Book Reviews

ANCIENT AND MEDIEVAL

The Local Merchants of Prato: Small Entrepreneurs in the Late Medieval Economy. By Richard K. Marshall. Baltimore and London: The Johns Hopkins University Press, 1999. Pp. xvi, 191. $42.50.

This modest book examines the world of petty entrepreneurs in a late medieval Italian town. It is based on a rare collection of 46 account books from small tradesmen in the archives of Prato, a town better known for the extraordinarily rich archive of business and personal records left by the wealthy merchant Francesco Datini. The author was guided in his study by Richard Goldthwaite, a leading expert on late medieval account books. The ledgers provide a look at a little-studied social and economic group. They are especially revealing because they were used not only for business entries but also to record family events, including births, deaths, and marriages. As Marco Spallanzani points out in a preface to the book, most scholarship on medieval tradesmen centers on guilds and guild members. The people represented in these ledgers operated outside of economic corporations. They were not a homogeneous group but included a thirteenth-century moneylender, fourteenth-century druggists, cheese sellers, cloth sellers, a tailor, a doublet maker, a grain seller, a secondhand dealer, a broker, a cloth shearer, butchers, wallers, paper makers, and a family of innkeepers who were responsible for seventeen account books. As Marshall notes, the business atmosphere was casual, and this list is a bit deceptive because people's stated occupations were not always how they made their money. One man named as a second-hand dealer in fact used a broker to trade commodities, including 318 pounds of Sardinian cheese, bought one day and sold at a profit the next.

This is a careful and useful descriptive study. It holds few surprises. After an introductory look at Prato and the nature of the account books, the author draws on the ledgers to describe their way of life. Some had meager resources, but on average their incomes allowed them a varied diet, adequate household goods, clothing, and even a few luxuries like silk or squirrel fur. Assessments of their net worth are speculative. Like wealthy merchants and bankers, they were quick to invest in farms and urban rental properties. It is not obvious how they accumulated their capital, a question that has been debated for elites as well. Marshall reasonably suggests that some were investing their wives' dowries. These tradesmen also liked to keep large amounts of cash on hand.

Much of the book examines aspects of the conduct of business, including useful sketches of the roles of a petty broker, termed a *sensale*, and the operations of a successful inn. The entrepreneurs of Prato did not use notarial contracts but rather recorded their own accounts in single-entry ledgers, calculated in money of account. This suggests a high degree of literacy and mathematical competency. Business was conducted in a thoroughly monetized local economy. Marshall stresses the reliance on credit in daily transactions, though it should be noted that cash sales were not recorded in the ledgers and thus are not easily recoverable. Day-to-day reliance on credit was a way of life for the whole society. "Credit based on trust," he writes, "that same *fiducia* that Federigo Melis so passionately insisted was the essential element in merchant society at the highest ranks—sealed the close relations of these more modest operators with people of all walks of life . . ." Marshall argues that these shopkeepers, not pawnbrokers, were the primary source of small loans for the Pratesi. Credit purchases and loans were most commonly secured by third-party guarantors. He calculates that while collection was often slow, collection rates were high: the average rate for credit purchases was 95 percent; that for loans approached 98 percent.

The author suggests finally that this look at a local economy can contribute to the history of the rise of modern deposit banking. Federigo Melis argued that the reliance on bankers' lines of credit was a crucial development. Marshall finds that by the last decades of the fourteenth century the tradesmen of Prato, who had long operated within a climate of local *fiducia* and credit, began to turn to merchant bankers to establish lines of credit in order to facilitate purchases from distant suppliers.

CAROL LANSING, *University of California, Santa Barbara*

MODERN EUROPE

Fifty Years of the Deutsche Mark: Central Bank and the Currency in Germany since 1948. Edited by the Deutsche Bundesbank. Oxford: Oxford University Press, 1999. Pp. xxvi, 836. $90.00.

This volume was written in commemoration of West Germany's postwar currency reform and its product, the Deutschmark. In June 1948 the U.S. Army distributed a new currency in the Western occupation zones of Germany. These banknotes, which had been printed in the United States, carried no signatures and made no mention of an issuing authority. But they did carry the name *Deutsche Mark*. Seldom has a military initiative created such a successful brand name, but that is not the theme of this book. Its purpose, rather, is to burnish the Bundesbank's once-formidable reputation as an inflation fighter. As the European Central Bank is taking a decidedly different course, the magic of the Bundesbank is vanishing at an amazing speed. The Bundesbank has been downgraded to a mere member bank, has suffered severe budget cuts, and has had to lay off half its personnel. Meanwhile, those who remain regard the new system with apparent bewilderment and sometimes outright fury.

This volume is actually the third *festschrift* issued by the Bundesbank. The first, appearing in 1976 to commemorate the centenary of the Bundesbank's predecessor, the Reichsbank, was a collection of very carefully edited essays (*Währung und Wirtschaft in Deutschland 1876–1975*. Frankfurt/M.: Knapp, 1976) plus a statistical companion volume (*Deutsches Geld- und Bankwesen in Zahlen*. Frankfurt/M.: Knapp, 1976). The second, published in 1988, celebrated forty years of the Deutschmark with an updated data set for postwar Germany (*40 Jahre Deutsche Mark. Monetäre Statistiken 1948 bis 1987*. Frankfurt/M.: Knapp, 1988). Given the recent fate of the Bundesbank and the imminent disappearance of the Deutschmark, the present, third *festschrift* may well be the last. It combines a rather weighty volume of essays with a CD-ROM, which is a further update of the Bundesbank's data on West Germany.

Harold James provides a historical retrospective on the Reichsbank (1876–1945), commenting also on the role of the notorious Dr. Schacht in financing Hitler's war preparations. James argues quite convincingly that except for the brief 1924–33 interlude of the gold standard, the general theme of German central banking was a religious belief in a radical version of the banking doctrine. The credit expansion of the early 1930s was thus not just an Schacht's invention, but rather followed a firmly established intellectual tradition within the bank (of which, lest it be forgotten, the hyperinflation formed an integral part).

In light of this background, the saving grace of the new currency was its non-German origins. After World War II, Germany remained under military administration of the four Allied powers, and attempts to establish joint administrative bodies soon fell victim to the emerging Cold War. Attempts by the United States in 1947 to overcome the deadlock and establish separate structures in the Western zones included preparations for currency reform, which was seen as the only way to cope with the monetary overhang created by the Nazi war debt. For this, a separate central bank (called the "Bank deutscher Länder") was created, an act that both manifested and deepened the political division between East and West. Christoph Buchheim presents a chapter on the institutional background of the 1948 currency reform, which provides a meticulous review of the scattered literature on the reorganization of central banking prior to that date. Indeed, it is notable that currency reform and a unified central bank preceded political reform in the late 1940s; the Federal Republic was established a full year after the Deutschmark had been introduced.

Carl-Ludwig Holtfrerich reviews monetary policies during the Bretton Woods era. He views German monetary policy in the 1950s as having been led by traditional credit-expansion doctrines; these, he argues, clashed with the need for monetary stabilization beginning in the middle of the decade. This appears to be a minority view, though. A more standard interpretation might be that German monetary policy in the 1950s was merely reacting to increasing capital mobility, and that it did so in a roughly consistent way. But clearly, if there were lessons to be learned for what later became the Mundell-Fleming model, West Germany provided a case in point. From the mid-1950s onward, the country experienced growing capital inflows, and whatever monetary sterilization measures were attempted proved to be counterproductive.

Among the many other contributions in this book, which cover everything from the legal aspects of Bundesbank independence to the policies of monetary unification in 1990, one that this reviewer particularly liked is a thoughtful essay by Jürgen von Hagen on the Bundesbank after the collapse of Bretton Woods. Being a monetary economist, Von Hagen sets out to find archival evidence supporting the view that central-bank behavior is consistent with Taylor-type rules and that internal decision processes can be explained by the board members' differing sensitivities toward unemployment along party lines. This is an interesting experiment, and the result is actually a very nice piece of monetary history. Contrary to his original intention, Von Hagen ends up dismantling one myth after another, ranging from money-base targeting to any kind of rule-based decision making whatsoever.

The picture that emerges of the Bundesbank in the critical 1970s is that of a battered institution, trying to recover from the Bretton Woods disaster and to overcome its internal divisions by creating a new public image. It was only in this era that the Bundesbank established its reputation as a committed inflation fighter. In retrospect, it is noteworthy that large parts of Germany's monetary history after about 1980 consisted in the Bundesbank's attempts to punish finance ministers for their fiscal laxity by tightening its monetary stance. Given that German national debt has about tripled since 1980, the ultimate success of these policies amounts to closing the barn door after the horses have escaped, but that is another matter.

In all, this *festschrift* provides the reader with a very rich account of West German monetary history. Taken together with the two earlier volumes and the companion statistics, the Bundesbank has certainly achieved one goal: to document its legacy and provide some of the best material in German historical statistics.

ALBRECHT RITSCHL, *University of Zürich*

Women, Gender and Industrialisation in England, 1700–1870. By Katrina Honeyman. New York: St. Martin's Press, 2000. Pp. viii, 204. $59.95.

The process of industrial change energized generations of historians to explain the transformations that reformed the modern world. Over much of the twentieth century, historians of Marxist and liberal persuasions staged mighty contests to define most persuasively the effects of factory production, work discipline, class, and capital formation. The sometimes triumphalist chronicle of factory reform, capital accumulation, and trade-union growth were painted across the nineteenth century; more detailed studies of communities, corporations, and institutions had to fit in as best they could. Until the 1970s, as graduate students signed up for one or the other of these interpretative camps, few authors, liberal or Marxist, considered the experience of women. Adding women's history and gender analysis to the study of industrialization has brought about one of the most significant transformations of this chronicle. Katrina Honeyman's most recent contribution stands as an essential text for those teaching and working in this field.

Honeyman summarizes and interprets the considerable output in this field, with due attention to recent and important contributions. This is a work of creative synthesis, in which compelling claims are made for the centrality of gender in the making of the industrial revolution. "Equally, industrialisation was important in the making of gender" (p. 146). But not all women, or all elements of gender, are assessed in this volume. The middle class—its creation, character, or the function of gender therein—is not considered. Honeyman focuses exclusively on working-class experiences and the differential functions of gender among laboring men and women. She follows in the tradition of Ivy Pinchbeck, Sally Alexander, Angela John, Edward Higgs, Sonya Rose, and Pamela Sharpe by focusing on women's working lives. These are assessed against the shifting and often conflicting priorities of working-class men, and the changing patterns of industrial and agricultural production. Working-class men struggled to preserve their standing against mechanical deskilling and the stigma of domestic equality. Growing male exclusivism in the trade-union movement was tied to a rising distaste for formal female employment, determining women's working opportunities and collective experiences.

The volume begins with a survey of feminist historiography, set against the interpretations of the industrial revolution before and after historians put gender at the forefront of their research agendas. The results of this latter research, explored in the subsequent chapters, make clear that neither industrialization, class, nor gender norms can be fully understood without reference to their interaction over this era. Honeyman adheres to traditional chronologies, identifying the greatest changes and most significant outcomes with the late eighteenth century and the first half of the nineteenth. This choice leads to a periodic overemphasis on changes in that era, at the cost of neglecting their far deeper roots. In this context, the characterization of the eighteenth century is slightly less satisfying, even while the economic contributions of women are set in place.

The classic era of the industrial revolution is one of the principal foci of this volume, as too are the themes of earlier generations of historians. But these themes and the industrial revolution itself are substantially reformed. Honeyman notes that there is no one experience characteristic of all laboring women. Maxine Berg and Pat Hudson have shown the importance of regional and time-sensitive studies to ensure a balanced interpretation of women's experiences. Some women benefitted, for a time, from the introduction of new industrial techniques, working with machines not yet gendered male; young single females, in particular, found an abundance of better-paid employment. Women also sweated in the home, performing tasks adjunct to factory production. Honeyman traces the variables at play over this era, even as she explains the major shifts in opportunities and expectations that increasingly defined gender roles among the working class.

Skill became a central element of contested gender norms. Productive skills were increasingly seen as male attributes. I would argue with her on the timing of these events; nonetheless, Honeyman effectively summarizes the general consolidation of these views and incorporates the important new work of Anna Clark, *The Struggle for the Breeches:*

Gender and the Making of the British Working Class (Berkeley: University of California Press, 1995). Honeyman, highlighting the work of Clark and others, paints a subtle image of industrial society, where men's and women's roles take on ever-different hues: women's work is more informal and invisible, men's work is center-stage in trade and political movements. Class-based priorities of working-class men and women had, at times, antagonistic aims over this era. The characteristics of both work and class receive detailed reevaluations in the light of new gender analyses. Our understanding of industrialization and the industrial working class is transformed as a result.

BEVERLY LEMIRE, *University of New Brunswick*

An Economic History of Sweden. By Lars Magnusson. London: Routledge, 2000. Pp. xvii, 305. $70.00.

This book is an English translation of the abridged edition of Professor Magnusson's grand survey of Swedish economic history. The latter has emerged as the standard introductory text on the subject within Sweden, and this book is likely finally to replace Eli Heckscher's venerable work of the same title (actually a translation of a "popular" work first published in 1957) as the Anglophone's introduction of choice to Swedish economic history. In exchange for later coverage and a more up-to-date approach, however, the reader will have to forego any discussion of the period before 1750. While Magnusson's Swedish text covers earlier centuries, this material was apparently deemed inessential for Anglo-Saxons. Unfortunately, there will also be some loss of reading pleasure. Although certainly competent and understandable, the present translation is excessively mechanical. The book's Swedish origin is apparent, without, however, preserving the smooth flow of Magnusson's prose. Nor is it up to the—admittedly very high—standard established by Goran Ohlin (with the active participation of Alexander Gerschenkron) with his translation of Heckscher.

The book opens with a chapter on the Swedish agrarian "revolution" of the eighteenth century. Although rather slow for a revolution, the growth of agricultural incomes plays an important role in Magnusson's scheme of things. Compared to the great emphasis placed on export demand by earlier generations of Swedish economic historians, he argues that Swedish industrialization was, to a much greater degree than previously thought, the result of growing domestic demand. This demand in turn was, at least to begin with, the result of increased agricultural productivity. Magnusson's emphasis on the positive role of the government, especially the central government, can also be seen as revisionist. It was not just a matter of abolishing mercantalistic regulations and letting the free market take off. Rather it is was a question of replacing old regulations and rules of the game— "institutions," in modern usage—with new ones. In addition, the government played a major role in financing infrastructure investment, education, and research.

I believe most readers will find the last third of the book to be particularly interesting. It provides an excellent discussion of the rise of Swedish "welfare capitalism" and the welfare state, as well as the rise and fall (?) of the so-called Swedish Model. This corporatist model, based on cooperation among national organizations representing workers and employers, as well as various national and local government bodies, is often seen by Swedish social scientists as the country's most distinguishing feature. For a considerable period following the end of World War II, it was perceived to be highly successful. Indeed, it was considered suitable for export. Sweden's increasingly obvious and painful relative economic decline of the past quarter century, however, has led to it being first questioned and then undermined.

When it comes to the analytical foundation of the book, Magnusson is just as thoroughly modern as Millie—at least Scandinavian modern. This is reflected especially in the emphasis on now-fashionable institutional theory. This, let me hasten to add, is not the institutional theory based on profit-maximizing behavior that has evolved out of classical price theory. Rather it is the highly general version associated with the more recent writings of Douglass North. Causality in this approach, however, is so complex that it seldom, if ever, yields testable predictions. Thus it is largely limited to rationalizing developments after the fact. While no doubt useful and sometimes productive of insights, the theory has a certain mushy, "anything goes" aspect. Simply put, it can never be wrong.

In my view, a good example of this problem emerges from the discussion of the positive role of the government in industrialization, particularly its role in creating new "institutions." It is certainly true that modern industrialization requires the creation of new institutions, not just the abolition of old and inefficient ones. It is also true that governments have played, and almost certainly had to play, a major role in this process. Still, there is a pronounced tendency to conclude that virtually all government actions accompanying successful economic modernization must have contributed to that process. There is little sense that alternative actions by the government might have been even more helpful. Even less appreciated is the possibility that sometimes no government action, leaving problems to be resolved privately, might have been better yet. After all, it is possible that some economic advances occurred despite, rather than because of, government intervention. I believe this is an area where further work, especially that involving theoretically based, quantitative analysis, could be of great value.

Such carping aside, however, this book undoubtedly is the best introduction to Swedish economic history now available. If I do not particularly like the spirit of the times, that is my problem. Maybe the next phase of the intellectual fashion cycle will be more to my liking?

LARS G. SANDBERG, *Ohio State University and the University of Uppsala*

The Economy of Obligation: The Culture of Credit and Social Relations in Early Modern England. By Craig Muldrew. London: Macmillan, 1998. Pp. xvii, 453. $69.95.

Craig Muldrew has written an imaginatively conceived and richly researched study of the meaning and practice of "credit" in early modern England. "Credit" today is understood almost exclusively in value-neutral terms, and applies principally to functional economic activities. Directed towards individuals, it refers primarily to their financial assets and their capacities to assume interest-bearing debt. This meaning was only entering into use in the later sixteenth century; it became common only in the later seventeenth. "Credit" in the medieval and early modern era referred paradigmatically to a person's *moral* worth, as this book abundantly demonstrates.

In portraying a social environment increasingly dependent on complex commercial and credit relationships, Muldrew perceptively opens this older world of meanings and practices to reveal the multifaceted pressures it faced as market-oriented activity assumed greater importance in public and private affairs. He also goes far in suggesting how by the end of the seventeenth century a new contract-based conception of sociability, emphasizing individual agency and equality of rights, had emerged from within the prevailing communal and hierarchical social order. Based on interpretations of literary works, social commentaries, law cases and legal texts, and archivally-based case studies of economic practices and debt litigation, his book provides a model of effective interdisciplinary scholarship.

To be a man or woman "of credit" in the early modern era was to be deemed honorable, to possess a good name and a reputation for honesty in word and deed. Credit, in this understanding, meant that the person was judged of virtuous habit and character, someone who could be trusted to meet obligations. This confidence was among one's principal assets; it affected one's capacity to relate to others of any rank or station and in every aspect of life, private and public. It was also widely understood to be essential to social adhesion—one of the principal bases of human sociability. Trust is a complex category, grounded as much in action as in character. We extend it to others in expectation that they will successfully meet their obligations, which requires a combination of good intentions with effective performance. To trust someone, therefore, we must have confidence not just in the person's honesty, but also in his or her capacity to do what has been promised in the given circumstances.

Under prevailing economic conditions, these features of trust made individuals critically dependent on one another for the performance of their own obligations. Coin was scarce and normally changed hands to clear debts in commercial transactions only after many months; most parties engaging in market exchange were simultaneously creditors to some of their fellows and debtors to others. In consequence, the confidence a debtor received from his creditors was vulnerable not just to his own misfortunes, errors, or personal failings, but to those of persons in debt to him. These circumstances made each party to a transaction in effect an equal agent in juridical terms, whatever their differences in wealth and status. But they also made it essential that the parties defend themselves against any threats to their credit, lest they lose their capacity to form bonds of obligation with others in and out of the market. Muldrew stresses the consequences of these structural features on the incidence of debt litigation. As market exchange grew in scope, scale, and importance in the sixteenth century, there was a marked increase in the number of cases for debt or breach of contract; it remained high into the early eighteenth century. In some urban places, the average was more than one suit per household per decade between about 1550 and 1650. These findings are consistent with, and might go a long way toward explaining, evidence of a similar growth in defamation cases over this same period, as reported by David Underdown and others.

Although Muldrew does not make a special point of the fact, a large portion of debt cases introduced into borough courts failed to reach a final judgment. The records themselves may be partly at fault; Muldrew gives a highly informative account of their limitations. But, as he acknowledges, it is also likely that a number of these cases were settled early in the process. This suggests that plaintiffs often looked to local courts not as a means to win a final victory over defendants, but as a way to bring more informal processes of dispute resolution into motion, while holding the sword of justice in reserve should they fail. Given the disparities of social rank frequently evident in these cases, the practice arguably was a way to call on the law to level the ground in negotiations between creditors, who might be mere shopkeepers or handicraftsmen, and their debtors, who often were their social betters. In his conclusion, Muldrew focuses on Hobbes's understanding of the role of law, understood as sovereign command, in securing peace between competitive individuals in the new contract-based, market-oriented environment of the seventeenth century. Perhaps there is something equally to be said for the role of the legal process itself, and of the social interchanges it encouraged within communities, in mediating between the state and civil society as these two realms became differentiated from one another during this period.

In this complexly argued and densely written book, Muldrew sets "deconstructing capitalism" as his motivating aim (p. 1). It is a large and daunting task, but worth the effort. All readers of his book will be grateful to him for undertaking it, and for the insights he has so far achieved.

DAVID HARRIS SACKS, *Reed College*

The Mediterranean Response to Globalization before 1950. Edited by Şevket Pamuk and Jeffrey Williamson. London and New York: Routledge, 2000. Pp. xvii, 430. $115.00.

Divergence, rather than convergence, has been the name of the game in worldwide "modern economic growth," at least up to the 1980s. This empirical observation no longer puzzles economists, as it did thirty years ago. In the last two decades, we have taken a more sophisticated approach to the issue of the long-run relative performance of pioneers and latecomers, of core and periphery, of rich and poor countries. If we are currently able to talk of "conditional convergence," "convergence clubs," and the like, this is the result of a tremendous cross-fertilization between the so-called "new growth theory," the pioneering work of Moses Abramovitz and a few others, and new approaches in global economic history. Amongst the latter, the monumental research program of Jeffrey Williamson stands in a class of its own. Thanks to his intellectual inputs and coordination efforts we have rediscovered the so-called "first globalization" and the backlash against it, and are thus better equipped to understand the causes and effects of the current globalization wave, and thus produce plausible scenarios for its future course.

A great deal of the work of Williamson and his associates has so far focused on the core Atlantic economy, an early "convergence club." Now, thirteen essays—collected and edited with Pamuk—focus on the response of the Mediterranean periphery to the challenges of both pre-1914 globalization and its subsequent reversal.

The individual chapters in this book contain interesting, if not entirely new or unexpected, findings. The region's mid-nineteenth-century retardation is confirmed (Williamson, Reis). Booming international trade is shown to have had the expected effects on cash-crop specialization (Akarli, Ramon-Munoz, Morilla Critz, Olmstead and Rhode, Metzler) and on the growth of maritime trade (Harlaftis and Karadasis). Pre-1914 Italian protectionism is yet again found to have been relatively mild (Federico and O'Rourke). It is intriguingly argued that exchange-rate depreciation hampered Spanish emigration (Sanchez-Alonso). Other chapters tell us that a well-conceived autarky was a good second-best strategy for sustaining Turkish growth in the 1930s (Pamuk), that institutions matter (Foreman-Peck and Lains), and that labor efficiency was the main determinant in choice of technology in the cotton textile industry prior to 1860 (Ramon Rosés).

The essays collected by Pamuk and Williamson represent a state-of-the-art sample of research in nineteenth- and twentieth-century Mediterranean economic history. They make available in the *lingua franca* a harvest of data and findings about an area of extraordinary historical and economic interest, as yet not adequately investigated in the international literature. Still, the book fails to answer—and in the case of most chapters even to address—Williamson's introductory questions about the area's unsatisfactory convergence to the core during the so-called first globalization. This is, of course, nobody's fault: it merely reflects our current stock of knowledge in the field. Williamson's own chapter, therefore, rightly concludes by proposing a research agenda sufficient to keep a generation of economic historians busy. And yet a useful perspective seems to be missed there. After the Second World War, the European side of the Mediterranean—both market and socialist economies—caught up with the "core" Northwestern powers remarkably quickly, soon acquiring full membership in the European "convergence club." What prevented a similar process from taking place a century earlier? What was missing then that was present now? It is likely that only by understanding the factors responsible for the strong recent convergence we shall be able to shed significant light on the previous relative weakness thereof.

GIANNI TONIOLO, *Università di Roma Tor Vergata and Duke University*

British Society 1680–1880: Dynamism, Change and Containment. By Richard Price. Cambridge: Cambridge University Press, 1999. Pp. 349. $59.95, cloth; $22.95, paper.

