

Book Reviews

MODERN EUROPE

Lokales Leben, atlantische Welt: Die Entscheidung zur Auswanderung vom Rhein nach Nordamerika im 18. Jahrhundert. By Georg Fertig. Osnabrück: Rasch, 2000. Pp. 466.

Georg Fertig's *Lokales Leben, atlantische Welt* is an in-depth account of eighteenth-century migration from Southwest Germany to North America. Above all, the author seeks to explain the nature of migration decisions. Section 1 discusses theories of migration that have been entertained by historians and economists. Section 2 offers a comprehensive characterization of eighteenth-century migration from Germany to North America. Section 3, the monograph's centerpiece, concerns local migration behavior in the village of Göbrichen, located in Baden. Migration decisions in the eighteenth century were made for the most part at the individual and family level; it is at this "micro" level that Fertig explores the many aspects of migration behavior. Much of this section is devoted to explaining, in the manner of David Sabean, the social and economic relationships in this village and how they changed over time. With a motto that the best stuff lies in the details, Fertig provides a plethora of possible demographic, economic, and social reasons why Göbrichen residents might want to leave town.

Fertig offers a critical framework for the scholarly discussion of migration behavior. A key purpose of this work is to pinpoint factors that made a difference in encouraging individuals and families to emigrate, and to identify those factors that did not. He argues, for instance, that overpopulation is a vague concept and suggests a more exact description of what overpopulation entails and how it might affect the incentive to emigrate. He suggests such indicators as food shortages, on the basis of which he rejects the hypothesis that Göbrichen experienced overpopulation as such, or reached some kind of maximum carrying-capacity. He also assesses the impact of laws restricting land ownership, physical movement beyond the village, or entrance into a marriage contract, and finds that they were for the most part unimportant. In these respects and others, this book increases our knowledge of what mattered to Southwest Germans contemplating emigration in the eighteenth century.

According to Fertig, the clearest predictor of emigration from Göbrichen was that an individual's father had been born outside the village. Previous family migration experience, he argues, may have provided children with "migration know-how." Those who had experienced downward mobility were also more likely to participate in migration, albeit mostly within Germany. Poverty was not correlated with transatlantic migration from Göbrichen; individual wealth was, however, inversely related to internal migration from the nearby village of Bauschlott.

In his discussion of land and its possible effects on various demographic and family-structure outcomes in Göbrichen, as well as in his analysis of emigration determinants, Fertig computes correlations between various socioeconomic variables, land ownership, and emigration. In future work the author might consider enhancing this part of the study with panel regressions, much as he uses regression analysis in other parts of the book. Such a model would take advantage of the interrelationships between variables. These techniques would also specify better the exact nature of the relationship between emigration and the various family determinants he is proposing as possible reasons why people left Göbrichen.

Ultimately, the importance of Fertig's findings depend on how typical we think Göbrichen's migrants were. Fertig chose Göbrichen for its archival resources and for its significant, but not outrageous, number of emigrants. Duplication of such a local study for other communities would obviously be desirable, but necessarily time-consuming in terms

of the analysis of family reconstitution data; it may not even be feasible. Still, only a modest number of emigrants, 36 in total from Göbrichen, are involved in his analysis. Fertig seems frustrated that he cannot identify more patterns. In this case, surely a larger sample size (more villages) would improve the data analysis, but the tradeoff would inevitably be a less careful analysis of qualitative evidence at the local level. In addition, Göbrichen was peculiar in that its economy was based primarily on agriculture. It would be interesting to explore how migration strategies differed in places with a wider variety of economic activities.

One must emphasize that the Göbrichen emigrants are what Fertig terms “pioneer” emigrants, among the first from their village to emigrate to North America. He finds little evidence that they resettled in close proximity, very much to the contrary of the “chain migration” pattern associated with later waves. This characterization provides a particular bias in terms of their own migration behavior, and must be considered in any comparison to other migrant groups.

Fertig is careful in his reasoning and—needless to say—detail-oriented. One quickly senses that he has thought carefully about migration, and considered many different literatures in forming his own ideas about what really mattered to those who contemplated long-distance migration. With the use of extensive sources from both sides of the Atlantic, he argues his side of the story with numerous types of evidence, both qualitative and quantitative, including emigrant letters, archival records, newspapers, family reconstitution records, and other data series. His probing style of questioning, and his knack at interweaving vastly different types of sources to form his overall argument—he regularly combines stories about actual Göbrichen emigrants with regression analysis—provide for an interesting and valuable work. Although the author was surely constrained by editorial guidelines, I must add that a subject index as well as an author index would have greatly enhanced the usefulness of his work for future research on migration behavior.

Few historical studies take migration behavior as seriously as does Fertig’s, particularly in its analysis of migration behavior at the micro level. We need more such studies to generate new data, test existing theories about migration processes, suggest determinants that made the difference to emigrants and nonemigrants, and propose new hypotheses for further research. Fertig’s addition to the literature on migration decisions is certainly valuable progress.

SIMONE A. WEGGE, *College of Staten Island—CUNY*

Industrializing English Law: Entrepreneurship and Business Organization, 1720–1844.

By Ron Harris. Cambridge: Cambridge University Press, 2000. Pp. xvi, 331. \$59.95.

This important book considers changes in the legal framework of business organization in England in the era of the industrial revolution. The significance of that theme has long been recognized, but it has grown over the past two decades because of increasing emphasis, within both economics and economic history, on the need to understand institutional contexts. That said, Harris is returning to often complex sources that have not been considered closely since W. R. Scott, A. B. DuBois, and N. C. Hunt in the first half of the last century. Moreover, he brings to the task a very wide range of skills. The result is a book for the times, but one that deserves to have a very long shelf life.

The bare subject matter of this book, a consideration of changes in the law surrounding partnerships, corporations, and limited liability in the era between the passage of the so-called “Bubble Act” of 1720 and the Companies Act of 1844, initially appears somewhat

traditional. But at the very outset the subject matter is contextualized in two important ways: with an exploration of the relevant legal concepts, and with a crisp and effective discussion of the evolution of companies in the two centuries prior to the Bubble Act. This combination of careful detailing characterizes the book as a whole, which weaves together legal, political, and economic history with care and imagination. Drawing on an article published in this JOURNAL in 1994, Harris moves on to explain the passage and significance of the Bubble Act. His account here is definitive, a model of how careful research can overturn a host of misconceptions. The second part of the book looks at the period between 1721 and 1810, a period often glossed over as relatively insignificant for an understanding of the evolution of business forms. By looking at developments in businesses dealing in transport and insurance, Harris gently corrects this misperception, just as he does when discussing the legal advantages corporations retained in relation to partnerships, trusts, and unincorporated companies through the eighteenth century. A final chapter in this part provides some important speculations on the scale of capitalization of joint-stock companies in England in the early stages of the industrial revolution. Harris concludes that joint-stock companies grew rapidly in importance, accounting for nearly 14 percent of Britain's total net reproducible fixed capital stock by 1810, and for nearly 25 percent by 1840. In the last part of the book attention is directed mainly at the reasons behind the repeal of the Bubble Act in 1825 and the passage of the Companies Act in 1844. These decisions are related to changing attitudes towards monopolies, speculation, and government intervention. Considerable weight is attached to the legal problems of enforcing the Bubble Act and the difficulties its existence posed for Parliament.

Clearly, to write a book such as this requires mastery of legal, political, and economic history. Such mastery, however, is not enough, because of the demands it makes upon the reader (not least because it renders a unified narrative infeasible). Harris is indeed ever-careful to link together the fractured parts of his story so that the reader does not get lost (though the prose is clear rather than compelling). If this is a book decidedly rooted in empiricism and fine detail, wider theories and models are nevertheless always kept in view. At the very end, for example, Harris considers how his findings challenge interpretations—such as those set forth by Douglass North—with regard to property rights, transaction costs, and market efficiency. Here his avoidance of the neo-Whiggism of many modernization theories provides an interpretation with very wide implications. In sum, this is a book that deserves to influence approaches to the study of market imperfections in England's industrial revolution generally.

JULIAN HOPPIT, *University College London*

Privilege and the Politics of Taxation in Eighteenth-Century France: Liberté, Égalité, Fiscalité. By Michael Kwass. Cambridge: Cambridge University Press, 2000. Pp. xvii, 353. \$69.95.

Historians commonly attribute the origins of the French Revolution to the crown's relative inability to tax members of the elite. The proliferation of tax-exemptions, the argument goes, contributed to the fiscal crisis that brought about the collapse of absolutism. While Michael Kwass sets out to disprove this thesis, taxation nonetheless remains central to his understanding of the Revolution's origins. The crown did indeed succeed in taxing nobles and other members of the privileged elite, he argues, and yet the very debates and contestations that arose from such policies laid the groundwork for a new revolutionary political culture.

Kwass agrees with revisionist historians that the revolutionary impact of fiscal protests can be understood only with reference to the evolving political culture of the eighteenth century. Yet he takes his analysis one step further to uncover the sociopolitical causes of the cultural, linguistic, and ideological phenomena that gave rise to the Revolution. Inspired in part by the “post-revisionist” embrace of both Tocqueville and Habermas, whose works take seriously the changing social and political context of the eighteenth century, Kwass focuses on that “omnipresent” issue, taxation, and considers “how long-term changes in fiscal institutions altered the relationship between monarchy and social hierarchy and, in turn, helped to transform political culture” (p. 13).

