ogborn argues that the Company built its empire in Bengal at that time on the security and certainty that printing appeared to provide. Yet, as he shows, even as printing became integral to a political philosophy of imperial rule, it also enabled a critique of empire. Written communication failed to compensate for the growing British disdain for conversing with Indians, an interesting, albeit derived, argument that could bear greater elaboration.

Certain scholars view the Company’s activities in India as an epistemic rupture from earlier forms of knowledge production in the region and, hence, rule; others underscore the material continuities that underwrote Company’s knowledge production. Ogborn’s methodological innovation of privileging the spatial qualities of concrete writing practices has great potential to overcome this intractable divide regarding the Company’s role in the production of colonial knowledge about India. He demonstrates that multiple forms of “Indian ink” played a crucial role in the making of the Company Empire, creating a model of state control envisaged through print and script, and securing relationships between distant places.

Ogborn’s attempt to bring a “geographical interpretation to both the history of empire and the history of the book,” however, has some demonstrable limits. He is right to suggest that “Company printing in India came with empire rather than trade” (199), but he runs shy of extending his meditation on writing and authority to the relationship between the Company’s pen and the Company’s sword. Company militarism—armed trade in the seventeenth century and field armies in the eighteenth century—was crucial to its ideology and practice. Abstracting writing and printing from its pre-eminent role in connecting the Company’s military and fiscal domains sanitizes the relationship between print and empire.

Bhavani Raman
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Certain Other Countries: Homicide, Gender and National Identity in Late Nineteenth Century England, Ireland, Scotland, and Wales. By Carolyn A. Conley (Columbus, Ohio State University Press, 2007) 255 pp. $49.95 cloth $9.95 CD

Trial lawyers devote great efforts, through their use of challenges, to craft juries that they believe will be sympathetic to their arguments. A
specialized field of jury psychology helps them to play upon jurors’ emotions or personal backgrounds. Attorneys, though they can not control which judge will be hearing their case, know that some judges will be sympathetic and others indifferent or hostile to their client. According to Conley, however, the solid men of property who comprised the juries in England, Scotland, Wales, and Ireland between 1867 and 1892 responded to homicide cases according to simple notions of “national character,” class behavior, and conventional expectations about gender and family roles. Likewise, judges sentenced harshly or moderately according to such viewpoints. Nineteenth-century judges and juries classified murders under four categories: “as an act of barbarism representative of an alien mentality, a comprehensible response to provocation, as the unfortunate result of a careless moment, or as a fundamental threat to the existing order.” Judges and juries in England, Ireland, Wales, and Scotland differed in the way that they placed homicides under these categories; they felt “obligated to demonstrate what their own nation would and would not allow” (209).

Conley delves into the complexities of these responses, tracing general trends from homicide statistics and court reports and bringing them to life with accounts of specific cases. Among her findings are that English judges and juries were actually harsher to respectable citizens who committed murder, believing that they should know better. Contrary to expectations, they were more lenient to working-class killers, often reasoning that they were too uncivilized to help themselves. If the respectable person were a woman, they often found her to be insane. These views were not universal; exemplary executions for murder occurred as well.

In Ireland, jurors regarded premeditated murder as an English characteristic alien to the Irish, and they were lenient toward Irishmen who killed somebody in a “recreational” fight. Since jurors often considered Irish homicides the result of fights between evenly matched men, they thought that only a man who had lost his mind would be capable of killing a weaker woman.

Scots judges and jurors were harsher toward Irish migrants to their country, but in general expected some sort of atonement for a murderer’s sin. Given clear mitigating circumstances, they considered time in jail while awaiting trial sufficient atonement. Because Scots courts often considered women as capable of reason as men, they were less likely to find women insane than English juries were. Wales had few homicides compared to other parts of the United Kingdom. Conley suggests that punishments were more likely to remain under the jurisdiction of local communities than the English-controlled courts.

Conley writes clearly, whether developing her arguments or describing murders. Even those readers who are statistically challenged, however, may well think that the book would have benefited from more tables; the one that Conley provides shows the percentage of convictions and insanity verdicts, and the average sentence for killings ac-
cording to gender in the four parts of the nineteenth-century U.K. (93). But the lack of numerical data is hardly a major defect. Certain Other Countries is highly recommended for anybody interested in the comparative study of crime and the responses to crime.

Wilbur R. Miller
State University of New York, Stony Brook


Hennock has in the past made an important contribution to the comparative history of British and German social policy. This volume takes his work further by examining poor relief, sickness and industrial injury, invalidity, old age, and unemployment policies between the mid-nineteenth century and 1914. Its main focus is the new light shed on German social policy by three volumes of documents on German (mainly Prussian, as Hennock points out) social policy; a fourth volume is forthcoming. As Hennock also makes clear, the survey of social policy in England and Wales (Scottish social policy often followed different tracks) is essentially a synthesis of secondary work, much of it a summary of sections in his British Social Reform and German Precedents: The Case of Social Insurance (New York, 1987) and of other, sometimes dated, secondary works by other authors. A central purpose of the volume is to bring the new German material to the attention of English speakers and to provide a fresh perspective on the history of British policy through this comparison.

Hennock initially supposed a consistent difference between German and British policy—the former based upon compulsion, state bureaucratic control, and professionalism and the latter on voluntarism, local initiative, and amateurism—as much influential British opinion insisted during the formative years of the welfare states of both countries. This new work, however, does much to substantiate, and to complicate, such a comparison.

The perception that Germany under Bismarck pioneered modern social policy with the introduction of sickness and old age insurance in the 1880s is a commonplace of comparative social-science analyses. As Hennock interestingly points out, both parts of divided post-1945 Germany invoked the precedent: West Germany lauded it, but East Germany challenged it as insufficiently redistributive, echoing the conventional criticism that its original intent was to undermine the emerging

1 Quellensammsammlung zur Geschichte der deutschen Sozialpolitik 1967–1914 (Mainz, Germany, 1997–) in four volumes: I (1867–1881) and II (1881–1890), edited by Florian Tennstedt; III (1890–1904), still to come; and IV (1904–1914), edited by Karl Erich Born and Hansjoachim Henning.
Social Democratic Party by demonstrating the beneficence of the liberal state, not actually to extend benefits.

Hennock repeatedly challenges this view, arguing that Bismarck’s particular aim was to stimulate German economic growth by giving workers greater security. But he does not demolish it entirely, since he shows Otto von Bismarck commending state pensions “as a way to undermine socialist influence among workers” as early as 1880. A new finding is that Bismarck was hostile to extending the insurance principle to pensions, preferring direct state funding, although he was overruled by his civil servants. Unfortunately, the documents on which Hennock relies do not enable him adequately to explain Bismarck’s reasons.

The book is stronger on processes of policy formation than on outcomes, partly because statistics in Germany—for example, on coverage and costs of provision—are less comprehensive than in Britain. It provides much new information on Germany but no significant new perspectives on British policy. The British government was not, as Hennock suggests, “ignorant” of the German pension system. The British examined it thoroughly and made a conscious choice to adopt a different, noncontributory model. As Hennock, and others, suggest, the two schemes had different aims—the German one to increase the security and productivity of skilled workers and the British one to relieve most of the poor. Gender does not loom large in Hennock’s analysis, but the British government was aware that females were difficult to fit into an insurance system based upon regular work at adequate pay.

Hennock also fails to understand that Britain introduced health insurance in 1911 to serve a different purpose than pensions—namely, to stimulate, as in Germany, worker security and productivity rather than to reduce severe poverty. He provides no clear comparison between the beneficiaries of these different schemes in the two countries. He rightly, however, points to an important, enduring difference between the two systems. British state benefits have always been, and remain, minimal, relying on individuals to supplement them with their own saving, whereas the German state, more realistically, believes that without compulsion, most people will not, or cannot, save. Hence, Britain has the lowest state benefits in Western Europe, and many older people, especially women, are poverty-stricken.

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Medical Lives in the Age of Surgical Revolution. By M. Anne Crowther and Margaret W. Dupree (New York, Cambridge University Press, 2007) 425 pp. $120.00

Few disciplines have felt the death of biography, proclaimed by several generations of social historians, so keenly as the history of medicine.
Continued emphasis on writing “history from below,” and a tendency to dismiss all individual biography as hagiography, has encouraged historians of medicine to seek other ways to place patients and physicians in their historical context. Crowther and Dupree have responded to this challenge by combining a broadly sociological methodology—collective biography—with passages of thick description drawn from the micro-historical tradition. They offer a collective biography of 1,938 Scottish medical students who began their studies at Edinburgh or Glasgow—then the two largest medical schools in the British Empire—between 1866 and 1874.

Crowther and Dupree begin with a spirited reconstruction of the first day at medical school and spend the first four chapters exploring the social, professional, intellectual, and economic dimensions of student life. They use photographs to explore the expression of these associations and distinctions through dress and deportment—for instance, the silk top hats worn by Edinburgh students who had passed their first professional qualification. By choosing to study a well-defined group, and by paying particular attention to the cultural fault-lines within that group, they get around the biggest challenge of this method—selecting a body of subjects without imposing a false sense of coherence.

This dual methodology permits Crowther and Dupree to relate the experiences of their cohort to the social networks that they fashioned at all stages of their lives. In many cases, these networks followed lines of communication underpinned by the British colonial project. Some students were “old India hands” who had served in the Indian Medical Service for decades but now required a medical degree to meet the requirements of the 1858 Medical Act. Others were some of the earliest women to seek a medical education; their experiences shaped the entry of women into British medicine in subsequent decades. By focusing on Scottish medical schools (and, by implication, decentering London), Crowther and Dupree open a fascinating window on the framing of British national identity, revealing some of the ways in which the members of a geographically and ethnically diverse group came to think of themselves as British. Roughly half of the cohort were not Scottish by birth, but their experiences in Edinburgh and Glasgow imparted a style of medical thought and practice that was both distinctively Scottish and typically British.

Crowther and Dupree have clearly (and justly) aimed this book at those with a decent working knowledge of British medical history. The first chapter dives straight into a historiographical analysis of medical professionalization, making only cursory efforts to open the subject to workers in other disciplines. But notwithstanding this minor quibble, this book represents collective biography in its most humane sense, combining sociological insight and historical analysis to illuminate individuals and groups in their time and place.

Richard Barnett
University College London
This is an intriguing and important book in intellectual, and to a degree, social history. Pattison, a nineteenth-century intellectual and Oxford don (which are not, as Jones emphasizes, necessarily the same thing) is barely remembered nowadays as a major thinker or shaper of his times, although he was undoubtedly important, as this study successfully argues. He might be better known for allegedly serving as the model for Edward Casaubon in George Eliot’s *Middlemarch* (London, 1872). Pattison wrote a study of Isaac Casaubon, a sixteenth-century classical scholar and, like Eliot’s creation, he had an unhappy marriage characterized by sexual incompatibility. This connection should not suggest that Pattison also demonstrated an aridity and paucity of scholarship. In fact, Pattison was a prolific writer on a variety of subjects, though his corpus of work is diffuse. The prevailing idea that his campaign in favor of the modern research university constituted his major contribution turns out to be true, but not in the way commonly supposed. Jones argues convincingly that Pattison is part of a continuum, heavily influenced by his early experience as a Tractarian and as a follower of John Henry Newman. His conception of the university, however, is generally to be taken to be at the opposite end of that spectrum. Even though he remained an Anglican priest, Pattison became increasingly secular in his outlook, but Newman’s influence persisted.

This book is divided into two sections, one on Pattison’s life and the other on his ideas. It provides a good sense of Pattison’s early days as the son of a vicar, and what Oxford was like when he arrived in 1832. His “home” schooling badly prepared him for the university where he was to spend the rest of his days, but he adapted to it. Oxford was (and is) intensely parochial, though no longer small-scale. Nine fellows elected him head (rector) of Lincoln College, a post that he held for many years without doing much work as an administrator. The dons usually had some connection with the world beyond the university, possibly through their students or through their efforts to secure preferment in the Church of England. Pattison’s derived most notably through his essays in the great nonacademic journals of the time. He was also a member of the Metaphysical Society and the Athenæum. He represented the don as an intellectual, and as an early practitioner of the history of ideas, his greatest scholarly achievement. In his day, he was a well-known and successful figure, leaving the considerable estate of £46,000.

Jones shows that Pattison’s campaign for the endowment of research aimed to reduce the reliance on classics and mathematics and encourage original scholarship and research-oriented studies: “He thought specialization and research offered a more effective way of achieving the goals of rounded character” (213). His belief that undergraduates would become “better” in this educational climate was no less an act of faith than was the traditional teaching method that he sought to replace.
Though not totally repudiating his earlier more religious training, Pattison argued that new knowledge should be a crucial part of the university, ultimately to be shared with the outer world though not as its main purpose. The new dons that he envisioned (undoubtedly men in Pattison’s time, although he was supportive of women’s education) were not yet, for better or worse, the “modern” researchers who populate today’s universities, many of whom view the professional world to be at least as important as the world of teaching.

Peter Stansky
Stanford University


The Mass-Observation studies of mid-century working-class life in England are iconic examples of twentieth-century social science. Like the earlier social-survey tradition that examined urban poverty and social class—for example, Charles Booth’s study of Londoners’ lives (1886–1903), the Russell Sage Foundation’s Pittsburgh Survey (1907–1908) or Richard Staugton Lynd and Helen Merrell Lynd’s Middletown studies of Muncie, Indiana, during the 1920s and 1930s—the “M-O” studies have had an enormous impact on social-science methods and on the general understanding of the social and class relations of their day. Also like the work of Booth, the Chicago School, Kinsey Associates, or the Lynds, the studies have recently generated a theoretical and historical literature examining the motives and intentions of the original researchers, the day-to-day practice of the studies, as well as re-examinations of the original data in light of later developments in cultural theory and the social sciences. Hubble adds to this literature by placing his history of the Mass-Observation project into the larger context of mid-twentieth century intellectual life and later theoretical debates about class and culture in Britain.