How long is a century? In British history, the answer is rarely a hundred years. The last two decades of historiography have extended the eighteenth century deep into the nineteenth. In his new book, Richard Price both develops and modifies this tendency by suggesting that the two centuries from 1680 to 1880 constitute a single phase in the broad sweep of British history.

As a general history, the book inevitably has its inclusions and exclusions. These are signaled by the starting point. 1688 looms large in Price's story. Its importance has been less apparent in other synthetic studies of the period. Linda Colley (*Britons: Forging the Nation, 1707–1837.* New Haven: Yale University Press, 1992), for example, begins with the Act of Union linking Scotland to England and Wales. Price acknowledges that his is not a work of four-nations history, though his claim that this simply reflects the historiography should be met with scepticism. Price's purported focus is Britain, but the unit of analysis is basically England. The neglect of Ireland seems odd, particularly when a strong chapter on the ambiguities of free trade omits the Irish famine completely.

Price builds on established trends in economic history to argue that Britain remained a small-scale manufacturing economy through to the 1870s. Britain's commercial predominance was established by the early eighteenth century, and was apparent in the importance of the reexport trade to the balance of payments (p. 61). Industrialization emerges as a primarily regional phenomenon (p. 47) that did not disrupt the existing patterns of economic life. Labor, capital, and credit markets remained localized throughout the period (p. 49), but effective linkages existed between the differing sectors. These were disturbed after 1870 by the rise of industry in Britain and abroad, the revival of protectionism on the Continent, and the new imperialism. Lack of integration between finance and industry appears as an essentially twentieth-century problem (pp. 86–87).

The transition towards a free-trade regime in the 1820s is not portrayed as a caesura. Price emphasizes the importance of war in promoting British commerce (p. 62) and, with telling brevity, the role of the state in regulating domestic and imperial labor markets (p. 64). Free trade and protection should not be seen as mutually exclusive, since individuals often combined both perspectives (p. 97). Furthermore, adherence to both was informed by the accepted need to expand trade. Conflict over trade policy reflected the differing perspectives of particular interest groups. Price tends to treat economic interests as self-evident rather than constructed, though he gestures towards the importance of evangelicalism in fostering a free-trade political culture. Price regards localism as dominant throughout the period. Parliament thus "mirrored the provincialism of local priorities" (p. 157). This view has much to commend it, but it underestimates the impact of 1832 in rejuvenating Parliament and in encouraging pressure groups, especially nonconformists, to lobby its members. The boundaries between state and civil society reflected the resilience of localism by limiting the role of the former and upholding the importance of the later. Feminist challenges to the policing of prostitution from the 1860s asserted the autonomy of civil society, but, through their complicity with moral interventionists, ironically succeeded in expanding the remit of the state and diminishing the scope of civil society.

While the limited impact of 1832 on the electorate is emphasized, 1867 emerges as a significant moment, presaging the advent of a nationalized political culture and the demise of rowdy traditions of community politics. In the light of Jon Lawrence's work (*Speaking for the People: Party, Language and Popular Politics in England, 1867–1914*. Cambridge: Cambridge University Press, 1998), this irenic view of late Victorian political culture fails to convince. If for Price the close of the nineteenth century sees the emergence of twentieth-century politics, it also witnesses the arrival of twentieth-century class relations. Price is confident that class consciousness existed in eighteenth-century Britain, but argues that it is only from the 1880s that institutional forms permitted class relations, as society came to be understood in terms of collectivities rooted in economic realities.

British Society is a stimulating book that deserves a wide readership. However, its argument about the continuities between 1680 and 1880 depends on important omissions, and does not seem equally applicable to all of its themes. The greater visibility of nonconformity from the 1780s did much to shape Victorian politics, culture, and society, as Price acknowledges in passing (p. 275). The forms of nineteenth-century politics importantly persisted into the twentieth century, as free trade nicely exemplifies. This remains, though, a brave and striking attempt to reperiodize British history, one which demonstrates the value of reconsidering accepted chronologies.

JAMES THOMPSON, *Cambridge University*

Education and Economic Decline in Britain, 1870 to the 1990s. By Michael Sanderson. (New Studies in Economic and Social History Series.) New York: Cambridge University Press, 1999. Pp. viii, 124. $39.95, cloth; $11.95, paper.

Michael Sanderson, author of several influential books on British education and industry, is the general editor of the series in which this volume appears. It admirably fulfills the aim of the series—to survey the current state of scholarship for students and their teachers—by reviewing an extensive literature on British schooling and its relation to the economy in seven tightly organized, substantive chapters. If a succinct overview of this work is what you need, this is the book to get.

Three themes, crudely summarized, suffuse the volume. First, the adherence to gentlemanly values and the liberal arts has persistently shaped British schooling by relegating economically meaningful education to the periphery of the educational system. Efforts to build technical and vocational programs have repeatedly succumbed to entrenched attitudes and interests. Second, the apathy of employers towards technical and managerial education has further undermined technical–vocational initiatives by limiting the demand for graduates. Labor unions have reinforced these tendencies by seeking to monopolize and limit training for the benefit of their members. Third, the British educational system has been insufficiently meritocratic, restricting the access talented people from the lower social classes; insufficiently fair, spending a disproportionate share of educational funds on higher education; and insufficiently market-oriented, funneling too few elite graduates into business and industry.

Sanderson documents these leitmotifs with a subtle discussion of British schooling from 1870 to the 1990s. At the same time, he tempers excessive contemporary and subsequent scholarly disparagement of pre-1945 technical education and workforce development in Britain by pointing out the sustained role of apprenticeships and the proliferation of technically-oriented colleges, institutes, and secondary schools in these years. Chapter 1 reviews the growth of literacy and schooling through 1914, a period during which—in contrast to the United States—British child labor declined. Chapter 2 summarizes the literature on Victorian and Edwardian Britain's deficient technical–vocational programs and their role in economic decline. Chapter 3 accesses it. This work explicitly identifies Germany as the exemplar against which British educational practices pale. Extending the critique into the interwar and postwar years, Sanderson implicitly does the same in chapters 5 and 6. He devotes the remaining two chapters to elite education. Chapter 4 highlights

Oxbridge and the public-school tradition between 1870 and 1914. Chapter 7 broadens the perspective, while extending the analysis into 1990s. Both chapters stress the enduring influence of genteel, anticommercial values upon elite school culture.

Unfortunately, the nature of the series has foreclosed a more ambitious cut on the subject. Consequently, those who seek to understand the impact of education in the broadest sense on British economic fortunes will have to mine this volume with diligence and care. Reflecting the literature which it fruitfully summarizes, it focuses too heavily on schooling, at the expense of other forms of education and training (especially at work); it fails to situate skill needs and institutional growth sufficiently within a contingent historical framework (shifting labor demand, government priorities, and employer strategies arguably contributed more to educational reform efforts in the 1910s and 40s, and to their subsequent failures, than did ingrained social values); it does not give the economy its full due (its treatment is episodic and unsystematic); it pays too little attention to who governs educational practice and how (technical–vocational education worked so well in Germany because of employers' and workers' oversight roles within it); and it uses a comparative framework that is insufficiently comparative (one cannot usefully deploy Germany as a measuring stick without saying more about how the German vocational training and technical education systems work and why).

Analytically, this is an educational history in which the economy gets short shrift. Economic forces and educational practices interact far more dynamically than this volume implies, particularly through the labor market. The composition of the workforce changed dramatically between 1870 and 1990. Preoccupied with manufacturing, Sanderson neglects the secular shift to services and says nothing about Stephen Broadberry's contention that the loss of leadership in service-sector productivity has been key to lagging British fortunes. Business cycles, international competition, war, and other historical contingencies that altered the demand for skills—and, in turn, for education and training—receive less attention than they deserve. Still, as an introduction to the literature, the book provides a valuable service. This was, after all, its aim.

HAL HANSEN, *Suffolk University*

Merchants, Markets and Manufacture: The English Wool Textile Industry in the Eighteenth Century. By John Smail. London: Macmillan, 1999. Pp. x, 198. $65.00.

This is a strong contribution to a cultural interpretation of the dynamics of industrialization. Critical of quantitative debates and their limited insights into the processes of change, critical too of polarized views about the primacy of supply (production) or demand (consumption), Smail's study is directed at the ways in which merchants and merchant manufacturers fought to reconcile the *shape* of supply with that of markets, and thus brought about fundamental changes in the processes of production and the source and scale of profitability. Smail's central thesis concerns the shift, as he sees it, from a Smithian to a Schumpeterian economy: from activity based upon the extension of operations and growing sophistication in the division of labor, to an economy founded upon continuous product and design innovation and intensive technological as well as organizational change. His concern is with the cultural and economic conditions under which producers and merchants shifted from maximizing profits by manipulating the marketing system (through niche products and niche markets) to maximizing profits by changing the mode of production. He dates this shift, in the case of the woolens sector, from the middle third of the eighteenth century. Focusing upon the interplay between production and marketing systems, he carefully documents change in the four major regions of manufacture in England: East Anglia, Devon, the West Country, and West Yorkshire. He places particular emphasis upon Yorkshire, which quickly emerged as the most responsive region and which dominated the trade in the second half of the century.

England's wool textile industry in this period is not under-researched: regional shifts in markets, merchanting, and products have received attention, and innovations in organization and technology, particularly in West Yorkshire, have been the subject of a number of major studies. Many historians have attempted to explain Yorkshire's success, and in these analyses the closer integration between merchanting and manufacturing in the region than elsewhere has been recognized. Smail is clearly familiar with all this work and with the wider literature of debate, empirical and theoretical, on the nature of the industrialization process. His achievement lies in synthesizing what we know; adding new research on a varied and voluminous array of correspondence between manufacturers and merchants, memoranda, and business accounts; placing renewed emphasis upon the nature of links between each region's producers and their markets; and, finally, in demonstrating very forcefully how these links were vital both in creating and in foreclosing potential paths to growth. He argues that close connections between producers and their markets in the late eighteenth century, especially in Yorkshire, created an economic and cultural environment in which entrepreneurs were induced to focus upon changing production technologies as the main route to greater profitability. His argument is particularly strong and interesting in documenting the origins and implications of vertical disintegration (the separation of spinning from weaving) in the worsted trade, and the emergence of a separate marketing structure for yarn a decade or more before machine-spun yarn became available. In the woollen sector, again through the medium of stories of particular businesses and businessmen, Smail shows how changes in the mode of production owed as much to the activities of merchants and the volatility of markets as it did to the logic of the production process. Smail's important point, reminiscent of a Marxian argument about the shift from mercantile to industrial capitalism, is that this change involved a fundamental reorientation in the ways in which manufacturers conceptualized their economic activities. It certainly involved a wholly different attitude to the investment of fixed capital and to cost accounting, and Smail's argument would have been strengthened by more prominent reference to the literature on these subjects. Nevertheless, we are in Smail's debt for producing a cogent and well-researched argument that gets at the human relationships and exchanges which generate and shift both supply and demand curves.

PAT HUDSON, *Cardiff University*

Economics of Transport: The Swedish Case 1780–1980. By Thomas Thorburn. Södertälje, Sweden: Almqvist and Wicksell International, 2000. Pp. 589. SEK 422.

This is an important book, with implications far beyond the Swedish case. The author has painstakingly constructed a unique aggregate series of transport expenditures covering two centuries (the book contains 260 pages of appendices). He uses this information in a series of models that analyze the crucial role of transport improvements in Swedish economic development, with a special focus on industrialization.

As with all attempts to develop estimates of historical aggregates, Thorburn has some solid data sources, on railway freight charges for example, but must make estimates from shaky bases in other areas, such as foot travel or wagon carriage. This, along with the tendency of transport historians to specialize, helps explain why no such aggregate series have been constructed elsewhere. The author laudably faces these shortcomings directly, and provides a range of plausible estimates as appropriate. Rarely do different assumptions have a significant impact on Thorburn's key insights into the course of development of Swedish transport: for example, that the increase in transport's share of GDP from 6–7 percent before 1850 to 14 percent in 1980 was *entirely* due to a tenfold increase in the share of passenger traffic, whereas the share of goods transport remained remarkably stable; that "efficiency" (roughly, total factor productivity) in goods transport increased by a factor of 37 in the century before 1973; that railways caused the contribution of roads to total internal transport costs to fall from over 80 percent in 1850 to 40 percent in 1913, while having little impact on the contribution of water transport; and that trucks subsequently pushed the contribution of roads back above 80 percent (largely by dominating short-haul transport) in 1973, while reducing water transport to barely 2 percent (but still 17 percent of ton-miles).

Both Thorburn's data and his expertise (he has combined careers in academia and the shipping industry) are strongest for the modern era. And while he often draws on research concerning other European countries, he does not always provide international comparisons. I found his assertions that commercial road transport was insignificant before 1818, that travel times by land barely fell before 1913, and that costs of road transport fell by a meager 0.5 percent per year between 1780 and 1870, to be at odds with results from other countries showing considerable improvements in roads, horses, and coaches over that period.

Thorburn emphasizes that both organizational and technical changes were important to transport improvement. In particular, he identifies some key advantages in the development of common carriers to replace part-time carriage by farmers: specialized staff, decreased time in terminals, and increased numbers of trips. There were further benefits to customers: a drop in transactions costs as fares were posted, increased confidence associated with regular schedules, and the legal liability associated with common-carrier status.

A little more than half of the text is devoted to an examination of the economic impact of transport. Thorburn details how transport improvements halved the cost-of-living disadvantage of urban areas in the century before 1913, and erased it thereafter; Swedish urbanization would otherwise have been severely limited. He is unable to estimate precisely transport's impact on industrial scale, but he provides evidence that it was substantial (though variable across regions). Laudably, he devotes a chapter to the distribution of consumer goods, and concludes that consumer goods could not be produced on a large scale in any sector until appropriate transport systems were ready to distribute them (speed, frequency, and reliability were of different relative importance to different sectors). In particular he details how large-scale production began with goods of a high value-to-bulk ratio, and spread as transport costs fell. This is a crucial insight: despite the work of Alfred Chandler, economic historians all too often focus on production processes and neglect the impact which changes in distribution have on firm decisions.

Thorburn notes at the outset the danger of ignoring those effects of transport which are not easily quantifiable, but he does not always heed his own advice. He recognizes that increased scale encourages technical innovation, but says nothing of the additional stimulus flowing from urbanization, regional specialization (mentioned only briefly), ease of travel (facilitating the interaction of innovators and entrepreneurs), access to a wider range of raw materials, or even division of labor (which is not identical to increased scale). Thorburn estimates the cost of a slow transport system in terms of goods-in-transit, and finds it to be modest (except for imports); but he neglects the much greater inventory costs that would be imposed on firms by a slow and unreliable transport network (and would discourage centralized, capital-intensive production), despite noting that damage and theft of goods-in-transit was a major problem before 1850. He discusses the role of peddlers, but does not discuss the fact that as catalogues and common carriers replaced face-to-face sales, industry had to produce a highly standardized output, with critical implications for technology and organization.

Thorburn provides a masterful survey of the development of the Swedish transport system, and provides detailed analyses of its impact on economic development. The fact that yet more causal linkages can be identified should only serve to encourage further research in the area.

RICK SZOSTAK, *University of Alberta*

The Development of Modern Spain. An Economic History of the Nineteenth and Twentieth Centuries. By Gabriel Tortella. Cambridge, MA: Harvard University Press, 2000. Pp. xvi, 528. $49.95.

The publication of this book in English is good news. Gabriel Tortella presents a synthesis of Spain's economic history during the last two centuries, written with scientific rigor and didactic guidance. He expertly combines economic theory and general historical analysis to formulate his hypotheses and to develop his arguments. The present volume contains some modifications of the original Spanish edition, and Valerie J. Herr's translation is excellent. Its 16 chapters include numerous tables, figures, and maps, and are followed by an updated bibliography and a chronology.

The book devotes equal space to the nineteenth and twentieth centuries, and is the first general treatment of the modern economic history of Spain to do so; the works of Nicolas Sanchez-Albornoz, Jordi Nadal, and David Ringrose, by contrast, are dedicated only to the nineteenth century. Throughout, Tortella's approach is sectoral and thematic, including significant analyses of human capital, the entrepreneurial factor, money and banking, and the foreign sector. This is a work of economic history, but it makes reference also to special events in the political history of Spain: regarding the twentieth century, in particular, the Civil War and the Franco regime are adduced to explain the economic backwardness of the 1940s and 1950s in comparison to the European democracies.

The Development of Modern Spain represents an interpretation of Spain's industrialization that dissents from the findings of Jordi Nadal (*El fracaso de la revolución industrial en España, 1814–1913.* Barcelona: Ariel, 1975). Tortella and Nadal agree in recognizing a relative failure of Spanish industrialization in the nineteenth century, but they differ in their explanations. Tortella attributes the failure to the absence in Spain of an entrepreneurial factor able to invest in or foster the new industries; this void, he argues, was filled by foreign businessmen, especially Frenchmen, who invested in mining, banking, and railroads, with their sights set more on speculation than long-term growth. The root cause of all this, according to Tortella, was the weak development of human capital in Spain. Spain's stunted economic growth is also traced to the crisis of 1866, which adversely affected the banks and the railroads (the latter being in any case quite cost-ineffective to run, given the lack of cargo); another reason relates to the protectionist policies of successive governments, especially the conservative ones of the political Restoration. In any case, Tortella says that the period 1868–1891 was a free-trade epoch for Spain. In my opinion the entrepreneurial factor could be an endogenous variable in the economic system.

The most pronounced counterpoint to professor Nadal, besides the disagreement over the implications of foreign-trade policy, is Tortella's claim that the failure of Spanish industrialization cannot be attributed simply to demand, and thereby to insufficient agrarian development, but rather to the failure of Spanish entrepreneurs to secure foreign markets for their industrial goods; in addition, Tortella is very critical of Spanish economists and protectionist policymakers of the day.

Tortella's other notable thesis is that the liberalization of Spain's domestic economy and its foreign trade, begun in the 1960s, was the main cause of the ensuing industrial take-off, and that monetary policy and banking played decisive roles in this process. After the political transition and the industrial reconversion came Spain's entry into the European Union. From my point of view, I would note the great importance of the tourist sector to the expansion.

In my opinion, Tortella places insufficient emphasis on international economic fluctuations, especially the Great Depression. Moreover, the book lacks a chapter on labor markets, which would complement the second part of the work. Even so, this book is the main synthesis of the modern economic history of Spain to date. It will prove very useful not only to researchers, but also to undergraduate students.

JUAN HERNÁNDEZ ANDREU, *Universidad Complutense and University of California, San Diego*

ASIA

America and the Japanese Miracle: The Cold War Context of Japan's Postwar Economic Revival, 1950–1960. By Aaron Forsberg. Chapel Hill: The University of North Carolina Press, 2000. Pp. xi, 332. $45.00.

This book is a history of how American foreign policy during the 1950s affected Japan's access to foreign markets. It is not an economic history per se, but rather a political history highlighting the interaction between Cold War security issues and U.S. trade policy. Extensive archival evidence paints a clear picture of U.S. policy toward Japan after the end of the Occupation. Forsberg's main thesis is that the United States contributed to the "Japanese miracle" by fostering an international environment in which Japan could export while maintaining protectionist policies.

After an introductory chapter that outlines the book's basic findings and contributions, there follow seven chapters which explore U.S.–Japanese trade relations in chronological order. The story starts with the Communist victory in China, which drove the United States to design policies to keep Japan "on our side," principally by integrating Japan into the world economy. The peace settlement made a good start on this goal by reducing Japan's reparations burden and outlining a path for its return to international markets. At the same time the United States faced opposition from the Japanese over its trading ties with mainland China, and both domestic and European opposition over trade with Japan. Forsberg uses archival evidence to describe the tensions between, on the one hand, containing communism by encouraging Japanese economic success, and on the other hand pleasing U.S. manufacturing interests and stimulating European recovery. He shows how these tensions led to the frequent use of voluntary export restrictions and to a tolerance for Japanese protection against U.S. imports, both of which seem so puzzling to many observers of current U.S.–Japanese trade relations.

A valuable contribution of this book is to show how American policy worked to support the establishment of government interference in Japanese markets even when that interference was to the detriment of U.S. companies. For example, Forsberg shows that the United States tolerated Japanese foreign-exchange controls because it feared Japan's unstable balance-of-payments situation. Despite documentation of this practice in 1956, the U.S. government was reluctant to press for changes to the system. When change did come (in 1960), it was not due U.S. efforts, but rather to European and internal Japanese pressures.

This book also documents Japan's readmission to into international trade, especially the fight to gain membership in the GATT. In contrast to the German case, many countries —including Britain—resisted Japan's inclusion and, failing that, then wanted to attach special conditions to its membership, including the right to discriminate against its exports. Forsberg carefully documents the pressures brought to bear on Britain, and the trade concessions required of the United States before Japan could be brought into the Western trading bloc.

This careful political history provides a great deal of useful information to scholars of U.S.–Japanese relations and of the internal politics of trade in both countries. However, Forsberg devotes less attention to the relationship between these political events and Japan's miraculous growth. No economist would deny that better access to world markets helped foster Japanese growth. But limiting access to foreign direct investment, protecting internal markets, and strengthening bureaucratic controls, all of which entered into the bargain as well, may well have had deleterious effects. Despite this issue, Forsberg does a great job showing how the Cold War shaped Japan's trade relationships.

JENNIFER L. FRANKL, *Williams College*

Empire of Free Trade: The East India Company and the Making of the Colonial Marketplace. By Sudipta Sen. Philadelphia: University of Pennsylvania Press, 1998. Pp. 225. $37.50.

This book, a revised University of Pennsylvania dissertation, takes up a "classic" topic in Indian history, that of the conquest of Bengal by the East India Company in the mid-eighteenth century. As much has been written about this portentous event, it is essential to ask what Sen brings to the discussion that is fresh. At the heart of the book's originality is Sen's insistence upon the central role of markets and marketplaces, subjects rarely examined by Indian historians, who have been primarily concerned with land tenure and rural social organization. As he traces the coming of colonialism to Bengal, Sen makes of the market, not the countryside, "the epicenter of the battle for colonial conquest" (p. 7). Sen's second objective is to challenge the revisionist historiography, associated especially with such scholars as David Washbrook, that insists on an easy transition, with little institutional disruption, from indigenous to early colonial rule. As Sen bluntly puts it in his introduction, he proposes to argue against "the interpretation that, under the surface of administrative and commercial expansion, Indian society moved along at its own pace, unaffected by the colonial rule to which it was being subjected" (p. 5). Contest over markets went hand in hand, in his view, with the creation of an intrusive colonial state.

In his first chapter Sen argues that the precolonial marketplace was not simply the site of economic transaction, but was embedded in relations of power and redistribution as well. In so doing he verges on a romanticized view of the traditional market as embodying ideals of moral reciprocity, and yet he retains an awareness of the coercive exactions and exploitative demands that it expressed as well. In his discussion of the British advance into Bengal—before and after the 1757 battle of Plassey—Sen describes how the British, refusing to accept Indian notions of reciprocity, abused trade passes to secure personal advantage. Much of this is well-known, but Sen suggestively shows how, by taking over the trade in salt, betelnut, and tobacco—"prestige goods" reserved for the *nawab*—and then farming out these rights as monopolies, the Company lodged itself inextricably in the matrix of authority that sustained the Bengal state. Sen then goes on to describe the "regulative frame" that the British established to uphold this new power. Particularly suggestive is his account of how the British, in the interests of "free trade," destroyed the old customs posts manned by local chiefs, only to set up their own; for the colonial state did not want to give up any source of revenue, or to share power. This new style of commercial regulation threw into disarray the old relationships of patronage centered on the market, and inaugurated a new "colonial regimen of surveillance" that complemented the Company's military conquests.

In his fourth chapter Sen indicates how this commercial strategy in India was connected to British notions of property, rights, and markets. The concluding chapter, after pointing to the contradiction between the promotion of liberal ideas of free trade on the one hand, and the intensely regulative policies of the colonial state on the other, takes up the historiography within which he wishes to place the study. It is not an easy matter for a young scholar to defy the currently established view, now with twenty years of history writing behind it, that the colonial state simply took over for itself, as a "country power," established Indian modes of governance. Of course, to throw down such a challenge to revisionist history opens Sen up to the criticism of writing either an old-fashioned Indian nationalist account, in which the British are seen as evildoers subverting a happy traditional society, or an old-fashioned Marxist account in which the coming of capitalism destroys a pre-existing feudal order. There is, to be sure, more than a touch of nationalist nostalgia for the old order in Sen's writing. But Sen consistently roots his views in a deeply researched account of the transformation of Bengal's market economy with the coming of the colonial era. A stimulating and important book, *Empire of Free Trade* should force scholars to think afresh about the larger connection between markets and colonialism, as well as the coming of British rule to India.