Beginning in 1695, the crown sought new sources of revenue by creating universal direct taxes, first the *capitation* and later the *dixième* (which became the *vingtième* in 1749). The first was a scaled poll tax assessed on an ambiguous combination of social standing and wealth; the latter two were taxes on net revenue measured according to declared—or, increasingly as time went on, assessed—wealth. All had in common a disregard for privileges, and their establishment fundamentally altered the relationship between subject and monarch. Whereas unprivileged persons had previously paid all direct taxes collectively, and had brought their disputes to local and regional law courts, these new taxes were universal, and royal administrators oversaw assessment, collection, and the arbitration of disputes.

From the outset, the crown justified universal taxes for the relief they would provide the people by distributing the tax burden more equitably. Kwass tests this claim and finds that the weight of the *capitation* on the privileged was “mildly burdensome” (p. 82), but in no way of help in alleviating the weight of taxation on commoners. The *dixième* and *vingtième*, on the other hand, did make inroads into the wealth of privileged subjects (still without effect for the overburdened majority); this was largely the result of efforts by the crown in the 1750s and 1770s to investigate and assess wealth. Kwass estimates that the privileged paid about 65 percent of what they would have owed had the assessors found all “hidden” property. “These were significant rates,” Kwass concludes, “when we recall that a century earlier the very idea of a direct tax on privileged landowners was inconceivable” (p. 92).

It was thus not the crown’s inability to tax the privileged that undermined royal authority prior to the Revolution, but the success with which it brought the privileged into contact with a tax-collecting administration. Several levels of contestation, dispute, and debate arose that ushered forth a revolutionary political culture infused with concepts such as “nation,” “citizenship,” “liberty,” and “estates.” Kwass thoroughly examines these layers of dispute, which include individual petitions for tax reassessment, increasingly public disputes between magistrates and royal ministers over tax legislation, literary debates about ideal systems of taxation, and revolutionary-era disputes over taxation and citizenship. The result is a sophisticated analysis that succeeds in tracing the origins of the Revolution to where cultural and institutional developments intersected in matters of taxation.

Kwass’s argument that the privileged bore a meaningful tax burden in the eighteenth century is certainly convincing. Readers of this JOURNAL, however, might wish for more analysis of the social incidence of taxation. What was the overall relative tax burden borne by the privileged? What inefficiencies arose from eighteenth-century tax policies? Of course, these lines of inquiry are not essential to Kwass’s argument, and his book cannot be all things to all people. More pertinent to his enterprise, though, would have been further discussion of the institutional particularities of Normandy, where Kwass did much of his research. The crown levied exceptionally high direct taxes in this province during the seventeenth century. Did this relative overtaxation continue in the eighteenth century? Was it affected in one way or another by the establishment of universal taxes, and did it contrib-

ute to the convictions of Norman magistrates who were, as Kwass points out, on the forefront of opposing royal tax policies? These minor points, however, should not detract from what amounts to a serious and valuable contribution to both the institutional–financial history of old-regime France, and to our understanding of the origins of the French Revolution.

MARK POTTER, *University of Wyoming*

The Russian Peasantry 1600–1930: The World the Peasants Made. By David Moon. London: Longman, 1999. Pp. xii, 396. £49.00, cloth; £17.99, paper.

As a result of social historians' desire to investigate nonelite groups, the historiography of the Russian peasantry has burgeoned in the last twenty years or so. Numerous studies on both the macro and micro levels have illuminated various aspects of Russian peasant life during serfdom, the postemancipation period, and the revolutionary era. Others have begun to document the systematic destruction of the Russian peasants' way of life during collectivization, and the peasants' stubborn resistance to that onslaught. It is therefore fitting for David Moon, a recognized scholar of the Russian peasantry in the last decades of serfdom, to write a book that synthesizes the latest scholarship, both Soviet and Western, as well as studies by prerevolutionary historians and observers. The numerous citations in each footnote testify to Moon's prodigious reading and mastery of the literature.

While the book covers over three centuries, its main focus is the period from the early eighteenth century to 1861, when state and landlord exploitation of the peasants was at its height. Given the relative neglect of the eighteenth century in Russian historiography, Moon's conscious decision to concentrate on the pre-emancipation period is sound. He also provides a cautionary note for those scholars of the prerevolutionary Russian peasantry who overemphasize change. In the context of the *longue durée* of the Russian peasantry's history, he argues, continuity rather than change emerges as the dominant characteristic, a characteristic typical of other peasant societies before industrialization dramatically altered the rural landscape.

Moon's approach is twofold. He sets out to demonstrate the rationality and pragmatism of Russian peasants, without however idealizing them. Intent on presenting the peasants' point of view, their needs, and their desires, he succeeds in illuminating the complexity of rural life, the tensions inherent within it as well as the circumstances that militated toward cooperation. His is not an idealized peasantry. In the second instance, Moon judiciously presents all sides of a historiographical debate before coming to his own conclusions. For example, Moon is far more Chayanovite than Leninist in his analysis of the peasantry, emphasizing demographic factors of household formation and the peasants' subsistence needs, rather than the profit motive. At the same time, he demonstrates the way in which Lenin's model of peasant differentiation and polarization before the late nineteenth century is more appropriate for a minority of villages in which handicrafts, trade, and wage labor played greater roles than did agriculture. Ultimately, however, Moon points out that the Chayanov model also has the limitation of overlooking the fact that more prosperous households were able to assert their authority, and maintain or increase their wealth, through a variety of strategies that had nothing to do with household size and structure. In another example, he contributes to the debate about whether or not rural living standards declined after emancipation by pointing to the lesser exploitation of the peasantry in the late nineteenth century, and to a peasant diet that was able to sustain heavy agricultural labor.

Moon sees the history of the Russian peasantry as falling neatly into three periods: one prior to the late seventeenth century, a second between the late 1600s and late 1800s, and

a third encompassing the late nineteenth and early twentieth centuries. It is only in the middle period that the so-called traditional Russian peasant commune and its repartitional practices arose, in response to state and landlord demands for taxes and obligations. Large complex households—a result of the postmortem divisions, early ages at marriage, and high fertility that also characterized peasant life under serfdom—could only be sustained by an abundance of land, a luxury that peasants in Western Europe did not enjoy. The middle period also witnessed continuous peasant migrations to outlying regions, which caused “a shift in the centre of gravity of Russian peasant settlement from the forest-heartland to the forested steppe” (p. 59). Once emancipation had abolished landlord controls, Moon argues, households began dividing once again, reverting back to the sizes and structures that had characterized Muscovite peasant Russia before the 1645–1679 tax on households. Given the inadequate data on Muscovite households, and the still-infant state of demographic studies of parish records for the modern period, however, such a conclusion is based more on speculation than fact. Moon is on firm ground in arguing that the peasant protests at the turn of the twentieth century had much in common with those in central Russia in the seventeenth century, and with others in the borderlands in the eighteenth century; in each case, peasants resorted to “mass and open protest to try to attain more radical objectives” rather than more limited forms of passive resistance (p. 351).

Nicely written, with attention to Russian peasants’ celebration of major life passages, inner-household relationships, and some aspects of peasants’ religiosity in addition to economic issues and regional variations, the book is intended more for the generalist than the specialist. It can also be profitably used at an advanced undergraduate level.

CHRISTINE D. WOROBEK, *Northern Illinois University*

Germany in Central America: Competitive Imperialism, 1821–1929. By Thomas Schoonover. Tuscaloosa and London: University of Alabama Press, 1998. Pp. xiii, 317. \$39.95.

Readers may obtain from this book an overview of German–Central American relations, the main points of which can be summarized briefly. Germany had a significant presence in the Central American republics since the second quarter of the nineteenth century. Well before Germany’s unification, they figured in German public discourse as potential areas for colonization, trade, investment, and performance of the rituals of great-power politics. First the Prussian state and then the Reich displayed a restrained concern for Central America, and engaged in competition with Britain, France, and the United States to advance the interests of German businesses and settlers. German commerce with the region rose at times to not-insignificant levels, especially when measured in terms of the total trade of individual Central American countries. Landowners, businesspeople, and professionals of German extraction have had a significant effect on the cultural and social histories of Guatemala and Costa Rica. However, at no time has Central America been of vital economic importance to Germany or at the forefront of German policy. In general, at least between the 1870s and 1929, German governments were unwilling to challenge U.S. hegemony in the area.

Readers interested in German–Central American relations may also find the book helpful because of the author’s copious use of European and U.S. archives: chiefly diplomatic and consular records, with some material from German trade organizations. (By contrast, he cites relatively few Central American primary sources.) Documents are carefully cited and are quoted or paraphrased at length. Unfortunately, this sometimes gets in the way of

presenting a clear narrative or a continuous interpretation. In places, readers must make an effort to extract an overview from the author's rather fragmented presentation, which tends to leap from document to document, and from particular issues in one Central American country to different ones in another. Statistical tables are also included, most taken directly from sources and not critically analyzed.

