On Deep History and the Brain. By Daniel Lord Smail (Berkeley, University of California Press, 2008) 271 pp. $21.95 cloth $15.95 paper

World historians are of two kinds—the professionals and the interlopers. Smail, in real life a talented historian of medieval Europe, is emphatically an interloper, but one with an unusually grand vision of what the study of the human past should be. In essence, his vigorous and spirited book is a plea for two things, each of them represented in his title.

The first is “deep history.” The depth is chronological: Smail is making a case for a history of our species that reaches far back into the Palaeolithic. He does not simply mean that historians ought to pay some attention to what prehistorians, archaeologists, and geneticists have to say about the Paleolithic background to history; such an approach would do no more than ask historians to cross a disciplinary boundary. His proposal is much more ambitious: He sees the Palaeolithic not as the background to history but rather as an integral part of it; in other words, he seeks to remove a disciplinary boundary. To this end, he launches a polemic against the normal restriction of “history,” understood as a label for what historians study, to the last few thousand years. To make his colleagues feel bad about this restricted usage, he tells them that what they are really doing is holding fast to the traditional Christian chronology, according to which the world was created around 4004 B.C. But he also turns his guns on what is surely the real reason for sticking to shallow history—namely, that it makes a big difference whether or not the past can be studied on the basis of human memory, typically as represented by written sources.

If there is a major problem with Smail’s argument, it is that he has not confronted the practical implications of his program. Would he really have people trained in the discipline of history excavating Palaeolithic sites and sequencing Palaeolithic DNA? And would he have prehistorians, archaeologists, and geneticists change their professional identity and move into history departments, pushing for academic cleansing to free up the necessary lab space? Such a program seems more like a recipe for indiscipline than for interdisciplinarity. But if this is not his intention, it is unclear just what he is proposing—unless, perhaps, he is trying to have it both ways, combining the modest substance of crossing a boundary with the rhetorical panache of abolishing one.

Smail’s other plea concerns the brain: He would like historians to give an ear to neuroscience. Indeed, one of his chapters bears the title “The New Neurohistory” (he does not divulge what the old neurohistory was). It is certainly in the interests of historians that some of their number should keep a watch on what scientists are doing. Geneticists, for example, do not simply confirm that our ancestors had genes; they provide solutions to historical problems that cannot be resolved from written sources. But can neuroscience, at least in its present state, match the contribution of genetics? It can reveal the physical underpinnings of our thoughts and feelings, and by implication those of our ancestors; but
with one possible exception (154), it is not clear from Smail’s account that it can provide new historical information in the way that genetics can and does.

Within the discussion of neuroscience, the last substantive chapter narrows its focus to the psychotropic—activities and substances that humans use to effect changes of mood in themselves and others. This is the chapter most likely to have a positive effect on the mood of readers. One of its high points is an account of the phenomenal growth of the psychotropic techniques at the disposal of eighteenth-century Europeans, from erotica to tea. Another is its analysis of the use of random violence to achieve social dominance by medieval castellans, male chimpanzees, and—lest the comparison seem unduly sexist—some female baboons.

On Deep History and the Brain falls somewhere between an overlong manifesto and an overshort book. Those passages in which Smail is content to leave off haranguing and, as he puts it himself, “just get on with it” (43), are by far his most convincing.

Michael Cook
Princeton University

Low Income, Social Growth, and Good Health: A History of Twelve Countries. By James C. Riley (Berkeley, University of California Press, 2008) 229 pp. $45.00

In this concise and highly readable book, Riley looks at the various routes to long life expectancy taken by a selection of diverse countries. In many ways, this book is the final panel of a triptych, following from the author’s Rising Life Expectancy: A Global History (New York, 2001) and Poverty and Life Expectancy: The Jamaica Paradox (New York, 2005). Rather than a global overview, or an in-depth case study, this volume compares twelve countries. Two of Riley’s data points, Costa Rica and Sri Lanka, are the canonical examples (along with India’s Kerala state) of good health despite low income, always referenced by UN agencies anxious to prove that long life expectancy is not the exclusive dominion of rich nations. Riley’s other ten nations are Japan and Korea (a colonizer and former colony), Panama (neighbor of Costa Rica), Cuba and Jamaica (two West Indian nations with different governmental styles), China and the Soviet Union (socialist economies), Oman and Venezuela (petroleum exporters), and Mexico (exemplifying certain characteristics of income inequality).

What these countries have in common is that all made great strides in health from 1920 to 1960 (except Oman, which, buoyed by oil revenues, transitioned slightly later). Riley employs long life expectancy as a proxy for good health, a crude equivalency, but one unlikely to be improved upon in the same historical context for such a diverse selection of
countries. According to Riley, these countries have little else in common. Using deftly juxtaposed examples (for instance, a chapter on Cuba and Jamaica), the book shows how the particulars of each country’s health transition have differed. In Cuba, where most of the pivotal gain in health occurred before the revolution, vaccines and vector control took center stage. In Jamaica, the introduction of latrines and education about hookworm led to wider educational campaigns against other diseases. Differences across chapters (that is, across further international comparisons) are even more pronounced.

What really unifies these diverse examples is the concept of social growth, or the accumulation of social capital, “physical assets plus human knowledge and skills” (3). The importance of knowledge is a thread that runs throughout the book. The profiled countries were better than others “at building the institutions, habits of mind, and behaviors of social growth” (9). Whether discussing pit latrines, the isolation of tuberculosis patients in sanatoria, or the control of malaria-spreading mosquitoes, Riley emphasizes knowledge and the concomitant change of behavior that leads to the institutionalization of norms promoting good health. As a counter to the country-specific studies, the book ends with a chapter discussing three key causes of death—malaria, tuberculosis, and diseases transmitted by fecal contamination. Certainly, these diseases were the common backdrop for the stage of mortality decline, even if the foreground varied from place to place.

The McKeown debate—about the relative importance of nutrition versus health interventions in the historical decline of mortality—persists.¹ Riley has been a voice of reason in this polemic, arguing against particularistic approaches by observing the many interconnected ways in which health has improved. In Low Income, Social Growth, and Good Health, Riley underscores another often-overlooked subtlety, namely, that the recipe can vary for different countries. This point seems a logical corollary of much past research about mortality decline, but people have often avoided connecting the dots, preferring instead to focus on one factor or the other.

One obvious desideratum is to apply the historical lessons of this volume to the present. The region most likely to benefit from the insights of this book is sub-Saharan Africa, as Riley briefly observes in the conclusion. He might have directed more attention, however, to the problems in that part of the world. Riley is well aware that sub-Saharan Africa currently faces an epidemic of HIV/AIDS, which none of his profiled countries faced during their pivotal periods. But Africa has other problems that are even more fundamental and harder to measure. Riley’s observation that reliance nowadays on antibiotic drugs may hinder the institutionalization of healthy habits is perspicacious (171).

¹ See Thomas McKeown, The Modern Rise of Population (New York, 1976), for the claim that better living standards were more responsible for mortality decline than medical intervention was.
In covering so much ground so efficiently, Riley inevitably leaves room for cavils about the details. John Snow seems to have been unfairly omitted from the brisk summary about the development of the germ theory *vis-à-vis* cholera and fecal contamination of drinking water (144). Nonetheless, this volume is an important contribution to the literature about mortality decline, and Riley’s argument that there are routes to low mortality (note the plural) will be hard to dismiss.

Andrew Noymer  
University of California, Irvine


In this remarkably lucid and deceptively accessible little book, Tilly presents a unified theory of the processes that shape transitions to and from democracy. Although he draws upon his vast knowledge as historian and sociologist, the exposition develops primarily through analytical narratives and comparisons of a variety of contemporary regimes: India, South Africa, Russia, Spain, and Venezuela. Occasionally, he expands the perspective historically through discussion of the French, Dutch, and American pre-twentieth-century experiences.

Tilly argues that there are no single prerequisites or general sequences that lead to democratization or its reversals. But he does point to “recurrent causal mechanisms” that “in varying combinations and sequences” produce changes in three processes that he deems essential to working democracy (78). These three processes are necessary conditions and constitute the major focus of the book. Democratization (the introduction or deepening of democracy) involves the integration of “trust networks” into public policies, the insulation of public policies from “categorical inequalities,” and decreasing autonomy of major power centers (23, 74–77). The opposite process of reducing some or all of the elements of democracy is characterized by contrary processes of the withdrawal of trust networks, the intrusion of categorical inequalities, or the increased autonomy of alternative power centers.

In his overview discussions, often summarized in handy tables and boxes, Tilly discusses the causal mechanisms that affect each of these processes and their interactions, some of which involve unique historical shocks, such as war or invasion. Others involve social and economic development, such as the movement from an agriculturally based economy to one based on industry or knowledge. He then produces several sketches of historical or contemporary experience with democracy in specific regimes. He also discusses alternative “paths” that are established primarily by the timing of the development of state capacity in relationship to the democratization processes.

The most attractive and challenging feature of Tilly’s approach is its

Miles traces the changing visual representation of the female breast in the Western world. According to her, the breast was predominantly a religious symbol in paintings made during the late Middle Ages, but by 1750, it had become a thoroughly erotic and medical phenomenon. This book expands on Miles’ influential essay, “The Virgin’s One Bare Breast,” first published in Susan Rubin Suleiman (ed.), The Female Body in Western Culture (Cambridge, Mass., 1985). Miles revisits familiar subject matter but includes a wider array of images of female breasts, placing them within a broad, pan-European context that features gradual changes in religious practice, medical concepts, women’s economic status, and the rise of witch hunting. The book’s methodology is traditionally art-historical and not interdisciplinary. The author arranges the images by date and theme, considering how they changed over time in relation to contemporary theological, medical, political, and philosophical written texts. Her historical evidence is primarily gleaned from work published by historians within the past thirty years or so.

The originality of Miles’ approach consists in her collection and analysis of the images themselves. This focus on visual evidence allows her to challenge Steinberg’s famous claim that Renaissance imagery draws attention to Christ’s penis in order to emphasize his embodied humanity.¹ Miles contends that the humanity of the Christ child is also

¹ Leo Steinberg, The Sexuality of Christ in Renaissance Painting and in Modern Oblivion (New York, 1984).
central to early modern paintings that show him breast-feeding—his fleshly body prominently figured in relation to his mother’s breast, thus drawing attention to the importance of the female body in early modern religious belief. This point is in accord with Bynum’s work.2

Scholars now regularly examine the shifting representation of the female breast in Western culture, focusing on debates about breast-feeding, medical understandings of lactation, and the significance of stories about supposedly single-breasted Amazons.3 Most of these publications are restricted to a single country or locale, noting the specificity of regional and historical beliefs about the breast. In contrast, Miles’ book takes a broad view of the subject, resembling the survey of representations of the female breast written by Marilyn Yalom—The History of the Breast (New York, 1997). Unlike Miles, Yalom takes her analysis into the modern period to consider the commercialization of the breast. Both authors portray, however, the gradual transformation of the breast from a sacred to an erotic object during the early modern period in Europe. Miles’ book, which is aimed at a general audience, provides a good starting point for those beginning to think about the historical nature of the human body.

Lianne McTavish
University of Alberta

Strangers Nowhere in the World: The Rise of Cosmopolitanism in Early Modern Europe. By Margaret C. Jacob (Philadelphia, University of Pennsylvania Press, 2006) 189 pp. $34.95

Jacob, well-known for her studies on the Scientific Revolution, Enlightenment, freemasonry, and the politics and culture of the late seventeenth and eighteenth century, has pulled all of these themes together in outlining the challenging story of “The Rise of Cosmopolitanism in Early Modern Europe.” Jacob employs a wide variety of sources and methods to illustrate how the ideas and ideals of being cosmopolitan took shape and were put into practice between 1650 and 1800. Her task is every bit as formidable as investigating the origins of toleration or capitalism.

Diderot defined cosmopolitans in the encyclopedia in the encyclopedia of 1751 as

“strangers no where in the world,” and Jacob expands the definition of “being cosmopolitan”—“the ability to experience people of different nations, creeds and colors with pleasure, curiosity and interest, and not with suspicion, disdain, or simply a disinterest that could occasionally turn into loathing” (1). As “a citizen of the world,” a cosmopolite practiced “a form of virtue” toward the other, whether hospitality or comfortable socialization with foreigners and people of different religions across “family taboos and regional parochialism” (2, 3, 5). Jacob aims at writing “a different kind of history” in “presenting practices, behavior, social habits, [and] mores from the quotidian long past” that illuminate the origins of today’s diversity, multiculturalism, and clash of cultures in its possibilities and limits (3–4).

The book’s five chapters chart cosmopolitanism not in relation to the more common sites of sociability—court society or civil society—but to religion, science, markets, freemasons, and revolutionary citizens in order to portray a world of cross-pollinating, interconnected spheres of thought, action, and passion. In Chapter 1, Jacob uses a postnationalist perspective to interrogate both Protestant and Catholic censors and inquisitors for insights about the mixing of Christians and Jews, orthodox and heterodox, and aristocrats and commoners to find proto-cosmopolites among the prosecuted.

Chapter 2 follows the need of modern scientific practice for witnesses to experiments, especially among alchemists and mechanists in London and Paris, as the basis for the social setting encouraging cosmopolitan mores. Currency and stock exchanges are the sites of economic as well as cosmopolitan exchange in Chapter 3, as international markets, global commerce, and the luxury trade from records in Antwerp, London, Paris, Lyon, and Marseille reveal a more complex reality of reciprocity than the vaunted ideal of market freedom. In Chapter 4, Jacob studies the new associational brotherhoods practiced by freemasons in depth through the recently returned archives of the Bordeaux lodge, highlighting the paradox of cosmopolitanism and egalitarianism versus secrecy and exclusivity, as exemplified in the restrictions against Jewish membership. But secret societies could become sites of radical republicanism and subversive revolution. Hence, in Chapter 5, Jacob introduces the British radicals and romantics who, in their optimism for crossing boundaries and being free from constraints, invented the “bohemian” as a citizen of the world.

Jacob’s essay into the origins of cosmopolitanism builds its argument on two methodological perspectives: (1) post-nationalism and post-confessionalism, which permit her to think across social, religious, and national boundaries; and (2) evidence from practices or habits, which permits her to view cultural practices and de facto mores, rather than theories or ideals, as the instruments of change in early modernity. Given their practices that bridged the divides between, and challenged the authority of, church, nation, order, and orthodoxy, these cosmo-
plites provide an explanation for the widespread fervor for reform and revolution during the late eighteenth century.

John A. Marino
University of California, San Diego

Divided by Faith: Religious Conflict and the Practice of Toleration in Early Modern Europe. By Benjamin J. Kaplan (Cambridge, Mass., Harvard University Press, 2007) 415 pp. $29.95

Popular depictions of early modern Europe in cinema and literature present intolerance as life’s daily menace; vivid scenes of religious violence and the martyred heretic perishing at the stake are the dramatic proof of a world unable to accommodate religious difference. In the academic realm, historians have focused upon the hot spots of religious rivalries, dissecting the causes of religious civil war, exploring the “rites of violence” that Catholics and Protestants inflicted upon one another and offering such rich case studies of intolerance as the infamous Saint Bartholomew Day’s massacre of 1572. More broadly, surveys of the period describe how Europe hardened into confessional blocs, state authority surged, and religious conflict was the order of the day. Not until the intellectual pioneers of the European Enlightenment arrived on the scene were theological dogma and the intellectual groundings of religious intolerance dislodged.

This standard narrative of early modern religious conflict has some truth to it, but it has also imposed parameters. Textbook writers often succumb to a binarism that paints the European map into differently coded religious denominations, and an intellectual Whiggishness that credits the rise of religious tolerance to progressive rationalists disenchanted with rank superstition and pointless violence. Specialists avoid such a trap, but their scholarly attraction to the great spasms of religious conflict has muted the quieter realm of religious coexistence. Kaplan wants to tell a different story about religious conflict and the reality of tolerance, grounded in neither the pressure points of religious violence nor the intellectual origins of tolerance. His story highlights the practical accommodation and improvised arrangements of religious coexistence. In his hands, the pioneers of religious tolerance are not the philosophes of the eighteenth-century salons and coffee houses but the neighbors, kinspeople, municipal and regional authorities, and government officials who puzzled through the often shifting borders of religious denominations to make ad hoc, practical arrangements for communities of different faiths—forms of pragmatic toleration avant la lettre.

Kaplan’s may be a revised story, but it is not one free of despair and bloodshed; persecutions, expulsions, and exclusions of both heretics and Muslim and Jewish infidels are integral to it. But his is a more nuanced story of porous borders, ecumenical arrangements, interfaith marriages,
and both secret and shared churches that provides a fresh perspective on religious accommodation in early modern Europe. Written for both specialists and generalists in ways that skillfully speak to both audiences, Kaplan’s book is an intellectual triumph that casts a wide net from Eastern Europe to the Mediterranean.

The book opens with three chapters on confessionalism in the sixteenth century, on the triggers in public life that sparked religious conflict among Catholics and the fledgling Protestant denominations, and on the interbraiding of the new religious map with the growing power of early modern princes. Protestants clashed with Catholics as they sought free expression in the public realm, while the political arrangements of *cuius regio, eius religio*, and *cuius dominium* calcified the political boundaries of faith communities.

Part two of Kaplan’s book brilliantly explores how these insuperable obstacles were, in fact, overcome by ecumenical experiments that historians have not hitherto fully understood—such as in the small German town of Goldenstedt where Catholics and Luthers produced a hybrid liturgy in the same church or, on a larger scale, in 1570 Poland where Luthers, Calvinists, and Bohemian Brethren participated in the sacraments at one another’s churches.

In three of the book’s most important chapters, Kaplan explores how Catholics and Protestants maneuvered through open borders, erected hidden churches and other places of worship, and even shared parish churches to bring religious pluralism to areas without any legal precedent for free expression of religious worship. His case studies include the practice of *auslauf*, or ambulation, best exemplified in the estate of Hernals outside Vienna, to which thousands of Protestants trekked on Sundays and religious holidays safely to practice their faith. Even more dramatic were Europe’s “schuilkerken” (hidden churches) at which adherents of officially proscribed faiths could worship out of the public eye, most famously in the Netherlands, which tolerated the private freedom of conscience, but elsewhere as well. Jews, for example, established several clandestine synagogues in Alsace during the seventeenth century. Kaplan also describes astonishing instances of shared churches, such as the parish church of Biberach, Upper Swabia, in which Catholics and Luthers worshipped at different hours of the day.