THOMAS R. METCALF, *University of California, Berkeley*

Japan's Economic Diplomacy with China, 1945–1978. By Yoshihide Soeya. Oxford: Clarendon Press, 1998. Pp. xi, 187. $60.00.

Yoshihide Soeya's book, which emerged from his University of Michigan dissertation, covers the period from the end of World War II until China's opening outward with the ascendancy of Deng Xiaoping in 1978. This was a period when inordinately complex legal and political arrangements, concocted by particularly astute Japanese politicians, enabled Japan to conduct trade with both Taiwan and the People's Republic of China (PRC). It was also a period, for the most part, in which China and Japan did not have diplomatic ties. Although sometimes a bit tedious in describing the intricacies of all this, Soeya has done quite a creditable job of explicating the often tortuous bilateral ties between the two countries over three decades.

As his title indicates, Soeya concentrates on the Japanese side of this equation. He has consulted a few Chinese sources, but the overwhelming majority of his primary and secondary materials are Japanese; he also includes interesting interview data. This Japanese side was far from monolithic: there were numerous competing political and economic forces interested in trade with China. For the first seven years of the period under study, 1945–1952, Japan was still under occupation by the United States; some in Japan sought to work together with U.S. interests in the region, while others wanted Japan to forge an autonomous foreign policy; NGOs sympathetic to China also lobbied actively. In short, Soeya argues for a "pluralistic structure" to Japan's China trade.

The book is structured chronologically, as Soeya takes us into the 1950s, the 1960s, and so on, all the time attempting to link political and economic developments. One interesting early point that he makes is that Japan's pro-PRC lobbies were not typically motivated in the first instance by common socialist or communist ideological aims, but by a widespread opposition to U.S. hegemony in Japan's foreign policy initiatives and to the "reverse course" struck after the outbreak of the Korean War.

The discussion of the "LT" and "MT" trade regimes (chapter 5) is particularly illuminating. LT stands for the two signatories—Liao Chengzhi and Takasaki Tatsunosuke—to a 1962 agreement enabling China and Japan to carry on trade despite the absence of diplomatic relations. LT trade has been discussed and analyzed before. Soeya makes a convincing case that the structure of this trade was essentially non-governmental. When the domestic Chinese political scene veered sharply to the left at the time of the Cultural Revolution in the mid-1960s, LT trade was reshuffled and renamed MT ("Memorandum Trade").

In 1972, as the United States was beginning to warm to the PRC in the run-up to President Nixon's historic visit to Beijing, China and Japan normalized diplomatic relations with what, at the time, seemed to be great speed. Actually, as we learn from this book, they had been inching in that direction for some years. The delineation of this process is another important component of Soeya's book.

This book does not make for scintillating reading. It has far too many acronyms—there is a list of 27 of them at the beginning—to keep track of all the players and actions. Nonetheless, in its very doggedness, the book offer a great deal of detail on the fascinating wranglings on the Japan side as it moved toward normal bilateral diplomatic and economic ties with China. As the two countries now do an enormous volume of trade, it would seem as if all the effort was well worth it.

JOSHUA A. FOGEL, *University of California, Santa Barbara*

Chinese Capitalism, 1522–1840. Edited by Xu Dixin and Wu Chengming. New York: St. Martin's Press, 2000. Pp. xl, 517. $79.50.

This book is a translation of the first volume of a trilogy entitled *History of the Development of Chinese Capitalism* published between 1985 and 1993 by a team of researchers at the Institute of Economics of the Chinese Academy of Social Sciences. It focuses on the emergence of what Chinese historians have called "embryonic capitalism" (often translated more literally as "the sprouts of capitalism") prior to the Opium War (1840–1842), which in Chinese historiography marks the beginning of China's modern era. The authors utilize an explicitly Marxian analysis to distinguish embryonic capitalism as a distinct phase of economic development. Many Western scholars have begun to define the period 1500–1800 as China's early modern era, finding in the emergence of free labor markets, regional specialization in agriculture and handicraft production, rural market integration, the growing autonomy of merchant groups, and increased foreign trade patterns of socioeconomic change comparable to contemporary developments in Europe. The authors of this study, in contrast, distance themselves from the hypothesis, once prevalent among Chinese historians, that China during this period was developing a capitalist economy along lines similar to Europe. Yet at the same time they challenge the tendency to view the late imperial economy as homeostatic, a characterization that gained considerable currency among Chinese historians during the 1980s, most notably in the "super-stability" hypothesis advanced by Jin Guantao and Liu Qingfeng in their 1983 book *Prosperity and Crisis: The Super-Stable Structure of China's Feudal Society* (Changsha: Renmin chubanshe). The introduction by Chris Bramall and Peter Nolan provides non-specialists with a useful guide to the historiographical background of this book and its methodological approach; but their review of alternative interpretations of the late imperial Chinese economy, which only considers scholarship published up to the mid-1980s, is outdated.

The book is divided into five parts, comprising 22 brief chapters on broad socioeconomic changes and specific sectors and industries, plus a conclusion spread across two chapters. Thus, chapters on topics like "Commodity Circulation and Merchant Capital" and "Changes in Tenancy and Hiring of Labor" are interspersed with studies of textile manufacture, sugar refining, paper-making, food processing, mining and metallurgy, shipping, etc. One of the book's signal contributions is its emphasis on using quantitative evidence to analyze the Chinese economy; as a result, it contains much empirical data not found elsewhere. The authors are generally cautious in using quantitative data from historical sources, but are obliged to rely on hypothetical estimates and indirect measures to obtain the figures they need for their analysis (for example, using a fixed estimate of food consumption, allowing for no temporal or spatial variation, to generate gross estimates of grain output across time). The inattention to regional variation is particularly problematic, given the uneven density of historical documentation. Although many of the industries are examined in regional profiles, isolated data (almost never reported serially) from a few areas is used to deduce conclusions at a national scale on critical issues like labor productivity and rates of tenancy. The study's most widely influential findings, its estimates of the size of the major commodity markets in the early nineteenth century (chapter 9), rest on indirect measures of consumption, none of which actually relates to trade. Non-specialists unfamiliar with the original sources should exercise caution in citing the numbers generated by such calculations.

The conclusions advanced by the authors merit careful consideration. They argue that prior to the Opium War, embryonic capitalism still played only a negligible role in the national economy. Most commodities entering the market were exchanged among peasants according to the principle of comparative advantage. Exchange between towns and the countryside remained minimal (here again regional variation is ignored). Yet the authors also insist that the changes in social relations and forces of production between the sixteenth and nineteenth centuries shaped the development of capitalism in the modern era. Imperialist penetration, they argue, did not devastate China's domestic industries, but rather strengthened capitalist relations of production within traditional industries, leading to a belated transition to a distinctive mode of capitalist development.

Ultimately the analysis presented in this book is handicapped by the confining conceptual framework of embryonic capitalism. Most scholars, specialists and non-specialists alike, will appreciate this book as a handy and concise reference for the empirical fruits of historical study on specific sectors of the Chinese economy. Readers interested in more recent Chinese scholarship that transcends the embryonic-capitalism approach, and also is more inflected by Western scholarship, should consult Bozhong Li's *Agricultural Development in Jiangnan, 1620–1850* (New York: St. Martin's Press, 1998), published in the same series.

RICHARD VON GLAHN, *University of California, Los Angeles*

AFRICA, MIDDLE EAST, AND LATIN AMERICA

Slaves, Freedmen, and Indentured Laborers in Colonial Mauritius. By Richard B. Allen. Cambridge: Cambridge University Press, 1999. Pp. xvii, 221. $64.95.

With this important collection of essays on the independent economic activities of slaves, freedmen, and indentured Indians in colonial Mauritius, Richard Allen takes the history of Mauritian labor well beyond the familiar themes of exploitation and resistance. This is careful economic history from below, with important implications for Mauritian social and cultural history. Allen's argument is that inhabitants of the vast bottoms of the social pyramid—*gens de couleur* (free persons of color), ex-apprentices, and Indian immigrants—not only shaped their own destinies in difficult legal and social conditions, but that their independent choices profoundly influenced the insular economy as a whole. Through attention to subalterns' acquisition and management of land and capital, Allen sheds new light on key questions in Mauritian social history (especially the social "disappearance" of the Afro-Mauritian population and the rise of Indian entrepreneurship in the late nineteenth and early twentieth centuries). As do all provocative and well-researched works, this one proposes solutions to old problems and allows us to ask new questions about the roles of Malagasy, Africans, Indians, and their descendants in Mauritian history.

Commencing with a brief introduction and a first chapter setting out the parameters of the studies that follow, the book is organized into two parts. The first, composed of two chapters, examines maroonage among slaves, and absenteeism, desertion, and vagrancy among indentured laborers. Here the author turns the tables on much of Mauritian historiography to place emphasis on the ways in which subaltern labor placed constraints on the management of agricultural estates. The second part takes the relationship between land and domestic capital as its organizing principle. Its three chapters examine the land acquisition strategies of *gens de couleur*, ex-apprentices, and Indian indentured and ex-indentured servants, respectively. What Allen demonstrates in these chapters is that each of the populations studied found multiple avenues to the acquisition of capital and land. While many persons never accumulated much of either, the capital resources commanded by these groups as a whole was crucial to investment in the sugar economy of colonial Mauritius. Through the subdivision and piecemeal sale of their estates to subalterns (a process known locally as *morcellement*), the larger landowners derived vital capital unavailable from outside of the colonial economy. In the process, small landowners, especially those of Indian origin, came to play key roles in sugar and food production for the colonial economy. By 1920, for example, Indians cultivated almost 45 percent of all land planted in cane, and accounted for more than one-fifth of all cane output.

Allen's method, long a staple in the study of plantation societies in the Americas, is new to Mauritius. He has carefully combed the colonial archives for both quantitative and qualitative data relating to land, labor, and capital among the subaltern populations. The most useful sources prove to be population census returns, maroon registers, Protector of Immigrants reports, and notary documents, especially those recording transfers of land. A variety of reports and documents published by the colonial government also prove useful. Each chapter summarizes the most significant data series in tabular form, while the text discusses the implications of those findings and explores the details of particular cases to illustrate the larger argument.

Taken together, these essays form a rich and complex study. Beyond its attention to the economic activities and strategies of colonial Mauritius's underclasses, the book proposes answers to important questions in Mauritian history. To my mind, these answers emerge most saliently in the second part of the book. Here, Allen documents the rise and fall of Afro-Mauritians in the colonial economy, showing how many of them acquired land and shifted into market gardening and waged professions. By 1806 the *gens de couleur* owned and managed about 10 percent of the island's agricultural wealth; by 1830 they comprised more than two-thirds of the island's free inhabitants and one-fifth of its total population, controlling some one-fifth or more of its agricultural wealth. The position of *gens de couleur* and ex-apprentices in the colonial economy eroded from midcentury, with the influx (beginning in 1842) of nearly half a million indentured servants from India. While scholars have long surmised a connection between Indian immigration to the effacement of Afro-Mauritians from the economy, Allen's work allows us to understand more specifically how it happened. Like the *gens de couleur* before them, Indian servants, too, acquired land and accumulated capital, but their greater numbers and tighter associations in villages and societies meant that they commanded greater capital resources. Physically isolated, pushed from wage labor and from the professions, the *gens de couleur* could not manage their estates and began to sell them. By the late nineteenth century they disappear from the archival record as a distinct group. With this history of Afro-Mauritian economic strategies and failures, social historians will more easily understand the declining presence of Malagasy and African influences in Mauritian public life.

Because of the fertility of its content, one comes away from Allen's book able to pose many new questions and (if one is also interested in social and cultural history, as I am) with the desire for a sustained narrative that treats exploitation and subaltern initiative in the same pages. These new questions and desires testify to the importance of Allen's work.

PIER M. LARSON, *Johns Hopkins University*

Colonization as Exploitation in the Amazon Rain Forest, 1758–1911. By Robin L. Anderson. Gainesville: University of Florida Press, 1999. Pp. x, 187. $49.95.

Studies of the social and economic history of the Amazon are few and far between, and long-term studies that attempt to link the history of the colonial and post-colonial periods are especially unusual. The demands of academic careers make it difficult for many scholars to undertake the time-consuming research required to produce a book covering such long time spans in a serious way, especially since the "long" nineteenth century saw three different political regimes in Brazil: colonial, imperial, and republican. Moreover, the isolation of the Amazon, the conditions of its archives and documentary collections, and its lack of a well-developed historiography further complicate efforts to study this vast region. Thus, Robin L. Anderson has undertaken an especially difficult task writing a book on the settlement of the Amazon from 1758 to 1911.

Anderson argues that efforts by Portuguese and Brazilian governments to settle the Amazon—more specifically, the province of Pará—were doomed to failure because they were based on flawed notions about the region and its environment, and because they were profoundly exploitative of the people to be settled. She explores her argument first in three chapters discussing the "Directorate," the set of policies under which Pará was to be governed between 1758 and 1798, finding that the exploitation of the Indians by the colonists and colonial administrators wrecked an idealistic, but unworkable, framework for settlement. Subsequent chapters deal with the period from independence in 1822 to 1911, when *paraense* elites attempted to bring Europeans, North Americans, and refugees from the droughts in the Brazilian northeast to the Amazon to settle the region. These efforts failed, because their expectations were too high and because they exploited the colonists—who often returned to their places of origin at the first opportunity.

In the process of evaluating the colonization efforts, Anderson devotes significant attention to basic trends in the Amazonian economy. So far as I am aware, this is the only book to attempt to gather statistics on the quantity and value of all Amazonian agricultural and forest products during the period. Rather than focus solely on rubber or cacao, as a less thorough scholar might do, Anderson has traced the trajectories of numerous commodities, including Brazil nuts, cacao, sugar, cotton, rice, rubber, and manioc, among others. Then she examines those statistics in light of local elites' constant complaints that the gathering of products found in the wild, especially rubber, was damaging to agriculture. Interestingly, she argues—*contra* the opinion of the local elite—that rubber was not responsible for the decline of agriculture in late nineteenth-century Pará. In fact, she maintains, agriculture did not decline: it only seemed to decline, because it could not keep up with the demands of Pará's growing population.

Anderson shows that the cultivation of manioc and other foodstuffs for consumption in Amazonian cities was an important element of the regional economy and of great concern to *paraense* elites bent on development. While state policymakers did hope that settlers would develop export agriculture, they were equally concerned about maintaining the food supply in Belém. In addressing the question of the domestic trade in food, Anderson joins a growing group of historians of Brazil, such as Hebe Maria Mattos de Castro and B. J. Barickman, who are showing that the old dichotomy between subsistence farming and export agriculture must be rethought.

Although Anderson makes several interesting arguments, her book leaves something to be desired. In commenting on agriculture for domestic consumption, she does not seem to be aware of the significance of what she is saying, or of the literature to which she is adding. Although she has read widely in the literature on Amazonian development issues, she did not draw it into the discussion of the historical period under study. Moreover, she made almost no effort to place Pará or the Amazon in the context of wider trends in Brazil during the period, and what little there is draws on a Brazilian historiography that is woefully out of date. This is unfortunate, because recent work on the social history of rural Brazil and of its indigenous peoples would have allowed her to show that the Amazon was not as different from other parts of Brazil as one might think. Thus Anderson has produced a valuable contribution to the literature, but one that falls short of its potential.

MARY ANN MAHONY, *University of Notre Dame*

Slavery and the Demographic and Economic History of Minas Gerais, Brazil, 1720–1888. By Laird W. Bergad. Cambridge: Cambridge University Press, 1999. Pp. 298. $54.95.

Forty years after it was first published, Celso Furtado's seminal *The Economic Growth of Brazil* (Berkeley: University of California Press, 1965), is still one of the most widely used books in undergraduate, and even graduate courses, on Brazilian economic history. Although short, Furtado's book set forth many hypotheses that generated several memorable academic debates amongst specialists in Brazilian history. Even when Furtado was wrong on a given topic, his analysis often stimulated discussions that pushed forward the frontiers of knowledge. Such is the case with the scholarly literature arising in the 1970s and 1980s around the development of the region of Minas Gerais after the abrupt end of the mining boom in 1760. Furtado's view, that the end of mining activity had brought a generalized economic decadence to the region, was widely accepted. It was believed that this collapse had caused a reversion to subsistence activities, together with dispersal of the population and underutilization of slaves, leading eventually to their emigration to other regions where coffee was becoming the center of the economy.

This view started to change with the Ph.D. thesis of Robert B. Martins in 1980 ("Growing in Silence: The Slave Economy of Nineteenth Century Minas Gerais, Brazil," Vanderbilt University) that portrayed post-boom Minas Gerais as a large, active, diversified, slave-based economy that was essentially self-sufficient and not directly based on any form of export activity. This sparked much debate on the topic, and focused attention on a region previously neglected by the literature. Since then much work has been done on the economy of Minas Gerais, and a general consensus has emerged regarding the functioning of its economy. However, some important points remain unresolved, and various scholars are currently doing research to prove hypotheses concerning some specific issues of the *mineiro* economy.

One of these issues is precisely the central thesis of the book under review. Bergad posits that the slave population of Minas Gerais, which was involved mostly in these internal provincial rather than export markets, managed to reproduce naturally. This is no small claim, as natural demographic increase of a slave population in the New World is something that has only been registered in the United States; it thus represents a complete break with many aspects of the prevailing view of slavery in Brazil. The central evidence used by Bergad to prove this thesis is a new data set collected in archives in Minas Gerais from estate inventories. The author examined over 10,000 inventories for the period 1713–1888, which include a total of 111,963 slaves, leading to what he claims is "the only systematic times-series primary source available in archival collections in Minas Gerais that contain data on the slave population on a year-by-year basis" (p. xxii).

The book starts with a couple of chapters surveying the economic and demographic history of Minas Gerais, already looking for evidence in the literature to bolster the notion of a slave population whose growth was not driven by new arrivals. In the third chapter the author tries to prove the same point using several well-known primary data sources typically used in the literature. Then in the fourth chapter his own data set is employed to prove his thesis. This is done by showing indicators such as the increasing number of female slaves of childbearing age, falling sex ratios, and high child–woman ratios. Bergad analyzes this data (which are made available in the appendices) and stresses how they paint a picture of Minas slavery that is essentially different from that of other Brazilian regions, where slave imports were undoubtedly the norm.

One of the most commendable aspects of Bergad's research is his search for new sources of data on colonial and imperial Brazil. Scholars of Brazilian economic history face a paucity of quantitative data to work with, a state of affairs that has led to much unsubstantiated argumentation. More such efforts to assemble new data sets, allowing empirical tests of many currently held theses, are badly needed.

With this book Bergad has painted a picture of the functioning of the post-mining economy of Minas Gerais that is significantly different than the prevailing view. If previous experience is anything to go by, this may lead to intense debate amongst specialists in the field. This reviewer's personal communications of with one leading specialist has made it clear that the book's thesis, as well as the data set on which it is based, will not go unchallenged.

BERNARDO MUELLER, *Universidade de Brasilia*

The Politics of Trade in Safavid Iran: Silk for Silver 1600–1730. By Rudolph P. Mathee. Cambridge: Cambridge University Press, 1999. Pp. xxi, 290. $64.95.

Since the revolution of 1979 there has opened a great gap in scholarship on the economic history of premodern Iran. As the author of the present study has appropriately noted, over the past two decades the historiography of premodern Iran has suffered from ideological constraints (on Iranian scholars) as well as lack of access to local sources for western scholars. In light of these limitations, the present study on the political economy of silk trade in Safavid Iran is a refreshing reminder that not all is lost in an otherwise hopeless situation. Using predominantly western (including Russian) archival sources, the author has made a significant contribution to the histories of western European (and particularly Dutch maritime) trade with Iran, of Safavid commerce, and of the politics of the Middle Eastern silk trade.

This study succeeds in revising the views of western scholars such as Immanuel Wallerstein (*The Modern World System*. New York: Academic Press, 1974) and Neils Steensgaard (*The Asian Trade Revolution of the Seventeenth Century: The East India Companies and the Decline of Caravan Trade*. Chicago: University of Chicago Press, 1974) on the incorporation of the Middle East into the world economy and its negative impact on the local commercial networks and trans-regional overland trade. Mathee has convincingly demonstrated that unlike Mughal India, Safavid Iran under the central rule of Shah ʿAbbas I (1587–1629) resisted English and Dutch pressures for open markets, access to the hinterland, and toll-free trade. Moreover, Iran's inhospitable terrain and its limited resources did not have much appeal for western traders beyond the cheap silk that they were forced to procure from royal agents in Isfahan. The local Armenian merchants showed much more resiliency: they negotiated with the shah for a privileged status in the silk trade, and they operated through a tried-and-true system of family firms. They controlled the overland Levant trade through Ottoman territories, as well as the Volga route through Russia. For these reasons, the European maritime companies turned their attention away from Iran and discovered the better-quality silk of Bengal. The silk trade between Iran and the Levant survived a bit longer, but ultimately it too declined as a result of Safavid disintegration, reduced demand in Europe for Iranian silk, and the development of sericulture in the Ottoman empire and Italy.

Mathee divides his study into eight chapters and an appendix. Chapter 1 sets the historical and regional context for the silk trade between Iran, the Levant, Russia, and Western Europe during the sixteenth century. He highlights the religious and political tensions between the Shiʿi Safavids and the Sunni Ottomans, and their use of silk embargos as an economic weapon against one another. The second chapter provides an important description of the cultivation, varieties, distribution, sale, and export of silk via all three routes, based largely on Western European diplomatic and commercial sources. The fourth chapter deals more specifically with ʿAbbas I's policy of rerouting the silk trade from Ottoman territories to the Persian Gulf, and establishing a royal monopoly on its purchase and resale to the English and Dutch maritime merchants. He also settled the Julfa Armenians in the new capital of Isfahan, in order to take advantage of their commercial skills, and encouraged the English and Dutch East India Companies to export from the new Persian Gulf port of Bandar Abbas, built in 1622. The fiscal needs of the state, the constant shortage of bullion, and the open hostilities with the Ottoman Empire continuing through his reign were at the core of his silk politics and his diplomatic alliances with Western Europe and Russia. These hostilities came to a brief end after his death in 1629 and the signing of the peace treaty of Zuhab with the Ottomans in 1639. The end of silk monopoly and the privatization of silk trade initiated by Shah Safi, and the rise of Russian connection from 1660s to 1690s, form the next set of themes (chapters 5–7). Finally chapter 8 reexamines the impact of these policies on the economic development of Iran, and sets the context for the final showdown between the Safavids and their hostile neighbors. The fate of the Dutch East India Company in Iran was sealed with the long siege and occupation of Isfahan in 1722, and with the violent murder of Shah Sultan Husayn and his sons by a group of Afghan marauders. This tragic end to the Safavid dynasty led to the Russian invasion of the Caspian Sea region in 1722, and the Ottoman occupation of western Iran and the Caucasus in 1725.

This book has set a new course for Iranian historiography that will be taken up once Iran opens up for research. Only then will historians be able to complement its findings and shed more light on some of the questions left untreated, such as the role of indigenous Muslim merchants in international trade, and the internal economic development of Iran outside the narrow orbit of the state and the interests of western European maritime companies.

FARIBA ZARINEBAF-SHAHR, *University of Chicago*

UNITED STATES AND CANADA

Forced Founders: Indians, Debtors, Slaves, and the Making of the American Revolution in Virginia. By Woody Holton. Chapel Hill and London: University of North Carolina Press, 1999. Pp. xxi, 231. $39.95, cloth; $14.95, paper.

Professor Woody Holton makes two important points in this fine study of rebellion: the elite gentlemen of Virginia did not always lead the Revolution, but were sometimes pushed from below, and the Chesapeake is not New England. Those of us who live and teach below the Mason-Dixon Line are often painfully aware that the general outlines of U.S. history are not the history of our region. As this detailed study of Virginia's path to Revolution makes clear, many of the generalizations about the origins of the American Revolution only hold for the northern colonies. But this work's greatest contribution is its carefully researched and documented argument that race and class mattered in creating a Revolution.