The Mass-Observation project was the creation of Tom Harrisson, (an ornithologist and anthropologist), Humphrey Jennings (a filmmaker), and Charles Madge (a journalist and poet) in 1937. They proposed to create a “science of ourselves” using “volunteer observers” to monitor and record the everyday life of ordinary Britons (5). One part of the project involved the observers reporting data from questionnaires developed by the project leaders; another involved a community study of Bolton and Blackpool, named “Worktown” in the publications. The published studies and data collection continued through World War II into the early 1950s. In 1970, the University of Sussex acquired the original data; in the early 1980s, the project was revived at Sussex. It continues to collect and archive data using volunteer observers. Details are available on the Mass Observation Archive website, http://www.massobs.org.uk/index.htm.
The project’s ambitious social and political goals were embedded in the larger political debates of the period. For example, Madge, Harrison, and Jennings began data collection during the 1937 abdication crisis, convinced that the journalistic coverage of the crisis did not capture working-class attitudes toward the decisions of Edward VIII to follow his heart and abandon his throne. The project’s survey of working-class attitudes about saving money helped to develop and evaluate Britain’s wartime fiscal policy, including the practice of income-tax withholding.

This book is a dense and detailed study about twentieth-century social science and cultural theory. For secondary users of the original data today, as well for those interested in the history of the social sciences and debates about twentieth-century class politics, Hubble provides an exhaustive investigation into the origins of the Mass-Observation studies as well as into their influence on successive generations of intellectuals and social critics.

Margo Anderson
University of Wisconsin, Milwaukee

Map-making, Landscapes and Memory: A Geography of Colonial and Early Modern Ireland c. 1530–1750. By William J. Smyth (Notre Dame, University of Notre Dame Press, 2007) 584 pp. $80.00

Historical geographers have made contributions to Irish historiography disproportionate to their numbers. After all, no serious student of Irish history can ignore Evans’ foundational work on Ulster archaeology and folkways, Houston and Seamus Smyth’s pioneering work on the transnational nature of Orangeism, Whelan’s wide-ranging insights on Irish nationalism and 1798 or Prunty and Murphy’s respective work on the urban geographies of Dublin. Rooted in a mastery of a remarkable array of sources, Smyth’s Map-making, Landscapes and Memory is a worthy addition to this lively scholarly tradition. Taken together with David Dickson’s magisterial Old World Colony: Cork and South Munster, 1630–1830 (Madison, 2005), this new book significantly improves our understanding of early modern Ireland.

Smyth’s central contention in this ambitious book is that Ireland’s uniqueness in Western Europe lies in the fact that it is both a colonial and early modern society. As a geographer, Smyth is particularly concerned with how space is organized, articulated, and transformed by the violent processes of change that dominated this era of Irish history. From such a perspective, William Petty’s maps were nearly as essential to the conquest of Ireland as Oliver Cromwell’s army was; Petty and other mapmakers made sense of the Irish landscape for English policymakers,

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creating colonial knowledge essential to the English project of remaking Ireland in its own image.

Smyth’s scope is wide; his narrative sprawls across 200 of the most eventful years in Irish history. His masterful and creative use of a wide array of evidence—from a variety of maps to the 1659 Census that he employs to reconstruct the destructive/creative process that led to the making of modern Ireland—is exemplary. Moreover, unlike many scholars in Irish Studies, he applies theoretical and methodological frameworks to help glean insights from his often sparse (if wide) source base. In a particularly deft chapter about the trauma and violence of the 1640s, he adapts the anthropological insights of Taussig to interpret the complex and powerful legacies of the atrocities detailed in the 1641 Depositions.2 The use of multidisciplinary and/or comparative approaches is still far too rare in Irish history; Smyth’s work displays its potential admirably.

Smyth complements his broad overview of the period with three detailed case studies of Dublin, Tipperary, and Kilkenny. They display the intricacies of local shifting power relations and underline the importance of regional variation in this turbulent process. Given Ulster’s important and unique place within the reformation of Ireland, however, Smyth might have thought to include a northern case study as well.

A more substantial problem centers on the ways in which Smyth’s narrative emphasizes binary colonial structures over the more complex relationships forged within early modern Ireland’s turbulent society. Although the late sixteenth- and seventeenth-century Irish experience has all the hallmarks of a violent colonial encounter, much to the advantage of English-speaking Protestant settlers, this binary model of English conquest and Irish defeat was complicated by a middle ground characterized by a certain social continuity. Smyth is at his best when his narrative transcends his binary model, but as the book progresses, the middle ground disappears, as illustrated by Smyth’s reductionist and dismissive treatment of Ulster Presbyterians and the United Irishmen.

But it would be unfair to end on such a negative note. This is a strong and learned book, full of insights for scholars interested in Irish history.

Sean Farrell
Northern Illinois University


Ruggiero has been thinking about Niccolò Machiavelli and sex for many years. I write with personal knowledge, since Ruggiero many years ago in Florence suggested that I and a girl that I was dating read

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Machiavelli’s *Mandragola* in the evening under the pretext of improving our Italian. The courtship was a smashing success, but the ensuing marriage was a failure—a romantic version of Cesare Borgia’s experience in the Romagna.

Ruggiero’s long acquaintance with Machiavelli, his eye for detail, and his generous sense of humor are fully evident in *Machiavelli in Love*. The volume is best understood as the third in a series with *Boundaries of Eros* (New York, 1980) and *Binding Passions* (New York, 1993). As in the previous volumes, most of the chapters offer one or two captivating stories. And the themes remain those of the earlier books—the relationship between sex and power, the creation of urban subcultures centered about sexual activity, and an attempt to define the Renaissance as a distinctive period in the history of sexuality.

Three things in particular are worth underlining. The first is that Ruggiero in this book applies to a group of what he considers to be high literary texts the methods and insights that he developed in years of studying the records of criminal archives. Thus, *Mandragola* (1518), Machiavelli’s correspondence with Francesco Vettori, Giovanni Boccaccio’s *Decameron* (1350–1353), Baldassarre Castiglione’s *Book of the Courtier* (1528), and Antonio Manetti’s fifteenth-century novella of the fat woodworker are among the texts that he explores for the ways in which they illuminate aspects of Renaissance sexuality. The second new element is an intermittent polemic with Foucault and/or certain “neo-Foucaultians” (not always identified by name) who argue that there was “virtually no sense of sexual identity before the modern period” (dust-jacket text).  

A third aspect is Ruggiero’s more forceful statement of the role of sexuality in our understanding of the Renaissance life cycle, in which youth (*gioventù*) assumes a pronounced role.

As always, Ruggiero knows how to tell a good tale. His version of Boccaccio’s *Rustico and Alibech*, in which the maid Alibech learns to “put the Devil back in Hell,” manages to be nearly as funny as the original, while also pointing out that one of the characteristics of Renaissance sexuality was its playfulness. Ruggiero’s Boccaccio was no “medieval” (as in Branca’s reading) but rather an early “Renaissance Man.” Far and away the most elegant of his discussions is an essay (previously published) on the fat woodworker, in which Ruggiero reaches as close as a historian can get to discussing what an individual’s sense of self may have been. Manetti’s novella reveals a Renaissance “self” that, because it lacked internal anchors, was reliant instead on an urban topography consisting of semiotic networks of friends and acquaintances.

Regarding Machiavelli (who figures in the title and in about half of the book’s substance), Ruggiero brings out the bawdy quality of the correspondence with Vettori—an aspect (as he delicately points out) ne-

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glected in Najemy’s relatively chaste treatment of the same letters. With respect to the view (argued by Martelli) that the correspondence suggests that Machiavelli and Vettori had homosexual affairs when they were young, and that as adults they may have had occasional affairs with young men while also carrying on with women, Ruggiero seems uncharacteristically indecisive. After some strangely harsh words for Martelli (in a note to page 109 and elsewhere), Ruggiero develops what he calls “an admittedly highly problematic reading” (albeit similar to Martelli’s), contending that the young Machiavelli had homosexual liaisons at the time of Savonarola’s preaching (129–130)—although Ruggiero subjoins this contention with a puzzling statement that he does not want “to argue that [proposition]” (130).

The indecisiveness about a seemingly central issue is confusing. Possibly Ruggiero was trying to meet the demands of a contrary reader for the press. At any rate, it remains one of Ruggiero’s basic contentions that young men in the urban elites of Renaissance cities had a sexual identity that was as yet unformed, meaning that they were given to experimentation on both sides of the gender line. Part of the problem may be a lack of familiarity with the classical sources that Machiavelli and Vettori knew so well. A reading of the poetry of Tibullus (54–19 B.C.) is in order. (And, more broadly, concerning the question of the life cycle, of Rudolph Bell’s How To Do It [Chicago, 1999]).

Treating Machiavelli’s plays, Mandragola and Clizia, Ruggiero seems more confident, mining rich veins of humor. The argument that the playwright presents himself as an aged, un-Machiavellian lover in the person of Messer Nicia is right on target. When Ruggiero turns to Castiglione’s Book of the Courtier, in which he might have been expected to explore the many amorous games at work, he offers instead a sober and well-argued treatment of the use of the word virtù. The thesis that virtù was an important word in both The Prince (1513–1515) and the Courtier makes perfect sense, although it may not have a similar range of meanings in both texts. Was Castiglione possibly attempting to “rescue” virtù by restoring meanings that Machiavelli had suppressed? Castiglione often appears to reply to Machiavelli’s Prince, which he surely read in manuscript.

Readers of Machiavelli in Love will certainly come away with a feeling for the playfulness of Renaissance sexuality. One of the book’s achievements is that it shows the extent to which the literature of high culture had deep roots in everyday experience. Few will ever again doubt the importance of sex in creating Renaissance identity.

William J. Connell
Seton Hall University

Property and Civil Society in South-Western Germany, 1820–1914. By Jona-
than Sperber (New York, Oxford University Press, 2007) 295 pp. $50.00

From eighteenth-century Scottish philosophers to Enlightenment law codes to the philosophy of Georg W. F. Hegel, the exercise of private property rights was a central and necessary element of civil society. Yet, curiously, the last two decades of scholarship about civil society has largely ignored the role of property, emphasizing instead bourgeois associational life and its public sphere. Sperber’s superb study corrects this oversight. Not disputing the significance of social networks and opinion formation, Sperber nonetheless illuminates another aspect of civil society—the familial and civic relationships that congealed around property. With stunning empirical richness, Sperber examines property disputes in court over the course of a century, showing how the cultural, social, political, and economic dimensions of land ownership formed the fabric of civil society. He thus injects locality and materiality into the discussion, showing how property relations not only structured family and community but also shaped normative values.

Accordingly, Sperber presents the nineteenth century as a “golden age of property,” when “the practice and culture of property transactions” articulated social norms, defined cultural boundaries, delimited economic opportunity, and expressed emotional relationships (9). In marked contrast to the restrictive property customs of the old regime, as well as the altered strategies of social mobility under the twentieth-century welfare state, the importance of property between 1815 and 1914, Sperber argues, was never greater. With its wide-ranging comparisons, magisterial command of the secondary literature, and its instructive periodization of “property regimes,” this well-crafted study elucidates the culture of property that framed the ideals and practices of civil society. Among Sperber’s numerous books on German and European political and social history, this one is arguably the most innovative of his prize-winning corpus.

The study rests on a sample of 1,646 property disputes in the German Palatinate, a noncontiguous Bavarian province in southwestern Germany that practiced Napoleonic law. This series is both dense and evenly distributed; most decades offer hundreds of disputes, further allowing Sperber to divide litigants by social group and religion. Significantly, the study cuts across most of Palatine society, examining not only such impersonal legal bodies as the state and railroad companies but also a full range of social classes, from the wealthy to day laborers. The social heterogeneity of ownership is striking. The “small producer” class of peasants, artisans, boatmen, and teamsters formed the largest group of plaintiffs (32 percent), but even the working class—day laborers, servants, and miners—constituted 17 percent of the pool (260). Although this diversity does not typify central Europe, Palatinate conditions are eminently comparable to areas in western Germany, France, England, the Netherlands, and North America.
Sperber exploits this database in various ways. While he provides information for historians, anthropologists, and political scientists interested in broader sociocultural comparisons, he also limns the social complexities of property ownership with individual case studies, bringing to life the struggles and ironies of family and community life. Sperber’s felicitous marriage of serial analysis with microhistory reveals a graceful expertise with a range of economic, legal, anthropological, and social-historical approaches. Not least, Sperber is concerned with discerning change over time, demonstrating civil society’s overlapping ties to both the old regime and the twentieth century.

Four large themes organize the study’s chapters: property’s impact on the family life cycle; the acquisition and transmission of property and its uses for credit; the legal, gendered, religious, and medical boundaries of property, which framed normative civic relations; and a narrative periodization of nineteenth-century property uses. As such, they offer arguments for numerous disciplines. Historians of the family will benefit from discussions on how property impinged on both family solidarity and individuals’ claims to independence. Sperber further explores how familial cooperation and emotional ties were simultaneously bound up in “rational” and “calculated” market interests. This book’s arguments on the granting and taking of credit—“a central and underappreciated feature of property transactions” (74)—will be of particular importance to economic and social historians. Similarly, the concreteness with which Sperber examines the mutually reinforcing constructs of property ownership with honor, respectability, medical competency, gender relations, and religious difference ought to draw a wide readership.

But the book’s arguments on property’s foundational relationship to civil society especially command attention; a comprehensive interpretation of civil society must incorporate the material and sensuous culture of property transactions. Sperber’s compact history of property is thus an outsized contribution to current scholarship on civil society, constituting a masterful blend of methods and literatures from one of the field’s leading scholars. It should stimulate discussion, invite comparison, and launch future research on “property regimes” in modern civil society.

James M. Brophy
University of Delaware

_Tulipmania: Money, Honor, and Knowledge in the Dutch Golden Age._ By Anne Goldgar (Chicago, University of Chicago Press, 2007) 425 pp. $30.00

This valuable and interesting study is the first genuinely scholarly investigation of tulipmania. It uses archival and other contemporary records rather than the usual, mostly scandalized contemporary pamphlet literature, which often verges on anti-tulipmania propaganda. Goldgar shows...
that the Dutch tulipmania of the 1630s was a highly complex social, cultural, and mass-psychological phenomenon concerned with taste, fashion, and connoisseurship and bolstered by intense discussion among close-knit communities of enthusiasts. She provides an opportunity to study in microcosm the problem of how, in a rapidly burgeoning commercial society, men and women struggled to relate fast-changing material values to time-honored social conventions. Thus, her work has much to teach about that highly dynamic intersection where early modern art, science, commerce, and religion converged.

Goldgar identifies the main body of the tulip fanciers as men of the prosperous middle strata of society, especially mid-level merchants and highly specialized master craftsmen. She shows that they tended to form closely interweaving networks in particular cities, notably Haarlem, Utrecht, Middelburg, and Amsterdam, and—what is especially significant—that they often belonged to Mennonite or other fringe Protestant congregations and often came from immigrant families originally from the southern Netherlands.