More generally, Kaplan canvasses the challenge of biconfessional communities, especially France, where even before the Edict of Nantes in 1598, scores of cities had accommodated Huguenot churches, sometimes within city walls, sometimes in the suburbs. Such commingling is a reminder that much of Europe was bi- or multiconfessional. Kaplan dedicates two additional chapters to examples of such coexistence, which was evident in mixed neighborhoods and in public venues like the marketplace, as well as in the phenomenon of intermarriage (a topic in need of further research).

In his penultimate chapter, Kaplan considers the enormous topic of Jewish and Muslim experience in early modern Europe. Like many re-
cent scholars in Jewish studies, he abandons the lachrymose narrative of persecution in favor of one that stresses civic dynamism and community organization, whether in the Italian ghettos or among the street merchants of Amsterdam. His investigation of the Muslim experience in Europe focuses upon the Moriscos of Spain, the victims of the largest forced expulsion in early modern Europe between 1609 and 1614. A final chapter takes stock of the Enlightenment and its fabled birthing of the concept of religious toleration, which Kaplan views as the culmination of two centuries of gritty compromises and accommodations.

Like any book of this scope, this one presents a few opportunities to quibble. Kaplan does not discuss the effect that overseas proselytizing and forced conversions had upon the European religious landscape. Furthermore, his portrait of the Enlightenment does not take full account of the newer literature that complicates its intellectual diversity. Nonetheless, his book merits all of the admiration and the wide attention that it will surely garner.

Peter Arnade
California State University, San Marcos

Placing the Enlightenment: Thinking Geographically about the Age of Reason.
By Charles W. J. Withers (Chicago, University of Chicago Press, 2007)
330 pp. $45.00

In this richly documented book, Withers makes the intriguing argument that “Enlightenment itself was geographical” (6). Respectfully bypassing recent discussions about the meaning of “the” Enlightenment (“what was it” and “what’s left of it”) and its historical context (“when” and “how” did it happen), Withers rephrases these standard questions by asking “where was it” and how do we “place” it? Once addressed in these terms, the Enlightenment was as much a spatial event as a “geographical thing” (5); it was as much an intellectual movement in whose wake European culture effectively redefined the meaning of space for non-Europeans, as it was a movement that had a geography of its own. Adapting familiar analytical models developed by such book historians as Darnton (the communication circuit) and science historians as Latour (centers of calculation), the book traces the enterprise of the Enlightenment geographically through an inter-European network of correspondence and global exchange of geographical information.1 It pays special attention to mapping projects and a burgeoning culture of geographical letters.

The book’s basic approach is as simple as it is effective. By substitut-

ing “geography” for “history,” Withers illustrates in Part 1 how definitions of the Enlightenment shaped and, in turn, were shaped by national interest and international rivalry in an increasingly decentralized European world between 1685 and 1815. Whereas in traditional historical accounts, Enlightenment theories and practices tend to be identified as individual contributions to a national collective, a geographical account demonstrates how European modes of knowledge production were a cosmopolitan enterprise beholden to the transnational dictates of communication (for example, the republic of letters and/or the world of belles lettres) and sociability (professional societies and salons). Having thus directed the focus to the spatial locales hosting Enlightenment practices, Part 2 of the book shows—using a wide range of sources, from travel accounts to natural history books and from geological theories about the origins of the earth to climatic theories determining the status of civilizations—how Enlightenment thinking was formed out of the nexus of geographical mobility and geographical discourse.

By the end of Part 2, Withers has delivered a general survey of European Enlightenment networks. On the one hand, he links the narrative and mapping habits that informed Royal Society members in London, middle-class travelers abroad, and even Native American elders, and, on the other hand, he presents a broad overview of English, Dutch, German, Italian, and French theories about the physical world and human difference. Joining with others who studied the literary foundations of the Enlightenment, Withers uses Part 3 to address the ultimate geographical locus of Enlightenment culture, the geographical text (maps and books), and the cultural institutions surrounding it—schools and academies, publishing houses, and the international book trade. The production and consumption of geographical writings become the book’s final setting. It thus suggests that the Enlightenment happened geographically, somewhere in the space between authors and readers or between pedagogic methods and popular demands.

This conclusion hardly comes as a surprise. Withers’ statement, two-thirds of the way into the book, “Geography was science as a form of knowledge—descriptive, locational, classificatory knowledge—not science as certainty grounded in demonstration” (179), offers the best description of how to make sense of the Enlightenment geographically. Throughout the book, Withers applies the term geography loosely and even ahistorically. It remains unclear, for example, how geography differs from such terms as place, locale, container, or field—all of which have been employed by a generation of scholars showing globalized modes of knowledge production during the period called the Enlightenment. But from the moment when Withers attends to the written forms of geographical knowledge, showing how maps and books circulated among philosophes and politicians, social societies and school children, the book assumes a compelling analytical framework. Because this framework comes late, the book’s sweeping synthesis of all things geographical can seem disorienting. But this sense of disorientation becomes a
vitre. After all, Withers’ goal is to remove the social history of the En-
lightenment away from the comfortable vantage point of metropolitan
institutions and to shed light on the spatial dynamics, in particular those
propagated by printed maps and geography books, underlying cultural
production during the long eighteenth century.

Martin Brückner
University of Delaware

The English Wool Market, c. 1230–1327. By Adrian R. Bell, Chris Brooks,
205 pp. $99.00

In the Middle Ages, English wool was a valuable commodity, particu-
larly for Italian merchants. Merchants had long been in the habit of of-
fering cash advances and long-term contracts to lay and monastic pro-
ducers for the later supply of a certain quantity of wool at an agreed
price. The fact that historians have been by turns overenthusiastic and
inaccurate in classifying these contracts as futures or loans raises the ques-
tion of how properly to describe them and—in a wider context—how
financially sophisticated medieval merchants were. Did they invent de-
rivatives like forward contracts and options centuries before their pre-
sumed, more recent development? Combining empirical historical re-
search with modern financial research methods, the authors aim (1) to
calculate the rates of interest charged on these contracts and determine
whether they fell within the normal medieval range and (2) to find out
whether the medieval wool market was efficient. These are difficult
tasks, because the rate of interest was normally concealed in view of the
Church’s prohibition of usury and because medieval price data are char-
acteristically not accurate, robust, or dense: It is not normally clear what
reported prices represent, whether they are comparable over time and
space, and whether the gaps in the information vitiate the analysis.

In the first part of the book, the authors analyze the 200 forward
contracts for wool—chiefly dating from 1270 to 1330—that they found
in a variety of sources (Public Record Office; The National Archives,
Kew, Richmond, Surrey; and the British Library). They then identify
the sellers (commonly monasteries, especially Cistercian) and buyers
(mostly Italian and Cahorsin merchants) and analyze their motives. The
monasteries were anxious to secure ready cash up front and a guaranteed
cash flow for the life of the contract, and to protect themselves against
the volatility of the wool spot market. The merchants were interested in
securing an assured supply of high-quality wool over the long term and
excluding their competitors. The authors discovered where (in London
and at the various fairs), when (in relation to the date of advance pay-

1 See also Bell, Brooks, and Dryburgh, Advance Contracts for the Sale of Wool c. 1200–1327
(Kew, 2006), 158–184).
ment), and by whom (monastic and merchant principals and agents) the contracts were negotiated, what penalty clauses were agreed in case of default, the mechanics of completing the contract (grading, packing, and delivering the wool), and what the factual consequences of default were for both parties (restructuring of the debt, often under royal pressure).

The following chapter highlights these issues in an in-depth study of the Cistercian abbey of Pipewell. The authors then apply modern financial research methods to the results of their empirical analysis, turning their attention initially to the determination of interest rates, which requires sophisticated and entirely plausible manipulation of the available price information. After surmising that the agreements in question were not futures, but were a cross between loans and forward contracts (130), the authors employ the formula for present-value calculation. Knowing the amount advanced (if it was the entire purchase price) and the spot prices for the life of the contract allows them to solve for the rate of interest hidden in the contract. Calculating both from the spot prices obtaining at the moment of the agreement and for each subsequent year of the life of the contract shows median interest rates of 22 percent and 18 percent, well within the normal medieval range. Next, they investigate the efficiency of the medieval wool market. In an efficiently functioning market, no information about a particular year’s spot prices or discount values should be able to predict the next year’s prices or discount. Despite the difficulties of the empirical evidence (which prohibit statistical analysis), the authors demonstrate that neither the amount of wool to be delivered nor the time span of the contract had any relationship to the size of the discount. Therefore, the “medieval wool markets were fairly efficient” (144).

This is a spectacularly successful investigation. The analysis is methodologically unassailable, and its conclusions are sound. Moreover, the book is superbly well written and supremely well organized. Those with no knowledge of modern financial research should have no qualms: The authors explain the most complex matters in layman’s terms.

Stuart Jenks
Universität Erlangen-Nürnberg

_Transatlantic Encounters: American Indians in Britain, 1500–1776._ By Alden T. Vaughan (New York, Cambridge University Press, 2006) 337 pp. $53.00

Vaughan has been a productive presence in the scholarship on colonial America for more than four decades, and his work has consistently been of high quality. Although historical anthropologists, those with stronger interests in Native Americans than in the English, or more theoretically oriented scholars of colonialism, might sometimes wish that Vaughn would focus more on their particular interests, they still approach his
work with great interest. His research is sound; he is reasonably open to interpretive approaches; and he writes well.

In *Transatlantic Encounters*, Vaughan handles the subject of American Indians who were taken to England prior to 1776 with his usual skill. He notes in his preface that he started out to focus on the (relatively few) well-documented cases, but “time and again, the experiences of less-prominent voyagers undermined my assumptions and generalizations and eventually persuaded me to make the narrative as comprehensive as possible” (xviii). His decision to explore the murky shadows and back-alleys of documentary passing mentions has produced a far more important work than would otherwise be the case: Vaughan is able to give accounts based on approximately 175 individuals instead of just a handful.

There are two threads to the argument—the lives and travels of individuals and the assessment of their contributions to the development of colonial America, and to some extent, of England. For most of the book, the former thread takes precedence in the narrative. The book presents its cases chronologically, beginning with an anonymous Baffin Islander taken to London by Martin Frobisher in 1576 and continuing through Samson Occam in the mid-eighteenth century; the penultimate chapter discusses several of the more obscure individuals from the 1770s before closing with Joseph Brant. The final chapter contextualizes these transatlantic encounters in the developing Anglo-Atlantic world, bringing together the threads that weave throughout the individual stories presented in the earlier chapters. This “Retrospect” could stand on its own, as could many of the individual stories, but the sum is even greater than its parts.

Some scholars might prefer an approach more grounded in the cultures of Native America and more oriented toward the roles of cultural mediators in their own societies. Others might want a more theoretically oriented study; discourse analysis could be productive with some of the texts, for example. Moreover, although Vaughan sometimes mentions Native Americans who were taken back to Spain, France, and elsewhere—whose stories can help to contextualize and understand those that Vaughan tells in detail—he does not portray them in depth because they were not his focus. Although his concentration on transportees to England is certainly understandable, the immensely challenging project of similarly treating other countries would be a valuable complement to this study.

Specialists in some of the areas covered in this book might quibble about a few missing facts. But they will also find new information from other cases, and a useful framework for thinking about the world of these encounters.

Frederic W. Gleach
Cornell University
In Harkness’ *The Jewel Box*, the city of London becomes a main protagonist of Elizabethan science. London provided the resources for a diverse population of craftsmen, botanists, instrument makers, prisoners, and others, both native and foreign, to explore the possibilities and utility of the natural world. Harkness’ rich descriptive study of this seething community of practitioners and students of nature provides a vital and original picture of a community of scientists whose existence was obscured by later developments in the history and historiography of science. Harkness has broadened the well-trodden path of the social history of science in a way that will challenge and shape any future efforts to describe the emergence of the Scientific Revolution.

Many cultural historians have appropriated the methodology and theories of anthropologists and sociologists, but Harkness’ use of “thick description” in an ethnographical account of an interwoven and complex scientific community is unique. Most social historians of science are concerned with the institutions, such as the Royal Society or the monarchial court, which shaped science. Harkness shows that science in the Elizabethan age was located and constructed through an urban marketplace of ideas, practices, and goods. By using manuscripts and books, church budgets, royal correspondence, broadsides, and prison notebooks, Harkness manages to re-animate and describe a lost population. She traces the networks of human and textual ties that made London a vital component of an emerging experimental and utilitarian culture. Her book will fascinate a broad audience of readers—social and urban historians, anthropologists, students of literary history, and anyone interested in a vivid picture of a vibrant past society.

Ethnographers want to establish rapport with their subjects; Harkness clearly could share an ale with hers. She examines foreign naturalists who lived in the Lime Street neighborhood of London, and who traded information on insects, tulips, and fossils. She explores the competition between medical practitioners and barber-surgeons, who sought profit and status. She recounts Elizabethan “Big Science,” an endeavor directed by William Cecil, the Queen’s minister, to use science to produce answers for monetary, navigational, and military problems.

Science transcended class and gender in Elizabethan London. Harkness’ examination of the unpublished notebooks of Clement Draper (c. 1541–1620), a merchant imprisoned for debt, reveals that “doing” science included reading and annotating books, as well as finding the proper recipe for the philosopher’s stone. Her exploration of the private and public roles of a gentleman brewer reveals the eclectic curiosity of students of nature, and how printing could obscure the many sources of scientific knowledge.

Harkness’ book ends with Francis Bacon, who, she argues, posthu-
mously redirected science in England. Bacon, born outside London, was unwilling to explore the sites in the city that provided a hands-on, experimental approach to science. Ironically, he coopted the practices of the nascent experimenters and advocated the ideal of learned scientists rather than those artisans, merchants and gentlemen who had earlier studied nature. In her splendid book, Harkness locates the Scientific Revolution in a place that Bacon chose to ignore, but now has regained its spot on the map.

Lisa T. Sarasohn
Oregon State University

Europe and the Making of England, 1660–1760. By Tony Claydon (Cambridge University Press, 2007) 370 pp. $95.00 cloth $34.99 paper

Claydon’s Europe and the Making of England, 1660–1760 is a revisionist study of English national identity from the Restoration of the monarchy in 1660 to the ascension of George III in 1760. Claydon seeks to demonstrate the continuing importance of religious commitment in an era that was once seen as a period of increasing secularization, partnered with a growing indifference to religion. Other revisionists, such as Clark, have made the same point.1 But Claydon’s book also challenges the widely accepted view of the English in the early eighteenth century as proudly inward-looking Protestants who had little to do with the alien, predominantly Catholic, cultures of the continent. Thus, Claydon challenges the powerful sway that Linda Colley’s, Britons: Forging the Nation, 1707–1837 (New Haven, 1994), has had over the profession. Claydon argues that far from being insular and xenophobic, the English were deeply concerned with the fate of Protestants in Europe. They also looked beyond what Claydon terms “the Protestant international,” seeing themselves as united with all followers of Jesus Christ, all of Christendom.

In order to understand how the English understood Europe, Claydon begins by examining travel guides to the continent. English travelogues were keenly sensitive to confessional divisions within Europe. Guide authors were not uncritical of Protestant lands, but they consistently saw Catholic regions as strange and alien, their people impoverished by the avarice of the Church and bedazzled by the trickery of priests. Whereas descriptions of Catholic Europe could all be set within a master narrative of priestly greed and blind superstition, accounts of Protestant nations were far more diverse, as a result of the Reformed religion’s own splintered nature. Nor were these descriptions always positive; the Dutch were just as often seen as greedy and cruel as they were thrifty and industrious. Nonetheless, England was part of a common Protestant world that crossed national borders. The English also recog-

1 See, for example, Jonathan C. D. Clark, The Language of Liberty, 1660–1832: Political Discourse and Social Dynamics in the Anglo-American World (New York, 1994).
nized that even if Catholic lands were “alien,” they were not nearly as strange and exotic as non-Christian realms. “The key division of the world was into its different faiths. Protestants and Catholics were huddled together in one corner: the differences between them were petty set against the bizarre beliefs which prevailed elsewhere” (65). In this sense, the English saw themselves as part of a larger Christian community.

The English also saw their confessional history as a part of European history, influenced by continental movements and ideas. Protestant dissenters and moderate Established Churchmen set the English Reformation within the broader context of Protestant Reformations in Germany, France, the Low Countries, and Scotland. The more conservative Laudians and Non-jurors stressed the Church of England’s heritage from the medieval Church. Either way, the Established Church was indebted to European traditions. Efforts to rewrite the history of the medieval church in England as completely independent of Roman authority were seen as “lame and incoherent,” as Brady, the constitutional historian, put it (113).2

The wars with the Dutch, followed by those with France, during this period also pulled England into the European orbit. Examining the various pro-war discourses in Anglican and Dissenting polemic, Claydon concludes that religious rhetoric, particularly arguments in the defense of Christendom, informed foreign-policy debates far more than any perceived threat of universal monarchy. Only when the English became increasingly war-weary did more secular “balance of power” arguments begin to appear. Claydon’s final chapter examines how the concepts of Christendom and the Protestant international were deployed in partisan polemic between 1660 and 1760. Although Protestant Europe has long been seen as important to Whig ideology, Claydon shows how the survival of Christianity abroad was equally vital to Tory polemic.

Not everyone will agree with everything that Claydon writes in this dense study of religious rhetoric during the first half of the long eighteenth century. But Claydon is certainly right to point out the important role that Protestantism abroad and Christianity in Western Europe played in the English imagination. Whether Claydon’s arguments can be applied to all English men and women is another story. One of the strengths of Colley’s work is the wide range of sources that she consulted to reach various social groups. Colley also saw women as consumers, creators, and propagators of a British identity. Claydon focuses entirely on tract literature—usually, though not always, written by divines. He has no interest in women as readers or writers; nor does he provide any evidence of the readership except to say that travel guides were popular. In the end, Claydon’s contribution to our understanding of national iden-

tity formation in England is certainly valuable, but until more evidence is provided, it can be only narrowly applied to the English people.

Melinda S. Zook
Purdue University

The Archaeology of Improvement in Britain, 1750–1850. By Sarah Tarlow (New York, Cambridge University Press, 2007) 222 pp. $80.00

In The Archaeology of Improvement, Tarlow outlines changes in British material life, describing how “change” itself became the modus operandi of Britain’s educated classes. For Tarlow, it was achieved through a cross-cutting ethic of “improvement” that is evident in literature and material culture associated with a vast array of practices, such as agriculture, landscape, town planning, social engineering, and industrial production. Tracking how nature, the built environment, and the British working population were the targets of improvement, Tarlow sets a stage for future historical archaeologies by presenting a materialized and active process that allowed change in the guise of reform, development, and progress to serve as the root of British culture in modernity.