Holton begins his study with an intriguing story of a Virginia gentleman who in April 1774 organized an attack on a jail and armed his slaves to battle the sheriff and his posse. What, he asks, would drive a gentleman to such desperate measures? The answer is one that delights economic historians: debt, deflation, and property rights. The purpose of his study is to show how Indians, debtors, and slaves interacted with the British and the ruling colonial elite to influence British policy and drive the gentry to rebellion.

His text is divided into three parts: Part one focuses on Indians and merchants and their attempts to use British rule to their own ends. Although the role the desire for Indian land played in the Revolution is not new, Holton emphasizes that the Indians played an active part in forming British policy and defeating Virginia's attempt to annex Kentucky in 1769. Fears of a general Indian insurgency forced the British to make policy changes detrimental to the economic interests of the white settlers. This section also examines in detail the conflict between British merchants and Virginia's debtors, focusing on the relationship between speculation in Indian land, debt and the burden of the Navigation Acts. Part two examines the boycotts from 1769 to 1774 and the influence of Virginia's debtor class of gentlemen and small land holders. Holton argues convincingly that while the import and export boycotts were aimed at combating parliamentary tyranny, they also gave indebted farmers and gentlemen a means to escape from payment of debt in a period of deflation and recession. Finally in part three, Holton argues that the boycotts created a set of circumstances that propelled the gentlemen (still reluctant revolutionaries in 1774) to rebellion. Here, of course, is the well-known story of Governor Dunmore's offer to emancipate slaves who joined the British cause. Things were not well on other fronts as shortages and hardships caused by the boycotts led to riots among the non-tobacco growers which fueled fears of a general agrarian insurgency. Revolution seemed the only way to preserve order. This brief outline does not do justice to Holton's lively and clearly presented argument.

One problem with any work that seeks to uncover the influence of the marginalized groups such as Indians, slaves, and the yeoman is a lack of direct testimony. Holton uses the traditional sources to try and ferret out the influences of these silenced groups. He finds that the records are not truly silent; indeed the gentry did discuss in private papers the problems with Indians, merchants, and slaves. Although the public proclamations are high-sounding calls to rebel against tyrants, the private papers often discuss debt relief, land deals, and the losses to Virginia tobacco growers caused by mercantilist policies. This careful study shows how the economic motivations for Revolution helped shape the political course of the Revolution. This study will be of great interest to anyone seeking a more complex view of why we rebelled. I do have one misgiving. This is the first book I have read in quite a while that is almost womanless. The role of Indian women is mentioned, a few prostitutes appear, but there is no discussion of gender or women's role. There has been so much recent work on women in the Revolutionary era that it seems important to explain why half the population has gone missing.

PAMELA J. NICKLESS, *University of North Carolina at Asheville*

Grand Master Workman: Terence Powderly and the Knights of Labor. By Craig Phelan. Westport, CT: Greenwood Press, 2000. Pp. 294. $65.00.

This volume is a throwback to an older style of labor history, one based on the leaders of the labor movement. Craig Phelan has produced an intriguing, and in places even inspiring, history of Terence Powderly, the leader of the Knights of Labor from 1879 to 1893. Powderly's story deserves to be retold. His history stands at the cross-roads of an older American set of ideals dating back to the founding of the nation and the emerging corporate values associated with the late-nineteenth-century rise of big business. This is a story of one of the earliest attempts to break from an elitist model of craft unions and replace it with a broad-based labor movement, one that welcomed skilled and unskilled, men and women, and people of all races. This is the story of how this movement was ultimately crushed by larger forces at work in society. It reminds us of what might have been, and perhaps encourages us to think of what still might be.

Powderly has not been treated kindly by historians. He is portrayed as arrogant, intellectual, and a "windbag." His strategies are called into question as is his dedication to his members and his willingness to reach deals with employers. Yet the impact of the Knights themselves is applauded and many appreciate how the Knights effectively challenged the existing social relations with a social vision based on cooperation, equality, and social responsibility. Their collapse, as pointed out by Kim Voss, contributed to the dominance of a more business-like style of unionism in the United States. Phelan's book is an attempt to understand why historians have been so critical of the leader of such a highly praised movement. Through careful research, Phelan shows that Powderly's actions were largely dictated by constraints beyond his control. Behind these actions remained an innovative leader dedicated to the ideals and principles that made the Knights of Labor such an important labor organization.

Powderly came to power upon the retirement of Uriah Stephens, who represented the original vision of the Knights, one that was backward looking towards small-town America and small-town moralism. Powderly, while committed to the all inclusive idealism of the Knights, represented the urban labor tradition of the wage worker, one based on the mines and the mills of the east. Collectivism was as much a strategy for preserving the past as a vehicle for raising the condition of workers in this new environment. His experience with strikes in the 1870s taught him to be cautious of them as a strategy for moving forward. The legal environment of the times made it difficult to win strikes, the union was unable to support them financially, and their failure often gave members a reason to leave their organizations. Powderly recognized that workers would and should use this tactic but encouraged the Knights to seek other alternatives as well. Here the very structure of the Knights, an all-inclusive labor organization, came into play. By organizing all types of workers into mixed locals, Powderly hoped both to broaden the range of debate to include items other than wages and working conditions, and in the process to create the basis for working-class solidarity. This would lead to enhanced political power that could be used to push through the same gains workers were seeking unsuccessfully through strikes.

Phelan shows how this strategy was caught in a contradiction resulting from the Knights' and Powderly's strong commitment to democracy and a bottom-up power structure. For Powderly, it was important that the organization of the Knights reflect the cooperative commonwealth he envisioned. Central control was kept to a minimum, local leadership was encouraged to promote strategies, and Powderly dedicated himself to implementing the will of the majority. In a hostile social and economic environment, the reliance on a bottom-up structure never resulted in the convergence on tactics, or the order and discipline Powderly knew was needed to be successful. The Knights were unable to sustain a national movement, build a powerful treasury, or establish the discipline over factions within the movement needed to build a political alternative or survive in the class-war environment of the Great Upheaval. By the 1890s the experiment with "horizontal unionism" was in shambles, and the business unionism espoused by Samuel Gompers and the American Federation of Labor on the ascendant.

In retelling one moment in time when a group of workers successfully promoted an alternative vision of society, Phelan has shown how leaders are themselves constrained by the social and economic context they inherent. He has also exposed a period when a very different vision of American development was on the table, a vision that those dissatisfied with current trends will find interesting.

WAYNE LEWCHUK, *McMaster University*

Big Business and the State: Historical Transitions and Corporate Transformation, 1880s–1990s. By Harland Prechel. Albany: State University of New York Press, 2000. Pp. xvi, 317. $75.50, cloth; $25.95, paper.

The history of the modern corporation is the history of institutional change and of the corporation's attempt to influence and respond to that change. In this book, Harland Prechel supplies a multilayered analysis of the development of big business from the 1880s to the 1990s. Prechel's approach is to draw linkages between the micro (individual managers), meso (corporate), and macro (institutional) levels of behavior and change. This multilayered analysis allows Prechel to expose the connections and also the inherent "irrationalities" between each layer of decision making.

The book contains a well-developed theoretical underpinning centered on capital-dependence theory, while incorporating and critiquing agency theory and transaction-cost economics. The premise of capital-dependence theory is that corporations attempt to shape their internal and market structures, along with their institutional environment, in response to "historically contingent capital-dependencies" that reduce profitability. Large corporations can, for instance, effect an increase in their power over consumers (and other smaller corporations) by changing the legal context in which they operate. Concurrently, they attempt to rationalize their internal structure to conform to the profit-making opportunities that the new institutional structure allows. The choice of a meso- or macro-level response to an exogenous change in the institutional environment depends on the historical context.

The incentive to alter the "rules of the game" is particularly strong following a period of "prolonged market instability and declining profits."

There are three major sections to the book, each examining one or more aspect of the micro-meso-macro linkages during three historical transitions of the corporation. The first section analyzes the corporate transformation of the late nineteenth and early twentieth centuries. Due to increased competition, firms desired to control markets in order to increase their power in relation to consumers. They lobbied for laws at the macro level that would facilitate the meso-level formation of trusts, holding companies, and finally multi-divisional-form corporations. Prechel argues that states "raced to the bottom" by passing ever more permissive laws, and that the federal government was initially incapable of offsetting the pace of consolidation of power in industry.

The second section explores the meso-micro developments that occurred during the switch from product cost controls to financial management using financial accounting. Prechel argues that the expansion of the scale and scope of corporations led to the use of financial accounting and that the new accounting method gave managers the incentive to cut labor rather than capital costs. The book's final section covers the corporation's evolution into multilayered-subsidiary form during the latter half of the twentieth century. This transformation was forced by declining profitability that resulted from the use of financial accounting and was facilitated by neo-Fordist management and the increased computerization of the decision-making process. Again, Prechel combines capital-dependence theory with many concrete examples, including a case study of the corporate transformation of the steel industry.

Prechel critiques other research traditions of firm behavior for not effectively analyzing the disincentives inherent in many corporate incentive structures. In particular, he argues that transaction cost analysis (TCA) "does not adequately acknowledge that social actors are embedded in social structures that create incentives and produce behaviors" (p. 113). This is exactly what the broader New Institutional Economics (NIE) attempts to do, but the critique of the TCA is not central to Prechel's analysis of the corporation. In fact, he gives many useful and interesting examples of the accounting contrivances, asset utilization incentives, and internal political costs that economists in the TCA tradition argue will occur due to moral-hazard problems within large corporations. Despite the distance Prechel tries to put between himself and the TCA and the NIE, much of his analysis concerning the incentive structures created by changing institutional forms is consistent with these research traditions. The NIE would offer a different interpretation of his facts, centered on the minimization of the sum of the transaction and production costs within a profit-maximizing firm. The NIE also asserts that firms will exert themselves on the meso (cost-minimizing) versus macro (political) level depending on the perceived relative payoffs. Prechel's analysis is interesting and well written, with a consistently applied theoretical alternative to the NIE and backed up with a detailed historical narrative.

TOMAS NONNENMACHER, *Allegheny College*

The Evolution of Retirement: An American Economic History, 1880–1990. By Dora Costa. Chicago: University of Chicago Press, 1998. Pp. xiii, 234. $40.00, £31.95, cloth; $19.00, £13.50, paper.

Buy this book, read it, and keep it on your shelf as a reference. Dora Costa has written an award-winning book about retirement over the past century. She examines long-term trends in the propensity to retire and the impact of health, public pensions, income, and other determinants on the retirement decision.

Costa argues that the long-term rise in retirement occurred both before and after the introduction of social security and then discusses the determinants of this rise. Many have suggested that the secular rise in incomes has allowed the elderly greater flexibility in choosing retirement. However, isolating the pure effect of income on retirement decisions is often difficult because income changes that are truly exogenous to the decision maker are not easily found. Costa finds a nifty way to resolve this problem and isolate the effect of income on retirement by examining changes in the Civil War pension for a longitudinal sample of Union army veterans at the turn of the century. She finds that the secular rise in income can account for a substantial portion of the increase in retirement up to around 1950 and a lesser amount thereafter. Part of this secular rise in income is associated with increases in public pensions. Costa examines the politics of public pensions with a focus on three key programs: the Civil War pensions, cash payments to the elderly poor under state and later state/federal old-age assistance, and then the modern social security old-age pension system. She argues that each of these programs was expanded and adopted in response to well-organized lobbies that were able to tap new sources of revenue and redistribute income in their favor.

Health is an important dimension of the retirement decision. Costa uses surveys of the Civil War and World War II veterans when they reached retirement age to show that the elderly are healthier on a variety of dimensions—chronic disease incidence, blindness, height, and body mass index (BMI)—in recent years than at the beginning of the twentieth century. She then documents the strong effect of ill health on nonparticipation in the labor force at various times. Among the retired, better health and higher incomes have contributed to substantial increases over the century in the percentage of retired persons living by themselves. Using the Consumer Expenditures Survey she shows further that the income elasticity of leisure expenditures has fallen over time; therefore, declines in income among the elderly are less likely now to lead to sharp rises in labor-force participation and reductions in recreational activity. She argues that technological change and public provision of many leisure goods have lowered the cost of recreational activities for the elderly. Thus, the modern retiree is likely to be substantially healthier, have more income, be more independent, and be able to enjoy more recreational activities than the retiree of a century ago.

I marvel at the impressive array of large data sets that Costa brings to bear on the issues. She asks interesting and provocative questions and than uses the data and appropriate econometric techniques to develop robust answers. A significant portion of the econometric work has appeared in journal articles and has been honed to a fine edge. Even if you have read many of the articles, however, you still should read this book. The volume adds quite a bit of institutional context to the econometric work that appeared in the journals. The book is well written and Costa uses several graphical techniques and simple statistics to enhance the clarity of her findings for nontechnical readers. More importantly, the book molds the broad array of findings into a cohesive story that makes the volume far more valuable than the sum of the journal articles.

There is one area in the literature about retirement trends that I wish Costa had addressed more fully. There has been a controversy about the trend in retirement between 1880 and 1940 that has played out in this JOURNAL. Between 1930 and 1940 the Census changed how they defined participation in market work from a concept of gainful employment to the modern concepts of labor-force participation. Roger Ransom and Richard Sutch ("The Labor of Older Americans: Retirement of Men On and Off the Job, 1870–1937," this JOURNAL 46, no. 1 (1986): 1–30 and "The Trend in the Rate of Labor Force Participation of Older Men, 1870–1930: A Reply to Moen," this JOURNAL 49, no. 1 (1989): 170–83) have argued that the Census definitions of retirement at the turn of the twentieth century do not adequately take into account the possibility that someone counted as unemployed might actually be retired. If we include the elderly who were unemployed for more than six months among the retired, we find very little change in retirement rates prior to the 1930s, which is consistent with a view that the introduction of social security led to a revolution in retirement. In response, Jon Moen ("The Labor of Older Men: A Comment," this JOURNAL 47, no. 3 (1987): 761–67) argues that use of consistent series under the old Census definitions still shows the long-term downward trend prior to 1940 and that the Census definitions of retirement were reasonable. Costa believes that Robert Margo ("The Labor Force Participation of Older Americans in 1900: Further Results," *Explorations in Economic History* 30, no. 4 (1993): 409–23) settled the issue by showing that the elderly retired look very little like the elderly who were unemployed for more than six months (pp. 7 and 8). After reading the series of articles, comments, and replies, I have to admit that my eyes glazed over in trying to sort out the disputes over what the Census instructions meant. It seems easier to rely on Margo's statistical evidence. Statistical evidence is not always the last word, however. Because some of the key conclusions of the book are affected by this issue, it is all the more important to hear what Costa has to say about the definitional debate.

This is excellent work. Abbie Hoffman would have recommended that you steal this book. Since I am a firm believer in the importance of intellectual property rights, I suggest that you purchase the book and thus provide appropriate financial incentives to encourage more work of similar quality.

PRICE V. FISHBACK, *University of Arizona and National Bureau of Economic Research*

American Agriculture and the Problem of Monopoly: The Political Economy of Grain Belt Farming, 1953–1980. By Jon Lauck. Lincoln and London: University of Nebraska Press, 2000. Pp. xiv, 254. $45.00, ^30.00.

Dr. Jon Lauck's book focuses on the problem of monopoly in grain-belt states from 1953 to 1980; in doing so, he analyzes a long-standing conflict that has touched the entire agricultural sector during the entire twentieth century. The late-nineteenth-century shift from small-scale farms, spread across the nation, to regional specialization in production, often on large farms, created a struggle in the agricultural and political community that has remained alive until today. Economic, political, and philosophical factors have been integral parts of the controversy. The media has been involved, as evidenced through discussions of unfair practices inflicted on chicken farmers by processors and discussion of rural outmigration. Environmentalists have added to the debate, suggesting that large farms deplete the environment by overusing resources. Politically, as the author states, many have felt that the USDA has supported agribusiness firms rather than the small-scale farmer. The political economy of these issues raises two questions. One, are the increases in farm and processor size efforts to gain market power, making it possible for these firms to engage in anticompetitive behavior? Two, farmers are often loath to leave the family farm behind, and are often thought of as the essence of U.S. history, raising questions as to whether policies should be implemented to maintain the family farm. Dr. Lauck's thorough documentation of the political economy of grain-belt farming from the late 1800s until the late 1980s indicates that answers to these questions are not easily found.

The author's thesis is that, from the late nineteenth century, monopoly was perceived as a threat to the American way of life. The popular notion was that erratic prices and increasing farm and processor concentration resulted from monopoly power. Dr. Lauck's analysis is divided into two parts: the first examines competitiveness in the livestock and grain sectors, focusing on food processing, meatpacking, and exports. The second analyzes the effectiveness of farmer voluntary organizations (collective bargaining and cooperative marketing) and government regulation (marketing quotas) in providing farmers with countervailing bargaining strength.

The author first traces the political climate in meatpacking; market power fears have been present since in the late 1800s (see Gary Libecap, "The Rise of the Chicago Meatpackers and the Origins of Meat Inspection and Antitrust," *Economic Inquiry*, April 1992). Much meatpacker power was removed by an FTC consent decree in the 1920s. After World War II, meatpackers faced competition from imports, and later, in the 1970s and 1980s, consumer demand shifted away from beef. The diminishing of processor power through the century was reversed in the 1980s, when vertical contracts began controlling a large share of livestock production. Lauck suggests that processor power over producers of grain, bean, and corn was similarly overstated; reduction in the number of processing firms was largely due to technological obsolescence, and competition from abroad and from the feed industry was enough to maintain economic competition. Lauck next examines whether the grain-trading cartels, the National Farmers Organization (NFO), farmer cooperative efforts, and state and federal regulation effectively increase prices. His exposition of the NFO suggests that countervailing bargaining power is possible, albeit difficult, to attain. The NFO turned towards collective bargaining, but its initial negotiations to receive higher prices from meatpackers failed. Next, taking more drastic steps, the NFO withheld livestock from the market, in one Missouri city for a week in 1959; three Midwestern cities for a week in 1961; and throughout the grain belt the next year. Prices responded only during the grain-belt-wide withholding in 1962, and the NFO claims to have entered forward-supply contracts after withholding livestock during this period. Political turmoil accompanied their efforts, and that while there is no hard evidence indicating that the number of contracts increased or that prices rose, the author presents some evidence suggesting that the NFO achieved limited success in securing contracts with meatpackers.

In sum, the author presents a fascinating, detailed account of the political and historical aspects of the long-standing debate over whether farmers have been victims of market power, dealt by agribusiness firms. His discussion reveals the difficulty inherent in analyzing agricultural economic history: researchers have not reached a consensus as to whether increased concentration, the diminished number of farms and processors, and erratic prices are the result of market power or market forces.

CAROLYN DIMITRI, *Economic Research Service, USDA*

Hard Work: The Making of Labor History. By Melvyn Dubofsky. Urbana and Chicago: University of Illinois Press, 2000. Pp. x, 249. $49.95, cloth; $17.95, paper.

The no-longer-so-New Labor History has reached that venerable age where its founders write reflective autobiographical essays to introduce collections of their publications. And well might Melvyn Dubofsky prepare such an essay and collection. Present at the creation of the New Labor History, Dubofsky has shaped the development of labor history as a teacher and researcher since the 1960s. He has written works essential to any study of American labor history, including the history of the International Workers of the World, *We Shall be All* (Chicago: Quadrangle Books, 1969), biographies of labor leaders John L. Lewis (*John L. Lewis: A Biography*. New York: Quadrangle/New York Times Book Co., 1977, written with Warren W. Van Tine) and William D. Haywood (*"Big Bill" Haywood*, Manchester: Manchester University Press, 1987), and a study of labor in American political economy, *The State and Labor in Modern America* (Chapel Hill: University of North Carolina Press, 1994).

With his contemporaries David Brody, Herbert Gutman, and David Montgomery, Dubofsky transformed labor history. In his introductory essay Dubofsky reminds us that when these four were preparing their dissertations and first publications in the late 1950s, labor history was a declining discipline dominated by economists associated with John R. Commons, Selig Perlman, and the University of Wisconsin. Culture and values, radical militants, nonunion workers: all these were largely ignored by a field absorbed with the evolution of organized labor. Instead, labor history was the study of trade-union institutions and their evolution away from radical class consciousness into responsible and conservative craft organizations. Studied as a teleological process, labor history was confined to the organized working class where institutions matured to adapt better to the conditions of modern capitalism. Instead of a labor history that made working people central to the broader field of American history, Commons and his followers wrote a history of trade unions largely isolated from the broad sweep of American history.

The New Labor History took as its mission expanding labor history to include the history of all working people, in or outside of trade unions, radicals as well as moderates. But if all the original four evangelists, Brody, Dubofsky, Gutman, Montgomery, rejected the narrow vision and conservative conclusions of the old labor history, they differed in their approach to labor history. Gutman, in particular, believed labor history should study workers without regard for their involvement in formal labor unions or other institutions of the traditional "labor movement." To him, labor history should be the history of changing popular attitudes and forms of association without regard for any framework imposed by social scientists' vision of the "proper" development of the working class.

Perhaps because it marked the sharpest break with the old labor history of Commons and Perlman, Gutman's cultural focus has come to be associated with the New Labor History. But his colleagues always maintained different perspectives. Like Gutman, Dubofsky sought to broaden the field beyond trade unions to study nonunion workers and popular protest outside of formal labor organization. But Dubofsky has continued to view workers as agents for social change and has looked to study the revolutionary potential of the working class. Directly challenging Commons and Perlman on their own terms, as scholars of the development of labor organization in the process of capitalist industrialization, Dubofsky has rejected their argument that some inexorable logic of capitalist industrialization leads workers towards narrow conservatism and craft organization. Instead, Dubofsky has defended the traditional Marxist view that conflicts will lead workers towards political radicalism. In his conclusions Dubofsky rejects the Commons-Perlman approach; but methodologically he has maintained their focus on trade-union institutions and politics.

Dubofsky's interests in labor organization, labor radicalism, and politics are well represented in this volume. Three articles come from his original research on the IWW, including his classic "The Origins of Western Working-Class Radicalism, 1890–1905" and a comparative biography of leading British and American syndicalists, "Tom Mann and William D. Haywood." These articles establish the compatibility of radical politics with American conditions. Indeed, *contre* Commons, Dubofsky argues that the success of the IWW and other radical unions in organizing workers in the capital-intensive industries of the American west shows that Marxists were right to expect that economic development would promote labor radicalism.

Establishing a link in this way between economic development and labor radicalism leaves unexplained the eventual failure of radical labor movements in the United States. This brought Dubofsky to look again at the relationship between worker and democratic government in the United States. In this volume, this work is represented by his papers on "The Wilson Administration and Organized Labor" and "American Industrial Workers and Political Parties from Roosevelt to Reagan." In these Dubofsky shows how the conduct of politics in the United States undermined the possibilities for radical politics, leaving labor little room "to maneuver politically" (p. 161). Unable to overcome ethnic divisions among the workers and the political impact of white racism, American labor was never able to establish a powerful, independent political force. Instead, the Democratic Party "captured and domesticated labor" (p. 163), leaving organized labor as one among many interest groups contending for favors from political authorities. The political struggles and events that led to this domesticated labor movement are central to Dubofsky's history because he puts the burden of American conservatism on these political processes rather than on capitalist industrialization as in Commons and Perlman.

Dubofsky has never believed that labor history can be studied apart from the history of employers, politics, and the development of modern capitalism and the modern world economy. These concerns separate Dubofsky from some of his colleagues in the New Labor History, but they make his work more accessible to social scientists and other scholars outside of labor history. I suspect that many economists and economic historians will not share his sympathy with wobblies, socialists, and other labor radicals. But all will benefit from spending time with the work of a premier labor historian who has thought long and deeply about the place of labor in our modern world economy.

GERALD FRIEDMAN, *University of Massachusetts at Amherst*

Wealth in America. By Lisa Keister. New York: Cambridge University Press, 2000. Pp. x, 307. $59.59, cloth; $19.95, paper.

Inheritance and Wealth in America. Edited by Robert K. Miller Jr. and Stephen J. McNamee. New York: Plenum Press, 1998. Pp. xiii, 226. $49.50.