Tulipmania is probably most well known for the steep rise in prices of certain categories of tulip bulbs in 1636/37 and the financial crash and accompanying scandal that followed; the crash reputedly bankrupted or caused heavy losses to vast numbers of investors. Yet, in actual fact, few were bankrupted, and hardly anyone was reduced to beggary. The reports claiming the contrary were tied to the fierce denunciation of the tulip craze and its participants, by respectable, primarily Calvinist, critics in subsequent years. The case of Jan van Goyen, the great landscape painter, who is known to have conducted several costly deals in tulip bulbs on the very eve of the crash, has often been cited as confirmation of the supposedly devastating effects of the craze. Goldgar, however, shows that the bulk of the artist’s massive debts, totaling 18,000 guilders at the time of his death, were due much less to investing in tulips than to his frenetic dealings in land and other forms of real estate. The general effect of the crash on the Dutch economy was undoubtedly slight.

There is much to be appreciated in this book. One general criticism, however, is that Goldgar makes little attempt to locate either the feverish spate of investment in tulips or the ensuing crash within the wider context of Dutch commercial history. The 1630s were a time of rapidly improving prosperity and rising wealth in Holland, especially from agriculture and industry, at a time when possibilities for investment in overseas trade remained severely constricted primarily because of the Spanish economic embargoes from 1621 to 1647. This commercial scenario was almost bound to fuel domestic investment crazes. Unfortunately, Goldgar makes little contribution to what has been said before about this important aspect of the subject.

Jonathan Israel
Institute for Advanced Study
The History of Finland. By Jason Lavery (Westport, Greenwood Press, 2006) 195 pp. $45.00

Lavery has produced a remarkable history of Finland, especially given the length of the book. He judiciously and engagingly covers not only the history and prehistory of the geographical place currently occupied by the Republic of Finland but also the limitations and possibilities of the historian’s craft. Although the author does not have the luxury of breaking new ground, he is conscientious and successful in selecting from the kind of recent research that makes this work much more than a repackaging of well-established narratives and theories.

Given the title of the series to which this book belongs—Histories of Modern Nations—it is fitting that Lavery devotes six of his nine chapters to the period from 1809 to the present. In many ways, he was assisted in this distribution of his attention by the fact that Finland had scarcely an autonomous existence prior to its separation from Sweden by force of Russian arms in 1808/09. Lavery negotiates this situation deftly in his first three chapters, neither exaggerating the early signs of regional and then protonational cultural and political identity within the eastern third of the Kingdom of Sweden nor celebrating them beyond their due within the context of the eighteenth century.

Lavery captures the distinguishing features of Finnish experience, including the darker side of Finland’s political history, and he points expertly in the notes and bibliography to further readings that can provide more detail about specific aspects of Finland’s past and present. His text is eminently readable, despite a few lapses in editing (misspellings, missing words, etc.). Less valuable are the thumbnail sketches of forty-five individuals who have contributed significantly to the cultural, intellectual, political, or sports history of Finland; any such abbreviated list is handicapped by the scores of profiles that would arguably have been equally worthy of inclusion. On balance, however, Lavery’s book is a welcome addition to the literature on Finnish history. It provides an excellent and up-to-date first bite at the apple for the foreign layman and foreign historian alike.

Michael F. Metcalf
University of Mississippi

Terror and Democracy in the Age of Stalin: The Social Dynamics of Repression. By Wendy Z. Goldman (New York, Cambridge University Press, 2007), 274 pp. $75.00 cloth $22.99 paper

This is an important study not only for Soviet historians but also for those scholars interested in labor and social history, political violence, popular mobilization, and populist components of terror. The book focuses on the participatory “democracy” campaign that accompanied the
political terror as well as the role of the labor unions in it. Although some scholars have touched on the populist element of the terror from 1937 to 1939, few have taken it seriously or tried to explain its complicated relationship between repression and democracy. Goldman shows both the power and the injustices of “democracy” as practiced during the Stalin era. Although it encompassed popular participation, accountability before the membership, and freedom to criticize bosses, it also allowed unions to remove unpopular leaders from office and to punish those accused of poor leadership, corruption, and the like.

Party leaders mobilized workers’ shop-floor anger and resentment into a national campaign against “wrecking,” in which workers were encouraged to equate accidents with deliberate damage by union officials and managers. Responding to pressure from above to search for hidden enemies, unionists became active agents in the expulsion and repression of their fellows, and there were as many victimizers as victims. In the end, this new democratic power did not transform the union leadership; more often than not, it simply replaced one group of bureaucrats with another. Nevertheless, the populist process managed to spark a prolonged public struggle within the unions that empowered workers (for good or ill), as it melded with the campaign against enemies. The resulting mess was a continuing cycle of accusations and arrests. Lines and structures of authority were undermined and the center lost control. The “snake devoured its tail” and “chaos reigned,” in Goldman’s felicitous phrasing.

Using heretofore unexploited Moscow archives, this book deals with policymaking at the top of the Soviet pyramid, showing the indecision and miscalculation that characterized the development of the terror even at the apex of power. Avoiding the usual simplistic approaches in which Stalin successfully planned everything and victimized everyone else, Goldman provides a more realistic account of confusion, unintended consequences, shifting alliances, and chaos.

The book shows the variety of issues and conflicts that pervade labor history, including status and authority conflicts on the shop floor, factional struggles within the union leadership, and struggles between workers and union and party leaders. Fissures and connections ran in all directions—from the top down, from the bottom up, and sideways, conditioned by a tradition of scapegoating and blame-shifting. This is one of the few studies that deals with Soviet labor unions before World War II. Although Goldman’s focus is on the politics of terror from above and from below, she also has much to say about the union activity, leadership, and structure.

Goldman’s story is replete with twists and turns, complicated maneuvers, and the often-opaque formulaic political discourse of the times. Nevertheless, she presents her analysis with grace and a lively writing style, generously interspersed with quotations from workers themselves.

J. Arch Getty
University of California, Los Angeles
The findings of the scholarship that underlies the study of slavery are in considerable need of comparative study. Because of the great strides that are being made in the reconstruction of the history of the African diaspora, periodic assessments of current trends and future possibilities are essential. However, such efforts at comparison are faced with the real dangers of synthesizing the wrong things too quickly and missing major advances in scholarship. In the case of the two books under review, this problem is front and center. Both books reflect the efforts of leading scholars to share their vast knowledge and insights. Bergad’s stated purpose is to integrate new interdisciplinary knowledge about three major slave societies—Cuba, Brazil, and the United States (why not Puerto Rico?)—in a digestible form for advanced undergraduates, graduate students, and the general public. Engerman’s book is an attempt to formalize a series of public lectures that were intended to provide a comparative overview of slavery and emancipation for an informed but a nonspecialist audience.

Both books turn out to be interesting but ultimately unsatisfactory efforts at synthesis and analysis. Bergad’s and Engerman’s focus on economic themes and influences is not unwelcome; the economic dimensions of slavery are often misunderstood or otherwise misinterpreted. The authors provide a clear view of the profitability and technology of slavery, although neither of them seems to have pondered the paradox that even though economics prefers tranquility to violence, slavery was premised on violence. How did slavery become profitable in the face of economic unease?

In examining slavery in world perspective, Engerman devotes half of his short book to considering how slavery in the United States conformed to, and differed from, wider patterns. He clearly demonstrates that slavery was not peculiar to North America, although, in some respects, the experience of slavery in the southern United States during the nineteenth century was unique. His discussion is coherent, informative, and hence useful in summarizing debate and making the issues generally accessible. However, he does not pursue certain implications that can be drawn from what happened in the United States, where slavery ended in violence on a massive scale. The ending in itself is not necessarily distinctive. What is missing from Engerman’s account is a full exposition of the importance of race and the issues of cultural survival and regeneration, which dominate current research elsewhere. Engerman draws on the statistical materials of the voyage database generated by David Eltis
and his associates, but he does not attempt to examine the implications of the new knowledge for migration patterns.

In textbook format, Bergad provides an effective overview of colonization and the rise of slavery as the primary system of production for each of his areas of study—North America (United States), Cuba, and Brazil, with some discussion of other parts of the Americas. Although Bergad conducted extensive research in Puerto Rico, he unfortunately does not include the history of slavery there; its similarities and differences might have been instructive within the context of broader patterns.

Bergad’s concentration on the nineteenth century highlights significant differences in the “mature” systems of slavery at a time when new technologies were being introduced to increase production, especially in Cuba and the United States. Engerman also identifies this significant development. The presentation of data regarding production, prices, investments, and demography in both books makes for an admirable introduction to the economics of slavery.

Where the two books do not achieve perspective is in relation to the people who actually suffered slavery. Engerman’s book is not at all concerned with slavery at the personal level, or with the cultural heritage of the enslaved. Bergad, however, attempts to use biographical accounts to give “voice” to the enslaved and to examine the ways in which slaves “made space” or otherwise resisted. However, when Bergad discusses a particular case, like that of Mahommah Gardo Baquaqua, he does not explain why the account is relevant (it does not help that he almost always misspells Baquaqua’s name [see, for example, 94]). How important is the fact that Mahommah was raised a Muslim? Must the text exclude any information about West Africa during the first half of the nineteenth century, when cotton was king in the United States, and South and West African slaves worked the fields?

Bergad makes serious factual errors about the African background of the enslaved. Certainly the fact that the area of the Bight of Benin was “wracked by warfare” had nothing to do with what was happening in “Bantu” Africa, and the number of “bantu-speaking slaves arriving in Bhaia from Angolan ports” had nothing to do with “repeated campaigns launched against them by Muslims from the north,” who, according to Bergad, had long been involved in shipping slaves to Brazil (226)! Abakúa in Cuba was certainly not “part of Yoruba culture”; nor were “Carabali” and “Ogboni” (181). Abakuá is traceable to the ekpe secret society in Calabar and Cameroon, not Yorubaland. Bergad claims that purchasing freedom was characteristic of Brazil but not elsewhere, but he subverts this point in examining the backgrounds of leaders of slave conspiracies in North America. The example of Venture Smith or the many apprentices in Newport, Rhode Island, are readily available to prove otherwise. These, and other, egregious errors and self-contradictions about Africa and the lives of Africans under slavery tend to undermine the stated educational purpose of the book.
Both books reveal the strengths and weaknesses of the comparative approach. What is gained in the discovery of broad similarities and differences has to be balanced with the potential loss of seemingly tangential subjects that might in fact be central to the study of the African diaspora—for instance, connections across the Atlantic, the cultural variations among the slave societies of the Americas, and the diverse forms of creole community. Although the predominantly economic issues that both Bergad and Engerman favor are crucial in understanding slavery and its demise, the experiences of those involved in slavery are just as critical. The comparative histories under review do not tread into this territory, but that is where the main advances are currently taking place in the study of slavery.

Paul E. Lovejoy
York University

Rebels Rising: Cities and the American Revolution. By Benjamin L. Carp (New York, Oxford University Press, 2007) 334 pp. $35.00


The originality of Carp’s approach lies less in the ground that he covers—though he builds on more than a generation of fresh, creative scholarship—than the manner in which he organizes his analysis. Eschewing a purely city-by-city approach, Carp invites readers to consider institutions: the waterfront (especially in Boston), the tavern (especially in New York), religion (especially in Newport), domestic spaces (especially in Charleston), and politics (especially in Philadelphia). He systematically reaches back into the first half of the eighteenth century to provide context for the stormy events of the 1760s and 1770s. In contrast to Bridenbaugh, who reified cities as direct participants in the Revolution, Carp treats the port towns as “sites of radical change” (9). In these places,
he explains, pluralistic culture and society, social unrest, and conflict with the royal government were concentrated and communicated to rural America.

Absent the cities, Carp—like Schlesinger, Bridenbaugh, and Nash before him—argues that Yankee farmers and Tidewater gentry would not have made a revolution. In Boston, for example, waterfront conflicts with royal authority served as catalysts for townwide and colonywide protests. Carp explains that tarring and feathering was a shipboard punishment dating from the twelfth century and that the image of death on a 1774 almanac wielded a fisherman’s spear to attack Governor Hutchinson. He explains that in Newport and Philadelphia, sectarian enmities shaped political allegiances and that the fear of Congregational and Presbyterian dictation was so strong in Philadelphia that Quakers and Baptists demanded that the Continental Congress affirm religious freedom as a condition of support for Boston in 1774.

Carp presents the most important insight of this learned and carefully crafted study in his epilogue, where he explains that though urban politics crucially shaped the coming of the Revolution, the cities became expendable and obsolete politically during the war years and thereafter. Having served as catalysts for the mobilization of the countryside, the cities’ influence shrank in the aftermath of independence. “Only Boston remained the center of government, economy, and society in Massachusetts” (219). Elsewhere, he points to the political ascendancy of the cities and to the arc of their relative decline thereafter.

Richard D. Brown
University of Connecticut


Ten years ago, prevailing histories of the American Revolution lived within the conceptual arc of the “cultural turn.” They explored revolutionary politics in terms of cultural expression and ideological ferment, often locating their subjects between the familiar poles of republican virtue and liberal self-interest. Since then, however, historians such as Holton and Young have revived the Progressive approach once associated with Beard, who stressed the class interests of the Revolutionary elite and the resulting betrayals of the nonelite masses.1 In Taming Democracy, Bouton makes a major contribution to the neo-Progressive renaissance. He asks, “What kind of democracy did common folk want

from the Revolution?” and “How happy were they with the version of democracy the Revolution brought?” (3). His answers produce an extremely important contribution to the literature about the Revolution.

Bouton’s approach rests on two lines of inquiry, each with its inevitable shortcomings. First, his geographical focus on the state of Pennsylvania makes sense: Pennsylvania was home to an ethnically diverse but overwhelmingly rural population, much like the new nation as a whole, and it played host to the major political events and institutions of the time. Most of Bouton’s eleven chapters concentrate on Pennsylvania while also alluding to analogous developments in other American states.

Second, his research, which is primarily concerned with the economic outlooks and political values of “the people” on the one hand and “the elite” on the other also makes sense, because from the 1760s to 1790s Pennsylvania endured an economic crisis comparable to the Great Depression of the 1930s. Studying the Revolution without that crisis in mind, Bouton suggests, would be as fruitless as exploring the New Deal without close attention to the Depression.