The bulk of the book considers various improvement discourses in British culture from the late eighteenth to the early nineteenth century. Tarlow outlines the general processes and effects of improvement and details the material efforts of the planning, technology, and labor involved. She begins with the most well-known arena of improvement, agriculture and the enclosure of common fields. According to Tarlow, enclosure may be tied simultaneously to technological, economic, and social interests. Enclosure was intended to increase productivity through such rational landownership and land maintenance strategies as field drainage and fertility and the development of new technologies. Enclosure also applied to social and personal improvement: For example, “forcing Scottish Peasants into modern agriculture was thus bringing them into the present and, as reformers saw it, into civilisation” (87), or, more powerfully, “being an agricultural improver . . . made one a qualitatively different sort of person—a fully modern man” (88).

Tarlow traces this sweeping ethic of improvement in several other pursuits, including town planning, street lighting, sewerage, street cleaning, suburban cemeteries, and several attempts at reforming workhouses, prisons, and mechanics’ institutes. Archaeologists and historians will appreciate her encyclopedic approach that treats each improvement through the lenses of its historical and social impact and material form sequentially. Archaeologists will especially enjoy her final substantive chapter, which engages specific forms of material culture that both relate the technologies and impacts of improvement and show how this ethic may be discovered in even the most “common and mundane types of artefact” (164). For example, Tarlow identifies how developments in bleachworks, window glass, ceramics, and rubbish pits connect visibility,
cleanliness, and taste to the presentation of an improved British body distinguished through consumer choices. Tarlow concludes with a review of the main points, findings, and unanswered questions of the book.

Tarlow is adamant throughout the study that improvement was an empowering moral ideology, an end unto itself rather than evidence of latent economic motivations or a desire for power. She is also frustrated by the lack of evidence that speaks directly about improvement across class lines in modern Britain. Although plenty of evidence suggests that poorer people were the subject of improvement, the reaction of these people to being improved is hard to determine.

These issues of ideology and class relate in a way that allows for a productive critique. Tarlow effectively shows that cost outlays for improvement were not necessarily recouped; improved lands, industries, and populations were more symbols of possibility than effective profit-producing forces. However, the basis of this argument places motivation in a simplistic and abstract economic model of outlay and return that misses an important theoretical condition for understanding improvement—competition. As radical social forces undermined traditional church and royal authority, modern society created new opportunities for advancement that re-situated authority in those people with the capacity to combine a control of resources with an ability to contribute to the larger national good; modernity, largely through capitalism, shifted the burden of national and personal well-being increasingly to individuals.

Tarlow could have showed the ideology of improvement to have been widely embraced. Members of British society competed with one another to be regarded as most improved—in other words, those best suited to earn both profits and social distinction, regardless of which came first. Tarlow’s difficulty in finding the working-class response to this ethic of improvement is itself evidence of the broad success of an ideology that tied social and self-improvement to making a living. This criticism, however, is based primarily on the application of Tarlow’s own idea. Her main goal of proposing “improvement” as a working model for approaching the evidence recovered in later historical archaeology is profound and important.

Christopher Matthews
Hofstra University


*Fashionable Acts* amply fulfills its stated objective to explore “the nexus between the opera, elite society, and British political culture from 1780 to 1880.” Hall-Witt argues “that the transformation of audience behav-
ior occurred in large part because of changes in how the upper class conducted itself at the opera as commercializing forces eroded the opera’s subscription system and as elite culture itself was transformed” (3). The book is cleverly organized in two “acts” (complete with “overtures”—introductions to each act—and a “finale”). The first, comprising Chapters 1 through 3, attends to the Georgian period and the second comprising Chapters 4 through 6, to the Victorian era. As an expansion and revision of the author’s dissertation, this dense and detailed study covers an extremely large range of topics related primarily to political, social, and economic history, but it also touches on theater and music history. All in all, it provides a valuable glimpse of society and culture through a lens that focuses on a century-long shift in opera audiences’ make-up and behavior.

The author has drawn from abundant evidence, including primary sources such as archival documents related to subscribers and ticket sales at opera houses; contemporary journalistic criticism and reports; and correspondence, memoirs, and diaries of the era. Given the breadth of the subject matter, it is hardly surprisingly that Hall-Witt often relies heavily (and sometimes indiscriminately) on secondary sources, as attested by the copious endnotes and the frequent text references to the scholarship of others. (The book could have benefited from a bibliography, although the author probably had to defer to the publisher’s norms). That few of Hall-Witt’s secondary sources, which draw from a wide array of disciplines, can be considered “musicological” (or musical) in scope is odd, especially given the ever-growing body of scholarship about British music, opera in England, and, especially, the Victorian musical press that she could have used to tease out deeper meanings in her study. The small proportion of musical studies represented gives one pause in considering the extent and nature of the interdisciplinarity of the methodology.

The importance of this book to those interested in opera spectatorship is beyond doubt. But since the book is not about “opera” or “music” per se, some readers might miss discussion of certain topics. For example, in her discussion of repertory (surely a primary consideration in considering audience), Hall-Witt refers to Cowgill’s excellent work with regard to Mozart, but otherwise she gives relatively little attention to the particular operatic works that were performed.¹

Hall-Witt compares opera audiences in London with those in Paris (because of existing studies about audiences there), but her argument might have been even more compelling had she compared English op-

era audiences with those for other forms of theatrical entertainment in London. Quibbles aside, however, *Fashionable Acts* furnishes valuable information about the social context of opera in London during the eighteenth and nineteenth centuries. It is an important addition to social history that will assist scholars in grasping the complex interactions responsible for transforming the opera experience in a single country during a thriving era.

Roberta Montemorra Marvin  
University of Iowa


Near the end of this fascinating yet frustrating book, Steedman wonders, “[W]ho will notice that this is not just another social history of life, love, labour and religion in the West Riding of Yorkshire, 1786–1806?” (228). Readers will certainly notice. In fact, Steedman never answers the questions that most social historians want answered about her subjects. The information that Steedman provides at the beginning of the book—that Phoebe Beatson, Reverend John Murgatroyd’s servant, became pregnant by George Thorp, who refused to marry her, and that Murgatroyd retained Beatson (and her infant daughter) in his service and left them both an inheritance after his death—does not increase at the end. Steedman never explains why Murgatroyd—whose diaries, notebooks and other papers provide the bulk of the documentary evidence about the situation in his household—did not turn Beatson out or why Thorp refused to marry her in the first place. Did Phoebe love Thorp? Was Beatson embarrassed or ashamed when required to give evidence to the magistate? Steedman does not say. None of the types of documentary evidence that social historians generally employ to fill in the details in such cases—settlement and bastardy examinations, depositions, affiliation orders, etc.—still seem to exist in this case. What kind of social history is this?

This frustrating lack of answers about the Murgatroyd/Beatson/Thorp relationship inspires Steedman to write, instead, a series of essays about the nature of history and historical narrative. The fact that Beatson is silent, existing primarily in Murgatroyd’s writings about her, allows Steedman to ponder the changing nature of the master-servant relationship in itself and as a metaphor for domestic life in general.

That the events in Steedman’s book occurred at the same time, and on the same ground, as those in Edward P. Thompson’s *Making of the English Working Class* (New York, 1964) (without Murgatroyd ever noticing the agitation around him) prompts Steedman to reconsider the iconic status of Thompson’s work. Steedman finds that Thompson’s
foundational myth of English class creation—depending as it does on narratives about Methodism, industrial work, and gender—has drowned out other voices and other experiences. Steedman attends to this issue in a series of deft and nuanced essays that reconsider the content of Enlightenment Anglicanism as social thought, attempting to reintegrate domestic service into the story of class creation and explore the belief systems of ordinary people.

A last essay on *Wuthering Heights*, in which Nelly Dean seems to speak for Beatson, illuminates modern identity formation through the relationship between history and literature. Although Steedman’s essays in this book never quench the desire to know more about the “real” people who inspired it, they contribute significantly to our understanding of the eighteenth century.

Patty Seleski
California State University, San Marcos


France’s Juridictions Consulaires, founded in the sixteenth century, were civil tribunals with local authority over disputes between merchants, including especially suits for nonpayment of debt. Forerunners of the modern Tribunal de Commerce, these merchant courts were staffed not by royal judges but by merchants whom the local guilds elected annually. They prided themselves on rapid and inexpensive justice, basing their rulings as much on common sense and knowledge of prevailing business practices as on the letter of the law. The papers of the Juridiction Consulaire de Paris at the Archives de Paris constitute one of our richest collections of sources for the business history of the old regime. Kessler has written the first book-length study of the Juridiction Consulaire de Paris in nearly a century, and by far the best.

Kessler works at the intersection of legal history, business history, and history of discourse. She seeks to understand not only commercial law as it was lived and practiced in eighteenth-century Paris but also the rhetoric through which merchants justified their place in society, and she has identified a profound shift in that rhetoric. In early modern France, proponents of commerce argued that merchants could be trusted because they were virtuous and selfless, stressing the need for regulatory agencies, including guilds and merchant courts, to maintain the virtue of the business community. The eighteenth-century guilds and merchant courts, however, began to insist instead on the utility of commerce, regardless of the morals of those who practiced it. If Kessler is correct, the sort of laissez-faire economics usually associated with great Enlighten-
ment writers was also a popular movement that penetrated the commercial class. A central paradox of her book is that the very guild leaders who developed this utilitarian ethic also sought to preserve guild privileges. The abolition of the guilds by the French Revolution was the ultimate, but unintended, consequence of their efforts.

Kessler draws heavily on the documentation generated in jurisdictional disputes between the Juridiction Consulaire and the royal courts. The heart of the book, however, is her analysis of hand-written reports filed by the independent arbiters whom the merchant court appointed to research and mediate its more complex cases. Arbiters’ reports do not lend themselves to quantitative analysis, but they offer a wealth of anecdotal information on practices, attitudes, and daily lives. Kessler shows how a wide variety of conflicts typically arose and found resolution, from unpaid wages to disputed marriage contracts. Her most groundbreaking chapter concerns the evolution of business associations, the liquidation of which often led to litigation. Eighteenth-century Parisians experimented with new forms of joint-stock ownership and limited liability that presaged the modern corporation. These novelties are nowhere described in the printed sources of the period. Finally, Kessler challenges much of the legal scholarship about the “law merchant” by showing that commercial law was not free from the edicts and institutions of the French monarchy.

Kessler’s central thesis is that the shift to a new utilitarian rhetoric of commerce was driven by the “rise of negotiability.” As negotiable credit instruments became more common in France, circulating wealth became more impersonal. The health of the economy required merchant courts to maintain the value of commercial paper through formal enforcement of certain obligations instead of through old-fashioned attempts to evaluate the morality of the parties. The “rise of negotiability” is at once the most fruitful and the most problematical concept in the book, since Kessler presents no quantitative evidence that negotiable credit instruments were, in fact, more common under Louis XV than under Louis XIV. Unlike notarized contracts, commercial paper tended to be destroyed after redemption; hence, its quantity in any given period is extremely difficult to estimate. Until further research clarifies this point, the link that Kessler asserts between the rise of negotiability and the shift in merchant discourse remains an exciting hypothesis rather than a fully demonstrated conclusion.

This is a splendid book, both for the quality and extent of Kessler’s original archival research and for the depth and significance of the concepts that she explores. It will appeal to cultural, legal, and business historians alike, as well as to anyone who hopes to understand the commercial bourgeoisie of old regime France.

Thomas M. Luckett
Portland State University

Davidson’s France after Revolution offers an original and important analysis of the role of gender in the establishment of the postrevolutionary social order. The model of separate spheres—a female sphere of home and family and a male sphere of work and politics—emerged from nineteenth-century rhetoric and influenced historians of women and gender throughout the 1980s. For the last fifteen years or so, this model has been called into question by works that demonstrate women’s involvement in the public sphere as consumers, philanthropists, journalists, artists, and writers. Davidson takes the critique of this model further, arguing that it “helps little in understanding male and female roles and experiences” in postrevolutionary France (187). As Davidson demonstrates, women were present in a variety of public arenas during the postrevolutionary period. At the same time, however, the emergence of a consensus that women served primarily as models of virtue, helpers and “ornaments,” resulted in a restriction of their opportunities to participate in public life.

This development was part of a larger process of delineating stricter definitions of class and gender identities during the early nineteenth century. Davidson draws on extensive archival research to examine political festivals, theaters, associations, cafés, and cabarets during the Napoleonic Empire and the Restoration in Lyon and Nantes. She demonstrates that during the Napoleonic period, social gatherings included individuals of both sexes and all classes, in part because public spaces, such as political festivals or the theater, allowed them to see how others performed class and gender identities. As new norms emerged, Davidson argues, the middle classes socialized among themselves; less frequent social contact with the working classes bred fear and distrust. Similarly, although women continued to be present in public gatherings, a more limited understanding of acceptable feminine behaviors changed the extent and nature of their presence.

Davidson emphasizes practices rather than language, but she acknowledges her debt to linguistic analysis in her effort to “analyze . . . the experience of social life in ways similar to the more familiar analysis of linguistic constructs” (9). Texts are therefore the means by which she approaches the question of what people did and how they were described while doing it. In adopting this method, Davidson draws inspiration from the work of de Certeau and Bourdieu to emphasize both individual agency in shaping cultural norms and the symbolic importance of everyday activities.1

Davidson’s approach to the formation and deployment of identity has fully absorbed the implications of both anthropological and sociolog-

Nature, Empire, and Nation: Explorations of the History of Science in the Iberian World. By Jorge Cañizares-Esguerra (Stanford, Stanford University Press, 2006) 230 pp. $60.00 cloth $24.95 paper

Cañizares-Esguerra’s collection of essays is not only, as its subtitle announces, a contribution to the history of science in the Iberian world but also to that of political and economic thought in both Spain and Spanish America. The collection covers a chronological span from the sixteenth to the mid-nineteenth century and ranges across a number of different areas of early modern natural knowledge. Throughout, the author explores views and interpretations of nature from two perspectives—that of metropolitan, imperial priorities and that of proto-national identity, or, to use his preferred term, patriotism among colonial, as well as metropolitan, elites and intelligentsia.

Calling attention to the role of Spanish sixteenth- and eighteenth-century imperial and commercial policies in gathering knowledge, searching for new natural resources, and developing technology, Cañizares-Esguerra argues strongly that the Iberian world deserves a more prominent place in the history of early modern science than it has usually received. In his view, the sources of this neglect lie in a modern historiography of science that long emphasized northern Europe and privileged the exact sciences, as well as in earlier hostile attitudes to Spanish society and religion among northern European writers, first under the influence of the Protestant Reformation and later under the influence of the Enlightenment. He points out that much still remains to be learned about the role of science and technology in the far-flung and long-lived Iberian colonial empires, notwithstanding the attention now paid to such disciplines as early modern botany and natural history.

Metropolitan concerns are treated most fully in the chapter “Eighteenth-Century Spanish Political Economy,” which shows that a series of patriotic authors argued vigorously about causes and remedies of perceived national decline, while reacting strongly against negative assessments coming from abroad. Other chapters trace the development of scientific interests, arguments, and institutions among intellectuals in
the largely autonomous viceroyalties that comprised the Spanish empire. Thus, the chapter “New World, New Stars” shows that seventeenth-century Creole reaction to claims (deriving from ancient theories linking the stars, the climate, and the character of peoples) that astral influences peculiar to the New World enfeebled its inhabitants played a part in the development of theories about a racialized human body.

The chapter “How Derivative was Humboldt” provides a thoughtful re-assessment of the relationship of Alexander von Humboldt’s investigation of bio-distribution in the Andes to earlier accounts of Andean climate and natural history, “a long-standing tradition of thinking about the American viceroyalties as Edenic microcosms” (128). The last chapter, “Landscapes and Identities,” extends the interdisciplinary range of the volume to the history of art in an exploration of the interaction of patriotism, historical narrative, and nature study in the work of Mexican landscape painters during the second half of the nineteenth century.

All of the essays in this book except one were previously published in earlier versions, but as revised and united in this volume they constitute a coherent whole. Therein, Cañizares-Esguerra contributes valuable insights and some incisive historiographical critiques.

Nancy G. Siraisi
Hunter College and the Graduate Center
City University of New York

*Raising an Empire: Children in Early Modern Iberia and Colonial Latin America.* Edited by Ondina E. González and Bianca Premo (Albuquerque, University of New Mexico Press, 2007) 258 pp. $24.95

The contributions to this welcome volume on the history of childhood in early modern Spain and Portugal and their American colonies reveal the inner workings of colonization—of creating, or “raising,” an empire. The authors explore changing concepts of childhood and reconstruct children’s experiences within several nested contexts: the family, the life course, institutions for children, and trends in adults’ expectations of children. Premo observes that this volume also pushes the history of childhood in new directions. Instead of limiting their inquiry to recovering hidden histories of childhood, the contributors present the complex interpersonal and political worlds that children inhabited and link “everyday patterns of growing up” to “traditional historical themes,” evincing an approach that illuminates children’s agency within the systems of power that surrounded and shaped them (244).

Geographically diverse and spanning more than three centuries, the chapters are organized in overlapping chronological order, beginning with sixteenth-century Portugal and ending with a study of child-rearing practices among African slaves in Brazil, where slavery lasted un-
til the late nineteenth century. The authors’ discussion of their sources adds to the strength of the collection. Given that much of their evidence comes from prescriptive texts, they note the difficulty of reconstructing children’s experiences, perspectives, and identities. Yet, with their analytical tools sharpened by questions about race, class, and gender, they mine well-used primary sources and familiar secondary sources for new insights into childhood and colonialism.

These studies explore a fundamental difference between past and present childhoods: In early modern Iberia and Latin America, most children grew up outside their natal homes. Children were moved into other households and institutions as infants to be nursed and raised and as older children to work, according to a practice called child circulation. Exploring the different terms under which children circulated exposes the diversity of their experiences. Isabel dos Guimarães Sá explains that in early modern Portugal, those terms were shaped by distinctions based on parents’ social and marital status and children’s sex. The same distinctions differentiated the asylums for children in eighteenth-century Seville, as described by Valentina Tikoff, and determined the degree of protection and training that children received in those institutions.

In the colonies, ethnicity and racial categories added new social cleavages. Teresa C. Vergara, for example, examines the practice of placing indigenous children and youth in Spanish households as servants and apprentices in seventeenth-century Lima and assesses acculturation as an indigenous strategy for survival in the new social and political order. Ann Twinam examines the consequences of elite resistance in Cuba to the 1794 royal decree that granted all foundlings the same privileges as white subjects of legitimate birth.