The first of the two books, *Wealth in America*, fills an important gap in the recent literature on inequality in America. The most recent comprehensive treatment of wealth inequality in the United States is, perhaps, Robert Lampman's book, *The Share of Top Wealth-Holders in National Wealth, 1922–56*, (Princeton, NJ: Princeton University Press, 1962). However, its coverage of wealth inequality ends in 1956.

A more recent work of the subject, *Top Heavy: A Study of Increasing Inequality of Wealth in America* by Edward Wolff (New York: The New Press, 1996) updates the wealth statistics to 1992 but is aimed at a more popular audience and does not contain nearly as much detail as the Keister volume. Given the growing interest of academic economists, as well as journalists, in the issue of rising inequality in America, this book should be very timely and command a wide interest in the subject.

The book serves a twofold purpose. The first is to present descriptive statistics on wealth accumulation in total, and the distribution of household wealth. This portion of the book relies on publicly available databases such as the Federal Reserve Board's Survey of Consumer Finances (SCF) and other published works, including several of my own. The second is to develop a microsimulation model of households to investigate the process of household wealth accumulation. This model combines survey data such as the SCF with aggregate household wealth data drawn from the Federal Reserve Board's *Flow of Funds*, data from estate tax records, and simulated data that synthesizes information from each of these sources. The model recreates the accumulation process for 14 components of household wealth beginning in 1962 and ending in 1992, with some projections into the late 1990s. The model provides for annual statistics on the distribution of wealth (publicly available databases such as the SCF are available only for intermittent years). It also tracks family formation as well as family break-up and thus allows for a comprehensive treatment of demographic changes on wealth accumulation. It also allows for a treatment of wealth mobility over time.

The first chapter treats the definition of wealth and the reasons why wealth, as opposed to income, is important. For example, wealth can provide for long-term security, while income is aimed mainly at meeting pressing consumption needs. The second chapter provides details on both the available household-survey and estate-tax data and the operation of the microsimulation model. The third chapter covers what is, perhaps, the most interesting topic—the changing inequality of wealth in America. This material includes descriptive statistics from household surveys and estate-tax data, as well as the results of the microsimulation model.

The fourth chapter details information on the rich in America, and the fifth, the wealth holdings of the middle class and the asset holdings (or lack thereof) of the poor. Chapter 6 provides a comparison of the wealth holdings of the baby-boom generation in comparison with those of their parents' generation. Chapter 7 looks at the effects of changes in family structure on wealth holdings—an analysis that is particularly suited to microsimulation of households. It also investigates the role of economic trends, such as those in real estate and financial markets, and stock ownership patterns on the wealth-accumulation process. In chapter 8, the author investigates wealth accumulation over the life cycle, while chapter 9 concentrates on wealth mobility.

The book represents a formidable exercise in data analysis. My major concern and caution is with regard to methodology. The analysis presented in the book is based on microsimulation of households. Though the simulated wealth holdings (and other characteristics) of families are periodically aligned to outside information about wealth holdings and population characteristics, the results of the analysis rely heavily on the validity of the equations used to generate the simulation of households over time. The author does present "validity checks" on the accuracy of their simulations by comparing simulated results to actual known characteristics of households. This is, of course, crucial. However, it is still sometimes confusing about what are simulation results and what are results derived directly from actual household survey data.

There is one other concern with regard to methodology. In the alignment and comparisons of the simulation results with outside sources, there is the additional problem that the outside sources are themselves *inconsistent*. Indeed, the Federal Reserve Board *Flow of Funds* data, the various *Surveys of Consumer Finances*, and the *Estate Tax* data, which the author uses in her study, are themselves very difficult to reconcile, so that trying to reconcile the simulation results with each of these three sources is itself problematic.

The book is clearly and intelligently written and accessible to a wide audience—over and above academic economists and sociologists. However, in a way it is written a little too nontechnically. For example, in chapter 8 in the discussion of the life-cycle model, there is a total absence of equations that are normally used to test the model. This is alright for a general audience, but it may frustrate academic economists who have worked on the subject.

The second of the two books, *Inheritance and Wealth in America*, while similar in title to the Keister volume, differs in two important ways. First, it is a collection of essays by different authors. Second, while it does contain some historical data on inheritance patterns, its coverage is more by topic than by historical period. However, it is a timely volume in light of recent legislative proposals to end the taxation of estates entirely.

Despite these differences, the book provides some very useful insights into the patterns of inheritances in the United States and their change over time. The editors note: "inheritance constitutes an integral component of family, economic, and legal institutions, and a basic mechanism of class stratification" (p. 2). While economists are primarily interested in how inheritance affects savings, consumption, and the distribution of wealth, legal historians are concerned with moral and political justifications of inheritance and its regulation through taxation; sociologists with its effect on stratification and the class structure; and anthropologists on its impact on the kinship system. This book therefore covers a wide spectrum of issues connected with bequests. I will focus here on the three papers that deal with the legal and economic issues involved.

I found the first paper in the volume, "Inheritance in American Legal Thought," by Ronald Chester, particularly enlightening. He presents a historical overview of how inheritance has been treated in American law from Revolutionary times to the present. Interestingly, even in the eighteenth century, a distinction was drawn between the property rights of current owners and those that governed estates. Whereas the ownership of property and its disposition were generally viewed as a "natural right," the right to transmit and receive property at the time of death of its owner was viewed as a "civil" or "positivistic" right— that is, one conferred and controlled by the state, not as a natural right. In other words, it was generally felt that the state had the power to regulate the transmission of property after death.

This approach was developed by Thomas Jefferson and his circle and was directly influenced by British utilitarians such as Jeremy Bentham and John Stuart Mill. Laws of succession to property were developed as pragmatic ways to avoid the "endless disturbances" that might otherwise result. The Jeffersonians believed that allowing the dead to control property is dangerous to civil society, that the earth belongs to the living, and that the dead have neither powers nor rights over it.

As a result, from the late eighteenth to the late twentieth century, the courts adhered to a separation of inheritance rights from property rights, such that the latter were defended as a deeper and more fundamental, based upon a combination of natural, positive, and common law, while inheritance rights were viewed as limited by civil society. Moreover, taxes on inheritances were considered not as direct taxation of property but as indirect taxation on the process of transferring that property from the dead to the living. However, in 1987 in *Hodel v. Irving*, the Supreme Court appeared to reverse course and ruled that the ability to transmit property at death is a constitutionally protected property right. Chester attributes this new interpretation to the conservative revolution associated with the Reagan administration. However, because of the nature of the case (in particular, involving Indian-held lands), Chester feels that this case has limited application.

In a second paper Paul Menchik and Nancy Jianakoplos outline some general issues in the economics of inheritance. One is how much does inheritance, as opposed to lifetime accumulation, represent in total household wealth accumulation. In their review of the literature, they find that estimates vary from as low as 20 percent to as high as 80 percent. A second concerns motivations for bequests. There are three major explanations: altruistic models, in which individuals care about the well-being of future generations; exchange models, in which parents exchange promised bequests for current services performed by their children; and strategic models, in which parents use the promise of inheritance to influence their children's behavior.

In a third paper Barry Johnson and Martha Eller present a history of federal estate taxation. The modern federal estate tax was introduced in 1916 and its structure has remained largely unchanged to date, except for changes in filing thresholds, tax brackets, and marginal tax rates. One alteration, adopted in the Tax Reform Act of 1976, was to create a unified estate and (intervivos) gift-tax framework. Despite all the hoopla over estate taxes, Johnson and Eller note that "revenue from federal estate and gift taxes has lingered between

1 and 2 percent of federal budget receipts since World War II . . ." (p. 83). Moreover, during the 1990s, fewer than 2 percent of all estates were subject to a federal estate tax.

EDWARD N. WOLFF, *New York University*

The Impact of International Trade on Wages. Edited by Robert C. Feenstra. Chicago: University of Chicago Press, 2000. Pp. ix, 406. $62.00.

The Impact of International Trade on Wages is a volume of ten papers, along with remarks by discussants, which were originally presented at a National Bureau of Economic Research conference in early 1998. In general, the papers have a strong empirical orientation, and they are focused squarely on the experience of the United States over the last thirty years or so. Although economic historians cannot expect to learn anything about the international economy prior to 1960 from this volume, they might learn a great deal about different methodological approaches to the study of international trade and factor prices.

For the sake of a brief summation, I will focus on the papers themselves rather than on the discussants' comments, though I do believe that those comments often contribute substantial insights. The papers are grouped into three sections. The first section considers shifts of demand for different kinds of labor; the second section centers on the impact of changes in product prices; and the third section explores interregional and interindustry variation in international trade and wage changes.

Paul Krugman's "And Now for Something Completely Different: An Alternative Model of Trade, Education, and Inequality" (chapter 1) contains the most innovative theoretical work in the volume. He uses a model of labor-quality signaling and multiple equilibria to show that movement from one equilibrium to another may generate rising inequality even when there has been no real change in the skill-bias of technology or in the influence of trade on wages. The point is that economists ought not limit their thinking about rising inequality to trade- and technology-based theoretical explanations. Chapter 2, "Effort and Wages: A New Look at Interindustry Wage Differentials" by Edward E. Leamer and Christopher F. Thornberg, also includes an interesting theoretical approach to thinking about wage inequality. The authors suggest that relatively capital-intensive industries should offer relatively high-wage, high-effort jobs. A convincing measure of effort is difficult to come by, but the authors' empirical work does appear to support the theory. Moreover, the authors attempt to interpret changes in the shape of the relationship between capital intensity, effort, and wages over the 1970s and 1980s in light of a variety of changing economic forces, including international trade. In chapter 3, "Offshore Assembly from the United States: Production Characteristics of the 9802 Program," Robert C. Feenstra, Gordon H. Hanson, and Deborah L. Swenson find some qualified evidence that outsourcing has shifted demand away from low-skilled labor in the United States.

The second section of the volume begins with a very useful survey of the product-price/factor-price literature by Matthew J. Slaughter (chapter 4). The Heckscher-Ohlin approach to linking trade and factor prices has guided a large a branch of the trade and wages literature, and Slaughter surveys that literature with considerable insight. James Harrigan's "International Trade and American Wages in General Equilibrium, 1967–1995" (chapter 5) uses a flexible general-equilibrium model to study changes in factor prices over the period. The results do not support the hypothesis that changing import prices had a sizable direct effect on wage inequality. In chapter 6, "Does a Kick in the Pants Get You Going or Does It Just Hurt?" Robert Z. Lawrence takes a step toward finding an empirical link between import competition and technological innovation. As one might imagine, the measurement issues are a formidable challenge, and ultimately Lawrence's results are statistically fragile, but the connection is undoubtedly worthy of further study.

Andrew B. Bernard and J. Bradford Jenson lead off the volume's third and final section with "Understanding Increasing *and* Decreasing Wage Inequality" (chapter 7). The authors document differences and changes in returns to education and in residual wage inequality across states (that is, inequality unaccounted for by differences in observable worker characteristics). There is a substantial amount of variation across states, implying that regional labor markets are not extremely well integrated and that studies that focus on national averages may be ignoring important clues as to what has driven changes in wage inequality. Linda Goldberg and Joseph Tracy's "Exchange Rates and Local Labor Markets" (chapter 8) attempts to measure the extent to which exchange-rate movements affect local labor markets depending on the location's industrial composition. The estimated impact appears to be small in magnitude. In chapter 9, "Trade Flows and Wage Premiums: Does Who or What Matter?" Mary E. Lovely and J. David Richardson explore correlations between industry wage premiums and international trade during the 1980s. They pay careful attention to industry wage premiums by skill level and to the level of development of trading partners, and they find relatively strong effects coming from trade with newly industrialized countries, especially for skilled workers. Finally, in chapter 10, "Trade and Job Loss in U.S. Manufacturing, 1979– 1994," Lori G. Kletzer studies the process of labor reallocation that is popularly associated with increased exposure to international trade. She does not find strong evidence that increased foreign competition accounts for a substantial part of labor displacement from manufacturing. She does find that workers displaced from manufacturing often experience large earnings losses, especially if reemployed in trade and service industries.

Again, this volume has very little to say about economic history. Indeed, for the most part, the papers proceed as if international trade began sometime in the 1960s or 1970s. By ignoring historical experience and international comparisons, the book disregards a great deal of useful information. In return for the narrowness of view, the volume does manage to maintain levels of focus, cohesion, and evenness that are commendable, especially for a collection of conference papers. Anyone in search of a deeper understanding of the complex connections between trade and factor prices or hoping to learn more about how one might get an empirical grip on these issues will find this volume to be an outstanding guide.

WILLIAM J. COLLINS, *Vanderbilt University*

A Nation Transformed by Information: How Information Has Shaped the United States from Colonial Times to the Present. Edited by Alfred D. Chandler Jr. and James W. Cortada. New York: Oxford University Press, 2000. Pp. xii, 380. $39.95.

With the word information on nearly everyone's lips, it seems an opportune time for historians to finally join the bandwagon and bless this new factor of production that apparently is giving rise to a "new economy." (And they had better do so fast, before the darkening clouds of recession rain on the parade.) In this book, experts drawn from history departments and business schools take a look back at how we got to the present, and more than that, the unique role that information has played in economics, business, politics, and culture in the United States.

From the premise that "[i]nformation has always played a profound role in American society" (p. 298), these essays examine the evolution of information technology and infrastructure, from posts, telegraphs, telephones and radio, to computers and the internet. Each essay tries, with varying degrees of success, to explore as well how information has been used in production or consumed by consumers. The latter task is somewhat harder than the former, because information has been used in so many ways by so many individuals and institutions. But then, like all studies of the topic this one too confronts the challenge of definitional vagueness. What is information, and how can it be measured? There are no references to the earlier work by economists Fritz Machlup and Marc Porat, who attempted quantitative assessments of the subject. Though to be fair, with the many ramifications of information, ranging from the management of large organizations, to the homogenization of culture, to the strengthening of the nation state, it is also not clear if one could come up with a single, measurable unit.

Given this definitional murkiness, the book relies rather heavily on lists of hardware. Besides the familiar infrastructure noted above, we also get chronologies of major computer systems, operating systems, and software applications. Sometimes this materialist approach seems a bit literal minded, compared to the broader conception of the term that brings in politics and culture. Surprisingly though, for a book written largely by economic and business historians, there is little quantitative assessment even of the hardware of the information economy. Nor does any author take on board the controversy over the relationship between information technology and productivity. The statement "no nation would invest more than 5 percent of its GNP year after year in a technology that it did not perceive made economic sense" (p. 214) places an enormous amount of logical weight on that word "perceive."

Some of the best chapters, on the other hand, deal with the subtle interplay between the economics and politics of information. Looking back, it becomes clear how important and consistent has been the role of the American state in creating the informational infrastructure for the modern economy. Essays by Richard Brown and Richard John, for example, delineate the ways the competitive, decentralized American political system led to wide circulation of news and rapid construction of inexpensive postal facilities. Direct subsidies to computers and the internet followed this earlier tradition. Margaret Graham discusses the important role of the state in the formation of radio networks, and the subsequent role of antitrust policy in opening up space for new cable broadcast networks. In other cases, as with telephones, semiconductors and computers, procompetitive antitrust cases helped new entrepreneurs and start-up firms get hold of important new technology and permitted vital periods of competition. This tradition may continue with the current Microsoft case.

My feeling is that the book would have been stronger had it taken a somewhat narrower focus like this throughout. Attempting to look at both the supply and demand side, while considering politics, culture, and business sometimes seems like just too much. The introductory and concluding chapters have to rest with broad generalizations about America's cultural receptiveness to information and information technologies. Only a brief final essay by Lee Sproull on computers in United States households deals directly with distributive issues, pointing out how skewed by class and race access to computers has become. Any generalizations about the "U.S. love affair with information" (p. vi) begs for comparisons, and this book provides virtually none.

KENNETH LIPARTITO, *Florida International University*

Struggling With "Iowa's Pride": Labor Relations, Unionism, and Politics in the Rural Midwest Since 1877. By Wilson J. Warren. Iowa City: University of Iowa Press, 2000. Pp. xv, 185. $34.95, cloth; $19.95, paper.

This fascinating little book recounts a century of labor struggles at the John Morrell and Company meatpacking plant in Ottumwa, a small city in south-central Iowa and the author's hometown. The book is based on a series of articles that appeared in the *Annals of Iowa*. "Iowa's Pride" in the title refers to a trademark once used by Morrell.

The book begins with the establishment of the plant by the Morrell family from England in 1877. A period of personal but paternalistic labor relations prevailed until World War I. This was followed by a period of "welfare capitalism": new, formal employee benefit programs that nevertheless failed to resolve workers' dissatisfaction with the pace of work and abuse of authority by supervisors. Militant unionism took hold in the late 1930s and 1940s in the form of the Local 1 of the United Packinghouse Workers of America, resulting in much higher wages, sharply curtailed management control, and frequent strikes. But militancy declined at the Morrell plant in the 1950s and 1960s as a new generation of unionists sought greater cooperation with the company. Meanwhile unionists from Ottumwa and elsewhere revived the Democratic Party in Iowa, perhaps their only permanent legacy. After several layoffs, the Morrell plant closed in 1973. It was replaced by a smaller plant operated by Hormel until 1984 and then sold to Excel. The Excel plant is still in operation, but it employs fewer workers and pays much lower wages than Morrell did.

This book is *labor history*, not labor economics, by Gavin Wright's classification ("Labor History and Labor Economics." in Alexander J. Field, ed., *The Future of Economic History*. Boston: Kluwer-Nijhoff, 1987). The focus is not on markets but on the consciousness and experiences of workers, especially union activists. The author paints a colorful picture using interviews with several generations of Morrell workers, and documents in detail where these workers lived, where they worshiped, to what fraternal organizations they belonged, and even at what bars they drank, using information from the census, telephone books, Morrell documents, and the *Ottumwa Courier*. The author's intimate knowledge of Ottumwa is obvious throughout.

The author's main thesis is that labor militancy at Morrell was driven by *worker homogeneity*, both ethnic and residential. Militancy was low in the early days of when Morrell's skilled workers were immigrants (mostly from the United Kingdom) and unskilled workers were native-born. Militancy grew in the 1930s as the entire workforce became native-born. (By contrast, immigrants remained in management.) Moreover, most of the workforce lived in the same neighborhood. But militancy declined in the 1950s when workers no longer lived together but became dispersed throughout the city and the surrounding rural areas.

These local details are fascinating, but what do they explain? It turns out that the history of unionism at Morrell's Ottumwa plant closely paralleled the history at all Midwestern meatpacking plants—even those with different demographics such as the immigrant-dominated plants in Chicago. Unionism flourished briefly throughout meatpacking under federal government protection during World War I but was wiped out in the subsequent recession. Unionism returned to meatpacking in militant form under protection of the National Labor Relations Board, beginning in 1937, and the War Labor Board in the early 1940s, and with the assistance of the CIO. Since the early 1970s the entire industry has experienced falling wages, falling employment, declining unionism, and numerous plant closings, most likely due to changes in product-market demand, production technology, and management strategy (Wallace E. Huffman and John A. Miranowski, "Immigration, Meat Packing, and Trade: Implications for Iowa." Iowa State University Department of Economics Staff Paper no. 285. December 1996). Given these industry-wide shifts with apparently national causes (especially changes in federal policy) little seems to remain for purely local details to explain. The story seems to be told at the wrong level of aggregation (Wright, "Labor History and Labor Economics": 331) to qualify as *economic history*.

Only in the penultimate chapter, while describing the closing of the Morrell plant, does the author consider industry-wide shifts. Yet even here, the author finds the reasons for the closing "veiled in secrecy" (p. 120). Like his interviewee, he suspects a "grudge operation," attributing the closing to personal antagonism (p. 114). A labor economist would more likely suspect an end-game operation: once a union captures the quasi-rents from long-lived plant capital, the firm will cease investing in new capital. (See surveys by John T. Addison and Barry T. Hirsch, "Union Effects on Productivity, Profits, and Growth: Has the Long Run Arrived?" *Journal of Labor Economics* 7, no. 1 [1989]: 72–105; and by Peter Kuhn. "Unions and the Economy: What We Know; What We Should Know." *Canadian Journal of Economics* 31, no. 5 [1998]: 1033–56).

Nevertheless, this book provides a wonderful feel for how the dramatic labor struggles of twentieth-century America were experienced by workers and residents of a small Iowa city, all the more fascinating to an Iowa resident such as this reviewer.

WILLIAM M. BOAL, *Drake University*

From Ellis Island to JFK: New York's Two Great Waves of Immigration. By Nancy Foner. New Haven, CT and London: Yale University Press; and New York: Russell Sage Foundation, 2000. Pp. x, 334. $29.95.

In this impressive study of American immigration Nancy Foner presents an "interpretive synthesis" that "brings together strands from the mass of literature on past and present immigration" (p. 4). She argues that the historical and contemporary literatures have developed separately and that there is virtue in bringing them together. Here the historical period covers 1880 to 1920, and the contemporary period begins in the mid-1960s. Of necessity, Foner selects a narrow and manageable range of topics to review. Geographically, Foner limits her coverage to New York City, the "quintessential immigrant city." In terms of immigrant groups, for the earlier period Foner considers only the experiences of Italians and of Russian Jews, who dominated that immigration; for the later period, in which no two national groups are so dominant, she emphasizes the experiences of West Indians, Asian Indians, Chinese, and Korean immigrants. The chapters consider who the immigrants were, where they lived, the work they did (with a separate chapter on immigrant women and their work), the dynamics of race and prejudice, the maintenance of transnational ties, and the role of schooling.

This work is a review of the two identified literatures. No new material is presented. End notes comprise 44 pages; the references, and there are in excess of 625, fill another 34 pages. Despite these numbers, there is little recognition of the contribution of economists and economic historians to the immigration literature. Fewer than a dozen of the references cited can generously be identified as coming from the economics literature broadly construed. Nowhere, for instance, are the contributions of George Borjas, Barry Chiswick, or Michael Greenwood dealing with the contemporary migration found, and with respect to the historical migration four works of Claudia Goldin appear in the list of references; works by Timothy Hatton and Jeffrey Williamson appear but once. Foner's view of economics, furthermore, is unduly narrow: "migration is not simply a matter of rational calculations in response to market forces, as neoclassical and new economic theory would suggest" (p. 19). Yet, readers of this JOURNAL will recognize key themes that recur throughout the book. The roles of immigrant self-selection, of English language competency, of prior migrants in establishing ethnic niches in terms of both occupation and residence, and of utility maximization by immigrants are all key in both the historical and the modern stories that are presented. Economic historians will leave this book strongly aware not only that the historical and contemporary literatures have developed separately, but even more so that disciplinary literatures can be starkly independent of each other.

Foner reminds us of the importance of fate, of community, and of the immigrants' own background in shaping the path of their experience in America. For instance, we are told that "Russian Jews arrived at a propitious moment, when the garment trades were undergoing a rapid expansion as the demand for factory-made clothing surged," that "the industry was already in the hands of Jews—albeit German Jews who had immigrated in large numbers in the mid-nineteenth century," and that "a high proportion of the Russian Jewish newcomers had had tailoring experience in the old country" (pp. 79, 80).

Foner presents an extended and exceptionally rich discussion of the process by which immigrants develop niches in certain occupations (pp. 91,ff.), and her discussion (in chapter 6) of transnational ties is unusually complete and fascinating. Foner also reminds us that immigrant schools in the "good old days" were often harsh and unproductive and that assimilation was (and is) a complex and time-consuming process. She presents a profoundly disturbing slice of today's research that suggests that becoming American in our contemporary setting can have high cost for immigrant youth. The argument, basically, is that American teenagers are horrible role models, that they lead immigrant youth toward activities that give quick gratification and away from making the investments necessary for developing needed education and skills. The literature Foner cites suggests this problem is especially critical for black and Hispanic immigrants who adopt a peer culture that has its roots in racial inequality and poverty (pp. 211–12).

Overall, however, Foner is optimistic. She debunks the sanitized view of the inevitable upward progress of our immigrant grandparents, and she debunks the often found modern view that today's immigrants are not up to the standards set by the previous immigration. The final chapter identifies the changing perceptions of race, the prospects for multigenerational occupational mobility, and immigrants' transnational connections as being key to the story yet to be written for today's migrants.