Drawing from a massive evidentiary base of tax rolls, court records, petitions, and correspondence, Bouton shows how British policies during the 1760s and 1770s occasioned a liquidity and credit shortage for farmers and artisans. The afflicted masses protested with a widely resonant ideology of liberty and equality that drew from both Whig radicalism and Christian morality. “Guided by those beliefs, Pennsylvania’s revolutionary generation—rich and poor alike—turned the Revolution into an attempt to expand political rights and ensure a relatively equal distribution of wealth” (32). Ordinary farmers devised comprehensive plans for progressive taxation and the equitable repayment of both private and public debts. They pushed cautious gentlemen to declare independence and to draft a state constitution in 1776 that not only empowered ordinary white men but also enabled free black men to vote. Pennsylvania, for a moment, was a liberal democracy in the full sense of both words—although, as Bouton duly notes, “the people” were also eager to kill Indians and bully Quakers.

That democracy did not last. In a “stunning reversal,” Pennsylvania’s elite refashioned itself in the mold of the British oppressor (61). Due to a determined, shameless campaign by Robert Morris and a disheartening series of betrayals by arriviste politicians, the state government embraced many of the policies that had provoked the Revolution (and the hard times) in the first place. Bouton provides startling evidence that Morris and his cohort consciously planned to enrich the wealthy at the expense of everyone else and to deploy state power on behalf of the moneyed elite. After further protest from the people, elites turned to the federal Constitution, the prohibitions of which on paper currency and debt relief “effectively outlawed most of the . . . popular reforms that ordinary Pennsylvanians had tried to enact” (178). Rushed into law, the Constitution dissolved the “rings of protection” that rural communities had built to ward off the devastating effects of pro-creditor and pro-
speculator policy (146). By the 1790s, the people resorted to desperate insurrections, which the national and state leadership first crushed and then belittled.

The overall story is convincing, not least because Bouton strikes an admirable balance between quantitative and qualitative evidence. He shows, for example, that courts issued enough writs of foreclosure to cover more than 60 percent of the taxable population in certain counties during the 1780s. He also conveys what a sheriff who issued those writs would have seen and heard as he made his way through Pennsylvania. Bouton writes with quiet passion, laying out evidence in careful sequence, creating sympathy for ordinary people without romanticizing them. At certain points, he might have described the economic values that animated the people in greater detail or made finer distinctions between different strata of the elite. He also might have done more to explain why the urban populace embraced some of the gentry’s plans and how the democratic revolution and genteel counter-revolution inflected the Atlantic-wide debate over African slavery. But these are minor issues that have more to do with readers’ preferences than the author’s contentions.

*Taming Democracy* brings social conflict and economic analysis back to the center of Revolutionary historiography. It also casts doubt on the reigning narratives of democratization in the United States, which generally applaud the elections of Thomas Jefferson in 1800 and Andrew Jackson in 1828 as watershed triumphs. Perhaps the real Revolution, the Revolution that most people had wanted, had already been defeated when Jefferson claimed victory in their name. Perhaps democracy veered off the path set down by the “Spirit of 1776” before the “Revolution of 1800,” leaving Americans with a diminished sense of freedom, equality, and the rule of the people.

J. M. Opal
Colby College

*Many Identities, One Nation: The Revolution and Its Legacy in the Mid-Atlantic.* By Liam Riordan (Philadelphia, University of Pennsylvania Press, 2007) 392 pp. $49.95

In *Many Identities, One Nation,* Riordan seeks to connect political history and social history in order to explore the diverse effects of the American Revolution on a local and national level by examining changes in group (and some individual) identities in three Delaware river towns between the 1760s and the 1820s. Riordan applies local-history methods to analyze changes in Quaker Burlington, New Jersey, in New Castle, Delaware (heavily populated with African Americans), and in Easton, Pennsylvania, where Pennsylvania Germans struggled for power. Riordan
brings a complex analysis of architecture, religion, electoral politics, demographic change, and reform movements to bear on post-Revolutionary change in a pluralistic society, where religious, racial, and ethnic categories became increasingly important.

The book’s seven chapters proceed chronologically as they juxtapose discussion of each locality against national political and social changes. The first two chapters, which establish local context for the towns, discuss how the American Revolution worked as the engine of social and political change in each of them. The third chapter examines the interaction of local political conflicts with national politics in the early republic, arguing that shifts in personal identity were connected with changes in partisanship. The fourth and fifth chapters focus on crosscurrents in Protestant religion as the reforming impulse grew at the turn of the nineteenth century. The final two chapters examine the impact of Jacksonian democracy on lived experience and continuing demographic change in the Delaware River valley.

Riordan’s addition of careful, local evidence to the scholarship on the growth of national identity and post-Revolutionary society is welcome. The towns that he chose prove excellent case studies about local variation in national trends. Riordan is particularly successful at integrating political and religious history. He illuminates the texture of social change for many African Americans, German Lutherans, and Quakers in a way that an analysis of national politics alone could never duplicate.

Anyone interested in the mechanisms of social and political change—the ways in which personal relationships interact with deeply held beliefs, ethnic traditions, and national trends—could benefit from a careful reading of Riordan’s book.

Although the picture of change is clear, Riordan never offers a convincing framework to integrate the concept of identity—one of his major concerns. He applies his several definitions of identity inconsistently across the chapters. He mixes constructionist definitions with structur- alist concepts, while also employing the anachronistic term “Revolutionary identity politics” to describe ethnic conflict throughout the book. Sometimes Riordan effectively relates events to changes in identity (when discussing Fries’ Rebellion or the appeal of evangelicalism), but other times he fails to demonstrate how change was internalized as a matter of identity (when he relates demographic changes in New Castle’s black community).

Ultimately, Riordan’s book stands as a good combination of political and social history, but his bid to expand the definition of identity in the early republic is less convincing.

Sarah J. Purcell
Grinnell College
Hotels have provided the backdrop for a wide range of important events in American history, including the “Sack of Lawrence” during the sectional crisis and the infamous “smoke-filled room” that propelled Warren G. Harding to the White House in 1920. But historians have seldom treated these institutions as more than passive, inert settings. In an engaging, lavishly illustrated book, Sandoval-Strausz focuses on the hotel itself, exploring its development as a distinctive social setting and architectural form during the long nineteenth century. Although the volume devotes considerable attention to the great luxury hotels of the era, the narrative also discusses resorts, residential hotels, and flophouses.

Sandoval-Strausz argues that the development of these specialized buildings reflected the rise of “capitalism” and “modernity” in the United States. American hotels became distinct from public inns during the 1790s and proliferated with the growth of long-distance commerce and the increasing mobility of the American population. As market relations extended to all corners of the nation, merchants and other travelers needed reliable, safe accommodations in both major urban centers and far-flung settings. Hotels developed and expanded to fill this need.

But Sandoval-Strausz also asserts that hotels accelerated economic growth during the nineteenth century. They institutionalized and commercialized “hospitality,” creating specialized forms of space, rules of social comportment, and workforces that enabled strangers to interact, conduct business, and even engage in illicit activities, especially adultery. “In a travel-dependent economy,” Sandoval-Strausz explains, “hotels played the same role in facilitating human mobility that banks played with respect to capital and warehouses, lumberyards, and stockyards did with respect to commodities” (237). Moreover, the “common law of innkeepers” helped to maintain predictable, familiar standards for lodging and public accommodation. Other scholars, most notably Janowitz, described similar social mechanisms, in which customs and institutions—such as voluntary associations, political parties, chainstores, and letters for transfer for church membership—integrated outsiders into both local and national networks. Sandoval-Strausz, however, links this sociological process of establishing “voluntary communities” or “communities of limited liability” to a specific physical setting.

The book’s broad conceptual framework and overarching argument account for both the strengths and the relative weaknesses of the analysis. Sandoval-Strausz skillfully connects the history of the hotel to macrocosmic changes in nineteenth-century American society, especially the rise of cities and the expansion of capitalism. To support his argument, he draws from an impressive range of sources, including legal

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Andrew Jackson and the Constitution: The Rise and Fall of Generational Regimes. By Gerard M. Magliocca (Lawrence, University Press of Kansas, 2007) 196 pp. $29.95

Beware of lawyers who ignore history. Magliocca begins this book with a gentle admonition to his fellow legalists to approach the Constitution as a living document, a product of circumstance instead of a set of eternal verities. Once we learn to see its provisions as artifacts of conflict rather than articulations of timeless principle, we can uncover, and even foretell, the dynamic by which it evolves.

That dynamic, says Magliocca, is cyclical. Each political generation seeks to overthrow its predecessor’s constitutional settlement. Whether it moves by formal amendment or judicial interpretation or presidential or congressional action matters little. What counts is who wins. Constitutional and political battles are indistinguishable, and “constitutional theory itself is usually nothing more than a reaction to recent events” (134).

Although historians always try to set texts in proper perspective, the reduction of, say, the Bill of Rights to “nothing more” than a fleeting political effusion might make even the most dedicated historicist blanch. So would Magliocca’s parallel assertion that “the only difference” between the Supreme Court cases of Worcester v. Georgia and Dred Scott v. Sandford is their degree of comportment with modern values (100). Simplifications multiply as Magliocca sets his scheme in motion. Mixing ideas drawn from Kuhnian paradigms, Schlesingerian cycles of liberalism and conservatism, Marxist dialectic, and electoral realignment theory, Magliocca posits a revolving struggle of generational cohorts. The Jacksonian states’-rights generation rebelled against the nationalist constitutional settlement embodied in John Marshall’s opinions. Overrunning the rearguard action of Worcester, they enshrined their victory in the Indian Removal Act and Jackson’s Bank Veto, which in effect nullified
M’Culloch v. Maryland. But even as Jacksonians consolidated their gains by capturing the Supreme Court, the succeeding “abolitionist” generation was coming of age. When it achieved power, the Fourteenth Amendment would resurrect Worcester, and national authority would again become ascendant. Round and round we go.

As should be evident, Magliocca’s scheme operates at a high-concept level of generalization. In legal technicality, Worcester, the Indian Removal Act, and the Fourteenth Amendment did not speak to each other: The glue that adheres them is their mutual address to the broad idea of minority rights. But this abstraction is the least of Magliocca’s problems. Fitting the messy sprawl of history into a neat cyclical model requires a lot of shoehorning and cherry picking. In truth, the Jacksonian political struggle was not, in any concrete sense, generational. Marshall was a relict of an older time, but Whig leader Henry Clay, Jackson’s junior by ten years, was not. Magliocca often labels Jackson’s opponents “traditionalists” and “conservatives,” a distinction less of age than of outlook and ideology.

Magliocca plucks documents nearly at random to sustain his thesis, citing a single Reconstruction-era Senate committee report praising Worcester to prove its reinstatement as constitutional dogma. On the other side, since the nullification crisis contradicts nearly every main point of his argument, he shoves it aside in a few quick sentences. One might think a book on Jackson and the Constitution would have to address this subject at length, but one would be wrong.

Magliocca’s relentless teleology—much of the book is written in present or future tense, not as narrative but as forecast—beguiles him into confusing cause and consequence. As example, Jackson was not elected to pursue what Magliocca calls “the incipient agenda of Jacksonian Democracy.” He defined that agenda once in office, reversing many of his previous positions and surprising and alienating many followers (16). A last flaw of Magliocca’s scheme is that of deterministic models generally: They work until they fail, and predicting when they will fail is impossible. The Supreme Court, Magliocca observes, “represents the established constitutional order.” Justices are hostile by training and temperament to radical change. “Accordingly, courts are apt to resist claims by a rising movement that its legal vision should displace past wisdom” (34). Try fitting the Warren Court and the civil rights movement into that formula.

Magliocca’s goal of placing the Constitution in historical context is laudable, and he does good service in locating Indian removal at the heart of Jacksonian controversy. But a persuasive historian, unlike a persuasive advocate, must avoid tendentious reasoning, selective use of evidence, and rhetorical overkill. The aim is to apprehend the past, not to win a case. Be cautious of lawyers writing history, even when they mean well.

Daniel Feller
University of Tennessee
Driven Out: The Forgotten War Against Chinese Americans. By Jean Pfaelzer (New York, Random House, 2007) 400 pp. $27.95 cloth $4.95 paper

Well-researched and well-written, Pfaelzer’s Driven Out represents an important contribution to Chinese American history. Based on a wide range of primary sources, including census materials, municipal and legal records, and newspaper accounts, it is the first comprehensive study to chronicle in detail the brutality of the racist movement that attempted to eradicate the presence of the Chinese throughout the Pacific Northwest from 1850 to 1906. By the end of the 1880s, “close to two hundred towns . . . had driven out their Chinese residents” (253). Pfaelzer’s in-depth discussion of anti-Chinese activities in various towns sheds light on the popular support that the anti-Chinese movement enjoyed, and provides convincing evidence that the government was involved as well: “The forces for driving out the Chinese had spread—through arms, through the laws, through the courts” (335). Insightfully placing the ethnic cleansing of the late nineteenth-century American West into a broader context, Pfaelzer notes “two other paths toward racial purity converging in the West,” one targeting Native Americans and the other, African Americans (xxi). What happened to the Chinese, the author points out, brings to mind “Kristallnacht, the night in 1939 when Nazi Germany violently exposed its intention to remove the Jews,” and it anticipated the history of Poland and Greece in the 1930s and 1940s and, more recently, of Rwanda, Indonesia, and Bosnia” (xxviii–xxiv).

Pfaelzer does not represent Chinese Americans merely as victims; she fully recognizes their historical agency, documenting their strenuous efforts in resisting the politically powerful and physically violent anti-Chinese movement. In spite of its bloody nature as the “largest single mass lynching in California,” the massacre of the Chinese in Los Angeles in 1871, for instance, failed to remove their community (54). Just in the first decade after the passage of the 1882 Chinese Exclusion Act, the Chinese filed more than 7,000 court cases to fight for their rights.