Against the book's strengths—as a source in itself, a guide to other sources, and a summary of its topic—must be set a number of weaknesses, the most important of which lie in the area of interpretation. The author attempts to place the detailed events he describes—inspection tours by German officials and their consequences, disputes over tariffs, rivalries between German businesses and those of other countries—in an extremely broad context defined by such constructs as “social imperialism,” “organized capitalism,” “competitive imperialism,” “dependency,” and, in the case of the Central American states, a perennial ideological conflict between liberalism and conservatism. These interpretive constructs date, for the most part, from the 1960s and 1970s and could stand some updating in the light of recent discussion. They are given essentially textbook definitions, with little critical consideration of their advantages and limitations as theoretical formulations or as frameworks for historical explanation. More important, however, is the way in which they are used. At intervals, Schoonover gives brief, very general outlines of major socioeconomic and political developments in Germany and in Central America during the period under study. These are typically summaries of familiar interpretations couched in terms of social imperialism, organized capitalism, and dependency theory. He then moves immediately into a detailed recounting of the events and impressions revealed in his sources. He occasionally makes reference in passing to the broad theoretical constructs, but the connections are not developed. There is, in other words, a curious gap in the discussion. Where readers might expect to find a consideration of links between individual German initiatives and broader topics of public discourse in Germany, or between German activity in Central America and parallel enterprises elsewhere, and where readers might look for a critical discussion of ways in which such links fit into one of the interpretive patterns the author cites, what they will generally find is a short statement that vaults over the problem. For example: “Social imperialism guided German relations with Central America in the 1870s and 1880s” (p. 84). In fact, very little of the evidence about specific German activities in Central America that the author cites for that period seems to have anything in particular to do with “social imperialism” as the term is normally used—that is, to refer to a deliberate attempt by elites in industrial countries to use imperialism to manipulate domestic class antagonisms or to solve social problems created by economic change. In the absence of a fuller analysis, the cited evidence seems to be saying instead that the desire for profits guided German relations with Central America. The two statements are not necessarily incompatible, but the first must be demonstrated, not simply asserted, because it is not immediately apparent in the documents the author discusses.

This is unquestionably a useful study. It is more useful, however, as a summary and as a guide to source materials than as an interpretation of its subject.

WOODRUFF D. SMITH, *University of Massachusetts, Boston*

Manipulating Hegemony: State Power, Labour and the Marshall Plan in Britain. By Rhiannon Vickers. New York: St. Martin's Press, 2000. Pp. xvi, 185. \$69.95.

The precise role of the Marshall Plan in stimulating economic recovery in Western Europe after 1945 remains problematical. One thing, however, is for sure: writing about

the Plan has given employment to many scholars. Over 50 years on, the scholarly literature is extensive and growing. Happily, a consensus of sorts has emerged. Firstly regarding the overall purpose and impact of the Plan, the New Left's contention of naked American imperialism has been replaced by the view that Marshall aid was, in Geir Lundestad's words, an instance of "empire by consent." Secondly, it has been established that while the role of Marshall aid in fueling the West European economy was marginal, that margin was critical. The workings of what Charles Maier has called "a consensual American hegemony" have been investigated in several key works. Michael J. Hogan's *The Marshall Plan* (New York: Cambridge University Press, 1987) has shown how and why the British Labour government fought so tenaciously against America's vision for the post-World War II world, concluding that "the Marshall Plan had aimed to remake Europe in an American mode. In the end America was made the European way" (p. 445). Anthony Carew's *Labour under the Marshall Plan* (Manchester: Manchester University Press, 1987) is equally ambitious, focusing on American initiatives and on their reception in four major European countries (Germany, France, Italy, and Britain); Carew does inquire in detail how the British Labour leaders and Trades Union movement influenced the Marshall program. In this context *Manipulating Hegemony* is an important new study, because it offers the first in-depth scrutiny of British responses.

Utilizing the neo-Gramscian conception of hegemony, which stresses consent as well as coercion, Vickers surveys the motivation and operation of the Plan. An assessment of Western and Soviet responses is followed by a discussion of the Plan's scale and impact on Britain in particular. But the core of the book is a perceptive discussion of the central British players: officials in Clement Attlee's Labour government, Bevanite critics of Labour's foreign policy, and the Trades Union Congress. Especially valuable is the analysis of the two trade-union bodies that emerged in response to Marshall: the Economic Recovery Trade Union Advisory Committee and the Anglo-American Council on Productivity (AACP). These two organizations, in addition to providing institutional structures for the control of organized labor responses, helped to legitimize the Plan in the eyes of British and European labor. Notably, the AACP became the "single most important institution through which the 'politics of productivity' was transferred to Britain" (p. 4). Vickers points out that "rather than the US imposing the agenda of the AACP . . . the Labour leadership welcomed the new discourse of productivity according to their own vision of a modernised British economy" (p. 5). Attlee, when asked what he considered to be foreign secretary Ernest Bevin's supreme achievement, replied that it was "Standing up to the Americans." To be sure, Hogan and Carew have themselves demonstrated that British leaders did indeed stand up to the United States, but Vickers illuminates in detail their motivation and policies. This is primarily an empirical study, one anchored in archival material and the secondary literature. Although the limitations of the neo-Gramscian approach—for example the downgrading of domestic actors—are addressed, the theoretical thread is not pursued. Vickers concludes convincingly that the Plan marked the incorporation of union leadership into the governing structure, "with trade unions acting not only as a power for their members but as a power over their members, thus strengthening state power" (p. 5).

Vickers's revisionism should serve as a warning of the danger of expecting too much from aid programs. Calls in the 1990s for a Marshall Plan for Russia and Eastern Europe sprang from the belief that the original plan represented America's most successful aid program. And so it did. Yet it was successful only because European leaders succeeded in adapting and shaping it to their own ends. Vickers has produced a closely researched, cogently argued study that contributes substantially to an understanding of the Plan as it developed and operated in practice.

ANTHONY ADAMTHWAITE, *University of California, Berkeley*

ASIA

Encountering Chinese Networks: Western, Japanese, and Chinese Corporations in China, 1880–1937. By Sherman Cochran. Berkeley: University of California Press, 2000. Pp. xii, 257. \$40.00.

One Industry, Two Chinas: Silk Filatures and Peasant–Family Production in Wuxi County, 1865–1937. By Lynda S. Bell. Stanford, CA: Stanford University Press, 1999. Pp. xvi, 290. \$49.50.

Patterns of commercial development have varied widely across the world. At one end of the spectrum is the experience of premodern Europe, where commercial centers grew up in cities that were independent of the surrounding feudal political system. Over the centuries these commercial centers developed their own laws and other institutions that laid the foundation for the capitalist economic system based on the rule of law that exists today. In regions such as Southeast Asia and East Africa, by contrast, commerce was mostly a foreign import with citizens of the colonial power running the larger commercial enterprises and immigrants from China or South Asia owning and staffing the rest of the system. China fit neither of these patterns: over the centuries it developed a commercial system that was entirely Chinese, but was based on networks of traders who set many of their own rules and yet were in no sense independent of the governing bureaucracy whose power ultimately rested with the emperor. This system led to a level of commercial development that was marveled at by European visitors into the eighteenth century.

By the late nineteenth and early twentieth centuries traders from the industrialized world no longer marveled at the size and sophistication of Chinese commerce, but they still wanted to expand sales to China, purchases from it, and modern industry within it. Chinese merchants and entrepreneurs, of course, wanted to do the same thing. The question then was how to do this. Did one import the organizations and management techniques of the already industrialized world, or did one attempt to accomplish one's aims by working through the existing institutions and networks that had been built up in China over the previous centuries?

Both of the books reviewed here attempt to answer that question by analyzing various cases of commercial and industrial development in China from the last quarter of the nineteenth century through 1937, when the Japanese invasion brought such initiatives to a halt. Sherman Cochran deals with this question by looking at the experience of two Western, two Japanese, and two Chinese-owned firms. Lynda S. Bell focuses on one sector, the modern silk industry, in one part of the country, Wuxi County in Jiangsu Province, not far from Shanghai.

Cochran confronts this question head-on. Much of his analysis is focused on the degree to which these companies, both Chinese and foreign, attempted to import management techniques from the West—"managerial capitalism operating through hierarchies," in the words of Alfred Chandler, on whose work Cochran bases his definition of Western management technique. Cochran's analysis centers on two primary questions. The first question is whether these firms relied on existing Chinese commercial networks to trade, or rather attempted to create their own system based on patterns they had followed elsewhere. The second question is whether industrial firms hired workers directly, or relied instead on the traditional Chinese approach whereby a foreman (a "Number One") hired, guaranteed, and paid the labor force.

Cochran does show that it was possible for a Western or Japanese firm to eventually replace Chinese trading networks with a sales force of its own; both Standard Oil and

Mitsui Trading did it. The Japanese industrial firm (Naigai Cotton Company) also managed to introduce direct hiring and to get rid of the Number Ones. The two Chinese firms, in contrast, relied heavily on existing social networks based largely in their home district, although they also tried to shape these networks to their own ends. Shengxin Cotton Mills was able to get rid of Number Ones and thus deal directly with its workforce, but China Match was not.

Cochran tends to emphasize the ability of the foreign firms to break away from traditional Chinese modes of doing business. I would tend to emphasize more the extraordinary tenacity of these networks; and how only the largest and most powerful foreign companies could effectively replace them—even then, it took decades to do so. Nor is it entirely clear whether it was really to the advantage of individual firms to set up their own systems rather than rely on existing networks. Getting rid of the Number Ones was undoubtedly desirable from a firm and nationwide development perspective, but the existing commercial networks often won out, not because they had a nationalist government behind them, but simply because they could often out-compete direct efforts at marketing by foreign firms. If anything, the government, thanks to the treaties imposed on China, was more favorable to the foreigners than to its own merchants and factory owners. It should also be noted that, even when a foreign company succeeded in circumventing the traditional networks, they still largely employed Chinese to do the work at all but top management levels. The exception was the Japanese, who then (as now) relied on their own nationals to staff many even lower-level positions to a far greater degree than other multinationals. My differences of interpretation with Cochran are more in the nature of a quibble than a major disagreement. The material in the book is very clearly presented and the reader can easily reach his or her own conclusions about these questions.