Economic historians may wonder why their literature was overlooked, and they may note where straightforward economic argument would have facilitated the development of the story. Nevertheless, for those who want a concise statement of the sociological literature dealing with these two great migrations, Foner's book is an excellent source.

JAMES A. DUNLEVY, *Miami University*

Bloodless Victories: The Rise and Fall of the Open Shop in the Philadelphia Metal Trades, 1890–1940. By Howell John Harris. Cambridge: Cambridge University Press, 2000. Pp. xvii, 456. $44.95.

The trajectories of American labor-management relations over the industrial era have exerted an enduring fascination dating back to such works as John R. Commons' *Trade Unionism and Labor Problems* (1905) and Werner Sombart's *Why Is There No Socialism in the United States?* (1906). In the intervening century, historians from a range of subdisciplines have offered a welter of hypotheses to explain the absence of working-class solidarity, the comparative weakness of American labor unions, and the strength of alliances (paradoxical in a democratic society) that businesses achieved with agencies of the state to counter workers' efforts to achieve political or economic power.

In all these studies, it is axiomatic that American business leaders would viscerally (perhaps congenitally) oppose any collective efforts by workers to enhance their bargaining power in the labor markets that governed their working lives. But until now, no scholar has entered the historiography with a fine-grained portrait of just how employers at the local level gained and exerted control over those markets to wage war against organizing drives and to preserve the open shop. Practical difficulties argue against such a study because the local employers' associations that carried that fight seldom left any records at all. As important, the topic demands a rare interlocutor: even-handed with sympathy and understanding for both sides in the battles between capital and labor.

In his account of Philadelphia's Metal Manufacturers' Association (MMA, 1903 –present), Howell Harris has written an exemplary narrative with insightful analysis that traces the successes and failures of a typical employers' association in its efforts to gain an upper hand in local labor markets while also responding to evolving industrial policies originating at the state and national level, and to secular trends in the economy at large. Sympathetic to proprietors and workers, Harris's study effectively roots his local Philadelphia story within the national political, institutional, and economic contexts that, by the 1930s, came to exert a dominant influence on the MMA and its latitude for action. Casting a broad research net, Harris drew upon a fine but fragmentary MMA archive, papers of individual participants, corporate and labor-union records, the National Metal Trades Association records, state industrial reports, the papers of a host of federal agencies, and dozens of trade and labor publications. By creating supple databases, he melded and extended these sources in ways that serve (rather than overwhelm) his narrative and analysis, or the reader.

To give an all-too-brief summary: the MMA originated in 1903 among those mid-sized Philadelphia metalworking firms that are now well known thanks to Philip Scranton's *Endless Novelty* (Princeton, NJ: Princeton University Press, 1997). Harris offers a nuanced account of the local entrepreneurial class to show how and why this group of acquisitive individualists felt compelled to take collective action to preserve their right to manage. While Philadelphia's machinery builders may still strike some modern readers as an aberrant batch alternative to the mass production monolith, Harris takes care to show that national forces and institutions drove the MMA's rise as labor-management contests heated up around the country. In Philadelphia (as elsewhere) employers' associations gained their initial rationale (1904–1914) in vanquishing union organizing drives by providing tactics and replacement workers for member firms that held the line for the open shop. While the MMA's Labor Bureau clearly served capital's interests, Harris notes that this recruitment tool only succeeded because job-seekers benefited as well. With its tactical advice and labor referrals, the MMA and its member firms had little need for the injunctive powers of the local judiciary, although Philadelphia's governmental machinery clearly favored employers in this period. In leading the city's open-shop drive of the pre–World War I era, the MMA earned the first "bloodless victory" of the book's title.

This dramatic period dominates the first half of the book, but developments after 1914 (and particularly after 1923) are perhaps more intriguing. Worker insurgency grew with the full-employment conditions of World War I. Notwithstanding the half-hearted support for labor provided by various Wilsonian agencies, the MMA preserved its dominance over the city's labor markets. With Harding's normalcy, however, the MMA seemed to outlived its purpose, as organized labor scarcely had a pulse in Philadelphia during the 1920s. With nothing left to fight, the MMA evolved in this period to take up a late-Progressive rationalizing agenda. Mirroring the national rise of welfare capitalism, the MMA established ties with researchers at the Wharton School of the University of Pennsylvania, it undertook inquiries into the causes of labor turnover, and it offered expanded personnel-management services to member firms. These initiatives mirrored the national rise of welfare capitalism. But Harris is equally nuanced and convincing in tracing the Orthodox Quaker reformist impulse that came to dominate in the MMA during the 1920s. Seeking to reconstruct "the very basis of right human relations," MMA President Morris Leeds genuinely sought to cure the ills of capitalism (p. 347, quoting Leeds). Harris describes Leeds' substantive efforts to that end, but good intentions and the MMA itself would soon take a severe battering in the prolonged depression of the 1930s.

In three closing chapters, Harris takes the MMA through the Great Depression and down to the present. After organized labor gained its New Deal patrons, the open shop was doomed—the second "bloodless victory" of the title. But Harris credits the MMA with "speed, good grace, and intelligence" in adjusting to the new political economy (p. 354). Indeed the rise of the regulatory state and newly powerful unions gave the institution a new rationale after 1937: to guide its members in understanding and negotiating with the new institutional players whose legitimacy could no longer be denied.

Bloodless Victories is essential reading for anyone interested in the industrial history of the twentieth century. For economic historians in particular, this account shows the complex, constructed, and historicized qualities of "the market." While his immediate focus is labor markets in Philadelphia, Harris correctly and resolutely claims in his very first sentence, "This is a book about power" (p. i). This study succeeds admirably and even-handedly in tracing, for one representative arena, the ubiquitous contest of the industrial age.

JOHN K. BROWN, *University of Virginia*

Making the Nonprofit Sector in the United States: A Reader. Edited with introductions by David C. Hammack. Bloomington: Indiana University Press, 1998. Pp. xix, 481. $19.95, paper.

At the dawn of the twenty-first century American historians are unusually qualified to advise on social policy. With the passage in 1996 of the Personal Responsibility and Work Opportunity Reconciliation Act, much of the nation's system of public assistance for the poor was given back to the states and localities. President Clinton promised to "end welfare as we know it," and he did. But President Clinton did not abolish welfare: in dimensions financial, legal, administrative, and moral, too polyphonous to name here, the new system is trying to revive welfare as *historians of the 1870s know it*. Likewise, with the inauguration of President-elect George W. Bush, the government's social services—from poverty alleviation to drug abuse treatment—will go, Bush promises, increasingly into the hands of faith-based groups. Of course the economic viability and the constitutionality of the faith-based subsidy is being challenged. And again, history offers ample precedent. The vision of the President-elect, and the immediate reactions against it, are reminiscent of the 1820s and of the 1870s–1900s movement to organize charity. So it is especially fortunate that the historian David C. Hammack has published the essays and excerpts that comprise *Making the Nonprofit Sector in the United States: A Reader*.

The volume is designed for students and "the practical nonprofit leader" (p. xi). It contains 41 excerpts and essays on nonprofits and their subject matters, from the Elizabethan Poor Law of 1601 (pp. 9–13) to the essay of W. E. B. Du Bois on *Economic Cooperation among Negro Americans* (1907) (pp. 264–80). Interleaved are brief introductions by the editor which intend to situate each entry historically and in contemporary scholarship. The topical organization of the volume will be especially useful for Hammack's intended user: it is divided into readings on colonial theory and established churches; colonial reality and religious diversity; the Constitution, limited government, and disestablishment; voluntarism and the Constitution; the variety of religious nonprofits; nonprofits as alternative power structures; the rise of science, foundations, and professionalism in social service; and the rise of federal funding and regulation. Unfortunately, the benefit of good topical organization is offset by an absence of the quick-reference glossary and index that managers crave.

The number and the selection of entries within topics are sometimes hard to figure. Lack of scholarly coherence and institutional representation are particularly evident in Part 6, "Nonprofit Organizations as Alternative Power Structures." Here one finds three entries: the Du Bois excerpt, and one entry each (both published after 1980) on women in antebellum Virginia and on women and the voluntary sphere. The entries are written by acknowledged experts in the field. Yet the secondary literature on Du Bois is enormous and contested in a most exciting way; Du Bois has his Booker T. Washington, and the writings of transitional women reformers of the nineteenth century (such as Josephine Shaw Lowell) are no less seminal. In like fashion, Utopian, cooperative, and radical manifestos are missing from this section, as is any discussion of pivotal eighteenth-century self-help groups organized by and for African Americans.

The volume is hardly designed for historical economists. The intercalary chapters will frustrate any reader in search of a quantitative understanding. For example, in an introduction to the well-chosen essay of Amos Warner (his *Argument against Public Subsidies to Private Charities* [pp. 286–300]), Hammack asserts that nonprofits were "small" (p. 283). Hammack says that "[m]unicipal and county governments . . . had always provided most of the funds needed for . . . the care of the poor, the elderly, and the sick" (p. 284). The cliometric literature is still quite small, yet enough evidence has been collected to show that in the last quarter of the nineteenth century, in Indianapolis and in other cities, nonprofits for the care of the poor bulked as large or larger than public assistance (J. Hannon and S. Ziliak in S. Carter et al., eds., *Historical Statistics of the United States* [New York: Cambridge University Press and U.S. Bureau of the Census, forthcoming]). In like fashion, Hammack says "[f]or much of American history the nonprofit sector seems to have grown as fast the American economy as a whole" (p. xvii). And yet the economist Burton Weisbrod has shown that the percentage of national income originating out of the nonprofit sector collapsed by more than 50 percent from the Great Depression through the Second World War, and did not return to its pre-Depression percentage (of about 4.5 percent) until 1985 (*The Non-Profit Economy.* [Cambridge, MA: Harvard University Press, 1988: 65]). Unfortunately, the stark absence of quantitative measure is often eclipsed by a clean and confident style of writing that may, on balance, mislead decision-makers of more than a little influence.

Many will profit from the volume nevertheless. The inclusion of the 1908 piece by Warner (from chapter 17 of his classic, *American Charities.* [New York: Thomas Y. Crowell, (1894]) is probably itself worth the price of the book: in Warner one finds a leader of the (Protestant) Charity Organization Society, a professor of economics at Stanford University, and a hero of President-elect George W. Bush's spiritual advisor Marvin Olasky, who is at the turn of the century—at least in the mind of his detractors—a laissez-faireist and social Darwinist, and who is arguing fiercely *against* the use of public subsidies for faith-based charities. That makes for an interesting circle of history, a Mobius strip, to tease or to fear.

STEPHEN T. ZILIAK, *Georgia Institute of Technology and Bowling Green State University*

Martin C. Melosi. *The Sanitary City: Urban Infrastructure in America from Colonial Times to the Present*. Baltimore: Johns Hopkins University Press. 2000. Pp. xii, 578. $59.95.

The Sanitary City: Urban Infrastructure in America from Colonial Times to the Present by Martin V. Melosi is a big book in every sense. It spans a multitude of disciplines, including economics, environmental policy, history, law, political science, and public health. It raises important arguments that will provoke debates and stimulate further research for years to come. And it is thorough and comprehensive, revealing an author who has been reading and thinking about the issues at hand for a very long time. A book this big needed to be nearly 600 pages long.

The Sanitary City analyzes the development of urban water and waste-disposal (sewerage and garbage) systems in America from their inception to the present day. As I read the book, a central theme structures the largely chronological narrative: the construction of water and sewer systems did not take place in a vacuum. They were, instead, shaped by a larger urban and environmental context and, in turn, helped shape the broader social context. Important variables in this process were: diseases, particularly typhoid, and theories of disease; the thinking of engineers and public health experts such as Edwin Chadwick and Sedgwick; politics; the law; and economic and demographic change.

Melosi has done a fantastic job surveying the relevant secondary and primary historical sources. As a result, anyone working in this general area will want to consult *The Sanitary City* first. I for one have had the book only a few months and have already consulted the endnotes and bibliographic essay on multiple occasions. With one notable omission, which I discuss later, it cites and discusses all of the relevant sources and literature, both well-known and obscure. I was particularly impressed by Melosi's handling of how the law and legal institutions affected the development of urban infrastructure. The book even includes a discussion of the little known case of *Hawkins v. Shaw*, which involved a group of African Americans suing a small town in Mississippi for discriminating in the provision of water and sewerage services.

Chapters 1–5 focus on the period from colonial times to 1880. In these chapters Melosi describes how ideas about disease and sanitation spread from England to the early United States, and he describes how fear of disease, particularly cholera and yellow fever, helped fuel the construction of urban water and sewer systems. I find two aspects of this discussion especially well done. The first is Melosi's discussion of how the miasmatic theory of disease, although largely incorrect in terms of the underlying science, gave impetus to early efforts to construct water and sewer systems. The second is his discussion of Edwin Chadwick, who popularized the idea that sanitary cities were also healthy cities. Here Melosi argues that Chadwick's belief that the state should take an active role in making investments in public water and sewer systems, presumably because they generated large positive externalities, was inspired by the thinking of Bentham and Ricardo.

Chapters 6–13 focus on the period from 1880 through 1945. This period witnessed the rapid growth and development of water, sewerage, and waste-disposal systems. Several forces animated this development. For example, demographic changes, such as industrialization and urbanization, increased the desirability of water and sewer systems, and the reform movements of the Progressive Era promoted the idea that improved water and sewer systems had a "civilizing influence" and broad social benefits. At the same time, the development of the germ theory of disease undermined the miasmatic theory of disease and the idea that filth directly caused human sickness and poor health. Chapter 7, which explores the rise of publicly owned and operated water companies, is particularly well done. Unlike much previous historical work, Melosi does not blindly parrot the claims of Progressive-Era reformers, who believed that publicly owned water companies did a better job providing pure water than did private companies. Instead, he provides a balanced and careful consideration of the pros (such as low rates, broader service areas) and cons (such as inefficient pricing, patronage) of public ownership.

Chapters 14–20 focus on the post–World War II period. This period was marked by suburbanization, mounting concerns about industrial pollution, problems dealing with the disposal of garbage, and the imminent decay of water and sewer systems built during the late nineteenth and early twentieth centuries. A particularly important change during the twentieth century was the increasing emphasis on industrial, as opposed to bacteriological, sources of water pollution. During the early twentieth century, some experts even believed that an element of industrial pollution was a good thing because it killed off potentially harmful bacteria.

My only criticism is with Melosi's treatment of economics, which is not in keeping with this otherwise outstanding book. Graphs and tables are poorly presented and interpreted, particularly when they deal with economic or financial issues. For example, Melosi always presents data on local spending in nominal rather than constant dollars, making it very hard to say anything about historical trends in spending. There is also the sloppy use and discussion of economic concepts such as price elasticity of demand (p. 109), path dependence (pp. 8–14), and monopoly. Finally, Melosi omits from the book a discussion of the relevant economic literature on urban water utilities, particularly the work of Oliver Williamson, Pablo Spiller, and the like.

On the book jacket a reviewer predicts that this book will become the standard text on this topic for "many years to come." Whatever my quibbles with its treatment of economics, I agree. This is an excellent book. Anyone working in the area on public health or environmental history must have it on their shelf.

WERNER TROESKEN, *University of Pittsburgh*

Women in Labor: Mothers, Medicine, and Occupational Health in the United States, 1890–1980. By Allison L. Hepler. Columbus: Ohio State University Press, 2000. Pp. xii, 177. $18.95, paper.

Progressive Era reformers believed that women workers needed to be protected from the hazards of industry because women were mothers. These reformers defined motherhood broadly, maintaining that a mother's duties included caring for and rearing children and maintaining safe, moral, and healthy households. Maximum-hours limits and prohibitions on night work were intended to protect working women from being too fatigued to perform these duties. In *Muller v. Oregon*, the U.S. Supreme Court upheld gender-specific protection laws, justifying the state's role in this arena by asserting it was in the public interest "to preserve the strength and vigor of the race" (p. 23).

Allison Hepler argues that the view of women as mothers shaped discussions of women's occupational health throughout the twentieth century. But over time, the definition of motherhood changed from the socially constructed duties of childrearing to the biological role of childbearing, and increasingly, the responsibility for protecting workers' health was shifted from the state and employers to the workers themselves. Hepler argues that these changes in part resulted from the change in medical philosophy from a holistic, environmental view of health to a much more narrow and specialized focus on disease. Increasingly, medical professionals linked illnesses to particular pathogens rather than to bad environments and hence linked occupational diseases to particular activities within the workplace. But Hepler contends that the movement for the Equal Rights Amendment also played a role in this process. ERA supporters claimed that gender-specific protection laws promoted gender discrimination in the workforce and argued that individual rather than gender characteristics determined the suitability of a worker for a job. The mass entry of women into manufacturing during World War II served to reinforce this view, as employers found that some women were stronger and more capable than some men. The war experience also exposed the inconsistencies of protective labor laws. The maximum weight that women were allowed to lift and carry, for instance, varied greatly across states and in many states, was less than the weight of a toddler. But protectionists also used the wartime experience to support their position. They noted that absenteeism in war-related industries was higher among women than men and attributed this to women's responsibilities at home. Hepler argues that the protectionists' strategy had the unintended consequence, however, of supporting employers' claims that a worker's health was more dependent on her personal behaviors than on conditions in the workplace. In the decades following the war, gender-specific labor laws were eliminated or expanded to include men. In their wake, some private companies instituted fetal protection policies which prohibited childbearing women from certain areas of the workplace. Women once again were being targeted because they were mothers, but now motherhood was defined narrowly as the biological ability to bear children. In 1991 the U.S. Supreme Court ruled that fetal protection policies were unconstitutional and gave women the right to choose the risks they faced in the workplace.

Hepler provides a convincing account of how changes in medical theory and practice and the debates among feminists shaped discussions of women's occupational health in the twentieth century. Particularly effective is how Hepler weaves these themes together by examining the career of Alice Hamilton, an industrial toxicologist and protectionist who dropped her opposition to the ERA in 1952. But Hepler's history seems incomplete. The variation in protective legislation across states and across industries that Hepler notes at several points in the text call attention to the fact that these laws were the outcomes of a political process. Yet this political process receives almost no discussion. The motives and actions of unions and employers—groups that also played roles in shaping protective legislation—receive only brief mention. As Hepler notes, ideas about occupational health are the "complex product of political, economic, social, and cultural pressures by employers, workers, state and federal health officials and physicians" (p. 1). Hepler adds greatly to our understanding of some of these interactions but leaves other unaddressed.

One important conclusion that emerges from Hepler's research is that promoting equal treatment in the workplace has its costs. A recent study found that women, especially mothers, are more stressed at work than men. As Hepler writes, "For wage-earning mothers, this study must have seemed like a waste of research money" (p. 128). Even though labor laws no longer take into account women's domestic roles, these roles continue to impact women's occupational health.

CAROLYN MOEHLING, *Yale University*

The Boston Renaissance: Race, Space, and Economic Change in an American Metropolis. By Barry Bluestone and Mary Huff Stevenson, with contributions from Michael Massagli, Philip Moss, and Chris Tilly. New York: Russell Sage Foundation, 2000. Pp. xiii, 461. $45.00.

The greater Boston area has experienced a remarkable economic resurgence in the last two decades. Beginning in the late nineteenth century the declining fortunes of its leading manufacturing industries—textiles and boots and shoes—contributed to a sustained economic slide that was not reversed until the early 1980s. By 1982 a Brookings Institution study citing high and rising unemployment, rising crime rates, poor housing, municipal debt burden and tax disparity ranked the Boston SMSA near the bottom of urban America, below cities such as Detroit, Gary, Newark, and Oakland. These trends were sharply reversed in the 1980s and early 1990s, however. Propelled by the rise of high-technology industries, the growing importance of medical care in the economy, and the contributions of its concentration of colleges and universities, Boston ranked first among urban areas in growth of median family incomes during the 1980s, while its surrounding suburbs ranked second among suburban areas.

The Boston area's rapid recovery in the 1980s and early 1990s provides the context for this study, which explores the effects of the region's economic transformation on the lives and livelihoods of the region's residents. Drawing on a specially conducted survey—the Greater Boston Social Survey (GBSS)—and a set of interviews with area employers, the authors explore the links between labor-market outcomes on the one hand and residential location, ethnic and racial discrimination, and the region's shifting industrial structure on the other hand. Funded by the Russell Sage Foundation and the Ford Foundation, the GBSS was designed in collaboration with researchers studying three other cities—Los Angeles, Detroit, and Atlanta—where similar surveys were conducted, and collected data on residential location and characteristics, racial and ethnic attitudes, and labor-market experience from interviews with a stratified sample of 1,820 adults from throughout the metropolitan area.

After an introductory chapter that describes the transformation of the Boston economy, poses the questions that motivated their study, and articulates a theoretical framework for analyzing their results, the authors devote the next three chapters to laying out what they describe as Boston's triple revolution: the transformation of the region's demographic composition, industrial structure, and spatial organization. Demographically, the recent influx of blacks, Hispanics, and Asians has transformed a previously white ethnocentric region into a much more racially and ethnically diverse, and multicultural community. Industrially, the region has moved from a "mill-based" to a "mind-based" economy. Spatially, the central city's dominant role as hub of the regional economy has declined while surrounding regions have become more important, producing a truly metropolitan region.

Beginning in chapter 5, the book shifts gears, embarking on an extended analysis of the results of the GBSS in conjunction with data gleaned from federal censuses and a survey of Boston area employers. Chapter 5 introduces the survey and uses it to paint a quantitative picture of the region's population in the early 1990s. As this initial exploration makes clear, there are large differences in occupation, earnings, and residential characteristics by race and gender. In Chapter 6 the survey data are used to explore racial and ethnic attitudes—including how each racial and ethnic group sees itself and others, the prevalence of stereotypes, and the extent to which individuals have experienced racial or ethnic discrimination—as well as the extent of satisfaction with a wide range of community characteristics—including the quality of schools, police protection, and city services, among others. Chapter 7 documents the persistence of a high degree of racial and ethnic segregation in housing within the Boston area, and attempts to identify the reasons for this segregation. Although housing costs in the Boston area are generally quite high, the evidence shows that a large fraction of the region's minority families could afford to live in white communities. Thus affordability is not the issue. Instead, the authors point to discrimination, differences in access to information, and varying perceptions of desirability of particular communities as more important factors. Intriguingly, the authors document a pronounced incompatibility between different groups in their perception of the ideal racial and ethnic mix of residents in a community.

In Chapters 8 and 9, the survey data are used to explore in more depth the sources of differences in labor-market outcomes. Chapter 8 focuses in particular on less-educated workers, confining much of its analysis of individual variation in earnings to those with no more than a high school diploma. Chapter 9 offers a more general analysis of earnings. The results of these investigations suggest that while the substantial differences between white and Hispanic earnings are due largely to differences in human capital characteristics, simply increasing black human capital would not equalize earnings. Rather it appears that black workers in Boston continue to suffer from the effects of discrimination, as well as the negative consequences of what the author's term "cultural capital"—negative impacts of family background and residential location. The final substantive chapter, chapter 10, augments these findings with the results of a survey of Boston-area employers showing that employer attitudes, the location of jobs, and rising skill requirements for even entry-level jobs all pose problems for the economic advancement of minority workers.

The GBSS and employer survey that underlie this study are rich sources of data about the economy of the Boston area in the early 1990s, and the authors do a good job of presenting and analyzing these data. Their attention to the historical context that produced these data is also gratifying, but the authors do little to integrate this historical background into their analysis of the data. Further, as interesting as the data they have gathered may be, from the perspective of historians seeking to chart change over time and identify the explanations for these changes, they represent a single observation. One can only hope that subsequent studies will follow up with the collection of similarly rich sources of data in the future.

JOSHUA L. ROSENBLOOM, *University of Kansas*

GENERAL AND MISCELLANEOUS

Political Economy in Macroeconomics. By Allan Drazen. Princeton, NJ: Princeton University Press, 2000. Pp. xiv, 775. $55.00, £ 35.00.

This is a colossal volume—it is at once a compendium, a survey, a critical evaluation, and a clear exposition of a burgeoning field. It is a study of macroeconomic policy-making in the presence of heterogeneity across actors. Allan Drazen argues that the *sine qua non* of politics is the conflict of interests; and the core of political economy is that policies are the outcome of the interaction of competing individuals and groups whose concern is their own welfare and not necessarily that of any other individuals or the society as a whole.