Two contributions map the distant poles of colonial childhoods. Jorge Rojas Flores analyzes the spiritual autobiography of a seventeenth-century Chilean nun to show how her recollections of a privileged childhood were shaped by the conventions of the genre but also to illuminate the imaginative space of childhood and her awareness of her position and influence in a complex web of family relationships. At the opposite social pole were the childhoods of African slaves in Brazil, studied by Elizabeth Kuznesof. Her findings challenge the received wisdom that slavery destroyed African culture in Brazil. The importation of slaves from Africa well into the nineteenth century meant that African practices shaped Brazilian slave children’s upbringing.

The volume’s clarity and accessibility belie its sophistication; students and advanced scholars have much to learn from these well-crafted studies.

Ann S. Blum
University of Massachusetts, Boston

The Culture Wars of the Late Renaissance has its origins in Muir’s essay “Why Venice? Venetian Society and the Success of Early Opera,” which sought to find justification for the rise and flourishing of opera in Venice in the mid-seventeenth-century.\(^1\) The activities of the Incogniti, one of the most important academies in seventeenth-century Venice, form the core of Muir’s discussions. The Incogniti have been the focus of numerous historical/literary studies, such as those by Spini, Miato, and Cannizzaro, and musicologists Bianconi and Walker, Rosand, Heller, and Calcagno have written persuasively concerning the importance of the Incogniti to the history of opera.\(^2\) Muir helpfully devotes the first third of his book to Cesare Cremonini, who taught many of the Incogniti, and his environment in Padua, with an emphasis on Cremonini’s philosophical and educational conflicts with the Jesuits. The genesis of The Culture Wars came at a fortunate time, as new studies on Cremonini have emerged recently, with a monograph by Kuhn, and a two-volume set of essays.\(^3\)

In the second chapter, Muir spends considerable time on Ferrante Pallavicino and Antonio Rocco, two of the most outspoken of the Incogniti, as well as on Arcangela Tarabotti, a Benedictine nun and prolific writer, who had ties to the Incogniti and has become the object of numerous studies in recent years. The author also weaves into his arguments the various sexual and societal tensions that surfaced during the seventeenth century as many noblemen forsook marriage vows in the interest of maintaining the family wealth; likewise, large numbers of noblewomen entered convents, many of them more out of a sense of filial devotion rather than religious piety.

Most of the topics that Muir introduces in the first two chapters

---

reemerge in the last. The author keeps the Incogniti, problems associated with thwarted marriage, and the influences of the Jesuits well in focus in his attempt to explain the forces that led to the establishment of opera in Venice. These themes, however, receive too much emphasis in Muir’s explanations for the popularity of opera. Moreover, in focusing on the Incogniti and, for example, Giovanni Busenello’s and Claudio Monteverdi’s masterpiece L’incoronazione di Poppea (1642), Muir neglects the more representative body of works that flourished during the first few decades of public opera. For better or for worse, the majority of operas do not match the particular idiosyncratic brilliance of Poppea. Indeed, although the absence of the Jesuits from Venice during the first half of the seventeenth century may have enabled the publication of numerous libertine tracts (and works such as Poppea), most operas of the time would have been far less offensive to the Jesuits. Conversely, the decades following the reinstatement of the Jesuits saw a rise in the number of risqué productions on the stage despite their presence.

Since Muir bases his arguments on a wide array of secondary literature rather than on his own research of primary documents, it is often difficult to distinguish his ideas from those of the sources that he quotes. Moreover, his reliance on secondary literature, particularly regarding the history and business of opera, leads to numerous generalizations that will not hold up under close scrutiny. For example, no documentation yet confirms his thesis that the patrons and financiers of opera were libertines. Moreover, by pinpointing restricted marriage (and frustrated sexuality) as a boon to the popularity of opera, he ignores the numerous married noblemen who rented boxes and bequeathed them to their wives and children. Muir’s tendency to paint the opera boxes of Venice as filled with the unmarried nobles of Venice and their women friends, though certainly accurate in some cases, neglects the fact that operagoing was often a family affair, as evidenced by household accounts listing an evening’s opera expenses.

The Culture Wars provides a welcome introduction to the cultural world of late sixteenth-century Padua for scholars conversant with the Incogniti but not with their famous teacher. Likewise, Muir’s treatment of the Incogniti offers an entertaining look at this diverse group of personalities, who produced a wealth of literature and criticism. His arguments concerning opera in Venice and its audience, however, should be approached with caution.

Beth L. Glixon
University of Kentucky

4 Muir’s choice of secondary sources is, for the most part, exemplary, but on occasion he fails to mention a significant work such as Anne MacNeil, Music and Women of the Commedia dell’Arte in the Late-Sixteenth Century (New York, 2003), in his discussion of Isabella Andreini, and Jana Byars, “Concubines and Concubinage in Early Modern Venice”—unpub. Ph.D. diss. (Pennsylvania State University, 2006)—in his discussion of restricted patrician marriages.
Laboratory for World Destruction: Germans and Jews in Central Europe. By Robert S. Wistrich (Lincoln, University of Nebraska Press, 2007) 420 pp. $55.00

Is it good or bad for the Jews? To be sure, there are other important questions to be asked in the field of Jewish history, but readers may not find them equally developed in this book. Wistrich analyzes how a group of politicians and intellectuals approached this question in the context of the Habsburg Empire and Germany during the nineteenth and early twentieth centuries. In those specific contexts, the answers to this question changed according to perspective, from that of German speaking anti-Semites to that of Jewish intellectuals and Zionist thinkers. But the Jewish prism (and for some, a Jewish-centered view of the world) seemed to have been central to their global reading of events leading to the catastrophe of the Holocaust. Other significant observers of the time did not ask this question at all. Does the author blame them for this omission? Should he?

In this book, Wistrich is once more concerned with modern anti-Semitism, one of the most significant features of European modernity. The relation between antisemitism and the Holocaust is acknowledged in the title, but, more importantly, this relation is the driving force behind the book’s historical inquiry. With the Holocaust in sight (or shall we say in hindsight?), all actors who ignored antisemitism appear as participants in the “laboratories for world destruction.” Why Adolf Hitler would be included as one of the “researchers” in this destructive laboratory is certainly clear, but why Sigmund Freud should be a member of the destruction lab is not. Wistrich argues that “unlike [Theodor] Herzl, however, Freud could offer no convincing political solution for the human drive to aggression, war violence, and injustice.”

Wistrich maintains that Jews who ignored their fate—that is, the ineluctable fact of their own incoming destruction—unwillingly collaborated with it, whereas Zionist thinkers like Max Nordau and Herzl did not. Moreover, Wistrich stresses the links between Jewish ideas of assimilation and the subsequent inability of their Jewish practitioners to see what was coming—a destruction prophesized and then delivered to them by Hitler. Wistrich argues that “the depth of Jewish assimilation did act as catalysts for antisemites, increasingly obsessed with the need to completely eradicate any Jewish presence in German society and culture” (21).

In this context, Germans and German Jews alike participated in a seemingly “symbiotic” environment in which victimization and integration were tied together. As Saul Friedlander reminds us in his sobering recent history of the Holocaust, Jews who were suddenly empowered with political subjectivity became visible objects of hatred and desire. Whereas Friedlander illustrates his Holocaust narrative with an innovative integration of structural process and private experiences that often belong to “ordinary people” as well as to intellectuals, Wistrich’s book
reads like a classic collection of informed vignettes about famous men (the only exception to this pattern being an interesting chapter on Rosa Luxemburg).  

Wistrich is mainly concerned with German-speaking Jews and non-Jewish German intellectuals. The book’s narrative places them in three categories: Jews with a conscience, Jews with an “assimilationist dogma,” and “Germans.” The author’s preference for the first group is apparent, though he does not neglect the others. Freud, Victor Adler, Karl Lueger, Luxemburg, Nordau, Herzl, Stefan Zweig, Friedrich Nietzsche, and Hitler are presented as inhabitants of a pre-Holocaust world that awaits its destiny.

Federico Finchelstein
The New School for Social Research

The Devil’s Handwriting: Precoloniality and the German Colonial State in Qingdao, Samoa, and Southwest Africa. By George Steinmetz (Chicago, University of Chicago Press, 2007) 640 pp. $90.00 cloth $33.00 paper

Recent years have seen a minor explosion of new work about German colonialism, much of it informed by postcolonial perspectives. In this study, Steinmetz places the histories of three German colonies in “precolonial” contexts. He attempts to explain the radical differences among the “native policies” adopted by German authorities in Southwest Africa (Namibia), Samoa, and Qingdao (the capital of the German navy’s small protectorate in China) as a consequence of ethnographies of the indigenous peoples of those colonies constructed before the full assertion of German colonial authority. It is a remarkable effort, based on a prodigious range of primary research. It should attract widespread engagement from historians of empire and from cultural and social theorists—both for many impressive contributions and for a few intriguing but problematical interpretations. The book has much to offer anyone who is interested in any aspect of colonialism—more than can be discussed in this brief review. One of its questionable aspects does, however, require comment.

As a historical sociologist, Steinmetz aims at re-asserting the significance of social limits to autonomous action and to discursive construction of reality. He advances a theoretical approach that combines a Lacanian analysis of symbolic and imaginary identification with Bourdieu’s concept of a social field in an attempt to define the “colonial state.”1 He describes native policy mainly as a result of projections by German colonial authorities of their own class origins onto the array of

existing ethnographical representations of the particular peoples whom they claimed to rule. The approach is serviceable, but the attempt leads Steinmetz to exaggerate the political autonomy of the three colonial states that he discusses and to underestimate the breadth of the array of factors that impinged on the aims and framed the imaginations of their administrators.

This problem arises in part because Steinmetz’s approach does not seem to recognize “politics” as a field. He thus misses a vital feature of modern colonialism clearly displayed by the German colonies: The field of colonial politics was not co-extensive with the colonial state but incorporated the public politics of the imperial metropole. In two of Steinmetz’s cases, metropolitan interventions shaped by domestic political considerations were decisive. The strongest influence on German policy in Southwest Africa was the way that the colony was portrayed in German domestic politics as a prime location for white settlement. This portrayal had much more to do with ideology in Germany than with the environmental realities of Namibia. The changes in policy and leadership that occurred during the Herero War of 1904/5 were largely informed by the ways in which the German public and the Reichstag viewed the attempt to exterminate the Herero. The shift that Steinmetz describes in Qingdao toward a native policy respectful of Chinese culture depended heavily on Alfred von Tirpitz’s decision to use Qingdao as a showplace for the efficiency, modernity, and social and cultural broadmindedness of the German Navy. His imagined target was a public opinion that cut radically across German class lines.

Steinmetz’s interpretation works best for Samoa: Even there, however, the success of Governor Wilhelm Solf’s policy of protecting “traditional” Samoan culture (and of projecting himself as a person able to govern traditionally because of cultural understanding gained as an educated bourgeois) depended on Samoa’s relative insignificance in German public opinion after 1900 and Solf’s skill at presenting his image positively in Germany.

Steinmetz’s general approach could be usefully applied to the whole field of colonial politics, even though Steinmetz does not do so. It could certainly help to establish the relationships between imperialist ideologies and colonial interests. Both economic and ethnographic imaginaries were at work in shaping colonialism, and in Germany they were vastly more significant than the measurable economic impact of the colonies. Historians investigating these subjects in the future will find much that is helpful and suggestive in this book.

Woodruff D. Smith
University of Massachusetts, Boston

Consistently grounded in the kind of careful archival research that has traditionally been the coin of the realm for historians, Zatlin’s study of the nature and consequences of monetary policy in the German Democratic Republic (GDR) combines a variety of approaches. In this volume, the political historian’s concern for statecraft, policy, and individual leadership meets the social historian’s desire to uncover the experience and agency of ordinary citizens and the cultural historian’s interest in popular values, shared expressive practices, and common meanings. The book evinces an attention to the hard-and-cold realities of economics— institutions, policy, tools, and measures—that, unfashionable as it may be, is essential for an understanding of what happened to state socialism, especially in East Germany. The investigation is unusual in today’s historiographical mainstream for its sensitivity to economic theory and data and its sophistication in combining economic and historical insights. Zatlin manages this blending of techniques and interests skillfully; that the work sustains its analytical coherence throughout is a genuine accomplishment, given the breadth and variety of its subjects.

Set forth clearly and convincingly, the book’s chief findings represent important contributions not just to German history (developments in the Federal Republic receive thoughtful treatment herein, too) but also to the multidisciplinary study of socialism and to economic history more generally. Following an incisive review of Marxist political theory regarding the implications of market exchange, Zatlin shows how the stewards of the East German economy sought, without much success, to establish practices that would reflect their ideologically grounded suspicion of money. Insofar as possible, they hoped to eliminate it, or at least to diminish its importance using the tools of planning and distribution policy, and thereby to subvert the fundamentally conflict-ridden, alienating, “zero sum” logic of capitalist exchange. In its stead, they hoped to offer an alternative that would re-establish the relationship between producers and consumers on a new, nonmonetary basis that promised to advance the political and social ends of Marxism—to be sure, a tall order. The devil was in the details, and Zatlin explains, with prose at once engaging and rigorous, just how bedeviled those details were from the beginning of communist rule in the East, focusing on the period from the ascension of Erich Honecker in 1971 until the collapse and dissolution of the GDR in 1989/90.

The Currency of Socialism makes its case by demonstrating, throughout the two main divisions of the book dealing with money’s role in production and in consumption, what the Ostmark was supposed to accomplish and how it failed. Zatlin shows how the Honecker government, like the previous administration under Walter Ulbricht, struggled to make financial and commercial practice conform to what remained a
consistently distrustful vision of money in a socialist society. These efforts rested on a faith in the state’s ability to determine and fulfill the needs of the population. Money was to become “a purely notational device” in facilitating the provision of goods and services, nothing more than “a marker balancing production against consumption” (22). Policy initiatives (and subsequent policy correctives) led to a spiral of complications that could barely be contained. In short order, the disengagement of the East German currency from market signals, from the true costs of production, and from consumers’ actual desires (as opposed to their state-sanctioned “real” needs) resulted in unforeseen, unwanted, and perverse outcomes: unsustainable and irrational spending and investment, large trade imbalances, burdensome debts to the capitalist world, accounting chicanery, looming insolvency, and commercial policies bent on earning hard currency despite serious social costs. Critically, the West German Deutsche Mark was accommodated as an all-too-common alternative currency for use in a variety of transactions for which the “notational” Ostmark was found to lack sufficient value.

Zatlin’s treatment of East German money as an instrument of consumption shows, in rich detail, the painful outcomes of the communists’ theory and practice: serious shortages, flourishing black markets, muddled and corrupted allocation systems (seen vividly in a chapter about the Trabant automobile), and a crippling dependency on an expanding Westmark sector. These results attested to the ways in which the GDR was “mortgaging its future to the West—eating oranges now and worrying about paying for them later” (78). Worse yet, they showed how even that effort to live on borrowed, illusory wealth had failed. These disappointments, Zatlin argues, discredited the regime in the eyes of its citizens and created deep reservoirs of ill will that destabilized the polity, contributed to the collapse of communist rule, and ultimately made union with the Federal Republic and the adoption of its prized hard currency all the more attractive to East Germans, who were inadequately prepared for the financial dislocations that would follow. Written judiciously and with considerable sympathy for the humanistic aims of Marxist thought, The Currency of Socialism is, nevertheless, as damning as it is instructive. It is an indispensable contribution to the economic, political, and historical study of the state-socialist enterprise.

Patrick Hyder Patterson
University of California, San Diego

Epidemics and Enslavement: Biological Catastrophe in the Native Southeast, 1492–1715. By Paul Kelton (Lincoln, University of Nebraska Press, 2007) 288 pp. $50.00

Kelton’s skillful weaving together of archaeology, epidemiology, historical demography, and economic history, both illustrates the power of interdisciplinary history and provides a fresh interpretation of the native
experience with European invaders in what would become the southeastern United States. Historians, Kelton notes, shy away from epidemics because of their apparent accidental nature. “Epidemics after all seem out of human control, whereas other historical events such as wars and revolutions can be seen as the consequences of the decisions and actions, either collectively or individually . . . of human actors” (121). Although there is no evidence that Europeans deliberately tried to spread diseases to the region’s indigenous peoples during the colonial era, the impact of diseases on natives was not merely the accidental result of Europeans and Africans arriving among a people who had no prior experience with the Atlantic World’s deadliest diseases. “Instead, the larger aspects of colonialism shaped the impact that epidemics had on indigenous peoples” (121).

In the Southeast, the English slave trade, which integrated Native Americans into the Atlantic economy, occasioned the Great Southeastern Smallpox epidemic, which sharply reduced native populations. Ironically, the slave trade destroyed itself. Sharp reductions in numbers reduced supplies, which further declined as small bands joined together to form more powerful and thus less vulnerable confederacies. Finally, the combined impact of the slave trade and epidemic disease led to the native revolt known as the Yamasee War, which persuaded planter-legislators of the dangers entailed in continuing an unregulated trade in native people.

Although Kelton’s work is focused primarily on the Southeast, it has implications for our understanding of the Indian experience with the diseases of the Atlantic world throughout the Americas. Students of other regions would do well to follow his lead in exploring the interaction between epidemic disease and the nonbiological aspects of colonialism. Unfair as it may be to criticize so fine a work, Kelton fails to exploit fully the limited quantitative evidence that is available to estimate either the size of the Southeastern slave trade or the number of Indians enslaved in the region.

Russell R. Menard
University of Minnesota

“Let a Common Interest Bind Us Together”: Associations, Partisanship, and Culture in Philadelphia, 1775–1840. By Albrecht Koschnik (Charlottesville, University of Virginia Press, 2007) 368 pp. $45.00

Koschnik’s “Let a Common Interest Bind Us Together” aims to use Philadelphia’s “rise and fall of partisan parties and militia companies” to trace “the development of association and partisanship,” as these forces shaped the emergence of “a new civic culture, from the American Revolution into the 1830s.” Koschnik argues that this new civic behavior developed out of a “generation of Federalists” who aimed to “divert their aspirations from partisan politics into the creation of high culture”(7).
Through this lens, Koschnik promises to highlight what he views as an under-represented theme in the scholarly discourse about early American life.