Driven Out is a well-organized book; its themes and arguments are easy to follow. Chapter 1 deals with the purges of the Chinese in the mining areas during the 1850s and 1860s. It offers evidence that the attempt to expel the Chinese started upon their arrival, countering the long-standing popular belief that white Californians initially welcomed the Chinese. Chapter two covers anti-Chinese violence during the 1870s. Chapter 3 focuses on the experiences of women in such cities as Antioch and San Francisco. Chapters 4 and 5 discuss the expulsion in Eureka and Truckee, respectively—Eureka driving out the Chinese with swift pogroms and Truckee starving them out. Chapter 6 examines the legal challenges to ethnic cleansing, taking advantage of the loopholes in American laws. Chapter 7 is a “litany of hate,” namely, a chronological register of anti-Chinese ethnic cleansings in the 1880s. Chapter 8 discusses the requirement that all Chinese had to carry an identification card under the Geary Act and Chinese response to it.
One of the weaknesses of *Driven Out* is the lack of a coherently convincing explanation of what motivated the expulsions. Pfaelzer’s argument that “whites violently expelled the Chinese in part because they looked so familiar” ignores the fact that anti-Chinese sentiments ran across racial as well as socioeconomic boundaries (166). All in all, however, this thoughtful book is a welcome addition to the list of fine studies of anti-Chinese racism by Sandmeyer, Alexander, McClain, and Lee.1

Yong Chen
University of California, Irvine


To excavate the lives of early twentieth-century communists within Chinese and Japanese immigrant communities in America, Fowler went, of course, to Moscow. How else could someone, without Chinese or Japanese language abilities, learn about this small group of activists who have largely been forgotten by historians of communism, labor, Asian Americans, and Asian revolution?

Fowler, who was fluent in Russian, found tantalizing bits and pieces of the story in the Russian State Archive of Social and Political History, especially in the papers of Katayama Sen, the famous Japanese communist, in files on the U.S. Communist Party, and in other Moscow collections. Memos, reports, letters, and directives, along with rare published material such as newsletters and flyers, provided her with evidence of the activities, interrelationships, and connections with Moscow through the Comintern and other organizations. Sympathetic to the iconoclasts, Fowler understands them as facing virtually insurmountable odds: In addition to the social isolation and legally enforced racism of the United States, they had to contend with Great Russian chauvinism, prejudice and ignorance within the ranks of American communism, poverty, repression, the rightwing in their own communities, and endemic sectarianism.

Using approaches from historical geography, Fowler sees the communists as struggling to break away from “spaces of dependence” to “spaces of engagement” in an attempt to advance their own notions of proletarian and national liberation. Bourgeois and Leninist conceptions of state borders could not stop them from trying to forge connections

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that spanned the Pacific, Euro-Asia, and cities across the North American continent. They rebelled not only against the capitalist order but also against the strictures of international communism. At one point, Fowler calls Moscow the “metropole” and the Asian communist cells “colonies.” Geographies, she reminds us, are constructed and are continually contested. Her methodology helps to unify a story of disparate individuals, places, and activities and thus to supplement scholarship on international communism and Asian Americans.

Unusual and intriguing characters abound in Fowler’s narrative. Maniwa Suekichi, a Japanese sailor who had jumped ship to try the life of a cowboy in the western United States, eventually became disillusioned and joined New York City’s socialist movement. Eiitaro Ishigaki, a young artist, befriended Katayama and became his confidante. H. T. Tsiang, a Stanford student from China and a radical journalist, collected a group of young Chinese intellectuals around him, even as his associates suspected his fidelity to Leninism. Unfortunately, these and many other colorful individuals make only brief appearances in Fowler’s book. She fails to mention that Ishigaki later became one of the leading socialrealist artists in the country and that Tsiang tried his hand at creative writing and acting in Hollywood. By the end of Fowler’s impressive work, many names have cropped up, but few of them are accompanied by a full identity or insight into motives, ideals, or intentions.

Nonetheless, Fowler’s work is an ambitious effort to break from the confines of traditional intellectual approaches, craft a transnational history, and “liberate” the stories of those who had fallen within the crevices of established scholarship. Regrettably, Fowler ends her account in 1933, on the eve of the greatest period of communist activity among Asian Americans.

Gordon H. Chang
Stanford University

Picturing the New Negro: Harlem Renaissance Print Culture and Modern Black Identity. By Caroline Goeser (Lawrence, University Press of Kansas, 2007) 360 pp. $34.95

Goeser’s *Picturing the New Negro* contributes an important yet essentially ignored dimension to the scholarship about the Harlem Renaissance—the role of visual imagery in helping to shape a “modern black identity.” She offers a close thematic and stylistic reading of the illustrations that appeared on various covers as well as in magazines, novels, scholarly books, and advertisements. The “modernity” of these illustrations, both in content and form, is the central issue that the author addresses. She introduces an extensive range of individuals (black and white), whose names rarely appear in the literature about the Harlem Renaissance. Yet she also understands that illustration is more than iconography. As a form
of advertisement, it links to the larger consumer economy that emerged in the early twentieth century—a modernist medium connecting art, and those involved in its creation, to the marketing demands, concerns, and techniques of commercial culture.

To account for the artist’s imaginative conception of black identity as a form of modernist expression, Goeser draws upon Barker’s and Gates’ understanding of “Afro-American discursive modernism” (the language of trickery) to situate the artist in the larger cultural field of the 1920s (270). Thus, Douglas, and many others, used illustration to subvert the racial stereotypes of the day while portraying African Americans as socially, intellectually, and culturally sophisticated. Although the primary emphasis was to challenge the racial attitudes of whites, Goeser also notes that female illustrators—women such as Joyce Carrington, Elanor Paul, and Gwendolyn Bennett—also challenged the image of women that black male illustrators presented in their works. Thus, whereas males often tended to present women as “timeless ideals” of beauty and culture, and men as ready to enter the modern economy, women illustrators often linked women with the “beauty culture” of the day and/or presented them as no less ready than men to assume entrepreneurial roles. To be modern was also to understand that modern images of race and gender did not exclude homosexual relationships and transvestitism as well as boundaries that crossed racial lines.

Goeser’s reading of the images fashioned to construct both a “usable past” and a “usable present” is an important complement to previous literature about the Harlem Renaissance. Less successful is her suggestion that the reading audience of the day understood these illustrations as she does. She uses Walter Benjamin’s seminal essay “The Work of Art in the Age of Mechanical Reproduction” to demonstrate that bold illustration was especially suited to capture the attention of the modern reader—“the preoccupied consumer” (40). But when she addresses the questions of who exactly were the modern readers and whether they were further distinguished by class, race, and gender, her answers provide little evidence beyond her own imaginative insights.

Mark Helbling
University of Hawaii, Manoa


Benson, in this posthumously published monograph, argues convincingly that consumer culture made only the slightest inroad into the American working class before World War II. Insufficient wages and erratic incomes, she contends, made it impossible for most families to purchase much beyond basic necessities. For this reason, she situates consumption within the larger framework of family economy, considering various interrelated strategies that working-class families used to earn money as well as to buy goods and services.

Benson concludes that, contrary to popular belief, wives often took less pleasure in consumption than in wage earning, whereas husbands were more likely than wives to direct a portion of their earnings routinely toward luxuries for themselves. A similar pattern emerged for daughters and sons—the boys edging tended more toward discretionary spending than their sisters. Benson also covers the ways in which working-class families tried to lessen the stringencies of the market economy by exchanging goods, services, and labor with kin and neighbors. Moreover, such emerging financial practices as credit may have heightened middle-class engagement with consumption, but they had little appeal to working-class families, who chose second-hand goods over falling into debt. Remarkably, these strategies varied little across various ethnic groups, prompting Benson to conclude that class prevailed as the major factor determining consumption patterns during the interwar period.

The methodology that informs this important study is boldly qualitative. Before telling the larger story, Benson opens by relating her own family’s limited engagement with consumer culture during this period. She states clearly that her concern is “not with absolute levels of consumption, nor with the quantitative aspects of the family budget” (7). Instead, she uses the “raw data” of reports and surveys of working-class families conducted during the 1920s and 1930s to understand how working-class families made decisions and how they meshed their strategies with the desires and goals of individual family members (9).

Two main sources predominate—reports of the U.S. Department of Labor Women’s Bureau, compiled from home-visit interviews with wage-earning women, and studies of working-class families conducted by social scientists during the 1920s and 1930s. Benson admits that because she was less interested in the conclusions offered by these investigations than the raw data that informed them, she set aside the published reports and headed to the National Archives. There she retrieved mounds of descriptive material, including highly effective first-person narratives. For example, in examining the often tense relationship between working-class parents and their wage-earning daughters, she
quotes a Kansas City meatpacker who complained that her teenaged daughters “don’t make much and they want everything” (71).

David Montgomery writes in the afterword, “Susan Porter Benson has left a distinctive imprint on the writing of American social history primarily because of her independence of mind” (170). Household Accounts confirms his assessment as much by its methodology as by its important conclusions.

Mari Jo Buhle
Brown University

Kansas in the Great Depression: Work Relief, the Dole, and Rehabilitation. By Peter Fearon (Columbia, University of Missouri Press, 2007) 316 pp. $44.95

Kansas in the Great Depression is a detailed study of state and federal programs during the economic crisis of the 1930s. The New Deal agencies serve as the main focus in this investigation, but Fearon pays particular attention to the efforts of state and local administrators as well.

In the familiar territory of the Great Depression and the New Deal, Kansas has never emerged as a major player. Yet Fearon demonstrates how the state devised a workable welfare system while it attracted federal funding to cover the costs of direct aid and work relief. As he emphasizes, one key to the success of the New Deal in Kansas was the use of professionals to administer programs. Another prime factor was the leadership of John G. Stutz, executive secretary of the Kansas Relief Committee. Fearon provides pertinent information about Stutz’s background and his long career as a public servant in Kansas, crediting him with the ability to direct the state and, later, federal relief systems. According to Fearon, Stutz insisted that professionals serve at the local level to determine relief needs and to provide training to create efficient and competent social workers. This study is valuable in recognizing the challenges that social workers faced in the 1930s.

Fearon begins with a brief history of Kansas, its population characteristics, and its employment statistics, supplementing the narrative with a plethora of charts, tables, and statistics. Fearon culled much of this information, such as the number of industrial workers and the amount of land under cultivation, from extensive research in a variety of depositories and archives. Economists and sociologists interested in the impact of the Great Depression will find the numerical data valuable, though others may find it a distraction from the larger picture.

The emphasis on statistics, however, has a more troubling effect; it shortchanges the human side of the story. Rarely does Fearon provide a glimpse of how unemployment affected Kansans, or what the Dust Bowl did to the agricultural side of the economy through loss of the tax base and destruction of the land. He includes an analysis of the building
projects directed by the Works Progress Administration (WPA), but he fails to mention the employment of such professionals as writers, artists, and musicians. These concerns, which have been addressed elsewhere, are not the primary focus of Fearon’s study, but their inclusion would have added depth to his portrait of Kansas during the 1930s.

Despite these flaws, *Kansas in the Great Depression* serves as a resource for specific information regarding work relief in Kansas. It may not be the easiest book to read, but it does promote a better understanding of what the people in Kansas endured during the economic crisis.

Judith R. Johnson
Wichita State University


Students of environmental politics tend to assume that knowledge or values drive history—that new laws, agencies, and ways of thinking are the product of insights into our impact on the planet, or reflect a heightened sense of the intrinsic value of nature. But as Milazzo argues, this assumption can obscure as much as it explains. In particular, it fails to account for a crucial period in American environmental history. In the early 1950s, pollution was considered to be, as President Eisenhower described it, a “uniquely local blight” not meriting federal attention (3). Yet two decades later, it was a national responsibility, impelling Washington to use both standards and subsidies to compel action.

In explaining how this change occurred, Milazzo draws attention to those who pursued environmental initiatives not just in the Sierra Club but also in more unexpected locales. Most important was Congress, which assembled the distinctive American system of environmental regulation, often far in advance of public demands. Several individuals within Congress became unlikely environmentalists—John Blatnik of Minnesota, who as chairman of the Subcommittee on Rivers and Harbors prodded Congress into passing the 1956 Water Pollution Control Act, and Sen. Edmund Muskie of Maine, who in the 1960s played a central part in strengthening the federal role in environmental regulation.

Blatnik, Muskie, and others were not necessarily motivated by concern for unsullied nature. For some, pollution control was merely efficient resource management—the necessary task of ensuring adequate clean water for industry. Ideas regarding management of complex systems, imported from the military and aerospace industries, as well as old-fashioned pork-barrel politics, were also important.

Ecological ideas were also sometimes influential, but often only by association with more established ways of thinking. Old ideas sometimes
gained new life as they worked their way through complex institutions, demonstrating how some ways of doing things could persist even as the legislative landscape was transformed. One example was the Army Corps of Engineers’ adoption of pollution control when it became evident that the policy would complement rather than challenge its mission of building big dams. But ultimately, demands for action, as well as the requirements of the National Environmental Policy Act, strained the conventional apparatus of pollution control, forcing new initiatives, including the 1972 Clean Water Act—a law more rigorous than could have been imagined just a few years before.

In telling this story, Milazzo descends deeply into the detailed workings of congressional committees. In fact, the details are the story: In the forming of legislation, not just the ideas but also the institutional machinery that transformed these ideas into laws proved essential. In particular, a decentralized Congress gave opportunities for individuals like Muskie to make an area of policy their own, combining ideas and negotiating trade-offs to advance both the national agenda and their own ambitions.

Milazzo draws on many sources—legislative archives, interviews, and the published record. His approach is thoroughly interdisciplinary, combining brief biographies with legislative, political, and institutional histories, mingled with forays into environmental history and the history of science and sprinkled with insights from political science. He concludes by suggesting that his insights into the legislative process be applied to areas of policy beyond the environment. This is advice worth taking.

Stephen Bocking
Trent University


White Ethnic New York describes the tensions between Jewish and Catholic (Irish and Italian) New Yorkers after World War II. Employing chapter headings such as “Fascism,” and “Communism,” Zeitz seeks to bring the conflicts of the 1930s into the postwar era, but he does not provide the background needed to understand the events and attitudes indicated in the later period. He provides little information about the Spanish Civil War or Father Charles Coughlin’s rhetoric, which divided ethnic New Yorkers, and none about the Christian Front of the 1930s, which split the city’s New Deal Jewish-Catholic coalition and resulted in neighborhood violence. Nor does he discuss how the groups responded to the Rosenberg case after the war, although he manages to cover Sen. Joseph McCarthy’s influence in detail.

Zeitz fares better in his analysis of the cultural roots of Jewish and
Catholic political divisions and of the racial aspects of New York’s political and cultural life from the 1960s onward. According to Zeitz, Jews saw dissent, anti-authoritarianism, and activism as Jewish values, whereas Catholics held respect for authority and community as core Catholic values. These groups’ cultural norms clashed during various events and political campaigns, especially involving liberal and conservative issues.