This is a comprehensive analysis. After three introductory chapters on the tools of political economy (an extraordinarily useful, concise summary of the fundamental concepts— voting theory, lobbying, and transactions-costs politics as well as the core growth and overlapping generations models), Drazen makes a compelling case for a political interpretation of the time-consistency (TC) problem. In its usual form, an infinitely-lived, social-welfare-maximizing social planner "surprises" a representative agent with a higher policy (tax or inflation rate) after the agent has taken an irreversible action (chosen an investment level, or signed a fixed-term nominal contract) in the absence of a commitment technology. This structure (representative agent and social planner) seems as far from political economy as one can possibly reach. Yet Drazen argues that while all individuals are identical *ex ante*, there is heterogeneity *ex post*. If the representative agent is the average of many (perhaps identical) agents, any agent desires a low tax rate (to preserve income) but to have the representative agent taxed at a high rate (to provide more public goods). So the ordinary agent internalizes the government's budget constraint in its objective function while the representative agent does not, creating a heterogeneity across agents, and hence a political problem.

This is clearly not the standard view of politics, in which (ex ante) heterogeneity across preferences leads to conflict over the use and control of power, especially the power to tax and redistribute. Yet it is a somewhat compelling logic and does speak to the much-needed problem defining the breadth or scope of this field.

Part 3 adopts the standard approach to heterogeneity: across preferences over goods or policies, across incomes or ideologies. In a series of chapters on the political business cycle, redistribution of private and public goods and economic reform, politicians (who may be office- or policy-motivated) choose policy in a variety of contexts. They optimize over the policy space in the presence of an electorate who may vote them out of power, or to bind the hands of any successor, or to signal their competence (again with an eye to re-election), or to limit the policy choices of another branch of government. They might have to determine policy in the presence of information asymmetries —perhaps about the consequences of a reform, or a policy's impact on the voters—or amidst uncertainty about who might bear the burden of a policy reform, perhaps in the context of deteriorating economic conditions ("crisis"). Policymakers may even find themselves incapable of action given the political conditions, leading to delay, which may exacerbate a crisis even when all players know that reform is necessary ("sclerosis"). Drazen provides a lucid analysis and critique of these models, and leaves the reader with guideposts for future research.

The topics collected in Part 4 (growth, international political economy, and reform) are exercises in application of models of heterogeneity. The chapter of the political economy of growth sorts out the links between economic conditions (income or education inequalities, capital market imperfections, externalities, and so forth), political institutions (property rights, voting rules, socio-political instability, and democracy versus autocracy more generally) and factor accumulation.

The long, 91-page chapter on international political economy focuses on exchange rate arrangements (fixed versus floating, monetary union), policy interdependence and cooperation, and capital flows. Drazen relies again on the problems of TC and credibility as explanations for policy choices—analyses of other sorts of heterogeneity are sorely missing. Drazen reverts to the malaise facing the rest of the subfield here—he avoids identifying gainers and losers from alternative exchange-rate or capital-control regimes (this is tricky after all), and therefore the politics of exchange rates (or capital controls) is under-theorized. Attempts at explaining moves towards monetary union are consequently unsatisfactory because of the insistence of sticking to problems of credibility as the explanation. Drazen himself admits this frustration.

The chapter on reform and transition takes the usual economists' point of view: political conditions are immutable, and the problem is to establish who bears the burden of economic reforms in order to understand if these reforms are politically feasible. In fact, most major reforms take place in the midst of significant political changes (new constitutions, changes in power, attempts to cement a nascent democracy, and so on), and the success of these political reforms is often constrained by economic variables. Drazen avoids discussing the economic determinants of political change, especially the role macroeconomic conditions might play in affecting political reform.

The political economy of international trade is absent (worthy, no doubt of a book itself). Drazen also avoids another crucial dimension of policy-making—the role the political institutions might play in affecting policy choices. Heterogeneity alone is not sufficient to determine policy outcomes; how the conflict is mediated via the political institutions matters, too. Institutions solve problems of preference aggregation, act as sources of information, or facilitate information transmission. They act as coordination devices in the presence of multiple equilibria. Variations across institutional structures internationally (and hence policy-variation internationally) merit only passing reference.

In 700 pages Drazen has produced an ideal textbook that will be widely used in courses in macroeconomics and political economy. It is also a superb reference book for the scholar to dip into when the quick insight into an important subfield is needed, and for the researcher in the field, it provides a unified exposition with pointers as to where the existing work is deficient. This book informs, examines and stimulates. It will set the standard for years to come.

B. PETER ROSENDORFF, *University of Southern California*

Essays on the Great Depression. By Ben S. Bernanke. Princeton, NJ: Princeton University Press, 2000. Pp. vii, 310. $35.00.

Ben S. Bernanke's *Essays on the Great Depression* make satisfying reading. Spanning microeconomic foundations and macroeconomic outcomes, the book pulls together articles containing some of the best and most conclusive research on the economic catastrophe of the 1930s. Bernanke's work, with co-authors Harold James, Ilian Mihov, James L. Powell, Martin Parkinson, and Kevin Carey, tackles key questions head-on; here the reader will find lucid treatment of the role played in the crisis by worldwide operation of the gold standard, as well as dissection of key developments in interwar labor-market institutions. The work is on the methodological forefront, with a number of careful comparative analyses across nations and industries.

After decades of disquieting debate between Keynesians and monetarists, these essays make it clear that the economists who have been researching the causes of the Depression during the final decades of the twentieth century now know "what happened" to transform the Roaring Twenties into social disaster. First, ". . . there is now overwhelming evidence that the main factor depressing aggregate demand was a worldwide contraction in monetary supplies" (p. viii). Moreover, ". . . subsequent to 1931 or 1932, there was a sharp divergence between countries which remained on the gold standard and those that left it, and . . . this divergence arose because countries leaving the gold standard had greater freedom to initiate expansionary monetary policies" (p. 21).

Bernanke presents a comparative analysis drawing on data from some two dozen countries, in which he estimates relationships between production and prices, wages, measures of adherence to the gold standard through various phases of the crisis, and banking panics. He concludes, "Monetary and financial arrangements in the interwar period were badly flawed and were a major source of the fall in real output. Banking panics were one mechanism through which deflation had its effects on real output and panics in the United States may have contributed to the severity of the world deflation" (with James, p. 105).

Bernanke points out Irving Fisher's prescient understanding that debt-deflation "created pressure on nominal debtors, forcing them into distress sales of assets, which in turn led to further price declines and financial difficulties" (p. 24). Although FDR ultimately followed Fisher's reflation prescriptions, it was not until much more recently that mainstream economics fully embraced the idea that, "from an agency perspective, a debt-deflation that unexpectedly redistributes wealth away from borrowers is not a macroeconomically neutral event . . ." (p. 25). Of course continuing controversies over third-world debt, the Asian crisis of the late 1990s, the savings-and-loan meltdown of the 1980s, and the links of these issues to New Deal bank insurance and regulation schemes and new institutions such as the IMF, indicate that full consensus on optimal financial sector design is still a very long way off. Nonetheless, Bernanke's analysis of the role that bank panics played in deepening the Depression will continue to provide valuable insights as financial-sector reform efforts proceed around the world.

Bernanke and Parkinson ask whether the Depression and the New Deal hold any lessons for high European unemployment rates. They conclude, "Our own view at present is that the New Deal is better characterized as having 'cleared the way' for a natural recovery (for example, by ending deflation and rehabilitating the financial system), rather than as being the engine of recovery itself" (p. 250). It was a natural path of inquiry; while it is now clear that bankers and economists must bear much of any "blame" that is assessed, the labor market, and in particular the union movement, has been a target in the debate over "who lost the thirties" all along. This is not surprising, as it has been recognized by many that: ". . . the adjustment of nominal wages in response to declines in aggregate demand during the 1930s was surprisingly slow and incomplete. Instead of cutting wages, employers adjusted on other margins including the length of the workweek and the intensity of labor utilization" (pp. viii–ix), and "Legislatures also resisted wage (and price) cuts, for example, by measures designed to limit competition" (p. ix). Note as well that, "The tendency of real wages to rise despite high unemployment was especially striking during the major depression cycle (1929–37) . . ." (p. 207). Bernanke concludes in one article, ". . . the inertia of nominal wages must be given some role in the explanation of wage behavior" (p. 208). However, Bernanke and Parkinson also suggest, "Maybe Herbert Hoover and Henry Ford were right: higher real wages may have paid for themselves in the broader sense that their positive effect on aggregate demand compensated for their tendency to raise costs" (p. 253).

Bernanke untangles some of the puzzles of Depression labor markets with an econometric analysis of eight United States manufacturing industries. He uses a Conference Board data set of monthly observations spanning from 1923 through to the end of the 1930s. Bernanke's earnings equation includes the length of the industry workweek, industry output price, and a cost-of-living index, as well as variables aimed at measuring union power, operation of National Recovery Administration programs encouraging work-sharing and wage maintenance, and the impact of New Deal emergency work programs. The resulting estimates support the predictions of Bernanke's model that ". . . economies which rely more heavily on short workweeks (rather than employment reductions) as a way of reducing labor input are more likely to have countercyclical real wages" (p. 217). Nonetheless, in an important follow-up, Bernanke reports that simulations based on the labor-market equations reveal ". . . the assumption of perfect wage adjustment to the cost of living had virtually no effect on the ability of the model to track employment and hours . . . lagged adjustment . . . may not have had great allocative significance" (p. 236).

Economists will, of course, continue to turn to the Depression to mine its rich data on firm, worker, and consumer behavior, and their interplay with the financial sector, in the face of amplified economic forces. Macroeconomists will still find the Depression a rich testing ground for their evolving theories. Bernanke's essays suggest, however, that scholarship would now benefit from bringing other social scientists and historians more squarely into the center of the conversation about the Depression. Much of Depression scholarship outside economics has focused on the New Deal era. However, the central role played by the interwar gold standard, established by Bernanke, Eichengreen, and Kindleberger, among others, suggests that it would be valuable to bring to the forefront of inquiry the figures who handled, or mishandled the early years of the crisis. To what extent did Herbert Hoover and, for example, Eugene Meyer of the Federal Reserve, as well as central bankers and finance ministers in other countries, grasp, on any level, what was happening? Many parts of the story economists tell today were discerned, at least in hazy outline. When we move these players from backstage, and shine the spotlight on them, can we begin to understand the actions they took? To what extent did class consciousness, pure self-interest, or blind risk-averse adherence to conventional wisdom, take over in the face of uncertainty or partial ignorance? Bernanke seems on solid ground in concluding, "I believe that . . . the comparative international approach holds the most promise for improving our understanding . . . the comparative analysis will need to include political and institutional variables" (p. 34). Most importantly, are there lessons here for institutional design in an uncertain, ignorant world?

BARBARA J. ALEXANDER, *Charles River Associates*

Labour Unions, Public Policy and Economic Growth. By Tapio Palokangas. Cambridge: Cambridge University Press, 2000. Pp. xiv, 237. $64.95.

Some economists are confident that they understand labor unions. Viewing unions as monopolies, they believe unions raise their members' wages by reducing labor supply to unionized trades. In this monopoly-union view, nonmembers pay for union wage gains through higher prices for union-made goods and lower wages for nonunion workers due to crowding into nonunion industries and trades. By tampering with the free-market distribution of labor, unions distort the fair distribution of wages and income and lower output by pushing workers from more- into less-efficient occupations.

Developed by John Bates Clark in the 1880s, the monopoly-union view has survived powerful empirical challenges. The debate between institutional and theoretical labor economists has demonstrated the truism that data cannot substitute for a theory. The monopoly-union view has survived because it has compelling grounding in the theory of individual maximization. Perhaps, too, it has been natural for American economists to think of unions as isolated monopolies. Living in an economy with relatively few union members belonging to isolated and decentralized unions often little concerned with their impact on other workers or on society as a whole, it has been easy for American economists to accept the monopoly-union view. A background in Finland, a country with a strong and highly centralized labor movement, may have led Tapio Palokangas to see unions differently. But whatever the motivation, Palokangas has developed an alternative to the monopoly-union model. Instead of locating unions in a model of maximizing behavior by isolated individual workers and employers, he uses game theory to develop a model in which integrated unions bargain with employers over wages and a share of profits. By allowing unions and employers to make decisions conscious of the impact their behavior has on the others, Palokangas breaks away from methodological individualism and its orthodox implication: monopoly unionism.

Labour Unions is organized in chapters of increasing realism from simple models with a single union and employer to complex models with multiple unions and employers and dynamic elements, investment, and state policy. After an introductory chapter laying out the basic concepts of game theory, he develops a static model without investment where a single union and an employer bargain over wages and employment. In contrast with much of the previous literature focused on pure wage or profit-sharing systems, Palokangas develops a model with two-parameter bargaining where the collective agreement specifies a profit-sharing rate on top of a base wage. This two-part wage model allows Palokangas to avoid many of the negative effects found in earlier models of union bargaining because it directly links earnings to firm performance and the size of the rents earned in the employment relationship. Palokangas establishes in the simple one-union case that the union and employer will divide the rent from their relationship according to each side's bargaining power. This gives each an incentive to increase the total rents. Palokangas shows that even in the short-term, union wage demands will be constrained by the elasticity of labor demand. Both a highly centralized union conducting collective bargaining for much of the economy and an isolated union bargaining for a very small fraction of the economy will restrain their wage demands for fear of losing employment.

In later chapters, Palokangas develops his model of bargaining further to consider the role of state policy and investment. He shows that an efficient tax and subsidy policy can shift employment towards high-productivity sectors. Investment is critical to maintaining responsible collective bargaining. Unions, he argues, have a strong incentive not to renege on agreements because they have an interest in encouraging investment. For the same reason, he argues, governments have an incentive to strengthen unions because investors will avoid industries with weak unions unable to make credible contracts. Investment and growth, Palokangas argues, will be the greatest where strong unions can make credible contracts for both base wages and a share of profits. Thus, by changing the scope of the bargaining process to include profit-sharing as well as base wages and by considering the impact of contract-reneging on investment, Palokangas transforms the politics of collective bargaining. Instead of parasites feeding off of an otherwise perfectly efficient economy, Palokangas shows how unions can contribute to economic productivity by encouraging labor efficiency and facilitating long-term investment by providing credible assurances about future wages.

Labor Unions should be read by labor economists because it provides a useful corrective to the still too-standard model of monopoly unionism. Historians too will find interest in his book. Although it is a piece of abstract economic theory, Palokangas's work provides important insights into the functioning of a dynamic capitalist economy and points out important considerations such as bargaining credibility and the impact of union strategy on investment climates.

GERALD FRIEDMAN, *University of Massachusetts at Amherst*

Negotiating the World Economy. By John S. Odell. Ithaca, NY: Cornell University Press, 2000. Pp. xiii, 252. $45.00, cloth; $19.95, paper.

This is a book about bilateral bargaining between countries over economic issues such as trade and finance. As such it is of great interest to economic historians, who often need to understand why countries entered into specific treaties or established certain relations with other countries. However, in some respects it is an uncomfortable book for economists. This is so because the book is written from the point of view of the discipline of International Relations, which, unlike economics, does not have a well-developed theory. The author recognizes this and his objective in the book is precisely to take some first steps towards developing ". . . a better grounded and more useful theory" (p. 17). He is not after something similar to game theory or other formal models that economists have already developed. He reviews this literature, and although he recognizes its many strengths, he has strong reservations about the rationality assumption and the many simplifications that are necessary to make the models tractable. Under such circumstances it turns out that outcomes are determined by background conditions, that is, the parameters of the model, and therefore negotiation ends up having no role. That is, whichever economic diplomat a country sends to negotiate a treaty would reach the same end result.

Clearly a theory with these characteristics is unsatisfactory for a book whose main point is precisely to argue that the negotiation process matters. Instead the author works with the assumption of bounded rationality, which implies that decision makers have both limited knowledge and limited computational capacity. Once you have this it becomes possible for different negotiators to reach different outcomes given the same circumstances, and therefore the negotiation process ceases to be perfunctory and becomes a crucial part of the game. Thus, instead of being limited to a simple pair of strategies such as cooperate or defect, or some form of mixed strategy involving this pair, the author is looking for a theory that involves a larger range of possible strategies. Furthermore, all strategies, preferences, pay-offs, and beliefs would not be known at the outset, rather the negotiation procedures would be a process through which these elements would be uncovered (or not).

The theory developed by John Odell is based on a classification of the types of strategies that can be followed by negotiators, ranging from pure value-claiming strategies to pure value-creating strategies. The first are zero-sum in nature, while the second increase the size of the pie as well as each side's slices. In between theses extremes there is a whole continuum of strategies; mostly value-claiming but diluted with integrative moves, balanced claiming and integrative, and mostly value-creating but diluted with integrative moves. The idea is then to try to explain which strategy is chosen in a particular case by looking at different elements that may affect that choice. The elements investigated in the book are market conditions, negotiator's beliefs or biases, and each negotiator's domestic politics. Each of these is tested by comparing two case studies of economic negotiations between the United States and some other country during the last half of the twentieth century.

The idea of this "two-case method of difference" (p. 21) is to choose two cases that are similar in every respect except for a given variable, which is allowed to vary so as to test a specific hypothesis. For example, in order to test how the internal politics of each side affects the outcome when the negotiation involves a pure offensive move by one of the countries, the author compares two such attempts by the United States between 1985 and 1988. The first involved threats of economic penalties against Brazil unless it changed its market-reserve program for computers, and the second threats against the European Union if it did not compensate the United States for new barriers to American grain in Spain and Portugal. Although the relative strengths of Brazil and the European Union would lead one to expect the threats against the former to be more effective, in actual fact the European Union ended up making far greater concessions than Brazil. Both cases occurred at basically the same period in time and consequently many of the circumstances in the United States were the same. The author argues that what made the difference in the outcomes was the internal support for each threat within the United States. Whereas there was significant domestic opposition against implementing the threats against Brazil, the threat against the EU had a strong and united constituency. Realizing this, the negotiators in Brussels and Brasilia reacted accordingly, which helps explain the difference in outcomes.

Four pairs of case studies are presented to illustrate the author's approach to understanding international economic negotiations. These cases are entertaining and very rich in detail, however the reader is always left with the sense that perhaps the difference in the outcome in each pair of cases may have been due to some omitted factors rather than the variable highlighted by the author.

BERNARDO MUELLER, *Universidade de Brasilia*

Small Town and Rural Economic Development: A Case Studies Approach. Edited by Peter V. Schaeffer and Scott Loveridge. Westport, CT: Praeger Publishers, 2000. Pp. xvi, 293. $62.50.

Economic development is for many communities and policy makers a familiar and worthy goal, but sadly, it can also be an elusive goal. Familiar, because the standard by which many policy makers and their advisors are evaluated is their ability to attract industries and firms, to create jobs, and otherwise to expand the capacity and economic base of a community. Elusive, because not only are no two communities identical, but also because the outcomes of specific policies and actions can be difficult to predict with certainty. The collected case studies in this book offer more than 30 examples of programs and policies enacted in the pursuit of economic progress across various small towns and rural communities.

These case studies are presented in a succinct, matter-of-fact style, and reveal that despite the best intentions of both public and private initiatives, economic-development strategies predictably have mixed results. Rather, economic development is almost never an insignificant achievement and it is unwise to imagine that meaningful economic progress can be achieved by a single act or an occasional policy initiative. Take, for example, a community that is dependent upon a single manufacturer or industry and assume that non-local factors are shaping and restructuring the market for the firm's output. Serious questions probably remain as to whether or not the local firm has the necessary resources and the time to recast itself in order to remain competitive in a changing global economy. And these questions all have to be answered with respect to labor markets, financial conditions, infrastructure, education and other public services, as well as community leadership. However, as this book makes clear, industrial attraction and rural development are not necessarily synonymous. Economic development is, rather, a multifaceted process, and there are almost always viable alternatives, whether progress takes place in a small town, rural area, or metropolis.

When economic development strategies are popularly discussed, the examples that are often used are those that relate to larger and more complex communities or to comprehensive development projects. Because the case studies in this book are concerned with smaller towns and rural communities, it provides a more controlled comparative analysis of contemporary policies. Given that economies can be a jumble of institutional and cultural characteristics, comparative analysis is more straightforward in communities that have uniform traits. Smaller towns appear, by definition, to be more economically and culturally homogeneous than larger metropolitan areas would ever be expected to be. Although this book demonstrates that homogeneity, there are sufficient differences between and even within smaller communities that these case studies have application, and thus relevance, to other communities and to other situations. Even though the initial conditions and political, cultural, and economic context of any particular community make every case study distinctive, it is possible to draw practical inferences from various policies that ultimately achieved various degrees of success. Moreover, this book may be appealing to economic historians because, by examining the development strategies in small towns and rural communities, it is possible to gain some insight and historic context into the general process of economic development.

Inferences about the general process of economic development are enhanced not only by the range of topics covered in these diverse case studies but also by the breadth of background among the contributors. The more than 40 contributors to this volume include conventional and agricultural economists, sociologists, geographers, and public-policy analysts and practitioners. Collectively these concise case studies deal with issues such as community capacity, expanding rural and small businesses, attracting large-scale industry, maintaining and enhancing a community's economic base, as well as the relative relationship between the public and private sector. Despite being of uneven quality, the case studies are largely written in a candid and summary style that typically introduces the project and then provides objectives and activities, summaries of the findings and outcomes, and even a few discussion questions from the editors. And because the cases presented range from such locations as Alabama to Hawaii, from southern Utah to eastern Connecticut, from North America to Ireland, Denmark, and Belgium, there should be enough to appeal to a variety of readers. As might be expected, not every reader will find relevance in each of the 33 case studies, but there should be enough practical material to benefit and educate users about the processes and strategies of local economic development.

TIMOTHY E. SULLIVAN, *Towson University*

Institutions and the Evolution of Modern Business. Edited by Mark Casson and Mary B. Rose. London: Frank Cass, 1998. Pp. 184. $42.50, cloth; $19.50, paper.

This collection of eight-and-a-half essays makes an important contribution to both the theory and practice of business history. Six essays employ the new institutional economics and evolutionary theory to examine central issues in economic history and economic development as they arose in specific historical contexts in Western European countries. The other two-and-a-half, by S. R. H. Jones and the editors (the "half" is a brief but wonderful introduction), examine and critique the theoretical literature that provides the basis of the new institutional economics, laying out clearly its strengths and its limitations for contributing to our understanding of business and economic history. The volume thus provides a model of how business history should be done.

The collection puts particular emphasis on cultural differences at the local and national levels. Its point is not simply to note, as is too often the case, that culture influences business. Rather, each essay addresses, either directly or indirectly, the central question of economic history: What has determined the pace and path of economic development? The comparative perspective taken by these essays allows the authors to discuss very concretely the ways that culture has made a difference.

S. A. Caunce's essay on the development of the Yorkshire woolen industry is a lovely example of this approach. Caunce analyzes the social geography of the region, as well as the history of individual families, to explain the emergence of an internationally competitive industrial district in the late eighteenth century. This emergence cannot be explained by a local technological shock, local factors of productions, or the introduction of the factory. Access to and participation in markets was critical, but these markets were not disembodied institutions. To the contrary, access to and participation in markets arose from the embeddedness of those markets in local institutions. Households engaged in home production for the market. Their willingness to do so reflected the particular history and geography of the region: a substantial nonconformist population, a large number of relatively well-off freeholders, and a lack of resident gentry. The market in which they participated was a very particular institution—the local cloth hall— through which merchants provided information to producers. The existence of community, family, and religious ties did not impede market activity but rather supported it, by providing a strong reputational incentive to honor one's economic obligations, both legal and informal. Local diversity also facilitated development: if one strategy, organization, or method of production or distribution did not work, there were others to be tried.

Oliver Westall's essay on diversity in the organization of the British insurance industry argues that while transactions-cost economics can successfully explain the organization of firms at any point in time, an evolutionary approach is necessary to explain why it was that the "optimal" organizational form changed over time and was different for different branches of the industry. He also makes a point with which I am particularly sympathetic, namely that the growth and success of large bureaucratic organizations over the twentieth century was predicated on significant market power and the ability to limit price competition.