Lynda Bell's study is both broader and narrower than Cochran's. It is broader in the sense that it covers a wider range of issues, and narrower in that it focuses on one industry in one county and deals only with Chinese-owned firms. The main theme of this study of the silk industry in Wuxi is that Chinese industrialists attempting to build modern silk filatures still had to work primarily through traditional networks, even though the ultimate goal was to build a modern factory to export silk abroad. These networks' operation, however, changed over time. In part the Wuxi silk merchants and manufacturers themselves tried to alter the existing systems to suit their needs. Equally important, the national and provincial governments faced a new situation in the decades following the end of the Taiping Rebellion, one that altered the way the government related to these networks. Bell goes into rich detail on how the merchant industrialists and the government tried to use each other to serve their not-always-complementary goals.

Bell is also interested in how the development of the Wuxi silk industry affected the lives of the peasant farmers as well as the urban elites. The industry was a new one for Wuxi, even though it had existed for centuries in nearby counties and provinces. Mostly it was women who raised the cocoons, because tradition confined them to the home: thus only their labor had an opportunity cost low enough to justify an activity where the return was so low. Men could do better by going to the nearby towns to find work.

Bell's study, therefore, is less focused on a single argument than is Cochran's. It yields insights into the way government, elites, and peasants interacted in one of China's more developed and crowded counties. The silk industry provides the unifying theme, but the observations apply much more broadly. Because it is more focused on its main theme and more tightly argued, the Cochran volume will probably appeal more to economists. Bell's book, on the other hand, by concentrating on a single county and one industry, actually gives one a broader picture of the way the Chinese economy and society responded to the early efforts to modernize in the late nineteenth and early twentieth centuries. Both books

are based on long years of careful research in Chinese and company archives. Thanks to these two monographs, and to a handful of other studies that have been published over the past several decades, we are beginning to get a clear picture of the institutional arrangements that shaped China's efforts to modernize its economy in the era prior to the Second World War.

DWIGHT H. PERKINS, *Harvard University*

China and Historical Capitalism: Genealogies of Sinological Knowledge. Edited by Timothy Brook and Gregory Blue. Cambridge: Cambridge University Press, 1999. Pp. xii, 291. \$64.95.

The five thoughtful essays collected here "aim to identify distortions introduced in the understanding of China under the influence of cultural and ideological trends that took Western capitalist relations and patterns of development to be the norm" (p. 5). Two trace how intellectuals concerned with finding the patterns of change familiar from European history failed to see change in China; one describes the origins of European capitalism as an accident that says little about basic differences among civilizations; and two trace long-term Chinese developments to show important economic changes proceeding according to a noncapitalist logic.

Gregory Blue traces "Western" views of China from the Renaissance to debates among Soviet Marxists. He emphasizes the most important Western thinkers, not those best informed about China. Blue shows clearly how China became a strategic foil in Europeans' internal political debates and their quests for universal historical theories. Thus the "Western perspective" was never monolithic; but he also emphasizes that beneath their differences, Western writers shared a view of China as essentially changeless. This, Blue argues, reflects a problem we still have: the lack of a vocabulary to describe long-term changes that did not lead toward capitalism.

Timothy Brook continues this tale into modern Chinese and Japanese historiography. A crucial issue here is the relationship between "capitalism" and "modernity" as analytical concepts. As Brook notes, Chinese intellectuals usually labeled the features of a desired future as "modern," while regarding capitalism with much greater ambivalence. There was, however, little serious discussion of how capitalism was related to modernity, either in the West or more generally. Even today, when capitalism is viewed positively by many Chinese intellectuals (if not by the state), its relationship to modernity remains largely unexamined, and most Chinese writing of Chinese history is still structured by contrasts with "the rise of the West."

Immanuel Wallerstein reverses the "rise of the West" story, asking why Europe "fell" into a system driven by the need for capital accumulation. Setting aside moral issues, the principal analytic importance of such a move in this book is to "normalize" China, making capitalism the deviant outcome. Wallerstein rejects all links between Western Europe's post-1500 uniqueness and long-standing civilizational differences (he does not consider the possibility that the crucial divergence may have emerged later), arguing that capitalism was the accidental result of the unusual depth in Europe of a larger Eurasian crisis, coupled with historical contingencies that prevented a Mongol conquest. The argument will be familiar to those who have read Wallerstein's *The Modern World System* (vol. I, New York: Academic Press, 1974), but this a clear and compact version of his thesis, with some useful reflections on more recent scholarship (especially by Guy Bois and Robert Brenner) that emphasizes class struggle within Europe as the origin of capitalism.

Francesca Bray's essay on Chinese technology takes us to analysis of what actually did happen in Chinese economy and society. She gives a useful general account of changes in both the regional and the gender division of labor, relating both to changes in agricultural and handicraft technology. On this level the argument is driven by familiar factors: for instance, ecological differences between North and South China affecting optimal farm size and crop choices, and technical innovations that spread because they overcame production bottlenecks. However, the essay also looks at official views of technology. Their central concern, Bray argues, was with which kinds of work would foster moral families and good imperial subjects; thus, they were often distressed to see comparative advantage periodically reshuffling the division of labor, and sought a stable world of farming husbands and weaving wives. This very thoughtful analysis suggests further questions not addressed here: How were these two levels related? When, where, and how did elite discourse affect producers?

R. Bin Wong's essay investigates both the aims of imperial statecraft and its achievements. Wong's comparison with European mercantilism shows divergent aims and results: mercantilism, he argues, largely "failed" in its professed objectives (since no country could endlessly amass specie without affecting its price level and competitiveness), but it did create distinctive linkages between market development, private accumulation, state fiscal interests, and state power, which together formed a crucial matrix for a subsequent industrial capitalism. By contrast, the Chinese state embraced markets as a tool for ensuring subsistence and modest prosperity, but remained ambivalent about encouraging the profit motive (since, as Bray also noted, they wanted daily activities to foster certain kinds of character); nor did the Chinese state, despite the huge scale of Chinese commerce, link its fiscal destiny to the activities of large merchants. The result was a highly commercialized economy, but not capitalism. Wong briefly traces these legacies into the twentieth century, offering the interesting suggestion that they make current Chinese slogans endorsing a "socialist" market economy worthy of more serious analysis than they often receive. He thus provides an example of the future goal this volume points to: to conceptualize links between the Chinese past and present that both create and preclude certain choices, rather than simply failing to create capitalism.

KENNETH POMERANZ, *University of California, Irvine*

Swallows and Settlers: The Great Migration from North China to Manchuria. By Thomas R. Gottschang and Diana Lary. Ann Arbor: Center for Chinese Studies, University of Michigan, 2000. Pp. xvii, 231. \$45.00.

From the 1890s through the 1930s millions of Chinese farmers and workers migrated from North China, principally from Shandong and Hebei provinces, to seek employment in Manchuria. Their migration was motivated by the opportunities in new industries, as well as in agriculture, and was facilitated by the opening of railways at the turn of the century. Manchuria was one of the most unusual and prized pieces of geopolitical real estate in the world. The homeland of China's Manchu rulers, it was sparsely populated under their dynastic regime (1644–1911), partly because of its ban on migration, which was occasionally lifted during periods of famine in North China. With the decline of the dynasty's power in the nineteenth century, Manchuria became the focus of Russian territorial ambition, to be followed in the twentieth century by relentless Japanese encroachment culminating in the creation of the Japanese puppet state of Manchukuo in 1932. "From 1906 until 1931 Manchuria was a complex patchwork of Chinese, Japanese, and Russian spheres of authority" (p. 49).

Thomas Gottschang and Diana Lary have written an account of the migration of northern Chinese to Manchuria drawing on their separate academic strengths. Gottschang, an American-trained economist, contributes statistical data and economic analysis drawn from his doctoral dissertation; Lary, a British-trained historian, draws principally on interviews she conducted in the 1980s with former migrants. Not surprisingly for a collaboration that was begun after the research was largely completed, there is a certain awkwardness to this book. And yet it has much to offer. Although statistical information has previously been available in various Chinese and Japanese sources, this is the first attempt to subject such material to economic analysis while also illuminating the social context of the migration process.

The authors call this "the greatest migration in modern history," a claim they back up with statistical comparisons (p. 2); but the most striking aspect of this migration of 25 million people, mostly young men, between roughly 1890 and 1942 was that two-thirds of them returned home after a stay that varied from one season (the "swallows" of the title, many left in the spring and returned in autumn) up to a few decades. Each year's migration, sometimes amounting to a million persons, was offset by a return flow of an average of two-thirds that number. Hence this is not so much a story of the Chinese settlement of Manchuria as it is a large-scale example of the sojourning process that was frequently seen in Chinese history. The authors effectively show how the temporary migration of sons and husbands became a part of the local economy of Shandong and Hebei, and how income from employment in Manchuria figured importantly in individual family strategies for survival.

Although natural disasters, civil disorder, and poor economic conditions in general motivated young men to go to Manchuria to work, the authors stress that there was as much "pull" as "push" in the migratory process. It was the opportunity created by new industries (mining, railways, manufacturing) in Manchuria that provided the "pull," while the hard circumstances at home formed the "push." In fact, the authors provide some data to show that it was not the very poorest people who left home, but those from farm families of marginal incomes, and that the counties that sent the most migrants were among the wealthier, not the poorest (p. 8). Earnings from Manchuria did not make Shandong and Hebei families wealthy, but they could provide up to a third or a half of their income (p. 93).

The geographical pattern of migration was determined largely by relative transport costs. Reassuring to economists, and revealing to historians, is the documentation of the dramatic reduction in transport costs once the railroads were opened up, and how localities closest to rail transport or easy steamship transport were overwhelmingly the source of migration.