Zeitz effectively examines the Jewish–Black conflict, and the Ocean Hill–Brownsville controversy in 1968, although he does not offer Catholic views about these matters. Generally, Zeitz argues that changes in race relations tended to draw white ethnic opinion together, as he shows through voting results and public-opinion surveys. His analysis of Mayor John Lindsay’s campaigns and administrations as well as the influence of the New Left on ethnic views is useful.

When Zeitz deals with Catholic attitudes before the 1960s, he appears to be writing about the Irish exclusively. His sources are Irish newspapers and organizations. Because he includes no Italian sources, his presentation of viewpoints is severely limited. Also, although he uses oral-history collections, he conducted no interviews of his own. Given the recent history with which he deals, interviews would have helped to convey the thinking of ethnic communities and give voice to neighborhood tensions. The lack of an intense focus on neighborhoods limits the study. The methodology is generally traditional. A neighborhood approach, looking closely at those areas that saw Irish–Jewish and Irish–Italian conflict and later Jewish–Black conflict, would have provided the detail this city of distinct neighborhoods requires. The inclusion of voting statistics drawn from secondary sources offers important information, but, again, the data often do not reflect trends at the neighborhood level.

The book provides a good illustration that, contrary to the “whiteness” historians, “whiteness did not equal sameness,” at least in New York. This important point has been submerged in various historians’ efforts to declare each group as “white” during the 1940s or earlier. Differences remained into the 1970s at least, and evidence of diversity within the white ethnic communities continued afterward.

In all, Zeitz provides an interesting and, at times, trenchant account of ethnic relations after the war, but he omits too much to secure a full understanding of the behavior of these groups.

Ronald H. Bayor
Georgia Tech

The Liberals’ Moment: The McGovern Insurgency and the Identity Crisis of the Democratic Party. By Bruce Miroff (Lawrence, University of Kansas Press, 2007) 355 pp. $29.95

Explaining the seeming demise of postwar liberalism has preoccupied a generation of political scientists, historians, and journalists. Miroff’s fascinating and insightful Liberals’ Moment provides a new answer in Sen.
George McGovern’s failed 1972 presidential bid. Long overlooked as merely a landslide defeat, Miroff demands that McGovern’s campaign receive serious consideration because, even more than the election of 1968, it signaled a turning point and “introduced the era to come (2).” He persuasively argues that the campaign of the last unabashedly liberal candidate helped to define the contours of the contemporary Democratic party as it transformed its base from blue-collar urban ethnics to issue-orientated suburbanites, shifted its foreign-policy position, and influenced its adoption of a centrist image and agenda.

Miroff avoids both the conventions of a traditional campaign study and the standard empirical analysis of political science, offering, instead, what he dubs a form of “political therapy” (7). His nuanced analysis illuminates the ambiguous and important lessons of the McGovern candidacy. Not only does it bridge disciplinary divides; it also makes the book accessible and relevant to both scholarly and nonacademic audiences.

Part I offers a narrative of the events of the campaign. Miroff reveals how McGovern through his vigorous opposition to the Vietnam War built a well-organized grassroots movement of middle-class and educated activists that led to his unlikely capture of the presidential nomination. Part II provides a thoughtful consideration of the larger meanings of the campaign, particularly the fatal flaws in its “left-center” strategy and its formidable opposition. Miroff stresses that structural dilemmas doomed the McGovern camp, but he also convincingly contends that President Nixon’s 1972 strategy—“a textbook for attack politics” (229)—provided a series of Republican successors with a “successful template” of how to distort liberalism and discredit Democratic candidates (141).

In the final section, Miroff shows how that landslide loss has continued to haunt the party, leaving every subsequent Democratic candidate with an “identity crisis,” torn between ideological convictions and political calculations. Furthermore, he asserts that the 1972 election had a devastating effect on the politics of Gary Hart, Bob Shrum, John Podesta, and Bill Clinton, all of whom began their political careers working on McGovern’s campaign and all of whom took the lessons of its failure to heart. Tracing the political odyssey of these “McGovernites,” he suggests that their conscious efforts to distance themselves from the liberal image and message has produced a “muddled” centrisim that may be slightly more palatable to voters but lacks the conviction and passion of their mentor (303).

Miroff conducted nearly fifty interviews with campaign participants, including McGovern himself, enabling him to paint a candid and textured portrait. This methodological strength, nevertheless, creates a conceptual limitation. Miroff repeatedly invokes the trope of the “McGovern insurgency” and alludes to the importance of the grassroots army of suburbanites and college students, but most of his discussion focuses on the campaign’s upper echelons. Shifting his vantage away from the campaign headquarters more frequently might have helped to ex-
tend the important implications of his argument beyond the arena of presidential elections and into the complex political landscape of the period as a whole. Ultimately, however, *Liberals’ Moment* proves essential reading for anyone interested in the past or future of the Democratic Party and the fate of American liberalism.

Lily Geismer
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The DeShaney Case: Child Abuse, Family Rights, and the Dilemma of State Intervention. By Lynne Curry (Lawrence, University Press of Kansas, 2007). 164 pp. $15.95

“Poor Joshua,” Justice Blackmun wrote with rare emotion in his U.S. Supreme Court dissent in the case of *DeShaney v. Winnebago County* (1989). He was dissenting from the ruling by the six-justice majority in the Supreme Court that Joshua DeShaney did not have a constitutional right to state protection from child abuse. To Justice Brennan, because an abused child was dependent on state agencies for his physical safety, those agencies could be held liable when they failed to protect him. The judgment instead was that Joshua’s father, not the state or individual child protectors, was responsible for the harm done to this boy since the boy was not in the custody of the state. In reaching this decision, the Supreme Court signaled that it would side with state authority against expanding legal rights for aggrieved individuals. The majority of the court drew a major distinction between irresponsible action, which could be punished, and bureaucratic inaction, for which no one could be held responsible. Brennan, however, wrote that “inaction can be every bit as abusive as power of action. I cannot agree that our Constitution is indifferent to such indifference.”

Joshua’s father, an unemployed alcoholic, often high on cocaine, repeatedly denied having beaten his son during the fourteen months when he was under voluntary supervision by a social service agency. When Joshua ended up in a coma at Mercy Hospital in Oshkosh, doctors who ordered CT scans could see that the extensive damage to Joshua’s brain was consistent with being shaken many times. They strongly doubted the father’s story that Joshua had accidentally fallen down the stairs. Eventually, Joshua’s father went to prison on a felony charge of child abuse. His mother, divorced from the boy’s father when her son was about a year old, filed a civil law suit against the child-protection services agency of Winnebago County, Wisconsin, for violation of her son’s constitutional rights. According to the Fourteenth Amendment, a citizen cannot be deprived of liberty without due process of law. Her suit argued that social workers and their agencies had deprived Joshua of his constitutionally protected liberty by failing to act on his behalf. She decided upon this legal strategy because the state of Wis-
Wisconsin had placed a cap on the amount of damages handed out in liability cases; she wanted a substantial award so that she could pay for expensive private medical care for her son.

Curry has been able to reconstruct the story of Joshua and his mother’s legal suit through interviewing key participants and taking advantage of Wisconsin’s policy of keeping records open. She makes effective use of police reports, medical testimony, social workers’ case notes, and legal briefs. Curry’s wide-angle lens encompasses the history of child-protection services, beginning in the late nineteenth century. Her more immediate focus is the “family preservation model” of these services, adopted in the 1980s. Critics were complaining that too many children, especially minority children, were being removed from their families and placed in foster care, shuttled about without ever being legally adopted. Moreover, foster care was far more expensive than keeping a child at home and providing social services to the family. Most social-service agencies preached that case workers could preserve a child’s biological family by guiding the parent(s) to job counseling and parenting classes and ensuring that the child was enrolled in Head Start. Cases such as DeShaney, however, led to disenchantment with the failures of the family-preservation approach to protect abused and neglected children.

Curry adopts an objective voice in discussing the debate about state intervention in the family and children’s rights rather than the voice of the outraged journalist or child advocate. Nonetheless, in this deeply researched narrative, the police invariably emerge as the good guys because they tend to assume that the accused are lying. Curry is fair, probably too fair, to the social worker who considered the father to be as much entitled to her services as Joshua was. Unlike the police, the social worker tended to accept one excuse after the other about Joshua’s “bumps” and bruises. She was never punished or demoted. In fact, she was honored at her retirement by the local police for “her skills in dealing with abused children.”

Curry avoids the question of whether social workers and agencies should be liable for their mistakes; she does not adopt the position that Joshua’s suffering shows the need for a federal agency to deal with children at risk. The policy that Curry most clearly endorses is divided authority—giving child-protection services their due but giving police the option to petition the court when a child needs to be removed from home. Her position affirms the view that social workers cannot function both as therapists and as police. Curry also implies that voluntary supervision of suspected abusers is too lenient because potential abusers can always threaten to sever their relationship with a social-work agency. Because her narrative is so thorough, readers can reach their own conclusions about the proper balance between state authority and children’s rights.

Elizabeth Pleck
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Employing wills as her primary source, Graubart has produced a useful study of how the experiences of indigenous women in colonial Peru helped to shape the wider world of which they were a part. Her introduction is a helpful overview of the relevant historiography of the past twenty years. In general, the pendulum has swung from a picture of native women’s victimization to one of their ingenuity. Among the more recent studies, Graubart contrasts those that emphasize women’s strengths in the psychological realm (women wielding the threat of witchcraft, for example) with those, like her own, that argue for women’s actual power in the social world. She also maintains that her study delves further back in time than usual and places women’s testaments alongside those of men, thus creating a thicker and richer description.

Chapter by chapter, Graubart follows indigenous women in the region of Lima and Trujillo as they moved through their lives. In 1567, a group of elderly women in Chicuito lodged complaints about their cacique, who, departing from traditional norms, attempted to extract more labor from them—specifically, more weaving—than had traditionally been considered just. Graubart discusses the stressors afflicting indigenous women during this era—largely the pressure on them to produce the cloth on which the local economy depended; many women decided to leave rather than comply.

In the next chapter, Graubart explains how women managed to relocate to the cities, mostly by relying on domestic service and market vending to make a living. In chapters three and four, she shows these urban women using the law to their own advantage and defining themselves through dress, in both cases rendering ethnic categories much more fluid than the Spaniards might have preferred.

On first glance, the last chapter, which explores how elite, rural, indigenous women sometimes used the colonial apparatus to make themselves caciques and how Spanish women sometimes employed it to become encomenderas (holders of Indian labor grants), might seem incongruous. But Graubart makes a theoretical connection with the material that precedes it: “In the end, the meaning of these [legal] victories is that individuals—male, female, Spaniard, Indian—were rapidly becoming adept at managing the new rules, and in fact were participating in their creation” (185). Such, in short, is the point that she makes in each section.

The women who left testaments were, with some exceptions, well to do. The fact that these privileged women were able to gain some control over their post-conquest experiences does not necessarily mean that all, or even most, women, were so empowered—at least not often. The recent trend of emphasizing agency rather than disempowerment runs the risk of diminishing the difficulties that women in general faced,
especially in the earliest generations. Nonetheless, Graubart has provided a valuable contribution. Scholars who study non-sedentary and semi-sedentary Indians, north and south, would do well to take cues from her, and others like her. Traditional frames of reference did not stop indigenous people from exercising their creativity when faced with new rules of the game. Graubart shows how far historians in some fields have moved from that perspective.

Camilla Townsend
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The 1812 Aponte Rebellion in Cuba and the Struggle against Atlantic Slavery. By Matt D. Childs (Chapel Hill, University of North Carolina Press, 2006), 300 pp. $55.00 cloth $21.95 paper

Childs’ book tells the story of a well-known, yet understudied event in Cuban history—the Aponte Rebellion. From January through March 1812, a series of uprisings launched by slaves and free people of color erupted across Cuba. Initial revolts in the island’s interior were suppressed and/or thwarted by Spanish colonial authorities. A few months later, another insurrection broke out on the Peñas Altas sugar plantation outside of Havana. After extensive investigations, thirty-two rebels were put to death while hundreds of others were imprisoned. One of the rebels executed was José Antonio Aponte, a free black carpenter and former colonial militiaman whom colonial authorities saw as the mastermind of the Havana rebellion.

Childs’ analysis of the insurrections provides a snapshot into the world of Cuban slavery during the Age of Revolution. The book excellently situates the Aponte Rebellions within the historiography of the Atlantic World and Cuban/Caribbean slavery. Like Laurent Dubois, A Colony of Citizens: Revolution & Slave Emancipation in the French Caribbean, 1787–1804 (Chapel Hill, 2006), Childs’ book shows how insurgents in Cuba were part of the larger liberation struggles of slaves and free people of color throughout the Americas. The book argues that the uprisings were a consequence of the expansion of plantation slavery in Cuba during the late eighteenth and early nineteenth centuries. Moreover, Childs is attentive to the diversity of the African population on the island, skillfully highlighting the prevalence of African “ethnic” identifications among slaves. Like other recent historians of Cuban slavery, Childs highlights the importance of the cabildos de nación and the colonial militia as niches that Africans and their descendants carved out for themselves in Cuban slave society—precisely the institutions that rebels used to launch the rebellion. Childs’ research shows how Aponte himself—a person of African descent who experienced a tightening racial hierarchy that eroded the privileges of free persons of color—embodied the tensions of Cuban slave society during this period.

Childs is particularly adept at telling the stories of the rebellions.
The most important sources for Childs were the criminal testimony of accused rebels. Childs carefully extracts from these documents details about the lives of the rebels and the institutions that helped catalyze their participation in the uprisings. The most compelling evidence that authorities used to convict Aponte was his fascinating “book of drawings.” The sketches in it that were most alarming to colonial officials were his depictions of Haitian revolutionaries, maps of Havana’s fortifications, and images of blacks defeating whites in battle. Although the book of drawings has not been found, Childs’ attention to the discussion of its contents in the criminal records, combined with other evidence, illustrates the ideologies that inspired the liberation movement.