By and large, Koschnik makes good on his promise. In five carefully crafted and sturdily documented chapters, he explores issues of class and influence, age and power, and emerging definitions of “public service,” gliding across personalities, civic institutions, and the interplay between these variables to create what he rightly calls a “broad definition of . . . partisan behavior in the early republic” (153). In the process, he asks such seemingly obvious—but seldom-probed—questions as, “Is there a role for public servants and government beyond proclaiming laws to keep public order?” (3).

Koschnik argues that Federalists and Republicans answered that question differently. The Federalists—an economically and socially conservative minority—increasingly saw public service as the province of “common interest” associations designed to create a high culture of art and letters that would distinguish “manly,” “useful” professionals from lesser people.

Building his case from a variety of evidence—newspapers, association membership lists, church records, city directories, legal documents, broadsides, and other graphic materials—Koschnik maintains that, in the decades following the War of 1812, a select few Philadelphians intertwined politics, culture, and personal biography to design a definition of public responsibility that resulted in such institutions as the Athenaeum, the Law Library Company, the Pennsylvania Academy of Fine Arts, and the Historical Society of Pennsylvania, each intent on showing its members in their best light. On the basis of this evidence, he self-consciously does what he faults Alexis de Tocqueville in Democracy in America (London, 1835) for not doing: He moves the story through time, instead of reporting a narrative based on the freeze-frame of a short visit. Unlike de Tocqueville, Koschnik examines the social and political dynamics that preceded, and followed, the realities of 1832, to reach a more nuanced and provocative conclusion than could the Frenchman during his short visit.

By delving into the development of modern print culture and exploring the changing landscape of public association from the Society of Political Inquiry and the Whig Society to the First City Cavalry Troop and the Washington Benevolent Society, Koschnik far outstrips de Tocqueville’s one-dimensional view of American social life, which has dominated the narrative for more than seventeen decades. Along the way, Koschnik offers a provocative view of the Whiskey Rebellion and its meaning to Philadelphians who, defining their loyalty to the republic, and to public order, described the rebels as “anarchists” (108).

This passing reference to anarchists, however, is one of only a few references to those outside the charmed circle of manly professionals. Except for a brief discussion of how men defined women from their role as “citizen-soldiers,” Koschnik’s narrative has little to say about women.
It also ignores the variety of “others” against which those with “common bonds” defined themselves. There are no Jewish, Catholic, or African-American threads to Koschnik’s narrative, though each of these groups developed comparable institutions in this period. Even Quaker culture is given only cursory treatment, though Koschnik offers, in his concluding paragraph, a rationale for this perspective.

But no one scholar can be expected to do everything, and Kaschnik has succeeded admirably in grinding new lenses through which future researchers can view questions of ethnicity and race, culture, religion, and gender as they inform the building blocks of voluntarism and civic notions of civic responsibility in the early republic.

Emma Lapsansky-Werner
Haverford College

The Politics of War: Race, Class, and Conflict in Revolutionary Virginia. By Michael A. McDonnell (Chapel Hill, University of North Carolina Press, 2007) 544 pp. $45.00

In his wonderful Politics of War, McDonnell uses the difficulties that Virginia faced in fielding an army during the War of Independence as a way to probe the internal conflict about the direction and meaning of the American Revolution. This sweeping book about the social, political, and military dimensions of war presents a complex tapestry of civil—military clashes, shifting loyalties, slave and Indian uprisings, battlefield exploits, and legislative combat. McDonnell’s primary concern, however, centers on how Virginia’s method of recruiting soldiers upset the balance of power between the gentry, middling folk, and the lower sort. Although slaves play an important role in the story, McDonnell is interested chiefly in the class struggles among whites regarding who was going to fight the war and who was going to foot the bill.

The bulk of the action unfolds in a chronological narrative that traces Virginia’s mobilization for war from the initial enlistments in 1775 to its attempts to keep the army together until peace could be secured in 1783. McDonnell’s impressive research allows him to give a fully rounded story of the multifaceted conflict within Virginia. He moves deftly from contentious backcountry militia musters to Williamsburg and Richmond, where state legislators debated whether to accede to popular demands—among other things, an end to the draft and of the gentry’s exemption from service, increased militia pay, and progressive taxation—or to close ranks and impose the gentry’s will.

McDonnell creates vivid portraits—whether it be Lord Dunmore aboard a ship on the James River pondering freedom for Virginia slaves or a company of runaway slaves defending Great Bridge on the Elizabeth River near Norfolk for the British. He is equally adept at depicting small farmsteads, where draftees weighed the penalties of desertion
against the hardships of serving, which would mire their families in debt and privation, and George Washington’s Mount Vernon plantation, where the general’s caretaker cousin fretted about upstart plebian neighbors, slaves, and indentured servants. Across these landscapes, McDonnell reveals the ups and downs of waging war and, especially, the troubles that the gentry encountered when ordinary black and white Virginians refused to play their assigned docile roles.

Some of the near-constant recruiting problems were the product of a long, chaotic war, in which the home front was also a battlefield. Other troubles stemmed from the popular belief that the gentry was administering the war in elitist, antidemocratic, and self-interested ways. Predictably, the revolutionary government had a harder time mustering soldiers as the war continued. Farmers worried about leaving fields unattended and families exposed to attack from British regulars, Indians, and enslaved Virginians; governments and individuals endured financial setbacks as taxes mounted and currencies depreciated; enlistments plummeted when soldiers were expected to fight far away or when British armies invaded the state. McDonnell imbues even familiar conditions and events with fresh urgency by showing how they wreaked havoc on the lives of ordinary Virginians and created agonizing decisions about whether to serve or resist.

McDonnell’s most original arguments come in his discussion of how the gentry’s elitism often undermined recruitment efforts. He shows how the Virginia elite—many of whom had supported independence largely to reassert control over the common people—stifled popular enthusiasm for the war by trying to transform democratically run militia companies into a hierarchical, gentry-controlled, strictly disciplined corps of minutemen. Popular sentiment also waned when the gentry enacted regressive taxes to fund the war and exempted slaveholders from serving (which caused many others to balk at fighting in and paying for a war to defend the property of rich slaveholders who refused to sacrifice anything for the cause).

The gentry also proved tone deaf to the rhythms of Virginia’s agrarian society. Soldiers complained about long training musters and tours of duty that required considerable travel at the height of planting and harvest; they were outraged by new taxes imposed in January, when farmers had few ways of making the money needed to pay. In some of the best sections of the book, McDonnell shows how ordinary Virginians protested when the Revolutionary elite refused to acknowledge their complaints: They resisted the draft and halted tax collection, property foreclosure, and any other measure that violated their sense of right and wrong.

Ultimately, those who protested were able to wrest many concessions—greater political rights, progressive taxation, and high recruiting bounties, even the possibility of obtaining a slave. Many slaves obtained freedom when desperate recruiters let them enlist. Most of these concession came directly at the expense of a gentry forced to cede
some degree of political control and to open its purse to pay a consid-
erable share of the cost of war. Sometimes middling people managed
to shift burdens from themselves both upward to the gentry and down-
ward to the lower sort—such as when landowning yeomen, struggling
to keep their farms solvent, eventually were able to induce the gentry to
assume most of the war’s cost and landless men to do most of the
fighting.

McDonnell also reveals that many of these gains proved temporary.
Most runaway slaves were returned to bondage. High wartime bounties
eroded amid rampant inflation, leaving most of the Virginians who had
served the longest with little to show for their efforts. The lower-class
men who fought the war tended to remain propertyless, many of them
fleeing to Kentucky, where, more often than not, they failed to achieve
their dreams of landed independence. Even middling farmers found
their ideas of the Revolution scaled back when the gentry, deeply frus-
trated by wartime and postwar concessions, formed a new national gov-
ernment to advance its economic interests and curtail popular power.

Terry Bouton
University of Maryland, Baltimore County

Children at Play: An American History. By Howard P. Chudacoff (New
York, New York University Press, 2007) 288 pp. $27.95

In Children at Play, Chudacoff provides a short but sweeping account of
changes in the “playful behavior” of American preadolescents (six to
twelve years of age) from the colonial period to the present (25). He
identifies three key turning points in the play patterns of a wide range of
American children—black, white, Native Americans, rural, urban, sub-
urban, rich, and poor.

Between the American Revolution and the 1850s, children’s games
became more structured and supervised as play became a central ingredi-
ent for the nurturance and education of republican children. From 1900
to 1950, professional efforts to control children’s informal play climaxed,
as did children’s efforts to resist such supervision. Since the middle of the
last century, independent play has atrophied, overwhelmed by parental
anxiety and the manipulations of toy manufacturers and distributors. As
a result, contemporary play patterns differ markedly from those in the
early colonial period. Children are more likely to play structured games
with school peers and friends in formal play spaces using mass-produced
toys than they are to play pick-me-up games with family and neighbors
in city streets and woods using home-made toys.

Chudacoff’s argument that children’s opportunities to play inde-
pendently have diminished over time is fairly intuitive and dovetails
with other work on childhood, such as Steven Mintz’s Huck’s Raft: A
History of American Childhood (Cambridge, Mass., 2004). But Chudacoff’s
work stands out in terms of its methodology. Rather than describing children’s play from an adult perspective, Chudacoff seeks to uncover their play-based culture from their own perspective. His analytical approach—studying changes in the places where children have played, the objects that they have used for play, and the playmates that they have selected over time—is novel. However, the sources that Chudacoff uses to draw such conclusions, including autobiographies, child-rearing guide books, children’s literature, and Work Projects Administration interviews, tend to capture adult memories of childhood as much as, if not more than, children’s own experiences. Thus, at times, they limit his ability to pursue a child-centered perspective.

In addition, Chudacoff’s decision to divide U.S. history into five time periods (the first being 1600 to 1800 and the subsequent ones in fifty-year increments) may be heuristically practical but not always historically sensitive. For instance, he suggests that toy manufacturers began to market their wares directly to children in the 1950s. However, advertisements instructing children about how to convince adults to purchase goods for them appeared in American magazines and radio shows almost half a century earlier.¹

Finally, Chudacoff’s criticisms of modern parenting are less nuanced than his description of changes in children’s play. Instead of dismissing their concerns and weighing them equally with the manipulations of mass marketers, Chudacoff’s work would benefit from a clearer study of the complex forces that have motivated contemporary parents’ anxiety as well as a more subtle reading of the marketing strategies of toy companies.

Sharon Ann Musher
Richard Stockton College of New Jersey

The Supreme Court: An Essential History. By Peter Charles Hoffer, William James Hull Hoffer, and N. E. H. Hull (Lawrence, University Press of Kansas, 2007) 491 pp. $34.95

To cover in one volume the history of the U.S. Supreme Court, an institution that has produced a mountain of source data and inspired an intimidating scholarly literature, is a formidable task. This book is exactly what the title states it to be, placing the Court’s noteworthy opinions within their institutional, social, and political contexts. Its treatment of a broad number of themes, including race and gender, is laudable. That it does so elegantly is a testament to how well the authors—a family of scholars—work together.

The authors’ approach is thoroughly historical, informed by neither judicial decision-making theories nor statistical analysis. The narrative

arc is empirical, assessing the Court’s decisions in relation to the other political branches of government and the larger society that it served. Taking a cue from Ackerman, the authors divide the Court’s history into three periods: the Heroic Courts (through 1873), the Classical Courts (through 1941), and the Modern Courts (through 2005).

Individual chapters cover the tenures of chief justices, and each chapter begins with miniature biographies of the justices added during the tenure in question. If not the most creative way to organize this material, it has the supreme virtue of being both consistent and predictable. Given the brevity of the book, the number of legal topics covered is breathtaking. Whether dealing with advances in private or public law, or statutory interpretation or judicial review, the authors manage a sophisticated and thorough review of Supreme Court jurisprudence. At appropriate junctures, they provide explanations for legal terms that will aid nonspecialists (or aspiring specialists) in understanding the material.

The bulk of the book focuses on jurisprudence, but the narrative, contextual approach saves the book from myopic internalism. For example, confronting the dismal record of Chief Justice Morrison Waite’s Court in enforcing the Civil War Amendments to protect freedpeople in the South, the authors conclude that the Supreme Court “played its part” in a Republican retreat from civil rights enforcement that gave the South back to white supremacist Democrats after the 1876 election (138). In clear and concise language, the book covers the political context of race cases in which the Supreme Court narrowly interpreted the Fourteenth Amendment to restrict Congressional power to protect blacks in the South from violence both public and private. The authors revisit these cases in the section on Brown v. Board of Education, offering both an understanding of seismic shifts in the Court’s jurisprudence and a sense of continuity within the book itself.

Certain of their interpretive choices will raise complaints. Despite the revisionist assault, Marbury v. Madison receives top billing in this book as the case that established judicial supremacy (54–55). Although the authors contextualize this argument, demonstrating that John Marshall was able to assert this awesome power only because he avoided direct confrontation with President Jefferson and his secretary of state James Madison, they leave the question of how judicial supremacy was both established by the Court and accepted by the nation unanswered. To conflate Marshall’s limited claims for judicial review in Marbury with a robust (and modern) theory of judicial supremacy is more than a little Whiggish—perhaps even self-consciously so, since the authors admit in their preface that they “value history precisely because it speaks to our [present] concerns” (viii). As their rendering of Marbury makes appar-

1 Bruce Ackerman, We the People. I. Foundations (Cambridge, Mass., 1991).
ent, however, presentism has its own pitfalls. It can obscure the complex past by paving too neat a road from landmark to landmark.

This book deserves a large audience, from novices seeking to acquaint themselves with the Supreme Court’s history to legal scholars who want a one-volume reference book in their libraries. The helpful bibliographical essay is as elegantly written as the main text of the book.

H. Robert Baker
Georgia State University

Swing the Sickle for the Harvest Is Ripe: Gender and Slavery in Antebellum Georgia. By Daina Ramey Berry (Urbana, University of Illinois Press, 2007) 224 pp. $40.00

Berry has written a fascinating but slightly disjointed book that touches on several historiographical themes and utilizes a range of methodological techniques. Yet, ultimately, her book is no more than a fine case study; much of its findings will be familiar to those interested in the history of gender, labor, and familial relations under slavery.

Berry seeks to compare and contrast the lives of the enslaved in two counties of Georgia—Wilkes in the upcountry and Glyn in the lowcountry. Her comparative approach highlights well the variety of life experiences during bondage within the state, though many of her conclusions run parallel to those of other scholars who have compared tidewater areas with Piedmont areas. Berry employs an impressive range of quantitative and qualitative evidence, adding depth to our understanding of slavery at both a micro- and macrolevel. She illustrates, for example, the changing slave populations of both counties, and, more specifically, rates of hiring out among individual owners in tabular form. Berry also explores, in great detail, various “micro-studies” of slave owners and their plantations.

The book’s main themes are gender, labor, family, community, and economy, some of which she explores in greater depth than others. The strongest contributions of her book are easily identified—her nuanced consideration of “skilled” labor and her analysis of enslaved “breeding.” Berry argues persuasively that skill was not reserved exclusively for men or for domestic servants. Offering a flexible definition of the term as “doing something well,” she shows how skilled agricultural work crossed gender lines regardless of the type of labor system employed or crop grown.

Berry asserts that breeding was a method used by slaveholders to force slaves into partnerships or multiple relationships without their consent. This argument is controversial, as Berry herself points out in the endnotes. It would have been more convincing if she had included some sense of how prevalent this form of coercion was. She provides a few examples of it (some of them outside Georgia), but she is mute about its scale of occurrence.
Other areas of Berry’s research are more problematical. Certain sections within her chapters are short and/or descriptive, not contributing much to her overall arguments. For example, under the subheading “resistance,” she explores only runaway slaves, considering other forms of resistance elsewhere. She raises questions about the motivations of slaves who married but ventures no explanatory hypotheses. More detailed comparisons between the two counties, as well as between Georgia and other slaveholding regions, would have been beneficial. In general, quantifiable data might have helped to substantiate her arguments. As it stands, some of Berry’s interesting points read like unqualified assertions; they deserve better.

Ultimately, Berry’s book adds a great deal to our understanding of the variety and complexity of slave life and makes a valuable contribution to scholarship both about Georgia and the antebellum South as a whole.

Emily West
University of Reading

Coastal Encounters: The Transformation of the Gulf South in the Eighteenth Century. Edited by Richmond F. Brown (Lincoln, University of Nebraska Press, 2007) 313 pp. $24.95

The eleven articles in this volume do not center on a unified theme; instead, they highlight the diversity of racial, ethnic, cultural, and economic contacts that characterized the eighteenth-century colonial Gulf South during a time of rapid change. In the afterword, Ida Altman sums up this broad-based approach by concluding that “strikingly diverse societies” existed concurrently in the region.

The various contributions provide useful examples of current historiographical approaches to, and viewpoints about, the region. Topics include Native Americans and their relationship with Europeans, African Americans, women, exchanges among the groups living along the coast, economic development, and political loyalties. The essays primarily cover, in one fashion or another, a geographical area encompassing Florida through Louisiana to Texas from the late eighteenth to the early nineteenth century. All of the articles flow methodologically from the varieties of historical analysis that have emerged from the application of “world systems theory,” employing an analysis of “centers and peripheries,” of overlapping physical and mental spaces, and of the “contested grounds” held by competing groups of people both temporally and culturally.

The volume opens with a well-crafted, informative survey of the historical literature of the late eighteenth-century Gulf Coast. Four of the subsequent articles deal with aspects of Native American society, especially cultural contact, trade relations, identity, and community. Three
additional articles explore colonial Louisiana, including social customs and deviancy during the French era, patterns of property holding in New Orleans, and the tobacco trade in Natchitoches during the Spanish period. An additional article deals with cattle raising and ranching along the lower Rio Grande Valley. The final article examines motivations for the Anglo-American revolt in West Florida.

All of the contributions are well written and meticulously researched. Refreshingly, a helpful and thorough bibliography includes every item cited in each of the essays.

Light Townsend Cummins
Austin College

_Becoming Free in the Cotton South_. Susan Eva O’Donovan (Cambridge, Mass., Harvard University Press, 2007) 364 pp. $35.00

O’Donovan’s study of the transition from slavery to freedom unevenly covers a stretch of eighteen counties in Georgia’s southwestern corner. She disclaims her work as a conventional regional history, preferring to call it a “socio-ecological” reconstruction, by which she seems to mean a study that delves into multilevel interactions between human and environmental systems.