Swallows and Settlers provides a comprehensive and useful digest of information about Chinese migration to Manchuria, and it should be the first stop for a Western-language source on this topic. But hopefully it will not be the last word on this subject. So much of its fascinating material points the way to further analysis or comparison. The social history of twentieth-century settlement of Manchuria focusing on the "settlers," for example, has yet to be written. Much of the material from this book can also be used for other purposes. For example, observations about family sojourning strategies invite comparison with the better-known migration from South China to Southeast Asia or to the United States.

LILLIAN M. LI, *Swarthmore College*

Shifting Ground: The Changing Agricultural Soils of China and Indonesia. By Peter H. Lindert. Cambridge, MA: MIT Press, 2000. Pp. 351. \$45.00.

This book addresses the basic issue of human-induced soil degradation. It attempts to resolve several debatable issues, such as: (i) Is accelerated soil erosion the dominant factor

responsible for soil degradation? (ii) Does agricultural intensification improve soil quality? (iii) Does increased fertilizer use compensate for low levels of soil organic matter and nitrogen contents? (iv) Does increase in farm income enhance soil quality? (v) Is urban encroachment a principal cause of farmland depletion? (vi) Is soil degradation accelerating? and (vii) Are human activities responsible for exacerbating soil degradation? The author addresses these questions objectively and bases his conclusions on the historical data regarding temporal changes in quality of agricultural soils in China and Indonesia.

The book is divided into four sections. The first, "Judging Soil Trends," addresses the common perception that the world is losing its productive agricultural soils and that the damage is likely to be most severe in developing countries of the Second and Third Worlds. It is Lindert's hypothesis that management of soil chemistry, rather than the severity of erosion by water and wind, is the important factor determining soil quality. He goes on to document "severe flaws" in the quality of available data on soil degradation, highlighting three limitations of the GLASOD (Global Assessment of Soil Degradation) methodology: (i) it measures changes over time in the absence of data over time; (ii) it is based on a set of predictions and not on a census of soil conditions on non-experimental farms; and (iii) it assumes that all land degradation is human-induced. A similar critique is made for data obtained by other methods, such as silt loads in river, changes in cultivated land area, and experiments based on variable topsoil depth. The author makes a case that study of trends in soil quality needs quantitative history if it is to draw objective conclusions. The third chapter of this section provides a historical survey of the available soil-quality data from China and Indonesia: the former are available from the 1930s to the 1980s, the latter from the 1920s to the 1990s.

The two following sections focus on soil quality in China and Indonesia, respectively. The Chinese data show that organic matter and nitrogen have been depleted since 1940 in the more intensively cultivated lands in the east, while total phosphorus and potassium contents have increased. It is this depletion, Lindert argues, rather than erosion, that should command attention. He shows that China's cropland area was underreported by 38 to 44 percent around 1985, and that the conversion to urban land use is grossly exaggerated. He concludes that overall soil productivity did not decline between the 1950s and the 1980s, and that the deserts have not in fact advanced into farmland. With respect to Indonesia, Lindert's data show that the organic-matter content of Javanese soils diminished from 1940 to 1970, then rose slightly by 1990. In comparison, total phosphorus and potassium contents increased over the half-century as a whole. And erosion, strikingly, appears to have played no role at all. The analysis in chapter 8 traces soil changes in Indonesia through to their economic effects. There was a 4- to 6-percent decline in average soil quality between 1940 and 1990. But while labor and land productivity stagnated between 1940 and 1970, they jumped by more than two-thirds between 1970 and 1990.

The final section summarizes the anthropogenic impact on soils of China and Indonesia. Principal conclusions are that (i) Intensification of agriculture does not enhance soil organic matter and nitrogen contents, but often depletes them; (ii) Phosphorus and potassium content does, however, increase in intensively cultivated soils; (iii) Accelerated erosion is not the dominant cause of soil degradation; (iv) Deserts have not encroached on northwest China's farmlands; (v) Cropland in China and Indonesia has expanded despite urbanization; (v) Rising incomes may improve soil quality; (vi) Fertilizers have served as a substitute for soil quality and a cure for a poor soil; and (vii) Humans are not worsening the quality of the soil by mismanagement. The author concludes that future study of soil degradation should shift its emphasis from erosion processes to possible overcultivation relative to input.

Overall, the book is well written, the information presented strengthens the available database, and conclusions and interpretations are supported by the data. Lindert's optimism

and positive approach to natural-resource management are refreshing. He also shows that doomsday prophecies will be proven wrong in the twenty-first century as well. Though it contains a fifth of the world's population, China has and will maximize its ability to feed itself, for example by shifting some cropland from staples to horticulture. However, Lindert's definition of soil quality, based primarily on soil chemistry, is narrow. There is no mention anywhere in the book of the importance of physical factors affecting soil quality. Relevant among these are soil structure (affecting crusting, compaction, water-logging, and root growth), available water holding capacity, permeability to air and water, and infiltration capacity. Similarly, the author has completely ignored biological quality as influenced by microbial biomass carbon and by the activity and species diversity of soil fauna. Without the benefits of changes in soil physical and biological qualities, the interpretation may be biased and conclusions speculative. Overgeneralization may also lead to complacency, and thereby to the misuse of fragile soils and ecologically sensitive eco-regions. A cautious optimism, rather than Lindert's "can-do" attitude, would be a more prudent approach to sustainable management of natural resources.

The data made available in the book have numerous ancillary benefits. The information on temporal changes in soil organic matter and nitrogen contents is useful to estimate the amount of C (as CO₂) and N (as N₂O) emitted into the atmosphere due to agricultural activities. The estimates thus obtained provide a reference point with regards to the soil C-sink capacity upon conversion to an improved management or an appropriate land use. Contrary to the author's contention that agricultural intensification leads to depletion of organic matter and nitrogen pools, the upward trends in these constituents since the 1970s show that increased use of fertilizer and adoption of recommended agricultural practices have increased both C and N pools in soils of China and Indonesia. These trends show that conversion of croplands to horticultural, pastoral, and silvicultural land uses can sequester C and N in soil and reduce the risks of accelerated greenhouse effect (see for example Rattan Lal, "World Cropland Soils as a Source or Sink for Atmospheric Carbon." *Adv. Agron.* 71 [2000]: 145–91).

This book will be of interest to soil scientists, agronomists, geographers, economists, land managers and policy makers interested in food security and the sustainable management of soil resources.

RATTAN LAL, *Ohio State University*

AUSTRALIA AND LATIN AMERICA

The Centenary Companion to Australian Federation. Edited by Helen Irving. Cambridge: Cambridge University Press, 1999. Pp. xxii, 474. \$64.95.

Australia in the Global Economy: Continuity and Change. By David Meredith and Barrie Dyster. Cambridge: Cambridge University Press, 1999. Pp. xii, 388. \$69.95.

In 1901 six of the seven British colonies in Australasia federated to form the Commonwealth of Australia. The collaborative volume edited by Irving is one of a number of recent scholarly publications marking the centennial of that event. It chronicles a federation movement that began in the middle of the nineteenth century, with different contributors telling the story from the perspective of each colony. Emphasis is given to the role of colonial leaders in the drawn-out saga, for this book is designed for readers interested in the political and constitutional dimensions of the process. (It also contains over 100 pages

of “short entries,” in the style of a compendium, and reproduces associated documents such as the 1901 constitution.) Although economic issues were important, in Irving’s book they are only indirectly recognized.

Federation was prompted in substantial part by growing feelings of nationhood among the colonies: an increasing sense of distinctiveness from British culture and society, and heightened awareness of the geographical isolation that these fragments of empire shared. For example, the influx of Chinese gold miners, and Germany’s colonizing activities in the South Pacific, had alerted the European settlers to their Asian-Pacific location and triggered uncertainty about the stability of their immediate neighborhood. Hence immigration and defense were functions listed by advocates of federation as better undertaken by a united Australia than by the colonies acting separately.

On the economic front, the Australian colonies were already highly integrated. They were members of a wider currency union, the sterling area, and they had managed through intercolonial bargaining to harmonize many arrangements affecting business, finance, and intercolonial trade—though not, infamously, the gauge of their railroad systems. More difficult to resolve in the context of political union were the issues of taxation and external tariffs. The colonial governments differed in their dependence on various revenue bases, especially regarding the sale of crown lands and tariffs. New South Wales had remained relatively free-trading while Victoria had erected protective tariffs. Since federation required free trade between the states and a common external tariff, there were predictable alignments (both party-political and private-interest) into free-trade and protectionist groups. These issues surface constantly in the contributions to the Irving book, but unfortunately they are not systematically addressed. Nor is there discussion of the economic effects of federation: what difference did it make in the welfare of Australians, and when?

Some attention is, however, paid to the role of economic factors in explaining the timing of federation. The eastern Australian colonies suffered a severe and lengthy depression in the 1890s, which heightened awareness of their economic interdependence. In contrast, at that time the remote and sparsely settled colony of Western Australia boomed under the influence of its gold rush. Not surprisingly, this colony was the least enthusiastic about the benefits of federation.

It is timely to be reminded by Irving’s contributors of New Zealand’s participation in some stages of the negotiations that led to federation. Perhaps because the New Zealand economy revived more rapidly from the downturn of the early 1890s than did eastern Australia’s, the New Zealanders decided to stay out. In the last quarter of the twentieth century, by contrast, as its relative economic fortunes slipped, New Zealand entered first a free-trade agreement with Australia, then pursued more comprehensive economic integration. With discussion of a possible currency union now underway, the centenary of New Zealand’s decision not to join Australia in 1901 looks—at least on the economic front—to have been substantially reversed.