Hans Sjogren's essay uses a comparative analysis of a sample of financially distressed British and Swedish firms to make a similar point about the importance of contextualizing contractual relationships. The contractual relationships between, on the one hand, British lenders and the firms to which they lent, and on the other hand between Swedish lenders and their own borrowers, were quite different. Despite this, in both cases banks were willing to force distressed firms into bankruptcy. This was the case even for Swedish banks, which were themselves shareholders as well as creditors of the distressed firm. Similarly, British banks did not constrain themselves to the arms-length relationship that we might presume. In many cases, British banks stepped in to take control and replace the management of distressed firms.

Matthias Kipping's comparison of the development of business consulting in Britain, France, and Germany demonstrates the importance of national political and cultural institutions in the determination of industrial structure and growth. It also demonstrates the importance of national differences in the diffusion of ideas, such as Taylorism, about how firms should be organized. This argument complements recent work of Christopher McKenna showing that consultants themselves were extremely important agents in the diffusion of managerial ideas ("The Origins of Modern Management Consulting." *Business and Economic History* 24, no. 1 [1995]: 51–58). Kipping's essay places those consultants into a broader political and cultural context.

Sverre Knutsen shows how Norway's distinctive culture and political climate influenced its industrial policy. Norway placed greater emphasis on small banking institutions participating in government networks and providing finance to small, regional firms, rather than the creation of large banks financing a few privileged firms. Thus Knutsen shows that the "optimal" size of the firm depends in part on state policy. At a time when mergers and planet-sized organizations are treated as the only viable enterprises, it is useful to remember that it is often state policies, not economies of scale, that determine whether small, community-based firms can compete.

Mary Rose's essay also focuses on state policy, providing a clear and convincing explanation of why the United States maintained higher tariffs on textile imports than did Britain, despite their mutual commitment to free trade. Neither differences in cost structure nor differences in culture or ideology can explain the tariff differences. Instead she turns to something often ignored by business historians—political history. She argues that increasing concentration and growth in firm size, combined with a tactical alliance with labor on this issue, allowed the industry to pressure key members of Congress who had disproportionate power over the setting of tariffs in America's decentralized political system. In contrast, the British textile industry was internally divided, as small and large producers pursued distinct strategies; a much more centralized British state was able to ignore its interests whenever these conflicted with the state's overall policy goals.

In the volume's final essay Casson proposes a "cybernetic" model of the economy, in which firms are viewed as specialized intermediaries rather than as production functions. While most economic models explicitly or implicitly assume that intermediation is costless and the ability to intermediate abundant, Casson argues convincingly that intermediation is costly and that the ability to do so requires both highly specific information and a well-developed reputation for trustworthiness. Thus Casson provides us with the answer to the time-honored question: If you're so smart, why aren't you rich? Economists don't pick up that dollar bill because they don't realize that knowing that it's there is valuable information. Virtually all economic models assume that there is no money to be made from intermediation because such opportunities will quickly be arbitraged away. Intermediation is trivial when you view information as free, easy to get, and easy to transfer. But information is none of those things, and without that information, trade doesn't take place. People who have information that makes trade possible are richly rewarded.

MARGARET LEVENSTEIN, *University of Massachusetts–Amherst*

The Idea of Capitalism before the Industrial Revolution. By Richard Grassby. Lanham, MD: Rowman & Littlefield, 1999. Pp. ix, 87. $50.00, cloth; $14.95, paper.

Richard Grassby, an economic historian of Early Modern England now at the Institute for Advanced Studies, has written a sly, slender contribution to the Rowman & Littlefield "Critical Issues in History" series. An extended essay on the question of what capitalism is, it begins by examining the attempts of Marx, Weber, Sombart, and Schumpeter, among others, to locate the essence of capitalism in this or that particular feature of economic, social, or political life. Grassby's method is to select a theory and then debunk it by producing a piece of evidence for which it cannot account. Hence, for example, comparison of "traditional marxist teaching" to the basic facts, as he sees them, of agrarian capitalism in Early Modern England (p. 26). Where a Marxist theorist would assert that a change in the mode of production transformed the rural economy, a dogged empiricist like Grassby finds that the motors of change were "in fact" demographic pressure and "the market with some government support." To the extent that he allows himself a general theory, it is a non-linear, cyclical one reminiscent of the Enlightenment thinkers: equilibrium and disequilibrium, freedom and regulation, activity and decay all wrestle one another in eternal combat, and there is no final victory to be had (pp. 30, 45).

Wearing an immense amount of learning lightly, Grassby skims over the history and historiography of economic activity since ancient times in well under 100 pages. Travel at this speed and scope is only possible at a rather giddy height, but the author makes frequent downward swoops to illuminate certain events and their connections. Some of these aperçus are quite striking, as when he links Dutch money, Dutch painting, "realism and the idea of self" to the rise of Western consumer culture (p. 57); or when he points out the resort to anti-Semitic stereotyping in Sombart's account of the emergence of economic individualism (p. 53).

Not surprisingly, the book's central thesis turns out to be a negative one. In an ironic inversion of his title, Grassby concludes by announcing that there was no idea of capitalism before the Industrial Revolution. Even more audaciously he declares that no such thing as capitalism has ever existed. There is only industrialization, an economic and social phenomenon to which the term "capitalism" has been infelicitously applied. In a rather strident peroration (pp. 62–73) he goes on to offer a psychological explanation for the enduring power (at least until now) of this essentially mythic construct. On the one hand the notion of something called capitalism is "a convenient shorthand expression" for poseurs too lazy to do anything so taxing as actual historical research, those "theorists, both marxist and nonmarxist, [who] reject empiricism completely and flaunt their contempt for facts" (pp. 67, 63). On the other hand he says "it is important to recognize . . . that what purports to be an explanatory structure is in fact a belief system in which individuals can indulge the fantasy of final causes" (p. 67). According to Grassby, believers in the idea of capitalism—its critics and partisans alike—suffer from an illusion rooted in their own wish to eradicate injustice and free the oppressed. Thus deluded, they eschew disinterested inquiry into the past because they already know what they are looking for. Their sterile, conformist pseudo-scholarship amounts to the accumulation of useful examples in support of preconceived notions about what must have happened.

Little in this diatribe is new, much less daring at the present world-historical juncture, though Grassby's pungent style will inspire likeminded readers and infuriate others. It is nevertheless a book to buy and keep as it is bound to improve with age. Fifty years from now it will be an intriguing document of *fin-de-siècle* intellectual history. Graduate students, while deploring the lack of footnotes, will mine it for its bibliographical essay (pp. 75–80) and its pithy encapsulations of whole schools of thought. They and others will find in it an eloquent, if partial, record of the way we struggled to understand capitalism once communism was gone.

CHARLES P. HANSON, *Menlo School*

Globalization and History: The Evolution of a Nineteenth-Century Atlantic Economy. By Kevin H. O'Rourke and Jeffrey G. Williamson. Cambridge, MA: MIT Press, 1999. Pp. xii, 343. $45.00.

Globalization and History is an impressive book. It asks a big question: What was the economic impact of globalization in the late nineteenth century? To answer it, Kevin O'Rourke and Jeff Williamson deploy new data—principally purchasing-power-parity-adjusted real wages and land values for major economies in Europe and the Americas—and analyze them with regressions and computable general-equilibrium (CGE) models. The analysis is always incisive and frequently elegant; the writing is accessible to the general reader as well as the professional. The message is upbeat: nineteenth-century globalization was a "good thing" because it allowed poor countries to catch up to rich ones. But unskilled workers in the leading countries suffered, leading to a backlash against globalization. Today's leaders should take heed, lest history repeat itself.

While *Globalization and History* deserves all the attention it will get, its conclusions are often contestable. Some issues relate to measurement, some to interpretation. The two are related.

O'Rourke and Williamson propose a paradigm shift in the way we think about the late nineteenth century. In most histories of this period, the industrial revolution has been the protagonist, and its spread the plot. Britain had the early lead, Germany and America caught up or overtook her by the First World War, as industrial development was beginning in southern and eastern Europe. O'Rourke and Williamson tell us instead to concentrate on globalization—commodity-price equalization, international migration, and capital flows. The implicit claim is that these "external" developments are more important in explaining income changes than are "internal" developments like capital accumulation (physical and human) and technological change.

The redefinition of the question is matched by a shift of evidence from GDP to factor prices. Using GDP per head as the metric for growth supports the standard story of the nineteenth century: Between 1870 and 1913, Britain grew at only 1.01 percent per year and was overtaken by the United States, which grew at 1.81 percent, and by Germany at 1.63 percent. Scandinavia made good but unexceptional progress (1.44 percent), while the Mediterranean lagged (1.03 percent).

In contrast, O'Rourke and Williamson urge us to look at real wages. With that metric, the standard story dissolves: The real wage of unskilled worker grew at the same rate in Britain, Germany, and America (between 1.02 percent and 1.04 percent per year), the Mediterranean did even worse (0.85 percent), while Scandinavia (2.60 percent) and Ireland (1.79 percent) were spectacular successes. That is the O'Rourke–Williamson view of the world: a lethargically growing core—and no more Anglo-German rivalry!—a Northern periphery that converged on the leaders, and a Mediterranean periphery that failed to advance.

Globalization and History devotes little attention to skilled wages, and this is a major omission. For instance, the wages of skilled building workers were much higher in the United States than in Australia, Canada, or Great Britain from the late 1880s to 1913 (See, for example, my "Real Wages in the English-Speaking World, 1879–1913," in *Labour Market Evolution*, edited by George Grantham and Mary MacKinnon. London, Routledge, 1994: 119). O'Rourke and Williamson defend their concentration on unskilled wages with the claim that they measure the standard of living of workers in general. That is not true in view of the variation of pay ratios.

Moreover, the skill premium was rising in the United States for most of the period, and declining in Britain. These divergent trends are consistent with Williamson's earlier work, in which he argued that both countries traced out a Kuznets curve where the ratio of skilled wages to unskilled first rose and then fell. Great Britain industrialized first and was in the falling phase by the late nineteenth century, while the United States was near the inequality peak. If skilled wages are the indicator of convergence, then the late nineteenth century witnessed increasing divergence between the United States and Great Britain—a pattern consistent with the GDP evidence and inconsistent with O'Rourke and Williamson's interpretation.

Ignoring the history of the whole pay structure makes it much harder to draw useful lessons for the present. A common view today is that competition from poor countries with relatively unskilled labor is driving down unskilled wages in the United States and leading to unemployment among the unskilled in Europe. The standard prescription is more education to raise the skill level in the industrialized countries. More education would not only increase growth, but would also reduce inequality by cutting the ratio of skilled to unskilled wages. Was the nineteenth century an example of this process? Did educational expansion (for example, the high school movement in the United States) increase growth and reduce inequality? These questions do not arise in *Globalization and History*, because the wage data are limited to the wages of unskilled workers in the building trades.

Movements in the price of land were even more disparate than trends in skilled wages. Land increased rapidly in value in the Americas, declined sharply in Britain and Ireland, and showed little change on the Continent. But national averages obscure important differences among regions in each country. *Globalization and History* implicitly assumes that these trends represent convergence, but the matter is not pinned down. Was the rent of good-quality pasture in upstate New York approaching that of southwestern Cambridgeshire, or moving away from it? We do not know.

O'Rourke and Williamson have a classic view of the nineteenth century. Trade liberalization (for example, repeal of the Corn Laws) and transportation improvements equalized the prices of tradable goods around the world. What impact did price convergence have on the wages of the unskilled and the value of land? In an elegant analysis based on the Heckscher–Ohlin model, O'Rourke and Williamson calculate that commodity-price equalization raised the British unskilled wage by 20 percent, cut the rent of land by 50 percent, and explains half of the growth in the British wage–rental ratio. In the United States, price movements had no effect on wages, but they raised rents by 12 percent. In Sweden, price convergence also had little impact on the wage but cut the rent of land by 40 percent. Elsewhere, the effect of price equalization was minimal. Detailed analyses of the impact of the Corn Laws on British incomes, and of the differential response of European countries to the American grain invasion after 1870—these chapters are among the most impressive of the book—support similar assessments of commodity-price convergence. Globalization is the hero of this book, but outside of the United Kingdom, did trade flows matter at all?

Much of the book is devoted to analyzing international migration and its impact on wages and incomes in sending and receiving countries. Emigration explains all of the wage convergence between Ireland and the leading economies, most (if not all) of the Italian convergence, and a third to a half of the Scandinavian wage catch-up. They estimate that immigration lowered American unskilled wages by about 10 percent (pp. 152, 159), which was part of the convergence process. Most of the convergence, however, was driven by real wage growth in the sending countries. When O'Rourke and Williamson tell us that globalization led to convergence, they are talking about migration and not trade.

O'Rourke and Williamson resolutely avoid considering the effect of globalization on economic growth. Migration is the one exception, but its effect may be decisive in view of its importance in explaining wage trends. Had international migration ceased in 1870, the population of the New World would have been 29 percent smaller in 1910 than it actually was, and the population of the Old World would have been 11 percent larger. Argentina, Australia, and Canada would have had smaller populations, while there would have been more people in Ireland, Italy, and Scandinavia. The predicted course of per-capita income would not have differed much from its historical trajectory, however (p.155). O'Rourke and Williamson conclude that these results "certainly" lend "strong support to the hypothesis that mass migration made an important contribution to the late-nineteenth-century convergence" (p.162), but one could equally conclude from their calculations that globalization shifted millions of people across the oceans but did not otherwise make much difference to economic growth in the late nineteenth century. The usual argument in favor of globalization is that it will cause a large increase in output, but the lesson of the nineteenth century seems to be otherwise.

O'Rourke and Williamson devote two chapters to capital mobility. They explore the question of whether trade was a substitute or complement for factor mobility, with inconclusive results. Of greater interest is their integration of capital flows into the explanation of wage trends. They conclude that capital inflows accounted for one-third—in the case of Sweden perhaps as much as 43 percent—of the wage catch-up with the industrial core. This is quite extraordinary, but is not the end of the story. Capital flows were endogenous. While weak domestic saving could have contributed to growing foreign investment in Scandinavia, the decisive factor was surely a rapidly growing demand for capital. The wage convergence that O'Rourke and Williamson attribute to foreign capital inflows (and thus to "globalization") should actually be attributed to industrialization. If this conclusion is accepted, then industrialization made a major contribution to real wage growth in Scandinavia, especially in Sweden.

This reading of the evidence highlights one of the most remarkable features of *Globalization and History*: it contains no serious analysis of capital accumulation or technical change as determinants of wages. Instead, the focus is squarely on trade and migration. While these are important subjects that need to be integrated into the analysis of income changes, industrialization should not be ignored either.

This orientation is all the more remarkable given Williamson's earlier work on the Kuznets curve in the United States and Great Britain. There he used CGE models to explain changes in the ratio of skilled to unskilled wages in terms of capital accumulation and skill-biased technical change. These insights remain pertinent. As noted above, trends in skilled wages indicate a significant divergence in incomes between Great Britain and the United States. Presumably an American accumulation process biased in favor of skilled labor explains the growth in the US skilled wage. It is churlish to complain that anyone's CGE model is not complicated enough, but *Globalization and History* would have more useful lessons for the present if skilled labor appeared in the models along with unskilled. Then we could explore whether the expansion of education was a successful solution to rising wage inequality early in the twentieth century, or whether high tariffs and immigration restrictions were the only way to protect the standard of living of most Americans. That would be a useful historical lesson, indeed.

The pioneering work of O'Rourke and Williamson allows that question to be posed and suggests how to answer it. *Globalization and History* may not be the last word on the subject, but it is a pivotal book, for it defines the agenda for future research.

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Uncommon Grounds: The History of Coffee and How It Transformed Our World. By Mark Pendergrast. New York: Basic Books, 1999. Pp. xxi, 520. $27.50, cloth; $18.00, paper.

The history of coffee is a tale of romance and complexity, sometimes bitter, sometimes enlightened. Mark Pendergrast weaves this tale with the art of a master storyteller. From its legendary discovery in the ancient land of Abyssinia (now Ethiopia), coffee became a source of stimulus for new ideas, work, and social controversy. Traded throughout the Ottoman Empire in the sixteenth century, the curious beverage was brought to the hands of Pope Clement VIII by priests who wanted to ban it. He tasted and replied: "Why, this Satan's drink is so delicious, . . . it would be a pity to let the infidels have exclusive use of it" (p. 8). So the Western European romance with it began. By the eighteenth century, coffeehouses provided an egalitarian meeting place and a social context for emerging liberal institutions and the American and French Revolutions.

Despite its ambitious subtitle, the book is primarily a history of coffee consumption and commercialization in the United States. The Arbuckle Brothers produced the first marketing strategy in 1860, claiming (falsely) to have sealed in freshness by coating roasted beans with egg whites. Since then, there have been two major innovations in preservation: the vacuum can (1899, by Hills Brothers) and the one-way valve (1970). Claims made on behalf of vacuum-packed coffee were also overblown. Freshly roasted coffee had to be "staled" before packing, to prevent the can from swelling. But the claim mattered more than the freshness. Thousands of local and regional roasters gradually gave way nationwide distribution of vacuum-packed beans. The habit of consuming bad coffee worsened with nationwide consolidation, as large roasters substituted a mix of low-grade blends and heavy advertising for good product. As the quality of the "cuppa Joe" steadily declined, so too did consumption per capita, as young "boomers" increasingly opted for soft drinks. Salvation for coffee-lovers came in the 1960s with a new generation of fanatics and missionaries, such as Alfred Peet and Saul Zabar, who introduced high-grade specialty coffees to the American consumer. We learn of the founding Starbucks of Seattle by three of Peet's disciples, and of its eventual takeover by Howard Schultz, mastermind of the Starbucks Experience. When Philip Morris took over General Foods in 1985, CEO Hamish Maxwell visited the Maxwell House division and asked for a cup. None was at hand because no one in the office drank the stuff.

Although most comprehensive on the American coffee market, Pendergrast writes with an eye to the global social context that supplies American consumer habits. He compares coffee fashions in America and Europe. He wanders also into Latin America to observe the social conditions where coffee is grown. Although coffee harvesters are paid only a few dollars a day, he concludes that one cannot easily find victims and villains in the coffee business. He makes a point of showing that the American coffee market was not insulated from foreign social conditions and marketing schemes: the American coffee habit had to adapt to combinations of foreign buyers who tried to corner the market, and to a Brazilian-led international coffee cartel that engineered an artificial scarcity of high-grade beans.

What is wonderful about the narrative is the hundreds of anecdotes woven together to illustrate how "coffee provides one fascinating thread, stitching together the disciplines of history, anthropology, sociology, psychology, medicine, and business, and offering a way to follow the interactions that have formed a global economy" (p. 410). Pendergrast has read extensively in the literature on coffee in all of these disciplines, but he is not directly engaged in any of them. Author also of *For God, Country, and Coca-Cola* (New York: Basic Books, 1993), he is a true authority on the history of beverage marketing in the United States, who aims at the sophisticated nonspecialist reader. Academics will find it a thoroughly documented and useful source of information. For the economic historian, he stirs the waters of fascination, bringing to the surface numerous questions worthy of more rigorous analytical treatment. On these, Pendergrast is silent. He offers reliable narrative, not analysis. As an analytical companion, I recommend Robert Bates, *Open-Economy Politics: The Political Economy of the World Coffee Trade* (Princeton: Princeton University Press, 1997). But by all means, if you "romance the bean" and are curious about how we got from Folgers to Peet's to Starbucks, then brew yourself a good cup, sit down, and savor *Uncommon Grounds*.

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Grain Markets in Europe, 1500–1900: Integration and Deregulation. By Karl Gunnar Persson. Cambridge: Cambridge University Press, 1999. Pp. xx, 173. $59.95.

Karl Gunnar Persson engages a daunting scope of history in this book: four centuries of grain-market evolution covering all of Europe is a broad sweep indeed for some 154 pages of text. The task is facilitated by his view that the particular countries' experiences were sufficiently similar as to be seen as parallel examples of one more-or-less uniform story. Thus the book does not attempt to analyze and contrast distinct political or regional developments; the focus is on what Persson sees as the "uniformity of the European experience" (p. xv).

Persson's stylized story of that experience will be recognized by readers familiar with the literature on early modern market regulation. In agreement with the convention in that literature, Persson argues that the regulatory institutions of that era were designed to protect consumers from "market failures"—excessive price volatility and the possibility of "cornering the market." Prior to well-integrated trade, market incentives were not sufficient to produce price stability and socially optimal storage. Recalling E. P. Thompson's "moral economy" thesis, Persson argues that the quest to ensure subsistence resulted in popular demands on rulers to discipline the price volatility resulting from market failures. State institutions could and did serve a welfare-enhancing role. Only later, as information and transport costs fell and markets became effectively integrated, would state intervention become obsolete.

Did markets fail in ways that the state could remedy? Persson uses a variety of imaginative statistical tests to evaluate the performance of markets in stabilizing the price responses to supply shocks. He judges their performance to have been poor. He finds that storage was minimal, due to low expected earnings and high levels of uncertainty, and argues that such stocks as were held were not based on pecuniary interests. Regional trade was also ineffectual in reducing price variance, due to the high cost of transport and to monopoly power. To those arguments Persson adds what he describes as a new interpretation of market failures based on the economics of externality. Price stability carried with it a variety of social benefits, by reducing vagrancy (and the health costs associated with it), crime, and other threats to the social order. If left to markets, the non-exclusive nature of those benefits would have resulted in a suboptimal volume of trade and stockholding.

Having determined that state intervention was needed, Persson proceeds to examine the effectiveness of the policies applied. He presents a typology of institutions that are each linked to distinct "market failures," and finds them to have been effective when compared to the counterfactual of unfettered markets. Such institutions included rules limiting trade to open (transparent) local markets, restrictions on private stockholding, price controls, public granaries, and a variety of other such measures.

Persson argues that those institutions, effective though they were when enacted, eventually became unnecessary. As transport and information costs fell, markets became increasingly integrated and thus more effective in the stabilization of price shocks. Persson devotes a full chapter—and a great deal of statistical thrashing of price data—to showing that regional markets across Europe indeed became more closely integrated as the centuries passed, and that one result was reduced price variance. The resulting moderation in popular demand for state intervention, combined with growing intellectual support for liberal policies, resulted in the decline of grain-market regulations beginning in the late eighteenth century.

In the book's preface Persson notes that he sees the study as an opportunity to "confront" his hypothesis about institutional change—that "institutions thrive to the extent that they are useful and decay when a viable and better alternative emerges" (p. xv). Unfortunately he does not bother to confront that view with alternatives, or to support it with a contestable theory of institutional change. To the contrary, Persson writes that "I am not primarily concerned with the political game that generated the observed policies, but if the nature of public intervention conformed to the expectations provided by the economic theory of market failures" (p. 73).

That is a curious position to take in this book. The entire thesis rests on the view that grain market institutions were the results of ongoing official efforts to serve the welfare of the populace. This view of government motivation is not compared to alternatives, or critically examined in any way; it is simply accepted as fact. Yet the text itself contains many observations that suggest obvious alternatives. For example, Persson notes that "it was often the case that local governments, as well as representatives of the Crown, were actively involved in the grain trade" (pp. 77–78). He notes the "administrative tradition of giving exclusive rights to some merchants" (p. 4), and recognizes the potential tension between "local market networks" (based on royal grants of exclusive trading rights) and market integration. All point to interpretations that beg to be contrasted with the view of royal paternalism—the most obvious alternative being the Crown as rent-seeker. To engage such a contrast would require taking politics, and the work of institutional theorists such as Douglass North, seriously.

There are other points where the author appears to ignore relevant work that could make the research stronger. An example is the reference to "the economic theory of externalities" without recognition of Ronald Coase and his identification of the role of transaction costs. Another example is the overconfident use of statistical significance tests of market integration, for example his assertion that "'integration' here means that pairs of markets pass cointegration tests" (p. 152). D. McCloskey's warnings of this error should be well-known to economic historians.

The book provides a welcome reminder of the importance of grain markets to the development of preindustrial Europe. It is also a useful demonstration of how much is yet to be known about the institutional evolution of the grain trade. The book is packed with statistical tests of price data, and a good accessible list of data sources is provided, making follow-up research possible. The book will hopefully lead to such work; the author has done his part in revealing a number of questions that need and deserve more attention.

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