During the late antebellum period, slave gangs on short-staple cotton plantations transformed this understudied corner of Georgia into a southern black belt. In 1820, it had only a few hundred slaves; in 1860, it had more than 60,000, reaching 54 percent of the total population. A tiny fraction of free people of color inhabited the region; four of the eighteen counties in 1860 had one or none. The fall of the Confederacy wrought a new social order: In 1870, 60,000 whites confronted 90,000 freed persons about their civil rights. For O’Donovan’s purposes, the region offers a convenient compression of developmental stages in representative processes, although at times, her intellectual peregrinations might have been better served by the “analytical clutter” that she sought to avoid.

The first of five chapters focuses on the building of a slave society and the attendant rebuilding of slave lives and families broken by forced internal migration. O’Donovan’s evidence fails to sustain the rise of “an ominous new variant of slavery in southwest Georgia” but does speak to a familiar regimen imposed on slaves by intrusive, market-responsive, and profit-conscious planters determined to discipline both their laborers and an unforgiving frontier. Slave life and labor remained relatively untouched in southwestern Georgia until the last years of the Civil War. Refugee planters from eastern Georgia streamed into this “land of Goshen,” although, as O’Donovan points out in Chapter 2, the gradual shifting of the balance of forces in favor of the Union led Confederate
officials, as well as increasingly uppity slaves, to whittle away at the masters’ dominion over their private property.

Chapter 3 underscores the hollowness of black freedom without a material underpinning. Freedmen and freedwomen aspired to own land, constitute and reconstitute families, educate their children, and worship in independent churches. In tense, conflicted negotiations with their former masters in the immediate aftermath of slavery, freed persons quickly learned how to use the law to their advantage. With a vanished dream of forty acres and a mule, freedmen tended to reach labor agreements for themselves and their families that “edged their old masters out of their lives” (138).

O’Donovan proves particularly sensitive to issues of gender throughout the book. In Chapter 4, she examines the “domestication of free labor” (162), describing the nature of freedwomen’s work as farmhands, midwives, seamstresses, spinners, dressmakers, and laundresses. Freedwomen generally accepted the condition of coverture in which wives were bound by the legal authority of their husbands. But, as O’Donovan argues, freedwomen exerted pressure on freedmen to effect labor arrangements that would employ members of families on favorable terms.

The final chapter presages the rise of Jim Crow in the state. Night riders and paramilitary organizations intensified their attacks in 1866. Blacks responded by associating in self-defense. O’Donovan concludes with a sketch of the infamous “Camilla Riot” of 1868 in which white Democrats fired upon an armed crowd of predominantly black Republicans, killing and wounding fifty of them who were attempting to hold a political rally in the Mitchell county seat.

Despite a weakness for sentimental pronouncements over measured judgments, O’Donovan has written a readable, intelligent book. She succeeds in finding new angles to look at a revolutionary process by peering through a small window.

Robert L. Paquette
Hamilton College

Joining Places: Slave Neighborhoods in the Old South. By Anthony G. Kaye (Chapel Hill, University of North Carolina Press, 2007) 376 pp. $34.95

Kaye’s new book is based on path-breaking research that accomplishes something unthinkable at this late date: It excavates a too-rarely used, massive set of sources that reports new words from ex-slaves speaking about their experiences before emancipation. Kaye also essays a new interpretation of the nature of enslaved African Americans’ community and culture. Perhaps some readers will ultimately find his conclusions unconvincing; others might find Kaye persuasive. Yet, on the whole, his
book is a rewarding, even exciting contribution to the scholarship of slavery and African-American history.

In his investigation of enslaved people’s lives and community in the Natchez District of Mississippi, Kaye pioneers the use of the pension files of African-American Civil War veterans to study slavery. Historians have been daunted by this source’s massive size; the index alone runs to 2,800 reels of microfilm. What Kaye does with the applications for federal pensions is both clever and inspiring. Since veterans or their widows could apply, and since marriages in slavery could only be substantiated in retrospect by the testimony of people who recognized and remembered those marriages, pension seekers marshaled witnesses who had known them during slavery. From the patient sifting of their affidavits and depositions emerges a portrait of how enslaved people lived—their markers and milestones, children, break-ups, courtships and forced departures. Supplemented by Kaye’s thorough survey of extant planter manuscript sources, Joining Places presents a detailed, breathing portrait of slavery in the Natchez District, one that sometimes is shocking in its living tints.

Kaye also intervenes in a long-running argument about slave community and culture. He insists that studies of resistance and community have introduced an “anachronistic liberal framework” to the study of slavery, positing a “liberal” slave and then assigning him/her “autonomy.” Yet he also maintains that historians want to convince themselves and their audience that a form of racial consciousness served as the bedrock of slaves’ identity. Liberalism, with its focus on the autonomous individual, seems a different sort of ideology from the racialized group identity posited by the scholars of slave “community.” If this circle can be squared, Kaye does not quite accomplish it here.

Kaye proposes instead to introduce a new paradigm for understanding the nature of slaves’ ideas and deeply structured beliefs—their *habitus* (my term; Kaye leans on Anthony Giddens’ ideas about “structuration”). Specifically, he offers the concept that he first introduced in *Slavery and Abolition*, the “neighborhood.” Slaves, he argues, conceived of their world at the “neighborhood” level, viewing all those unfamiliar—even if they were also slaves—as outsiders. This concept gives us much to chew on, for it is made from the meat of ex-slaves’ own words. But Kaye provides tough chunks of gristle, too. Eventually, in Kaye’s account, “the neighborhood” starts to decide and to do things by itself. Explaining too much, he explains too little. Still, the point that slaves, like all people, constructed concentric circles of community (family, kin, individual quarter, and neighborhood) that shaped their

---

lives as they crisscrossed their borders is well taken. Many will find Kaye’s arguments challenging, and all who study slavery in North America need to read this important new work.

Edward E. Baptist
Cornell University

*Slavery and American Economic Development.* By Gavin Wright (Baton Rouge, Louisiana State University Press, 2006) 162 pp. $25.00

Wright’s *Slavery and American Economic Development* is a small book with a big interpretive punch. It is one of those rare books about a familiar subject that manages to seem fresh and new. Wright returns to first causes in measuring the impact of human bondage on the economic history of antebellum America: Slaves were, first and foremost, legal property.

Wright’s basic point is that almost everything responsible for the Old South’s economy sprang from what he terms the masters’ “property rights” in their slaves (ix). Other recent historians, most notably students of the domestic slave trade, have referred to this set of legal concepts as the “chattel principle.”¹ According to Wright, “These legal aspects of slavery and their economic implications are where we should look in trying to understand the place of slavery in American economic development” (7).

How so? Wright makes the persuasive argument that the slaves’ status as human chattel had profound economic consequences. It was much more determinative than the efficiencies of scale and organization that masters supposedly achieved by gang labor—as Fogel argued.² Planters, by and large, could direct their slave workers however they saw fit; they determined crop choice, work regimen, and hours in the field, despite the slaves’ best efforts to demand some control in such matters. Masters could also increase their workforce during the critical planting and harvesting seasons by forcing women and older children into the fields. Slave women often picked more cotton than slave men.

Equally important, slave owners could use their human chattel as rock-solid collateral for the credit needed to sustain and expand their operations and to open up rich southwestern areas for staple-crop production. Echoing a point forcefully made by Berlin recently, Wright notes, “Distasteful as it may seem to modern readers, slave economics functioned through elaborate legal and financial channels, as fully devel-

oped and in some ways more fully developed than their counterparts in the free-labor states” (12). The Cotton Kingdom spread west with extraordinary speed. Migrating slave owners, whom Wright aptly labels “pioneers with means,” could avoid shortages of labor and capital that might have delayed or even prevented them from riding the wave of prosperity created by the burgeoning worldwide demand for cotton during the decades before the Civil War (68).

Wright’s comparison of the economic development of the free-labor North with that of the slave-labor South arrives at some striking conclusions. For one thing, southerners “held their wealth mainly in the form of human property rather than land values” (61). He might have added that this condition had a direct bearing on the Deep South’s embrace of secession in 1860–61. Indeed, “slaveowners were justified in feeling that they were fully as successful as their northern counterparts in the game of wealth accumulation, if not more so,” but their success came at a heavy price (61). “The South also lagged the North by such criteria as urbanization, banking facilities, and transportation improvements” (62). Nor did southerners feel the need “to attract and retain a labor force through positive economic and political incentives” (126). Why build schools and towns to draw immigrants when “property rights in slaves” made such investments unnecessary? The roots of southern postwar “backwardness” were thus planted “firmly in the antebellum era” (124).

Wright, as always, has given students of American economic history a great deal to ponder. “Property rights in slaves” is a concept that historians of the South will ignore henceforth at their peril.

Charles B. Dew
Williams College


Historians seem obsessed with collective memory. Showing the influence of the latest work on the subject while operating within the traditional format of a state study, the eleven contributors to *Lone Star Pasts* treat memory as, first, socially constructed and, second, as contested. A few of the chapters explore conflicts about memory, but the editors reinforce the phenomenon by including discussions of alternative memories developed within the African-American and Tejano communities. All of the essays assume that groups construct memories more from their needs in the present than based on the realities of the past—a third generally accepted characteristic of memory.

In the volume’s concluding essay, Randolph B. Campbell argues that three events dominate the collective memory of Texans: the Alamo and the Texas Revolution, the Civil War and Reconstruction, and the Cattle Kingdom. The editors, surprisingly, chose not to include any essays based on the mythology of the Texas cowboy, but both the Texas Revolution and the Civil War receive substantial attention. Since some selections deal with memories of national events, they also speak to other studies of memory. For example, Turner’s discussion of June-sixteenth ceremonies that began in Texas but spread to other states includes interesting observations about the use of Abraham Lincoln’s image in them. Kelly McMichael’s discussion of the United Daughters of the Confederacy shows the important work of the Texas Daughters in erecting Confederate monuments but subtly questions the scholarly consensus that women controlled the white South’s celebration of the Civil War.

Texas remains the focus of most of the essays. Laura Lyons McLemore summarizes the work of early Texas historians, and Andrés Tijerina analyzes the construction of Tejano memory. James E. Crisp challenges the accuracy of various mythic treatments of the Alamo, emphasizing the importance of historians confronting distortions in public presentations of memory; Don Graham takes a different stance, criticizing the emphasis on historical accuracy in the film *The Alamo*. Other essays look at the memory of more recent events. Yvonne Davis Frear explores how African Americans in Texas remember the Civil Rights Movement, concluding that those involved in the movement remember it as a success but that later generations consider it either a failure or passé. Ricky Floyd Dobbs examines the fading memory of President Lyndon B. Johnson, arguing that Texas no longer celebrates Johnson in part because of his association with negative stereotypes of the state but mainly because of its currently conservative political climate.

The volume’s most important contribution to understanding Texas, and for expanding historians’ conception of memory, comes in essays by Walter L. Buenger and Gregg Cantrell. Buenger’s essay about the four strains of memory employed by the Ku Klux Klan and Cantrell’s account of the reburial of Stephen F. Austin, building on earlier work by Buenger, make a convincing case for how, during the early twentieth century, Texans used historical memory to transform the state’s sense of itself from being southern to being western and American.

*Lone Star Pasts* approaches the study of historical memory just as historians should. Its essays challenge collective memories but also seek to illustrate their diversity, analyze their construction, and, in the process, show how they shape society. The result is an enlightening book that will interest those fascinated by Texas and those who study historical memory.

Gaines M. Foster
Louisiana State University
Allen’s reconstruction of the diverse temporalities—or ways of experiencing time—that shaped Americans’ “social imagination” during the nineteenth century is a welcome contribution to the literature about nationhood and national belonging. Challenging Anderson’s view that the “imagined community” of the nation is homogenous and homogenizing, reducing its deracinated subjects to a single soul-destroying temporality, Allen argues that national time “is not a unity; it is multifarious and protean, riven with conflict” (219). National identity is “an ongoing negotiation, in narrative, of heterogeneous temporal modes” (4), not a functionalist fantasy of the mindless masses moving forward in monumental lockstep.

Americans did not experience new ways of telling and thinking about time as a narrowing and flattening of their temporal horizons. Even as mechanical timepieces facilitated market expansion, and the nation began to run on timetables, clock time promoted new subjectivities, enabling Americans to “remake themselves into members of a heterogeneous national community” (113). Technological and scientific developments facilitated the proliferation of temporalities and identities. A sense of the living past, or what modern people call “tradition,” emerged in tandem with the market’s homogenizing impulses; revivalists infused old-time religion with new vitality, even as geological “deep time” raised cosmic questions about man and nature and the secular pieties of liberal democracy. Contestation characterized all of these coexisting temporalities, but it was this contestation that gave Americans a common ground—a “locus for healthy dispute” (223)—and a shared sense of national belonging.

Allen’s achievement is to save the idea of the “nation” from totalizing, reductive critics who imagine a premodern or postmodern world where their conceptions of a more fully human life were, or might be, realized. Such constructions of alternative futures and reconstructions of alternative pasts are characteristic products of the intelligentsia in modern nations—even when they seek to transcend their national identities. Happily, Allen adds, “globalization does not mean the death of the nation” (224); nations provide the only plausible instruments “to create the social and political worlds that we would like to inhabit” (222). By contrast, self-professed postnational critics, imagining themselves out of the nation, offer no such hope.

A Republic in Time makes its important case by reconstructing the rich history of debate and negotiation that the American national project has entailed. Its animating premise is that Americans have always imagined themselves as a nation through time, in the future. American

exceptionalists—whether of the celebratory or critical variety—privilege space over time, supposing that “nature’s nation” has been, or has imagined itself to be, exempted from the constraints of history. But the “recursive and dynamic interactions” between “time and the nation” that Allen charts show Americans to be both precociously and unexceptionally “modern” (11).

For all their many sins, nineteenth-century Americans engaged with the many temporalities that constituted national identity in ways that should command our attention. If we listened more carefully to what Allen’s subjects have to say—Ralph Waldo Emerson, Catherine Beecher, and Emma Willard, as well as the ingenious and enterprising clockmakers and merchandisers who promoted modern time-keeping and the many other Americans who made sense of their world in and through time—we might be inspired to continue the debates that they initiated about who we have been and who we could be.

Peter S. Onuf
University of Virginia

Some Family: The Mormons and How Humanity Keeps Track of Itself. By Donald Harman Akenson (Montreal, McGill–Queen’s University Press, 2007) 349 pp. $29.95

Akenson’s Some Family promises an analysis of genealogical meta-narratives—particularly focusing on the Church of Jesus Christ of Latter-day Saints (lds, the Mormons) and their vast collection of genealogical materials. His ambitious project aims to demonstrate the usefulness of understanding genealogy as a unique narrative of history; Akenson’s contention that historians too often treat genealogists with “snobbery” is a point well taken (15). Nonetheless, despite some promising ideas about narratives of human history, Some Family displays a lack of clarity about the Mormons’ genealogical project and how it could benefit historians and historical narratives.

Akenson is at his best when describing the various types of genealogical narrative, but his analysis breaks down when he discusses the specific challenges and opportunities of the Family History Library (FHL)—the location of the Mormons’ genealogical collection in Salt Lake City. Akenson’s account blurs the distinctions between the large holdings of primary records (microfilmed and digitized) and the compiled online databases. He makes some apt critiques of the compiled databases that should help historians and genealogists use them better. But he does not explain the important differences between the databases. Moreover, he incorrectly asserts that the compiled information can never be correlated with original sources, claiming that such information, in fact, is “beyond verification” (211).1 Many entries in the databases contain source notes

1 Akenson also hints at times that the Mormon genealogical project involves the assimilation of primary sources into a cut-and-paste Mormon narrative that then jettisons the primary
and other information that can still be verified by exploring the primary sources available in the FHL. Considering that Akenson has used the collection in his own research, this puzzling imprecision leaves the false impression that the Mormons can offer genealogists and historians only a mishmash of useful and useless information in the online databases.

*Some Family* is built around the idea of genealogical narratives, but the attempt to describe the Mormon narrative creates a tension in the book. Akenson grants the LDS Church a massive, and mechanical, power over a “master narrative” of genealogy (87–88, 118, 155, 185–86, 191, 199). But the problems that he has with the databases stem from the LDS Family History Department policy to make submitters, rather than the Department, responsible for the quality of information they submit.

Hence, although the church collects records and compiles databases, it does not try to control how the tens of millions of people use those records and resources; nor does it attempt to check submissions for accuracy. Akenson discusses the difficulties that this policy raises, but, unfortunately, he does not address how they affect his description of the “LDS machinery” as “writing a world narrative” (189, 211). Otherwise, his analysis of the different strategies for compiling lineages (patrilineal, matrilineal, double, biological, social, etc) is excellent. If he had more accurately discussed the Mormon genealogical project and its usefulness to historians and if he had accounted for the many voices included in the Mormon genealogical narrative, he would have made his mark on both history and genealogy.

Amy Harris
Brigham Young University


Technological and economic historians of Western industrialization have paid much attention to cotton manufacture. Some of them have also given consideration to the woolen industry, but very few of them have written about linen and almost none of them about silk. The introduction to *American Silk* says, “With this book, the story of American silk begins to be told” (xxiv). Fortunately, this claim need not be taken literally since the book’s useful bibliography shows earlier entries.

The book’s three case studies, each well illustrated and well written by a different author, share six appendixes and an index, as well as the bibliography, but each could easily stand on its own—thus begging the
question, Why is this a book instead of three journal articles? Field’s introduction says only that the book “grew out of the chance meeting of three scholars . . . long engaged in silk industry research . . . each had adopted a different company, and each had found that the silk men and their mills did not give up their stories easily. . . . Serendipitously, the three studies dovetailed to span the development of the U.S. silk industry from its beginnings in the 1830s to its decline in the 1930s” (xx). In addition to their own black-and-white illustrations, the three articles share a section of color plates showing textures, colors, weaves, and designs for silk yarns, fabrics, trimmings, and garments.

Senechal, who wrote the book’s engaging first six chapters, describes the silk worms that an early nineteenth-century utopian community in Northampton, Massachusetts, unsuccessfully tried to raise for profit as voracious, fussy, delicate, pampered infants. Success evaded the business until the Nonotuck Silk Company (and the industry generally) came under more practical leadership and began importing raw (already reeled but not yet twisted) silk from Europe, China, and Japan. Thanks to development of machinery that provided uniformity of thickness, Nonotuck made silk thread that ran smoothly through Isaac Singer’s newly-invented sewing machines, creating a symbiotic relationship between the rising fortunes of the two enterprises. Only in the twentieth century did the company undergo expansion in size, locations, and products to include woven silk fabric, renaming itself “Corticelli” after its brand of “machine twist” thread.