The book by Meredith and Dyster has quite a different audience in mind. It is a revised edition of a text on Australian economic history, directed at introductory undergraduate courses (there is a glossary of elementary economic terms) and focusing primarily on the twentieth century. As hinted in the title, the treatment is designed to emphasize the interrelationships between the Australian and international economies—a desirable approach to the study of a small, open economy whose development has for 200 years been closely bound to fluctuations in, and opportunities offered by, global economic forces. As the only recently published survey text available, it offers both students and the general reader a convenient summary of events and of most major issues in the literature.

Less satisfactory, at least for readers seeking a comprehensive introduction to the development of Australia’s economy, is that there are really two books packaged in parallel. The

international economic context of the Australian story is not integrated into the domestic narrative. Rather, in each of the five subperiods considered, separate chapters on the world economy precede discussion of the domestic economy. Thus almost half the book is not about Australia. This may be useful for the teacher wishing to cover both Australian and international economic history in the one course. But the result is a necessarily thin coverage of both sets of material. If bringing the story into the 1990s is not essential, readers may thus prefer to consult fuller, if older, accounts of Australian economic history.

IAN W. MCLEAN, *Adelaide University*

Fruitless Trees: Portuguese Conservation and Brazil's Colonial Timber. By Shawn William Miller. Stanford, CA: Stanford University Press, 2000. Pp. xiii, 325. \$55.00.

The tropical forest of Brazil has been a symbol of America's abundance, potential, and mystery ever since the Portuguese first landed there in 1500; but harvesting its potential has never been easy. The complex reality of the forest has always made its exploitation complicated. Whereas the history of the Brazilian forest has previously been studied from an ecological point of view, particularly in Warren Dean's *With Broadax and Firebrand: The Destruction of the Brazilian Atlantic Forest* (Berkeley: University of California Press, 1995), the present book is less interested in what the Europeans *should have done* to save the forest than in what they actually *did* to turn the forest into the exploitable resource. Miller does not wish to condemn the colonists for exploiting the forest, nor does he think that slash-and-burn farming or timbering were wholly to blame for the resulting destruction. Instead, his story is centered on the negative effects of the *real corte*, a royal monopoly of the most attractive wood-bearing species, the so-called *madeiras de lei*, and on the long-term results of government monopoly on the economic development of the timber industry. In this, Miller follows the lead of F. W. O. Morton ("The Royal Timber in Late Colonial Bahia," *Hispanic American Historical Review* 58 (1978): 41–61), but he expands the boundaries of that important study both geographically and chronologically.

Miller begins with a discussion of the colonial landscape, including analyses of soil types and of the amazing variety of tree species in the Brazilian forests. (He provides a very useful appendix on timber types). Early colonists and the crown were not only interested in exploiting the dyewood *pau brasil*—which gave the colony its name—but also the many varieties that could be used for naval construction, and that gave ships built in Brazilian yards a reputation for soundness and durability. He then demonstrates how the Portuguese policy of claiming for the crown all trees—even those standing on private land—suitable for naval construction and cabinet-making, which began in the mid-seventeenth century, was far in excess of what was done by other colonial powers at the time. It set the crown in direct competition with the local sugar planters, who were voracious consumers of firewood, and with those colonists who wished to clear fields or exploit the wood themselves. This competition intensified in the 1790s, as the crown passed new legislation to appropriate all forested areas within about thirty miles of the coast. Portuguese royal forest policy, its limitations on the timbering and shipbuilding industries, and its overall failure in the colonial context, together form the core of the book.

Miller is at his most informative in describing the regional timbering economies, the process of tree selection and felling, the techniques and technology of timbering, the transatlantic shipping of timber, and in calculating the industry's output. Here he is somewhat a prisoner to his sources: he is forced to depend on the *balanças do comercio*, which exist for the period 1796–1819 but not earlier. Thus the book is focused on this period and

on the royal monopoly's operations. Timbering was one of the few types of work in colonial Brazil that, for obvious reasons, was not dominated by slaves. Miller is able to provide some information on the workforce, but without much detail.

The book covers many themes and a tremendous amount of information, but at times the focus is too limited. The chapter on "Shipbuilding and Tropical Timber," while very informative about the problems of Brazilian naval construction, might have profited from some comparison to shipbuilding in other tropical areas (see Satpal Sangwan, "The Sinking Ships: Colonial Policy and the Decline of Indian Shipping, 1735–1835." In *Technology and the Raj*, edited by R. MacLeod and D. Kuman. New Delhi: Sage, 1995). There is not much discussion of the interrelationship between the demands for firewood and the *real cortes*, or of the overall reaction to the monopoly. While Miller successfully contests Dean's assertion that Brazilians knew little of the forests that surrounded them, he does not provide much information on how common people felt about the governmental policies. Such information is difficult to obtain, but not impossible. For example, a group of escaped slaves in Ilhéus, who in 1789 proposed a series of conditions under which they would return to slavery, demanded of their former owner a dispensation whereby "each person can cut jacaranda or any other wood without having to account for this." This was a defiance of the royal monopoly and an indication of feelings and frustrations about it, which slaves may have shared with the rural population in general. On the other hand, throughout the book Miller provides some very interesting comparisons to the timber industry of British America, especially New England, which he argues was far more successful. While in the late eighteenth century timber did not make up 1 percent of exports from any region of Brazil, in British North America—even in the monocultural south—levels were considerably higher (p. 211).

Overall, this is an excellent monograph that will serve as the starting point for the study of the colonial Brazilian timber industry for years to come. It represents an effective response of economic history to historical ecology's condemnation of the way American forests have been exploited.

STUART B. SCHWARTZ, *Yale University*

UNITED STATES AND CANADA

Rooted in Barbarous Soil: People, Culture, and Community in Gold Rush California.

Edited by Kevin Starr and Richard J. Orsi. Berkeley: University of California Press, 2000. Pp. x, 364. \$60.00, cloth; \$24.95, paper.

This is the third volume of the *California History Sesquicentennial Series* from the University of California Press. The present volume contains 12 essays on social history, and is handsomely illustrated with original photos and artwork, including 16 color plates of California art from the first decades of the American period. Each essay covers a particular topic, such as education, migration, urbanization, popular culture, the status of women, and more. The overarching theme of the book is that the Gold Rush caused a period of barbarism in California society and culture characterized by racism, violence, and drunkenness, but as the seeds of civilization slowly took root, a relatively "normal" society emerged. One irony of this theme is that it undergirds the postmodern sensibilities of several essays that attempt to uncover the hidden narratives of women, minorities, and sexual desire.

The essays are literary and qualitative, with evidence drawn from archival sources and citations from other historians. The volume is devoid of economic analysis and quantitative

evidence is rarely presented. A few authors draw inferences from the 1850 census but neglect to mention that fires destroyed the returns from three of the most populous counties, including San Francisco, and that the severe undercount in the mining counties led to a second census in 1852.

The central theme of barbarism giving way to civilization may be mostly a literary device, but it is contradicted in several of the essays, for example those of Anthony Kirk on Gold Rush art, Michael Kowalewski on literature, and Gary Kurtz on popular culture. Kowalewski cites sources to the effect that immigrants were the most literate pioneers in American history (p. 207), and all three of these essays describe a high level of demand and supply of art, literature, and refined entertainment, in addition to drinking and gambling, and in spite of the carnivalesque atmosphere of the Gold Rush.

In one of the few essays that uses concepts drawn from the social sciences, Sucheng Chan examines the struggle of Chinese, African American, French, and Latin American argonauts against the unjust treatment doled out by the Anglo majority. To one degree or another, with greater and lesser success, members of these ethnic groups used a nontransparent legal system and political order to protect their interests. Chan speculates that the intensification of Anglo hostility towards non-Anglos, from ethnic identity to nativism to racism, parallels three periods of mining, beginning with the 1848 placers when gold was relatively easy to find, to the more difficult placer mining period from 1849 through the early 1850s, and finally after 1852 when the technology of gold mining became more capital intensive. The mechanism that connects mining conditions and techniques to the treatment of non-Anglos is unclear, but it is a suggestive hypothesis.

A number of other essays raise interesting economic questions that are not addressed. In an essay on the East Coast migration, Malcolm Rohrbach notes that the high cost of the journey created "ideal" conditions for "companies or individuals with investment ambitions" (p. 32), but there is no discussion of the principal-agent problem this creates. James Sandos's essay on the destruction of Native Americans leaves the reader with the mistaken impression that the white community was united over Indian policy, and asserts without evidence (p. 96) that systematic murder was the single greatest factor in Native American deaths in Northern California after 1848. Sandos may be right, but he ignores the issues surrounding Native American family life and the social and economic conditions that made it nearly impossible to form and maintain families.

In an essay on urbanism Robert Phelps asserts that Los Angeles was within the orbit of the state's urban system (p. 123), implying that the northern and southern parts of the state were well integrated. This is a questionable proposition at best, and is contradicted by the essay on religion by Steven Avella (p. 256). According to the 1850 manuscript census, wages in Los Angeles and the southern part of the state were significantly lower than elsewhere, and Los Angeles remained predominantly Hispanic and Catholic for many decades. Los Angeles traded with San Francisco and Sacramento, but so did Kowloon, Callao, and Sydney.