Childs’ prodigious research refutes Stephan Palmié’s claim in Wizards and Scientists: Explorations in Afro-Cuban Modernity and Tradition (Durham, 2002) that Aponte’s role in the rebellion was based more on historians’ fascination with the image of the slave rebel than on actual evidence. Although Childs cannot prove that Aponte was the leader of the uprisings, nor firmly establish that the rebellions were part of a coordinated island-wide conspiracy, he nevertheless provides revealing documentation about Aponte’s involvement in the Havana uprising

Readers of this journal might have wanted Childs to provide an explication of his key theoretical categories, such as “ethnicity” and “identity.” For example, his reasons for applying the term “ethnicity” to African political and cultural formations, contrary to documents from the era that refer to them as “nations,” are unclear. This lack of a justification for Childs’ theoretical tools overlooks the ways scholars have grappled with the utility of these analytical categories in recent years. However, these shortcomings do not detract from Childs’ contributions. The 1812 Aponte Rebellion is a passionately written book that will prove to be a valuable contribution to slavery studies, as well as to the historiography of Cuba and the African diaspora.

Frank Andre Guridy
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Indo-Caribbean Indenture: Resistance and Accommodation, 1838–1920. By Lomarsh Roopnarine (Mona, Jamaica, University of the West Indies Press, 2007) 176 pp. $25.00

This history of the indentured South Asian immigrants to the British Caribbean seeks to rescue their difficult experiences from the relative obscurity into which the author believes they have fallen among many modern West Indians. In narrating the story chronologically and thematically, Roopnarine shows a good familiarity with the historical literature on the Indian emigration schemes that were designed to relieve the labor crisis engendered by the British emancipation of slaves in the sugar colonies. The slender volume also references a number of published official documents from that era. However, in other ways, Roopnarine’s
approach is at variance with current standards of the historical profession.

Although Roopnarine does not explicitly specify an intended audience for the volume, he seems to have written it in the manner of an introduction, taking pains to insert definitions of terms that may be unfamiliar, such as *mercantilism*, *sepoy*, *laissez-faire*, and *philosophe*. He introduces most topics with some general discussion about the differing scholarly views surrounding them, but he generally moves with great speed to settle into a materialist perspective that was more common a half-century ago. He does not establish his materialist stance by refuting those who have chosen different methods or by offering compelling evidence to support his positions. Rather, using an approach that might be more familiar in philosophical argument, he argues deductively from the general to the particular. Thus, in one case, he concludes a brief sketch of sociopolitical circumstances in British India during the 1850s with a brief quotation from Vladimir Lenin on the destructive power of imperialism. From Lenin’s general statement he concludes, “The nature of East Indian emigration to the Caribbean revealed these dynamics” (23).

Although the field of migration history has profited greatly in recent decades from the analysis of statistics and demographic trends, quantitative data receive slender attention in this work. Tables of migration appear as appendixes, but they remain in the identical form in which they were probably compiled and not much is made of them. Roopnarine seems on even shakier ground in trying to understand the complex economic forces that shaped commodity production, wage rates, and population movements during the period under consideration. Basically, as he presents them, they are all reducible to capitalist exploitation.

Perhaps oddest of all is Roopnarine’s inability to grasp historical relativism. Rather than seeing, as Karl Marx did, each era’s ideas, values, and institutions as products of their historical circumstances, he seems to judge all actions by a decidedly modern ethical standard. In the conclusion he states, “The colonial government encouraged emigration and the establishment of settled East Indian communities, but hardly provided a positive environment for the development of a settled domestic life” (109). That statement may be perfectly true, but it seems mere political posturing to imply that nineteenth-century governments saw creating ideal communities as part of their mandates. Roopnarine attributes British failure to racism alone, but a more meaningful explanation would have to take into account the difficult social and economic conditions that also existed among British industrial and agricultural workers during this era or among the huge numbers of contemporary European emigrants. To do so would have required abandoning the idealist position and the narrowly circumscribed frame of reference that Roopnarine has adopted.

David Northrup
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The essential argument in this work is that Muhammad was no more than a revolutionary insurgent who changed the way in which Arabs fought. Gabriel argues that a close look at Muhammad’s military campaigns reveals all of the ingredients of an insurgency. The book is divided into thirteen chapters, seven of which are descriptions and analyses of Muhammad’s campaigns. The other six chapters are descriptions of pre-Islamic Arabia, Arab warfare, and the life of Muhammad. Unfortunately, the examination copy received had not yet put in either the maps or the index, thereby totally eliminating any possible redemptive quality for this work. Moreover, the quality of the editorial work is poor.

The title and the author’s background create expectations that are completely dashed upon reading the actual book. Had Gabriel stuck with his strengths, military history, the book might have been a good analysis of the tactics used by Muhammad in his campaigns against Mecca. Instead, he overreaches, deciding to include a “sexy” theory—an attempt to link the insurgency in Iraq and Afghanistan with Muhammad, who after all, was the “inventor” of this method of warfare.

Gabriel overlooks a number of facts: First and foremost, Muhammad and his movement do not fit any definition of insurgency, however large the latitude given to the definition of the word. The Department of Defense (DOD) Dictionary of Military Terms defines insurgency, “An organized movement aimed at the overthrow of a constituted government through use of subversion and armed conflict.”1 On first glance, Muhammad did seem to lead an organized movement that used subversion and armed conflict, but the most important aspect of the definition—“overthrow of a constituted government”—did not exist in seventh-century Arabia. Had Muhammad started his armed conflict in Mecca, then Gabriel would be accurate in calling it an insurgency, but such was not the case. The armed conflict started after Muhammad and all of his followers moved to Medina, which was not under the sphere of influence of Mecca but, at the very least, a “parallel structure” and, after Muhammad’s efforts, a genuine rival.

Gabriel’s theory that Muhammad was the “inventor of the methodology of insurgency” becomes the strainer through which all of the facts are squeezed (xxxi). For example (among countless other examples throughout the book), according to Gabriel, “The leader of an insurgency must take great care to guard his power from challenges, including those that come from within the movement itself” (xxv). Thus, Gabriel continues, Muhammad established a bodyguard, called the suffah, whose job was to protect Muhammad and to assassinate and terrorize (two fundamental methods of any insurgency) his opponents. The most glaring mistake in the above statement is the designation of the Suffah

1  http://books.google.com/books?id=g-M-Ey2YcYaC&pg=PR1&ots=RDBr5fQVv4&dq=DOD+dictionary+of+military+terms&sig=AQwfeFG0TKGsLfB4epzt8kTaR Fo.
more accurately *ahl al-Suffa*) as a bodyguard. This group of Muslims comprised the mendicants of the emigrants who did not have a confederate in Medina with whom they could live; they had only a portico/vestibule in the mosque of Medina as their living quarters. They were neither a bodyguard nor an intelligence service.

The next problem is more a question of interpretation. The fact that Muhammad did indeed order the assassination of certain enemies does not prove that he was a leader of an insurgency, because heads of states, which have a monopoly on violence, also can call for the elimination of enemies.

Finally, Gabriel suffers from the problem of not knowing the sources or the primary language. He dismisses the need for the latter: “Only rarely do the language skills of any researcher equal those of a scholarly translator” (xxix). Unfortunately for Gabriel, this is a rare case! The lack of knowledge of the language creates glaring mistakes that are a constant distraction for an informed reader. Not only are names butchered and terms misused; Gabriel also betrays a clear misunderstanding of the secondary texts on which he heavily relies.

Saliba’s wide-ranging book on Islamic astronomy is a fascinating, revisionist account of a science that blossomed in a golden age under the Baghdad caliphs before fading into obscurity. Saliba persuasively argues that the sciences flourished in the ninth and tenth centuries not so much because of enlightened patronage in a climate of toleration but because of a realignment of the skills and languages required for government positions. In the ensuing translation program, Greek sources replaced Persian and even Syriac writings.

Saliba presents evidence that Islamic astronomy continued to flourish, becoming more sophisticated and paving the way for the European Renaissance in astronomy. His is an argument for continuity, not revolutionary discontinuity. Because the work of Copernicus involved technical details similar to those employed by the Islamic geometricians, Saliba assumes a causal linkage: Islamic astronomy “was in a position to lay the foundation for a revolutionary upset of [the older Greek] tradition (173) and “it seems to have had a seminal impact on Renaissance science” (233). This part of the thesis is, I submit, seriously flawed; in fact Saliba’s account provides the basis for a conclusion diametrically opposed to the one expressed in the title of his book.

In the centuries following the initial burst of translations, geometry and trigonometry developed, leaving behind the clumsy chord function and the solutions afforded by the Menalaus theorem that had powered
Ptolemy’s *Almagest* (c. 150). The Islamic developments in trigonometry provided a heritage brilliantly employed by Regiomontanus, Copernicus, and other European mathematicians. But the Islamic astronomers busied themselves with the “absurdities” of Ptolemy, that is, his digressions from Aristotelian dogma. Their efforts resulted in ever-more intricate geometrical models composed from combinations of circles with uniform motions. Saliba is so imbued with a medieval Islamic viewpoint that it never occurs to him to notice that these solutions contain their own comparable absurdity—motion around empty points. Those astronomers were chasing a chimera, and ultimately a dead end. The much-criticized Ptolemaic equant is in fact closer to the physical approach of Johannes Kepler’s law of areas than to the thirteenth- and fourteenth-century models of Tusi or Ibn ash-Shatir. Furthermore, there is no convincing argument that their models in any way led to a heliocentric cosmology for Copernicus, and they certainly did not do so for the Islamic astronomers.

Islamic model building was almost entirely divorced from any increase in accuracy of predictions, the one obvious exception being Islamic lunar theory. However, even though this model resulted in an improvement with respect to the moon’s apparent size, no observational input seems to have followed to show that the model was still seriously deficient. Furthermore, a major anomaly in lunar longitude went unnoticed until Tycho Brahe’s measurements. An obvious unification awaited with respect to the unnecessarily complex Ptolemaic Mercury theory, but the Islamic astronomers simply interpreted Ptolemy rather than the sky.

What the Renaissance required was a strategic break with the past. Copernicus was still mired in Islamic-type intricacies (which were greatly admired by the majority of his contemporaries), but his stroke of genius was the heliocentric insight that started the scientific revolution. Islamic trigonometry was a notable factor, but Saliba’s attempt to hitch his star to the Islamic astronomy of uniform circular motion misses the essential transformation of Renaissance science.

At present, there appears only one small avenue where a specific geometrical insight from the Islamic world might have given an indispensable impetus toward the radical heliocentric rearrangement. Around 1430, ‘Ali Qushji, an Islamic astronomer, wrote a small treatise with a parallelogram transformation diagram that reappeared almost identically in Regiomontanus’ *Epitome of the Almagest*, a volume finally published in 1496. That book, known to have been used by Copernicus, is most likely the source for an essential step toward heliocentrism. Perhaps a connection between Islamic astronomy and the making of the European scientific Renaissance really does exist, but it seems not to be in the place where Saliba has been looking.

Owen Gingerich
Harvard-Smithsonian Center for Astrophysics
This book is a deeply learned study of the government and society of Eastern Iran and Central Asia under Shahrukh (1409–1447), the son and successor of Tamerlane. Iran was then governed by a Turko-Mongolian Chagatay military elite backed by a bureaucracy recruited among local administrators and religious officials. Local cooperation was the basis of central government power.

The crucial political fact of the era was that the Timurid regime did not have a monopoly of force. All strata of the population were organized and capable of military action. Manz thus sees Timurid political life in terms of the relations among the central and local elites. Her work follows the precedents set by Lapidus, *Muslim Cities in the Later Middle Ages* (Cambridge, Mass., 1967), stressing the relations among the major strata of the population, and by Roy Mottohedeh, *Loyalty and Leadership in an Early Islamic Society* (New York, 2001), emphasizing personal and clientele relationships.

The most important contribution of the book is its depiction of how fluid this society was. No sphere of power was exclusively controlled by one class or group. The local power holders varied from place to place. For example, in Samarqand, the major religious figures took the lead; in Isfahan, local headmen, viziers, and the chief judge shared power. No concept of specialized authority with dedicated functions or clear-cut boundaries between the various social strata was in evidence. Bureaucratic officials might also serve as military officers. Artisans and ‘ulama (religious leaders) might also have military roles. Timurid society functioned not through structured institutions but through a culture of personal loyalties and political entrepreneurship. Power was an individual achievement.

A marvelous and original chapter reveals that religious and spiritual authority were equally diffuse. Given the prevailing beliefs in the intercession of saints, visitation of graves to ask for divine favors, magic, charms, amulets, exorcism, as well as in Qur’an, law, theology, and mysticism, many people had access to the spiritual realm, including rulers, ‘ulama, sayyids (descendents of the prophet), Sufis, dervishes, poets and madmen, and ordinary worshipers. Even though offices, endowments, and family connections gave power to shaykhs, religious leaders had to attract followers by their personal qualities and performances. The diffusion of sources of power and authority within the religious realm, along with the fluid competition for recognition and influence, mirrored that within the political realm.

Yet this book’s atomistic image of the society is not entirely persuasive. It is like having a scorecard that identifies the players without seeing the game. A comparison with Michael Chamberlain, *Knowledge and Social Practice in Medieval Damascus, 1190–1350* (New York, 1995), points to a number of potentially important issues. The household organizations
that lay behind the power holders are invisible. So too are the network links and the moments of consultation and coordination, however informal, that created political factions among military lords, landlords, merchants, and urban headmen and their cohorts or among scholars, saints and shrines, and their devotees. Cultural values, methods of acquiring status and influence, career paths, and support systems—informal or otherwise—all are conspicuous by their absence.

This admirably researched book explores the historical events in exhaustive detail and provides rich anecdotal material about the policies and careers of the Timurid elites. Maybe the sources do not allow further exploration, or maybe Manz’s narrative method and conceptual agenda have shown their limitations.

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*The Early Modern Ottomans: Remapping the Empire.* Edited by Virginia H. Aksan and Daniel Goffman (New York, Cambridge University Press, 2007) 376 pp. $80.00 cloth $27.99 paper

Ottoman history is alive and well, as attested by the proliferation of scholarship over the past four decades (and the appearance of this edited volume). Inspired by the opening of the Ottoman state archives located in present-day Turkey and led by a pioneering generation of scholars adept in the languages and crabbed scribblings of imperial scribes, Ottoman history writing gained new impetus with the fall of the Soviet Union. Searches for the keys to empire in general and to understanding the current state of southeastern Europe in particular have brought invigorated attention to this field of study. A number of survey textbooks now entice general readers while many lacunae are disappearing under the impact of new research.