In Chapters 7 through 12, Field draws on company records to present a history of the Haskell Silk Company of Westbrook, Maine, which cotton manufacturer James Haskell started in 1874. It survived and thrived through two more generations of management by members of the same family before closing in 1930. Unlike Nonotuck, Haskell’s company moved relatively quickly from making thread into producing fabric, building a solid national reputation for high-quality “staple” silk yard goods and selling directly to retailers and to garment makers instead of relying on sales agents.

In the final four chapters, Shaw presents advertisements for, and externally written commentary about, H. R. Mallinson & Company, Inc. of New York, New Jersey, and Pennsylvania, to show how the company’s creation of an image for itself and for its product helped to attract fashion-conscious buyers of high-end luxury silk. Mallinson’s pioneered “branding” through print and graphic media.

Regarding the larger picture, Appendix VI offers a helpful timeline for these three northeastern American companies during the century preceding the Great Depression, when they fell victim to rising competition from “artificial silk” (rayon) and to lack of funds. The introduction provides a brief overview of the rise of the American silk industry, with a nod to silk’s ancient Asian origin, nineteenth-century mechanization, immigrant operatives, and encouragement by post-Civil War tariffs (xx-xxiii). Each case history occasionally mentions larger conditions in
the American silk industry but makes no systematic effort to indicate how each company’s accomplishments and travails compared with those of the industry in general, rarely mentioning either of the other two companies. Field’s five-page conclusion summarizes similarities and differences in the companies’ experiences.

The authors disparately call on sericulture, technology, chemistry, economics, and social psychology to explain various features of their companies’ histories. However, rather than carry out systematic analysis, they gravitate toward the method of unhyphenated “plain vanilla” narrative: They have gathered their documentary threads and woven them into stories.

Carolyn C. Cooper
Yale University

Emerald City: An Environmental History of Seattle. By Matthew Klingle (New Haven, Yale University Press, 2007) 368 pp. $30.00

In Emerald City, Klingle traces more than 150 years of Seattle’s natural history, from the arrival of American settlers to the 1999 protests against the World Trade Organization, showing, among other things, that salmon and the history of Seattle are intimately entangled. More specifically, he examines how Seattle, a city renowned for its outdoor life and breathtaking landscapes, transformed and refuged its hills, rivers, and lakes with often tragic consequences. Importantly, the author goes beyond the typical diatribes about human destruction of nature to see the impact of such actions on the region’s inhabitants.

Seattle residents’ efforts to “improve” upon the region’s natural blessings began with the first sawmill constructed on Puget Sound. By the early 1900s, city leaders had filled in tidelands to make room for railway yards and carved a channel to connect the waters of the Sound with Lake Washington. Klingle continually emphasizes the consequences—intended or not—that manipulating the landscape had on poor and marginalized residents. Tidelands and waterways, he argues, were vital to the subsistence economy upon which immigrant fishermen and local native populations depended. Creating property and profit may have enhanced Seattle’s identity, but not without significant cost in terms of environmental degradation and social inequality.

Emerald City offers a refreshing contribution to the story of human interaction with the environment by paying specific attention to the role of middle-class professionals. This tale is less about the exploitive endeavors of local business elites than about the progressive-minded actions of municipal engineers and landscape architects. Seattle municipal engineer Reginald H. Thompson, for example, believed that flattening the city’s daunting hills would remove natural barriers that segregated neighborhoods and impeded social unity. Alas, such goals proved elu-
sive. Likewise, when attempting to enhance the city’s quality of life by securing ample water from the nearby Cedar River, Thompson’s efforts instead brought deforestation, flooding, and declining salmon runs. Klingel concludes that the good intentions of professionals like Thompson and renowned landscape architect John C. Olmsted notwithstanding, nature stubbornly resisted these efforts, producing both new problems and exacerbating older ones.

By the late twentieth century, Native American activism, sympathetic courts, a booming economy, and modern environmentalists together helped to focus attention on Seattle’s emerging symbol, the salmon. Klingel, however, seems less sanguine about postwar undertakings to protect salmon, which he suggests suffer from many of the same misguided beliefs that produced the problem. He argues that a “sustainable ethic of place must face the legacies of history, providing redress for wronged communities in some cases, ensuring equity for the future in others, and the principle should apply not only to human communities but to the non-human as well.” (275)

Emerald City successfully weaves urban history and environmental history into a narrative that shows how much we are a part of nature and nature is a part of us. Some might quibble with Klingel’s singular focus on waterways at the expense of air quality or other environmental issues that might change the Seattle story. However, since salmon swim, not fly, such a criticism might be unfair. More important is the author’s passionate plea to move beyond a binary view of culture and nature to develop what he calls an ethic of place that “links the necessity for social justice to the importance of protecting the environment” (60). In short, as Klingel suggests, we can learn much from salmon.

John Putman
San Diego State University

Why Confederates Fought: Family and Nation in Civil War Virginia. By Aaron Sheehan-Dean (Chapel Hill, University of North Carolina Press, 2007) 291 pp. $34.95

Sheehan-Dean’s road may be the heavily traveled highway of Confederate nationalism, but his vehicle is distinctive. Most treatments of the subject take a nationwide perspective, but Sheehan-Dean has chosen to focus on wartime Virginia. The choice is eminently justifiable, not because Virginia typified the Confederacy but because on any number of measures, the state was the Confederates’ linchpin. The approach is also appealing because it allows detailed exploration of soldiers’ motivations and experiences in the context of military and political developments.

Studies of Confederate nationhood have developed their own consensus and conflict schools. Sheehan-Dean comes down squarely in the
former: class hostility and resentment of government policies notwithstanding, he finds an enduring commitment to the Confederacy, rooted in soldiers’ desire to defend their autonomy, their families, and slavery. He has little new to offer in his overall conclusion, but his intention is to contribute breadth and depth to other studies’ appreciation of Confederate nationalism.

To add breadth, Sheehan-Dean draws extensively on letters and diaries to illustrate the interlocking facets of soldiers’ motivation, particularly the devotion to protecting their families. A study that rests so heavily on testimony and limits its scope to one state would seem especially likely to address the methodological challenges inherent in such evidence. Guidance for this purpose exists: McPherson’s recent analysis of Confederates’ testimony acknowledges that extant letters and diaries overrepresent ideologically committed soldiers, and Murphey has proposed a test for idiosyncratic bias in written sources. Yet, Why Confederates Fought offers no discussion of these issues.

To add depth to his findings, Sheehan-Dean compiled quantitative data about enlistments and soldiers’ characteristics, the results of which run afoul of the concept of population at risk. First, using company-level enlistments to construct a numerator, Sheehan-Dean estimates that 89 percent of available men of military age joined Virginia’s units, a figure that constitutes an “important index of the willingness of Virginians to support the Confederacy” (3). The denominator for this calculation, however, is incompatible with the numerator. Adopting Confederate officials’ estimate of Union-occupied areas, the author used 80 percent of the state’s military-age population as his denominator, yet he included enlistments from counties comprising 94 percent of the population in the numerator. Using the same counties in numerator and denominator produces an enlistment rate of 75 percent, an impressive but less overwhelming level of Confederate support.

Sheehan-Dean’s finding about desertion is equally important to his argument. The author created a sample of nearly 1,000 Virginian soldiers, whose military records indicate that the number of desertions from the army peaked in 1862. On this basis, Sheehan-Dean asserts that “the longer the conflict extended in time, the less likely Virginia men were to leave the Confederate army” (3). Yet raw numbers say nothing about likelihood; again, we need the population at risk. As a rough guide, applying the ebb and flow of men present for duty in the Army of Northern Virginia to Sheehan-Dean’s sample results in an annualized desertion rate that remains essentially flat from 1861 to 1865. The author’s contention that “most” soldiers stood by their commitment may well be true, but desertion (and going AWOL) must be analyzed in light of an army

---

that continually changed through reorganization, death, disease, imprisonment, and absenteeism.

Sheehan-Dean’s intended project of illuminating a question of war and society is commendable. The outcome, however, is marred by missed opportunities to master the full range of demands imposed by evidence from the Civil War.

Larry M. Logue
Mississippi College

*West from Appomattox: The Reconstruction of America after the Civil War.* By Heather Cox Richardson (New Haven, Yale University Press, 2007)
396 pp. $30.00 cloth $20.00 paper

The trend most apparent to scholars studying the era after the Civil War has been the broadening of the concept of *Reconstruction*. Traditionally defined as the twelve years after the Civil War when troops were stationed in the formerly rebellious southern states, *Reconstruction* has come to encompass a longer period during and after the war. Richardson’s book not only expands the time period but also argues that the West was a crucial part of the rebuilding that was taking place in America. Richardson views *Reconstruction* as centered around citizenship and the role of the government in a period (1865 to 1900) crucial to forging the American state. Moreover, she believes that citizens then settled on essential notions about the role of government and the proper recipients of its aid. Her central question is, “How did nineteenth-century Americans justify the expansion of government activism and still retain their wholehearted belief in individualism?” (4)

To Richardson, the answer lies in a middle-class worldview in which government should aid deserving citizens but should not become the pawn of special interests. The West became the laboratory where the proper mix of individualism and government assistance was achieved. Ironically, this West, which actually depended heavily on governmental assistance, came to symbolize sturdy self-reliance to most Americans. Richardson gives a political and economic narrative of major events from the end of the Civil War through the McKinley administration.

Readers of Richardson’s two earlier books on the Civil War and Reconstruction who expect the same close analysis of newspapers and other writings to grace this one will find a different kind of book that works on a larger scale. Despite this broader sweep of time, Richardson wishes to inject a human note. Thus, she introduces the life stories of a variety of Americans into her grand narrative to enliven this unfolding of the large forces.

Some readers will question the composition of this middle class and the kinds of evidence that Richardson uses to construct its vision. Marxists and most other scholars with a social-science orientation are likely to
want a definition of middle class that has an economic component. Richardson lets her group delimit itself: “Those who believed they could make it on their own saw themselves as part of the ‘great middle’ between rich monopolists and the lazy poor” (2). Elsewhere, she stresses that the members of this “new middle class” “were distinguished not by their income but by their determination to hold what they believed was an even handed government steady from the demands of those at the top as well as those at the bottom of society” (147). Crucial to this worldview was the belief that the government must necessarily protect American individualism, especially in the West. Such a definition of middle class allows her to include Andrew Carnegie, the steel magnate, and Wade Hampton, who in the antebellum period had been one of the largest slave owners in America.

Although Richardson lets this vision define class, her method of ascertaining this new middle-class ideology seems problematical. The biographical accounts that she employs to establish the views of the middle class in many cases depend on memoirs. The shortcoming of such writings is their tendency to reflect one period, generally at the end of life, rather than chronicle a changing outlook more often caught by letters or diaries.

Although Richardson has written a fast-paced, highly readable narrative that covers major political and economic events of the period, some scholars may question whether the middle class during Reconstruction can be comprehended adequately through the concept of individualism. They may also doubt whether Richardson’s methods and sources lend themselves to a convincing interpretation of the reconstruction of late-nineteenth-century America.

Jane Turner Censer
George Mason University


In this book, Murray presents a thorough history of industrial-sickness funds and explains why these funds were able to persist in the face of efforts to replace them with publicly provided insurance. The programs that Murray examines were not comprehensive health-insurance plans. Rather, they were funds, sometimes firm-specific and sometimes union-specific, that paid a cash benefit to workers who were physically unable to work for a period of time. The purpose of these programs was not “to leave the worker and his family whole” but to keep them “out of the poor house” (7). The benefits provided amounted to a portion of the worker’s pay, usually about half.

According to Murray, Progressive reformers of the time felt that in-
Industrial-sickness funds were small and inadequate but that public provision of health insurance for workers was nonetheless thwarted by the political maneuverings of powerful interest groups. Although this view has shaped most of the history written about industrial-sickness funds, Murray argues that it is wrong. He presents evidence that as many as 9 million workers (about 40 percent of all nonagricultural workers) were covered by sickness funds in the 1910s. This figure is several times larger than the less-formal estimates on which some historians have relied. Murray also argues that the standard view does not place sufficient emphasis on the initiative of the workers themselves. Many of them (especially the young, who were healthy, and the old, who had savings) chose not to participate in sickness funds because it was not in their interest, and they were unlikely to find public-insurance programs any more attractive.

Murray recognizes that efforts by the American Association for Labor Legislation (AALL) to create public programs of this nature met resistance from several powerful interest groups, including physicians, commercial insurance companies, unions, and employers. Yet, he argues that these groups did not prevent the implementation of public programs that workers desired; workers, in fact, were not supportive of these public-insurance proposals either. Murray notes that a 1918 California referendum to create public insurance failed resoundingly. He also cites survey data from New York showing essentially no support among workers for public-insurance proposals in that state. Murray uses econometric evidence to demonstrate that the medical benefits incorporated into AALL proposals probably made them less appealing to workers. For a given premium, workers preferred higher benefits and longer benefit periods to medical care, which they felt to be ineffectual.

Murray provides an engaging and readable history of a neglected set of institutions, combined with a vigorous (but fair) argument about how some scholars have misunderstood the operation of these funds. He draws evidence from both qualitative historical records and econometric analysis, though in the latter case, he is careful to place the statistical work in service of the historical argument rather than the other way around.

In a few areas the evidence seems inadequate to the task asked of it. For instance, in discussing low rates of fund participation by older workers, Murray shows that older workers who were not members of funds were more likely to have savings than were older fund members and that their average level of savings was greater than that of older fund members. However, only a small minority of these older non-members, less than one-quarter, held any savings at all, and their average savings would not have provided many weeks of support at the level that funds could provide (Table 7.6, 166). Hence, the expected difference (higher savings among non-members) is present, but its magnitude is small, making Murray’s argument less persuasive. Such instances of insufficient evidence, however, are rare.
Although the sickness funds that Murray examines differ substantially from the health insurance of today, his analysis has important implications for current policy debates. Murray suggests that a stronger case can now be made for government involvement in the provision of health insurance, but he argues that advocates of such policies often make the same mistake that the Progressives did. They ignore the fact that the present system works tolerably well for a large number of people. If the system is to be changed, these people will have to be convinced that the proposed change will actually help them, or at least not harm them. Politicians and policy advocates would be well advised to take the historical lessons that Murray has unearthed to heart.

Thomas N. Maloney
University of Utah

Escape from Empire: The Developing World’s Journey through Heaven and Hell. By Alice H. Amsden (Cambridge, Mass., The MIT Press, 2007) 197 pp. $27.50

This little book is a tour de force about how postwar U.S. foreign economic policy shaped the development prospects of the so-called Third World. Amsden’s previous iconoclastic work on the political economy of late industrialization provides the organizing framework of this historically informed book. East Asia and Latin America predictably get most of the attention, whereas the pre-industrial “bottom billion” in Africa, the Caribbean, and western Asia receive short shrift.

The book comprises eleven birds-eye-view chapters. The first two provide an overview and historical context; the next six characterize what the author calls the First American Empire (\( \text{fae} \)), which fell between World War II and Diem Bien Phu; and the last four chapters focus on the Second American Empire (\( \text{sae} \)), which ushered in a dark age of stagnation and income polarization—the rise of China and India (Chindia) providing the turning point.

Amsden boldly encapsulates the central thesis: “The idea presented in this book is a hot potato because it turns the meaning of laissez-faire on its head: instead of free markets, laissez-faire signifies free developing economies. Freedom is truest when the great powers let the developing world chart its own economic course and choose its own mix of market and state, as done to a tee by the fastest-growing late developers”(vii). The author envisions two roads to development, the second being superior: “Economic development thus has two approaches. One, which supposedly is applicable to rich and poor countries alike, recommends free markets to maximize efficiency. . . . The other, a less formal body of thought, likens development to learning technological capabilities and getting institutions to work, including markets—themselves institutions”(163).
FAE (1950–1980) spearheaded a golden age of growth under the clouds of the Cold War decades. It was permissive to the extent that it tolerated autonomy for former European and Japanese colonies to learn and experiment; it was flexible in providing aid and foreign direct investment (FDI) while permitting protectionist policies of import-substituting industrial development and the nationalization of key resource-based industries; and it provided nonreciprocal access to the markets of developed countries. As a result, nationalist Asia thrived while comprador Latin America, despite its pre-war manufacturing experience, did not fare so well. The reason is that “without land reform, without the flight of foreign firms, and without the creation of a critical, minimum number of professionally managed national companies, Latin America was unable to exercise its skill to survive in a high-tech world” (147).

SAE (1980—), experiencing a number of shocks (Vietnam, OPEC, and the end of Communism), became too arrogant and too ideological (free markets) in insisting that all spheres besides labor occupy a level playing field; big finance successfully pushed for growth-stunting privatization, deregulation, and liberalization (especially of capital markets) in developing countries; and only a handful of developmentalist states managed to engineer growth miracles under this unforgiving regime (most notably Taiwan, the People’s Republic of China, India, and South Korea). The deceleration of productivity-driven growth in the era of the Washington Consensus, despite market-oriented reforms, is attributed to the neglect of key features of late development: “For the disadvantaged, specialization and comparative advantage were suffocating traps” (42). The recurring theme is that “unless a country has its own nationally owned firms, it can’t ‘globalize’ in the form of outward foreign investment” since “import substitution industrialization and its offspring, manufacturing exports, lay at the heart of the Third World’s experimentation” (24, 74).

Why would a postcolonial imperial power like the United States, inherently hostile to the emergence of nationalistic competitors, become so “heaven-sent” in the third quarter and so “hell-bound” in the fourth quarter of the twentieth century? Are external factors, imperial powers and their global governance institutions, really the preponderant drivers of the economic fate of five-sixths of the world’s population?

Amsden argues that FAE was pragmatically benign for a number of reasons—the imperative of keeping poor countries out of the Communist orbit, residual Keynesian influences concerning governed markets, the visceral association by the elites of newly independent countries of free trade with colonialism, and the preoccupation of domestic business lobbies in the imperialist countries with opening up markets for manufactures while protecting their mid-tech and agricultural sectors. SAE, however, accentuated its ideological hostility to uppity developmental states as a result of notable changes in global circumstances that included growing competition from a handful of fast industrializers, especially in
East Asia; the growing importance of Wall Street, which stood to benefit from deregulation of financial markets even where they were shallow; the emergence of the United States as the sole superpower, resulting in greater military and political freedom to support its multinationals; and the rise of market fundamentalism in academic and policy circles, with a misguided mantra regarding the evolution of markets and institutions as latecomers learned to build up domestic technological capability.