In his introductory essay Kevin Starr writes that the Gold Rush "more than any other event up to that time firstly and most boldly dramatized the increasingly global nature of American society in the nineteenth and twentieth centuries" (p. 4). Starr and the others take great care to stress the heterogeneity of California society, but it is an open question whether California in the late 1840s and 1850s can be interpreted as representative of American society in any meaningful sense other than that it belonged to the United States and that its political system and laws were (mainly) American. Its economy, its cultures, and its societies, may or may not have been part of a larger American society, but there is no formal or systematic attempt to find an answer to this fundamental question. Not only is it plausible to view California's relationship with the rest of the nation in the 1850s as

weak and relatively tenuous, but it also plausible that during its first decade and beyond, the state was several cultural and economic regions, loosely held together by a state government, but as different from one another as modern Sonora is from modern Oregon.

JAMES GERBER, *San Diego State University*

A House Dividing: Economic Development in Pennsylvania and Virginia Before the Civil War. By John Majewski. Cambridge: Cambridge University Press, 2000. Pp. xvii, 214. \$49.95.

A House Dividing serves as much-appreciated example of how narrative political economy remains a persuasive format for historical research. John Majewski addresses the topic of divergence in economic development between antebellum North and South by looking inside the highly political process of infrastructure development. Using case studies of Albemarle County, Virginia, and Cumberland County, Pennsylvania, Majewski shows how competition between rival small cities left Virginia without a coherent growth strategy. In marked contrast, eventual domination of Pennsylvania internal improvements by business interests in Philadelphia and Pittsburgh focused development efforts there.

Central to Majewski's argument is the process of forming and financing the infrastructure improvement and construction companies that were chartered by the states. He dubs the companies, "developmental corporations." Developmental corporation is an apt description of organizations that use local pride to get around the free-rider problem. It is a truly useful definition that allows him to avoid getting mired in the debate over America's "capitalist" roots.

Although Albemarle and Cumberland Counties are hardly representative in a statistical sense, they are excellent choices for comparison. Both counties were well within the market regions of potentially major cities (Richmond for Albemarle, Baltimore and Philadelphia for Cumberland). Both counties had rich soil and were well-settled by the early national period. Circumstances were ripe for infrastructure development. Developmental corporations harnessed local enthusiasm in both counties, first for canals and turnpikes, then for railroads. A survey of local histories would quickly reveal the same process underway up and down the Atlantic Coast during Majewski's time frame, and in the Northwest Territories and the New South slightly later.

The earliest developmental corporations in both North and South relied heavily on local investors, who bought shares mainly on the prospect of experiencing appreciation of their land values. In the 1830s, however, urban financiers in the North began investing in railroads, not for the indirect benefits, but with the expectation of dividends. Focusing on direct benefits led Northern financiers to build trunk lines that connected (and drew revenues from) users of the many local lines.

In *A House Dividing* historical statistics are deftly folded into a well-developed base of archival research. To bolster his argument that the indirect benefits of investing in developmental corporations were substantial, Majewski uses a sample of tax and census records linked together and linked across time. He then takes the research a further step by completing, as well as possible, the cumbersome task of discovering whether the residents who received large indirect benefits actually bought shares in developmental corporations.

As admirable for what it does not do as for what it does, *A House Dividing* does not employ formal game theory to describe the rivalry between the several smaller cities of Virginia. Instead, the space is devoted to careful description of the local politics and legal disputes that underscore how hotly contested the race to develop can be. The exhaustive

archival, manuscript evidence Majewski brings to bear highlights how many resources were wasted on rent-seeking behavior in Virginia.

Finally, graduate students will find the appendix on sources and methods particularly useful. Majewski writes that his appendix is meant to partially answer the question, "How do historians produce new knowledge?" (p. 173). I admire him for this effort. By narrating the pitfalls and pratfalls of his research, he may indeed (as he hopes) help prevent others from making the same sorts of mistakes he made. But more importantly, I think, by simply revealing that he took wrong turns on the path that eventually led to his substantial insight, Majewski offers hope to many a graduate student that they, too, will eventually complete the work.

MARY ESCHELBACH HANSEN, *American University*

The Atlanta Paradox. Edited by David L. Sjoquist. New York: Russell Sage Foundation, 2000. Pp. 300. \$34.95.

This book is the third volume (of a proposed four) in the Russell Sage Foundation's Multi-City Study of Urban Inequality. The paradox mentioned in the title refers to Atlanta's seemingly contradictory position as both a symbol of Sunbelt South racial and economic progress and a pit of persistent poverty. Yet for all the apparent progress of the post-World War II years, Atlanta in the 1990s remained a highly segregated city and its poverty rate ranked as the fifth highest in the nation. This is "the Atlanta paradox" that the authors set out to try and explain. The chief sources of data for this book, as with other volumes in the series, are household and employers surveys conducted between 1992 and 1994 as part of the Multi-City Study. A useful appendix summarizes the survey methodology (pp. 9–12). The surveys themselves will surely become a useful resource for other scholars.

Sjoquist posits several possible explanations for the economic difficulties experienced by Atlanta's black population explored by the authors. Briefly, the explanations include spatial mismatches between areas of job growth and residential location and, related to this, racial segregation in housing; racial differences in human capital, related to discrimination or inadequate education for African-Americans and, in a connected argument, a deficiency in social capital among less skilled workers; racial discrimination in hiring practices; and poor flows of information on job opportunities. These arguments are explored in the book's 11 succeeding chapters.

Truman Hartshorn and Keith Ihlanfeldt (chapter 2) document the vitality of Atlanta as a center of Sunbelt-era job creation and detail the increasing concentration of poverty in the inner city. Population boomed in Atlanta's predominantly white, northern suburbs in the 1980s. Marietta and other northern suburbs "attracted a growing number of successful blacks during the 1980s and experienced the lion's share of the region's employment growth" (pp. 29–31). In the predominantly black southern suburbs, the poverty rate fell by a third but black unemployment increased. The poverty and unemployment rates among blacks rose within the heavily African-American city of Atlanta during the same period. Escape from inner-city poverty is certainly possible, though difficult. Mark Thompson (chapter 5) expressed cautious optimism about the possibilities for integrating neighborhoods and thus reducing the spatial mismatch between people and jobs identified as a serious obstacle to employment among blacks by Sjoquist and Ihlanfeldt (chapter 6). Thompson concluded that "economic factors are not a significant contributor to housing segregation in Atlanta" (p. 112). He suggested that increased federal support and supervi-

sion could stimulate a gradual integration of neighborhoods and thus reduce the spatial mismatch that other contributors

Historian Ronald Bayor examines the historical roots of the Atlanta paradox and finds that “the history of race relations . . . laid the foundations for the Atlanta paradox.” Bayor produces ample evidence (much of it from his own substantial body of work on the city’s history) to document his assertion that “Atlanta chose for years to favor its white and neglect its black citizens with housing, employment, schools, transportation, and other city services” (p. 56). The political empowerment of blacks in the 1960s, he argues, produced only limited improvement. In the 1970s and 1980s the city’s black mayors focused most of their attention on improving employment opportunities among the city’s already substantial black middle class through affirmative action and the Minority Business Enterprise program. Atlanta’s poor remained isolated from the mainstream, generally lacking even the basic qualifications and skills necessary to qualify for help through affirmative-action programs.

In chapter 7 Ihlanfeldt and Sjoquist also examine the human-capital dimension of Atlanta’s paradox. Based on other studies of city employment growth and earnings, the authors observe that “Atlanta’s remarkable employment growth should have substantively and dramatically reduced racial disparities in earnings.” In spite of “favorable changes in the human capital of blacks,” earnings by Atlanta’s African-American workers declined relative to whites during the 1980s (p. 153). Other factors help account for this disparity. Gender considerations add nuance to the discussion of the paradox. Black women, especially single mothers, suffered from a double disadvantage because of their gender (Irene Browne and Leeann Tigges, chapter 8). Similarly, job-search strategies, assessed by Nikki McIntyre Finlay, played a role, though successful strategies seemed to be more closely linked with social networks than any other variable. Gary Green, Roger Hammer, and Tigges find “some evidence that the poor in Atlanta are more socially isolated than those who are not poor,” though this isolation should not be overstated. Such social networks capital reserves may be limited and insufficient to help improve job prospects, but the poor “continue to receive and provide help” to and from friends, relatives, and neighbors (p. 259).

Sjoquist ends with a series of policy recommendations. He is skeptical of the impact of Enterprise Zones; he suggests that small-scale, community-based unemployment centers might work more effectively. He also recommends a significant expansion of federal involvement to achieve the “deconcentration of the poor” (p. 274) and other strategies. Fundamentally, however, Sjoquist concludes on a relatively pessimistic note. He agrees with Glenn Loury’s *Brookings Review* assessment that the “problem of the underclass is ‘a race problem,’” and can only be fruitfully addressed as a result of changes in “white racial attitudes.” All the specific policy recommendations will, in the end, produce only sharply limited results unless we can achieve a substantial improvement in race relations.

The Atlanta Paradox contains a wealth of data and a number of solid analyses. While some may question Sjoquist’s insistence at the end on the primacy of race, the book makes a substantial contribution to our knowledge of poverty and progress in urban America and the modern South.

RANDALL L. PATTON, *Kennesaw State University*

Finding Jobs: Work and Welfare Reform. Edited by David E. Card and Rebecca M. Blank. New York: Russell Sage Foundation, 2000. Pp. viii, 549. \$55.00.