The volume under review falls into the category of new research, focusing on the early modern era, which it raggedly defines as between the later sixteenth and early nineteenth centuries. It consists of twelve contributions by a varied, competent cohort; a non-exhaustive list of topics includes Ottoman politics and diplomacy and military, cartographic, legal, and cultural history. Generally, the authors share the ongoing revisionist impulse in Ottoman history writing, viewing the Ottoman Empire within the context of world history and the body of premodern states that possess a variety of common characteristics. For them, the Ottoman Empire is no *sui generis*, exotic oddity but a state and society with its own features as well as others that it shares with its contemporaries across the globe.

The individual chapters are all worthy, and many offer stimulating assessments. Collectively, however, the work suffers from the usual defect of an edited volume—the lack of unifying themes, beyond the com-
mitment to revisionism. The two historiographical inquiries by Douglas A. Howard and Baki Tezcan sparkle in their originality, offering fresh insights into how and why historical writing changed in the early modern era. Leslie Peirce’s study of “the material world” is a fine overview for both generalists and specialist. Edhem Eldem examines epitaphs in Ottoman cemeteries to learn about the social ambitions and manners of the living, and Shirine Hamadeh gives an international comparison of imperial gardens and their evolving use as public places.

Donald Quataert
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As If Silent and Absent: Bonds of Enslavement in the Islamic Middle East. By Ehud R. Toledano (New Haven, Yale University Press, 2007) 273 pp. $50.00 cloth $24.00 paper

Slavery existed in various forms in all societies and eras of history. Until relatively recently, however, most modern scholarship dealt with forms of slavery related to the Atlantic slave trade in the modern era. A growing number of scholars have expanded the range of this scholarship, examining the history and institutions of slavery in many different regions and periods. Toledano’s study of Ottoman history and society is an important contributor to this development.

In Toledano’s words, As If Silent and Absent “is all about people, their actions, their feelings”; it undertakes the task of “understanding enslavement on the individual level” (256). This project involves a re-reading of Ottoman state and private records in a “process of experience reconstruction,” in which Toledano works to “establish what enslaved individuals did” and then immediately to “ask the questions What did they intend by so acting? What did they want to achieve by their deeds?” (35).

In Chapter 1, Toledano argues that enslavement “is a form of patronage relationship . . . requiring a measure of mutuality and exchange that posits a complex web of reciprocity” (8). Chapter 2 surveys the variety of responses by the enslaved when the bond of reciprocity is violated. Chapter 3 investigates the role of the Ottoman state in these experiences, noting that the Tanzimat modernization reforms of the late nineteenth century strengthened the responsibilities of the state as the patron of the enslaved. Chapter 4 examines criminal activity as a way for the enslaved to survive. The various criminal options included theft, arson, murder, prostitution, and acts of group defiance. Remarkably, in the Ottoman contexts, Toledano does not note any significant “slave rebellions.” In the final chapter, he describes “Ottoman cultural creolization”—that is, “the process by which enslaved Africans and Circassians retained ingredients of their origin—cultures” (204). The analysis concentrates on African “zar” traditions. The book’s conclusion provides an effective summary of the major themes.
Toledano’s analysis provides important insights into the lives of the enslaved but the presentation is a gallery of quick snapshots of many individuals. He might have given more attention to methods that expand the context of his subjects’ voices, like the in-depth reconstructions in Ulrich’s *A Midwife’s Tale*. Toledano cites important works by Price that uncover the stories of individual slave experiences, but he does not note Price’s innovative use of different type fonts to identify different voices in *Alabi’s World*. Toledano’s perspective is innovative, and his new reading of the sources is compelling. But his narrative method could benefit from the examples of scholars like Price and Ulrich.

This book represents a significant addition to the literature not only dealing with slavery but also with Ottoman history and with listening to the voices of nonelite people in world history. Toledano’s insights can benefit specialists and nonspecialists alike.

John O. Voll
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*Colonial Madness: Psychiatry in French North Africa.* By Richard C. Keller (Chicago, University of Chicago Press, 2007) 294 pp. $70.00 cloth $25.00 paper

For the past two decades, scholars have sought with dogged determination to demonstrate that the cultural traffic between the metropole and the colonies was a two-way street. In re-writing the history of French anthropology, medicine, sociology, and urban planning, Burke, Lorcin, Fogerty and Osborne, Pelis, Wright, Rabinow, and others have emphasized the experimental nature of what transpired in the French colonies and the contribution that practitioners in the periphery made to the development of modern professions back in L’Hexagone. In this book, Keller turns his lens to psychiatry and its offshoot ethnopsychiatry. In North Africa, both disciplines intersected with biology, anthropology,
Frantz Fanon first brought to the attention of the public the pernicious racism of psychiatry as conceived and practiced in Algeria, as well as the dehumanization inherent in colonialism. Keller contextualizes Fanon’s analysis by recounting the disciplinary history of French colonial psychiatry. He also introduces some of its less well-known indigenous North African critics, and he provides a trenchant commentary on colonial psychiatry’s contemporary legacy.

During the Algerian War, psychiatrically informed notions of racial difference contributed to the arsenal of the Cinquième Bureau, the division responsible for psychological warfare. Yet, as Keller argues, this ignominious final chapter should not mask French colonial psychiatry’s original progressive intent. Imbued with the myth of Philippe Pinel—the French revolutionary said to have unchained the insane in Paris’s Bicêtre and Salpêtrière hospitals—early champions of psychiatry in North Africa saw themselves as liberators. By introducing such modern mental hospitals as Blida in Algeria, Manouba in Tunisia, and Berrechid in Morocco, French psychiatrists sought a humane substitute for the nightmarish, local maristan and its often lethal alternative—transportation to France. Later, in their zeal to exploit North Africa’s potential as a social laboratory, disciples of Antoine Porot and others associated with the “School of Algiers” became enthusiastic proponents of then cutting-edge techniques like electro-shock therapy, insulin treatments, and psychosurgery.

Colonial Madness is frequently engaging, but it is also occasionally uneven. Additional prosopography in identifying the School of Algiers would have been helpful in the early chapters, and country specialists may not agree with Keller paving over differences between Algeria, Tunisia, and Morocco (for instance, Keller seems unfamiliar with Mohamed Boughali’s important Sociologie des maladies mentales au Maroc [Casablanca, 1988]). In his effort to locate the sources and ramifications of psychiatric racism, his discussions of biological difference, criminality, and primitivism tend to be repetitive.

These criticisms aside, the work has undeniable strengths. The case histories from the archives and the discussion of psychiatry in contemporary film and in the works of novelists like Kateb Yacine are thoroughly engaging. In his concluding chapters, Keller makes an excellent argument for continuity in the postcolonial world. In France today, psychiatrists still debate the role of cultural difference as the origin of pathology. Political wrangling over the integration of citizens with roots in France’s former colonial empire remains heavily tinged with racism. To his credit, Keller convincingly demonstrates why colonial-era debates over assimilation and association still resonate today.

Jonathan G. Katz
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A distinctive feature of China’s civilization was a belief shared by the emperor and his retinue that agriculture was their foundation and that to defeat famine was the sovereign’s moral responsibility. Li’s path-breaking work explains how North China, from the 1720s to the 1980s, struggled to overcome famine.

Li’s narrative explains why famines persisted in China. Increases in the harvest also tended to increase the population, which sometimes stressed the food supply. When bad weather, followed by poor harvests, turned into famines, the state mobilized to provide famine relief and restore farm production. The possibility of famine recurring, however, was always present, owing to environmental degradation and population increases.

In recent decades the Chinese have been able to stabilize their harvests by introducing a “green revolution,” which has alleviated some of the environmental deterioration caused by drought and water logging, and by relying on the global economy to stabilize harvest output. Li’s fascinating, multilayered history, tells how northern Zhili province and the metropolitan areas of Beijing and Tianjin cities worked to feed a steadily increasing population despite unstable harvests, an increasingly damaged environment, and a declining ratio of farm land per capita.

Between 1668 and 1900, thirty-one great floods or droughts occurred in Zhili’s Hai River Basin. Six great calamities occurred there between 1801 and 1895, and twelve major disasters and famines struck the Zhili–Hebei area from 1900 to 1949. Li vividly describes the regional calamities and the ensuing human misery caused by the Great Leap famine of 1958 to 1961, during the Marxist regime, in which millions perished.

The instability of the region’s eleven rivers and an escalating population on limited arable land were primarily responsible for the environmental degradation. Moreover, frequent weather fluctuations prevented many farmers from restoring their land to former levels of productivity. As a result, this northern region suffered the highest frequency of natural disasters within the empire. To make matters worse, as population increased, the expansion of cultivated land slowed, and the population became denser. Farmers retaliated by planting new crops and by applying new multiple-crop methods and weeding practices. Li, like Elvin, describes much of Chinese farming of the late eighteenth and nineteenth centuries as “increased labor inputs fail[ing] to increase productivity after reaching a certain level.”

To cope with harvest instability and to fight famine, state officials had gleaned from experience a large repertoire of methods to relieve famine: sending monthly reports to the throne, describing crop condi-

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1 Mark Elvin, The Pattern of the Chinese Past (Stanford, 1973), 110.
tions and grain prices; routing grain-filled ships to cities and areas to reduce prices; organizing different granary systems to issue money or grain to help the needy; lowering the annual land tax; and issuing seeds to farmers for the next harvest. In Beijing and Tianjin, state officials patrolled the grain markets, monitored merchants’ stocks and sales, and punished merchants who hoarded grain. These efforts helped to prevent prices from rising, but excessive market intervention often had the reverse effect of exacerbating scarcity in the market.

Li argues that historical records suggest a low degree of commercialization in most localities, with only a small-scale marketing of grain in the region. She believes local-market integration and their scale to have been slight. Using price data to produce coefficients for price variations of sorghum, millet, and wheat, as well as to trace market-price correlations, she convincingly shows that market integration for wheat and millet occurred in only a few large centers, such as Zhili’s Baoding district and Tianjin. Such limited market integration in North China suggests that a customary economy of huge size existed, in which barter, rather than market prices dominated. Thus, two different exchange structures coexisted; this phenomenon deserves more research.

Even so late as the early 1900s, environmental deterioration continued to spread in North China. Although commercialization of agriculture accelerated, political and social instability increased. According to Li, the region’s qualitative change in livelihood was like drinking “boiled water versus unboiled water.”

In spite of improving agriculture through the introduction of grain seeds that enhanced yields while resisting major diseases and drought, the Communist Party was still more occupied with feeding the people than cultivating political freedom or Western-style democracy. The first man-made famine under the new Communist team-farming system occurred between 1958 and 1961 when the government’s distribution systems proved unable to deliver grain to low-producing rural areas. The resulting severe food shortage and widespread undernourishment damaged the party’s reputation and morale.

Li’s study is a work of enormous erudition. She has applied statistical analyses, garnered new historical sources, and examined intensive farming and environmental decay to show, with skill and imagination, how China’s creative and hard-working authorities fulfilled their public-policy responsibilities (excluding the disgraceful famine under socialism). China’s famines are often man-made. According to Li, the environmental deterioration at the heart of the problem will not improve until the country’s leaders establish the rule of law, minimize corruption, and interrupt the inertia of the state apparatus.

Ramon Myers
Stanford University

Expunging Variola is a meticulously researched, dispassionate account of the coordinated (and not-so-coordinated) worldwide effort to eradicate smallpox from late twentieth-century India, one of the disease’s last major strongholds. Bhattacharya focuses on the complex relationships that emerged between, on the one hand, such international and metanational organizations as the U.S. Centers for Disease Control and the World Health Organization and, on the other, elements of the new Indian state’s sprawling national and state-level bureaucracies. The relationships were often contentious. Bhattacharya again and again points out that the battle against smallpox was not waged and won by a monolithic global public-health administration, or even by monolithic entities within a more cacophonous worldwide health network. Instead, policies and practices were contested, refigured, rejected, or sometimes deployed by local governments, politicians, and medical officials. Arguments even extended to vaccination practices and types of vaccine (wet versus dry, for instance).

In the context of the hysteria about bird flu that dominated headlines at the time of this book’s release, Expunging Variola’s dry cascades of detail effectively lay out the extraordinary amount of planning, preparation, skill, and luck that must go into large-scale medical campaigns while also highlighting the assets of steady hands and basic research. The book also touches on a number of interesting side issues, including the tense and contradictory relationship between international organizations housed in the West and newly formed postcolonial states still sensitive to any sign of Western imperialism. The reliance of public-health campaigns on the compulsive power of the state also comes into the picture.

A number of important points, however, escape the author’s attention. Bhattacharya only briefly mentions the “widespread [belief in the goddess Sitala] among Hindu communities in northern, central, and eastern India” (239).” For such believers, Sitala was the divine source of smallpox, for good or ill. This particular fact raises many thorny social and cultural issues: What is the role of religion more broadly in embracing or resisting public-health campaigns? How does one escape an Orientalist framework in approaching such issues? Bhattacharya does not address these questions in any rigorous way; nor does he provide an adequate analytical frame to understand them. Indeed, the smallpox campaign also encompassed issues of caste, class, gender, and region, all of which require much more contextualization. Expunging Variola would greatly have benefited from a much more extensive engagement with relevant contemporary scholarly literature, which would have allowed Bhattacharya to further frame his study in relation to twentieth-century anti-vaccination movements or larger debates about sovereignty and cit-
izenship, among other possibilities. Bhattacharya’s nearly exclusive focus on smallpox places serious limitations on this work.

The elimination of smallpox in India is an inherently exciting tale; the termination of one of the world’s oldest and most powerful killers took place amid rampant neo-imperialism, the international dynamics of the Cold War, a host of challenging new intellectual philosophies, and the blights of poverty, caste, and communalism. Bhattacharya is to be commended for guarding against sensationalism and romanticism in his approach, but he might have been a little too restrained. Interested primarily in the bureaucracy, he has written an extensively bureaucratic tract, reminiscent more of a government report than of a compelling social, political, or medical history, as illustrated by his heavy reliance on long quotations throughout the book. This material appears to serve the purpose of public disclosure more than to provide analysis. As Bhattacharya explains at the end of his introduction, “Large parts of this study are based on materials that have not systematically been used in academic studies before. . . . As this material is relatively difficult to access, this book quotes in detail from some of the interesting items, not least as some scholars might find this information useful” (11).

These limitations notwithstanding, Expunging Variola is an excellent source for understanding important aspects of one of the world’s largest public-health campaigns. In the context of continuing concern about a new influenza pandemic or bio-terrorism, it is also a useful reminder of how complex disease control can be.

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