Regarding the second question, Amsden unconvincingly asserts that external factors trump internal ones for a developing country. At times, she equivocates. She maintains that the rise of Chindia rendered global absolute power a thing of the past but also that the widening income gap between rich and poor countries has made imperial power stronger today (15, 17). Escapees from the trap of empire are a handful of countries with a socialistic bent (China and India) or those that enjoyed unprecedented autonomy due to a twist of fate (South Korea and Taiwan). The extraversion of late developing states, the book’s basic units of analysis, and the need for policy/institutional autonomy in the nurturing of mid-tech industries is a view that is also shared by such Third Worldist economists as Rodrik and Stiglitz (curiously not mentioned in the book). What these populist liberals bafflingly ignore is the agency of the poor and the nascent business class in restraining comprador elites who certainly do not deserve a free ride in the name of policy sovereignty.

The book has two strong suits. First, it debunks many neoliberal claims concerning the relationship between markets (capitalists) and states (bureaucrats), import substitution and export substitution, and inequality and shared growth. Second, it is written in an accessible and thought-provoking style. Amsden’s penchant for aphoristic claims abound in this book as in her previous ones—for example, in the British Empire and sae, “the sun never sets and wages never rise” (9); in the reciprocity-centered Nipponian model of development, “the high ideal of the government was to give nothing away for free” (10); and in the “rivalry between the United States and China,” success “will depend on the relative performance of Asia and Latin America” (16).

The analysis is unsatisfying in other respects, especially due to lack of a fine-grained theoretical framework, which makes it difficult to know the conditions under which one development strategy is preferable to another. After all, not all autonomous states thrived (the socialist countries); not all import-substitutors switched to export promotion at the right time (South Korea versus Egypt); and most ruling elites, lacking effective domestic restraint, headed horrendously parasitic states under the aegis of both empires. By focusing on countries that succeeded, and ignoring the many failures with similar strategies, the analysis suffers

1 See, for example, Dani Rodrik, One Economics, Many Recipes: Globalization, Institutions, and Economic Growth (Princeton, 2007); Joseph Stiglitz, Globalization and Its Discontents (New York, 2002).
a selection bias and a propensity to take apt counterexamples for systematic evidence. Witness, for example, Amsden’s dismissive preemption of such what-if questioning: “Counterfactual arguments are nothing more than guesses” (135).

Overall, *Escape from Empire* is an effective synthesis of the more strident critiques of triumphalist market fundamentalism as the American Century gives way to a Pacific one. We badly need a sober perspective on the right mix of private markets and public bureaucracies for late developers.

Berhanu Abegaz
College of William and Mary

*An Environmental History of Latin America*. By Shawn William Miller
(New York, Cambridge University Press, 2007) 257 pp. $65.00 cloth
$22.99 paper

Environmental historians draw upon a wide range of disciplines, in both social and ecological sciences, as they broaden traditional fields of history and historical geography to deal systematically with the intricate interactions between societies and their natural resources. The subject matter of environmental history is difficult to delimit. Fine-grained studies of limited locations and time periods have one set of challenges to meet; broad syntheses such as Miller’s survey have another.

The field of Latin American environmental history, which hardly existed ten years ago, is developing rapidly now, with publications in Spanish, Portuguese, English, and other European languages. A one-volume survey, such as Miller’s lucid and provocative book, has long been needed. His earlier monographic work on colonial Brazil established him as one of the most innovative historians of Portuguese America. Brazil as subject matter virtually required him to work deeply in both anthropological and ecological history, as well as more traditional forms of political, social, and intellectual history.¹ His new work builds on those strengths to encompass the entire range of Latin America.

One admirable quality of this survey is its balanced articulation of several major controversies, starting with the “pristine myth” of indigenous cultures before European contact. Carefully surveying the recent literature on population, agriculture (the most vital aspect of preindustrial cultures’ relations to nature), and cities, Miller portrays major population pressures on land in both mountain and lowland ecosystems. He avoids a common trap in environmental history writing, the declensionist theme of steady ecological degradation through history. In both pre-contact times and the demographically devastating sixteenth and seventeenth centuries, Miller points up the ebb and flow of human pressures on natural systems.

¹ Miller, *Fruitless Trees: Portuguese Conservation and Brazil’s Colonial Timber* (Stanford, 2000).
In “The Colonial Balance Sheet,” the author surveys the intensive Iberian sugar plantations, and the human and natural exploitation around the silver mines of Mexico and Bolivia. These short essays are as vivid and provocative as any in print, in part because he is adept at presenting the nuts and bolts of productive systems. Then, in a chapter on the local and global dimensions of post-independence Latin America in the early industrial age, he describes massive water-management projects and the mining of nitrogen-rich guano for world agriculture. He balances this topic with a chapter on urban environmental history, provocatively entitled “Asphyxiated Habitats.”

Both resource depletion and massive pollution led to the rise of environmental awareness in Latin America, including recent trends in citizens’ environmental action and national legislation, which Miller surveys in the last chapter, “Environmentalism.” He adds a provocative epilogue about the transformations of Cuban agriculture in the aftermath of both American and Soviet imperial power. Finally, his “Suggested Readings” is an excellent survey of a rapidly changing field.

Richard P. Tucker
University of Michigan

People of the Volcano: Andean Counterpoint in the Colca Valley of Peru. By Noble David Cook, with Alexandra Parma Cook (Durham, Duke University Press, 2007) 319 pp. $84.95 cloth $23.95 paper

Cook’s book is a masterful synthesis of methodologies from geography, demography, and anthropology. His point of departure in this study is ethnohistorical, exploring the relationship between the present and its dynamic past: “My hope is to provide a guide for those who, moved by the experience in the valley, may wish to understand better how its people transformed their landscape, making it what we see today” (xi). Cook clearly defines a turning point in this valley’s history: “Much of what one views in the valley is the result of what the outsiders, the Spanish, took in the sixteenth century and the way they modified Inca and pre-Inca foundations” (xi). This turning point is still relevant for our understanding of Andean societies. The Spanish conquest and colonization transformed the way the natives organized their lives, and its legacy continues into today.

Cook’s central argument is that the transformations of the people in the Colca Valley, the Cabanas and Collaguas during the sixteenth century, have been long-lasting: “The basic political and social structure of the Andean world have been remarkably constant over the past half millennium” (252—253). Despite the colonial assault and the threats of the Peruvian state, the “people of the volcano” have survived and their social structures still remain in place.
Cook’s use of such concepts as reciprocity, community self-sufficiency, redistributive state, resistance, accommodation, and destruction are good examples of his ability to summarize the important contributions made by Andean ethnohistorians as well as by scholars like Murra, Rostworowski, Pease, and Espinoza.¹ His new emphasis is the focus on the history of the landscape. Cook’s analysis of changes in the landscape evinces a familiarity with the geography of the region that, together with his extensive archival research, provides a fruitful approach for a regional study.

Cook’s choice of title for the book speaks to the importance of the environment for the populations of the Colca valley. He cites several environmental factors that have been decisive in the transformations of this valley. For example, he argues that the decline of the _encomendero_ elite was a result of ecological events, specifically natural disasters (floods, earthquakes, droughts, insect infestation of crops, and demographic factors). This new argument should be taken seriously and tested elsewhere in the Andes. Cook’s analysis of the function of duality in Andean societies is compelling. Additional monographs and ethnographies will confirm his remarks.

Although fascinating and meritorious in its focuses on the history of the Colca valley during the early colonial period, the book offers little information about late colonial and republican times. Cook defines the influence of the republican state on the Colca valley communities as negative, describing their relationship as conflictive and dysfunctional. However, these remarks are not backed by archival evidence or by the kind of ethnographical analysis that he brings to the colonial period.

Miguel Leon
State University of New York, Oneonta


This extremely important and informative book should put to rest any conceivable effort to minimize the brutally destructive impact of the Atlantic slave trade upon Africa and Africans or to blame the victims. West Central Africa was the only region that suffered from European occupation since the early stages of the maritime slave trade. Portugal was in charge. Roman Catholic missionaries from various orders, Portuguese and Afro-Portuguese traders, and government and military bureaucrats

1 John Murra, _The Economic Organization of the Inca State_ (Greenwich, Conn., 1980); Maria Rostworowski, _Historia del Tahuantinsuyu_ (Lima, 1988); Franklin Pease, _Del Tahuantinsuyu a la Historia del Peru_ (Lima, 1978); Valdemar Espinoza, _La destrucción del Imperio de los Incas_ (Lima, 1981).
deliberately created havoc and war in order to “produce” more and more slaves to export.

The documentation that Heywood and Thornton unearth is stunning. It includes extensive correspondence among the kings of Portugal and the kings of Kongo as well as reports from Catholic missionaries and government officials. Sustained conversion to Roman Catholicism, especially in the kingdom of Kongo, and later in Angola on and near the Atlantic coast, played a large role in what the authors refer to as Atlantic Creole culture. They recognize that some of the regions in Angola from which slaves were sent to the Americas were not Christian. But they argue that the peoples from the Christian areas were sincere in their conversion and in their adoption of European culture, making them more esteemed by European settlers in the Americas than were West Africans. The authors accept Berlin’s concept of the Charter Generation of slaves introduced into the Americas as Africans who had already been significantly acculturated to European values, including the Catholic faith.¹ This concept, however, is questionable, even for West Central Africa.

The title of this book is much broader than its contents. The book is primarily concerned with the impact of West Central Africans on English and Dutch colonies during their formative period. During the mid-seventeenth century, the Portuguese army moved inland in West Central Africa, allied with the Imbagala, creating chaos and warfare and tilting the trans-Atlantic slave trade heavily toward West Central Africa rather than Upper Guinea. The English and the Dutch entered the area as pirates raiding Portuguese slave-trade ships, bringing some of the captured slaves to their own colonies. Thus, the vast majority of the earliest slaves brought to English and Dutch colonies were West Central Africans.

Unfortunately, because the documentary evidence about the early English and Dutch colonies is extremely weak, the authors cannot establish the important statement in their conclusion that the culture of the West African slaves of a later period “was different and more alien to Euro American expectations.” Sweet, who consulted rich documents generated in the early Portuguese world, especially in Brazil—including Inquisition documents—convincingly discusses the fluid, hybrid nature of culture and religion before 1770. He also argues that Christian religious culture in West Central Africa was superficial.²

Despite these problems, this book is a valuable resource because of its stunning research about how West Central Africans were obtained for the trans-Atlantic slave trade.

Gwendolyn Midlo Hall
San Miguel de Allende, Mexico


The older literature about the African emigration movement generally focused on the American Colonization Society (ACS), the national organization that founded the colony of Liberia in the early 1820s and established Liberia as an independent Republic in 1847. More recently, Tyler-McGraw and others have begun to mine the rich records of the ACS for local and state history. Black emigrants to Liberia left North America often in response to local conditions, and in the antebellum years, much of the momentum within the movement was in the state auxiliaries of the ACS.

Tyler-McGraw’s valuable book examines the African emigration movement in Virginia, fertile ground for a state study given that it provided more emigrants for Liberia during the antebellum period than any other state—approximately one-third of the 11,000 American settlers (others estimate 13,000). Tyler-McGraw’s failure to simplify her findings into unifying themes or a coherent narrative might seem frustrating at first. But the Liberia project meant different things to white and black Virginians. Members of each race differed markedly in their stance on emigration, and positions tended to shift over time. Virginia provides an excellent case study, especially to dissect the diverging white positions regarding African emigration, since several of the founders and influential leaders of the ACS were Virginians—James Madison, John Marshall, William Meade, Charles Fenton Mercer, and Bushrod Washington, nephew of the first president.

White Virginians supported black emigration for diverse reasons. Some, particularly women of the ACS, saw emigration to Liberia as a prelude to abolition, an opportunity for black people to demonstrate that they could be nation builders with a civilizing mission. Yet, some planters, particularly after the 1831 Nat Turner Rebellion, took up the emigration cause as a vehicle to remove free blacks from the state and negate the notion that slaves could ever have a future of freedom west of the Atlantic. Virginia’s legislature even appropriated funds to send free blacks to Liberia. During the 1830s, Virginians mirrored the ACS in general, badly divided between those who saw Liberian emigration as a means to end slavery and those who viewed black exodus as a way to preserve it. From the late 1840s into 1850s, white Virginians again divided; those in the Shenandoah Valley supported emigration as a strategy to dilute the electoral clout of the Tidewater region, whereas those in the plantation districts, after the Fugitive Slave Act of 1850, again pushed free blacks to plan their future outside the United States.

The first two-thirds of this book focus on white Virginians, relegating people of color to serve primarily as objects of white action. In the last three chapters, Tyler-McGraw looks at the world that black Virginians made for themselves in Liberia. To some degree, they succeeded in
re-creating an American-style republic on the African coast. In the end, however, Tyler-McGraw concludes that black and white aspirations to build American institutions and spread Western civilization to Africa ultimately failed; the little Republic of Liberia emerged relatively isolated from both the West and black Africa. The tragedy of Liberian history may be that the settler elite never assimilated with the indigenous people of the area, as indicated by the coup of 1980, which toppled a government run by descendants of the American settlers.

Kenneth C. Barnes
University of Central Arkansas


We are living in a Golden Age of research and publication about the minority peoples of China’s borderlands, including the history of their relationships with China’s ruling dynasties and their reactions to the intermittent pressures of Chinese (Han) military garrisons and civilian settlers. Herman’s new book makes accessible for the first time the long story about how a portion of present-day Guizhou province in southwestern China, heavily populated by—and, until 1701, politically dominated by, an ethnic group known nowadays as the Nasu Yi—came under China’s control.

At great length, Herman successfully challenges the idea, set forth in Herold Wien’s influential China’s March to the Tropics (Hamden, Conn., 1954), that the indigenous peoples of the southwest were simply overwhelmed by the advanced technology and the Confucian civilization that land-hungry Chinese immigrants brought to them. By making fuller use of Chinese documents, as well as some Nasu Yi sources recently made available, Herman shows two things: first, that the incorporation of Guizhou into greater China was violent, the Yuan, Ming, and Qing states playing major roles in conquering and annexing that territory; second, that far from being overawed by China, the Nasu Yi boasted effective technologies and a script-based civilization of their own, and were never “swamped” by their big neighbor.

Herman’s narrative is valuable. Just because he does not encumber his text with postmodern or other fashionable theory, or engage the approaches of the social sciences, does not mean that he cannot deliver a sharp picture of the military and political interactions of the Nasu Yi (and various other southwestern peoples) with China. What the book lacks in interdisciplinary entanglements it more than recoups in vividness and detail. Real-world scenarios such as this one can offer evidentiary challenges to existing theoretical and social-science perspectives.

On the downside, Herman does not make a point of discussing the
size or range of the Nasu Yi archive, or the provenance or reliability of
the texts, and he seems to use modern Chinese translations rather than
the original Nasu Yi materials. Modern scholarship has developed an
understanding of the nature and limitations of the copious premodern
Chinese written sources but not of the Nasu Yi sources. Though the
flaw is not fatal, Herman’s blind acceptance of the Nasu Yi historiogra-
phy unfortunately leaves unsettled the issue of whether and to what ex-
tent the Nasu Yi had created a genuine “civilization” relative to China’s.
How widespread was its literacy? To what uses was its script put? Who
wrote and who read Nasu Yi texts and documents? Was native Nasu Yi
administration as dependent upon paper flow as China’s was?

Furthermore, Herman might have considered a few roughly analo-
gous cases in world history for the purposes of comparison—dominating
powers encountering smaller but literate, and by no means backward,
peoples. England and its Celtic fringe, Russia and the Kalmyks, or the
United States and the Cherokees spring to mind. A look at such cases
would have helped to create a clearer understanding of China’s behavior
and of Nasu Yi reactions.

John W. Dardess
University of Kansas

Communalism, Caste and Hindu Nationalism: The Violence in Gujarat. By
$85.00 cloth $29.99 paper

Ironically, Gujarat, the home of Mohandas K. Gandhi, has been vital for
the growth of Hindu communalism in India since the 1980s, having be-
come the site of recurring communal violence against both Muslims and
Christians. Now a nerve center for the Hindu nationalist movement, it
is known as the “Hindutva laboratory”—Hindutva meaning “Hindu-
ness.” In February 2002, government officials and police at every level
were complicit in allowing, even encouraging, the massacre of Mus-
lims in a number of areas. Many of these authorities had ties to a group
of extremist Hindu organizations known as Sang Parivar, the most
prominent organizations of which were the Rashtriya Swayamsevak
Sangh (known by the dreaded initials rss), the Vishva Hindu Parishad,
and the Bajrang Dal. The political face of the Hindutva movement, the
Bharatiya Janata Party, was founded in 1980 as a reconstructed Jana
Sangh Party, which had been created by Shyama Prasad Mookerji in
1951 under the rubric of “one country, one nation, one culture, and the
rule of law”—the “one” being Hindu.

This recrudescence of extreme Hindu nationalism has surprised
many people and led to a growing body of work aimed at explaining it,
including Shani’s. Communalism, which has a long history in India, is
normally understood as the sectarian conflict between Hindus and Mus-
lims. Shani, however, questions the sectarian nature of the conflict. Her argument is that caste conflicts within Hinduism fostered communalism in the 1980s and 1990s and exacerbate the anti-Muslim feeling and violence. Hindus, for example, flock to Hindu revivalism on the grounds that the state has appeased the Muslim minority though a quota system at the expense of opportunities for Hindus. As Hindu parties gained power and communal violence between Hindus and Muslims escalated, caste conflicts between the upper castes and the lower castes increased dramatically. The Hindutva rhetoric about the appeasement of Muslims and the threat to Hindus resonates deeply with upper-caste and urban middle-caste Hindus, who were also exceedingly anxious about employment and education advantages (reservations) given to lower- and backward-caste Hindus. They see the inferior castes and the Muslims as a single undifferentiated threat, but the Muslims are the most readily identifiable enemy, and the easiest to attack.

Shani supports this theory through a case study of the large-scale riots of 1985 in Ahmedabad, where she traces the unprecedented anti-Muslim communal violence to a rift among Hindus. Shani develops this thesis in three stages: (1) by examining the background of the politics of reservations and caste; (2) by analyzing the course of the Ahmedabad riots of 1985; and (3) by recounting the creation of “ethno-Hinduism.”

Shani makes her case with telling detail. The extensive bibliography is an excellent guide to the study of communalism and of ethnic competition and violence in India. Hindu-Muslim conflict in India is the result of a complex mix of local circumstances and national influences. The overarching lesson is that the responsibility of political leaders, including those at the highest level, is paramount. When these leaders harness the power of the state for racist and murderous purposes, the result is a disgrace to humanity.

Roger D. Long
Eastern Michigan University