This book is ahead of its time. While the title suggests that *Finding Jobs* might be a study of the labor market impacts of the 1996 Personal Responsibility and Work Opportu-

nity Reconciliation Act (PRWORA), the editors and authors make clear that it is simply too early to study these effects directly. Rather, their purpose is to examine the characteristics of low-wage, low-skill labor markets, along with the effects of policies directed at these labor markets, relying on data from the pre-PRWORA era. Their hope is that this analysis will clarify the questions that we will need to ask as the effects of welfare reform become more pronounced. While the chapters are of varying quality, the book as a whole accomplishes this goal.

There are 12 main chapters (in addition to an introductory chapter), representing the contributions of 23 authors. The first six chapters analyze the characteristics of low-wage jobs and low-skill workers, including changes in the employment and earnings of these workers over the business cycle, elasticities of supply and demand for these workers, turnover, returns to experience, differences in pay between men and women, and the provision of health benefits in low-wage jobs. Chapters 7 to 10 focus on the effects of various employment-related policies in this sector of the labor market: wage subsidies, public-service employment, financial incentives for promoting work, and child-care subsidies. Chapters 11 and 12 deal with two particular aspects of the 1996 reforms: restrictions on immigrants' access to support, and lifetime time-limits on receipt of aid.

Given the scarcity of direct evidence on the impact of welfare reform at this date, many of the authors are quite creative in using analogies to more well-understood phenomena in order to derive lessons for our analysis of welfare reform. Timothy Bartik uses results on the employment effects of changes in the minimum wage to inform his discussion of the elasticity of demand for low-skill workers; these elasticities will shape the broader impact of introducing more low-skill workers to the market (as a result of work incentives, work requirements, and time limits). Similarly, Robert A. Moffitt and LaDonna A. Pavetti draw on research on unemployment insurance, a policy that has an annual limit on weeks of support, in order to speculate about the likely impact of lifetime time limits for poor relief. As creative as the authors are, they are also generally guarded in their conclusions, as the unusual macroeconomic conditions of the late 1990s, along with changes in a variety of other policies that affect low-skill workers (such as the Earned Income Tax Credit), introduce substantial complications into the analysis of these issues.

All of the chapters combine theory, literature reviews, case studies of existing and past programs, and original quantitative work in a variety of ways. The policy-related chapters (7 through 12) tend to emphasize a mix of accessible theory with a thorough and careful review of case studies of relevant programs. These chapters are the most clear and will probably be the most useful for policy makers and nonspecialists. Three of these chapters are particularly worthwhile. The discussion of public-service employment and mandatory work by David T. Ellwood and Elizabeth D. Welty pays very careful attention to issues of worker displacement that might accompany public service work. They also very clearly analyze the tradeoffs involved in these programs (for example, jobs programs that promote skill acquisition are likely to be more expensive to run, and programs that focus on producing valuable output are likely to have greater displacement effects). Rebecca M. Blank, David E. Card, and Philip K. Robins examine the effects of several characteristics of work-incentive programs, including entry requirements and job-search assistance, in addition to the effects of different income disregards and repayment rates. Moffitt and Pavetti's chapter on time limits includes a highly informative comparison of the provisions of the individual state programs created under the welfare reform law. They describe how variation in specific program elements (diversion policies, sanctions for noncompliance with work or training requirements, benefit repayment rates) will cause variation in the characteristics of the population hitting the time limits in various states. Their discussion of time-limit provisions enacted in Connecticut and Florida prior to the 1996 law is also very useful.

The weaknesses of *Finding Jobs* are primarily those associated with edited volumes of this type. The intended audience is somewhat unclear, or at least it is inconsistent across chapters. For example, the introductory chapter appears to assume that readers may not know the definitions of “median” and of “real wage” (pointing out that the former means “middle” and the latter means “inflation adjusted”), while the chapter on child-care subsidies includes a detailed description of the structural equation model used by the authors. There is also substantial inconsistency in editing and a surprising number of instances of incorrectly referenced tables, repeated text, and the like. In isolation, none of these errors is profound, but collectively they add to the challenge of sorting through the truly impressive amount of information contained in this book.

THOMAS N. MALONEY, *University of Utah*

Land in the American West: Private Claims and the Common Good. Edited by William G. Robbins and James C. Foster. Seattle: University of Washington Press, 2000. Pp. xi, 222. \$20.00.

Over the years, land use has been the focus of much attention by students of the American West. A recurring theme in a large number of scholarly studies has been a marked tension, in many contexts, between private land use and the so-called common good. Private land use practices, which include water use, mining, grazing, and timber cutting, often appear to militate against the common good, loosely defined as the best interests of society at large. This has led to numerous calls for regulation or planning efforts in order to promote the common good, often in the face of fierce resistance from private land owners and proponents of smaller government. Such real-world conflicts have spurred considerable debate among economists regarding the relative virtues of largely free-market solutions versus some form of government policy. In recent years, as population growth in the West and environmental advocacy in general have both boomed, so have the frequency and sheer economic magnitude of land-use conflicts. Consequently, the rift between the governmental and free-market positions relating to land use in the West has widened into a full-fledged gulf.

Land in the American West is a collection of well-written and incisive essays which shed much light on both the governing influences and normative implications of western land-use conflicts. The book begins with three different general perspectives on property rights in land, which discuss western land rights in their larger historical and intellectual context, leading up to current political squabbles. These perspectives generally share the view that property rights in land are very much interrelated but differ widely regarding the appropriate amount of regulation necessary to address the inevitable conflicts that arise over use. The first essay presents the anti-Lockean view that private land rights have always been elastic and very much conditional on the larger common good, and it is appropriate that this should continue. The second essay, much more sympathetic to proponents of private property, is more apprehensive about regulating land rights, particularly through the political thicket of the legislative process. The third essay follows up by taking a closer look at the political process, casting a justifiably jaundiced eye on some of the tactics used by both environmentalists and private property proponents. The bottom line is that there is no firm bottom line, except perhaps that the free-market and governmental views both fall short in resolving land-use conflicts.

After a useful two-essay interlude that examines land use from the urban and rural vantage points, the book proceeds to an examination of several case studies of land use in

the West to show how politics has played out in particular land-use contexts. The case studies examine specific land-use issues in southern Colorado, Utah, and Alaska, and develop some additional themes that shed further light on western land use. One key theme is western suspicion of federal directives handed down from Washington, which occasionally blossoms into full-blown animosity. Another is the persistence of institutions over time, as evidenced by land-use practices in southern Colorado, which derive in part from previous Spanish/Mexican rule and differ in important ways from those developed under Anglo rule. Though hardly new to students of the West, the themes are developed in interesting ways in specific contexts that may not be familiar to many, and help illustrate some of the systematic legal and political divides that have long governed western land-use policy.

An important strength of this book is its overall attempt to be even-handed in treating the free-market and governmental positions, even if some of the individual essays fall a bit short in this regard. Though the tone of the book at times smacks of a crusade to attack the unmitigated evils of the free-market approach, there is enough that is also critical of the governmental approach to satisfy the objective reader. The purpose of the book is to provide a reasoned, readable introduction to issues related to land use in the West, and in this it succeeds admirably. Economists and economic historians interested, but largely inexperienced, in such issues will find much valuable new information in this book. Those already steeped in these issues will find much to either agree or disagree with.

MARK KANAZAWA, *Carleton College*

Selling the True Time: Nineteenth-Century Timekeeping in America. By Ian R. Bartky. Stanford, CA: Stanford University Press, 2000. Pp. xvii, 310. \$45.00.

Contrary to what its title suggests, this book is not a history of clock design, manufacture, or marketing. It is instead about the rise and fall of commercial time signal distribution by astronomical observatories in the nineteenth-century United States—time-telling rather than timekeeping. It narrates complex interactions of scientific, business, and governmental establishments around the activity of telling any buyer of the service, and therefore also the public at large, what time it was, accurately and consistently. It also discusses the adoption, late in the century, of American standard time zones and the eventual international agreement to accept the location of Greenwich in England as 0°, the prime meridian from which other meridians were calibrated.

After a lucid first chapter distinguishing different kinds of solar and stellar, mean and apparent “time,” Ian Bartky begins his story in the mid-1840s. Town jewelers or churches displayed large clocks to provide locally ascertained “public” or “civic” time, differing from that of the next town east or west. American ports and major inland cities harbored purveyors of clocks, maps, and scientific instruments to those who needed them for various purposes, including astronomy. Astronomical observations on land and sea, together with chronometers carried from known longitudes, were hoped to be determining longitudes in as-yet-uncharted places, but these measurements had not yet converged.

Morse’s electric telegraph, demonstrated successfully in 1844, spread alongside railroads, providing instant communication between stations. Its capability of allowing simultaneous readings of time in two different places were also early recognized as valuable for “determining meridian distances [longitude differences] more accurately than was before within the power of instruments and observers” (p. 32, quoting naval explorer Charles Wilkes). With electromagnetic connection to their clocks, observatories could also drop

their time balls at noon without human intervention and with greater accuracy, distributing public time to any interested watcher.

Astronomical observatories tended to belong to institutions of higher education. The Royal Observatory at Greenwich in England and the newly established Naval Observatory at Washington, DC, however, were supported by their respective national governments. Both were concerned with improvements to maritime navigation. So was the Harvard University Observatory, whose director from 1839 to 1859 was also head of William Bond & Son, purveyors of clocks, watches, marine chronometers and other scientific instruments to agencies of the federal government as well as private users.

Other Bond customers were newcomers to New England since the 1830s: its steam-powered railroads, each of which ran on its own time. Conductors in charge of trains were required to carry watches synchronized with the station clocks, and to check daily to see that they coincided. Most of the New England railroads agreed in 1849 to adopt a single regional time, that of the meridian 30 miles west of Boston. This was “two minutes later than Bond and Sons’ clock, No. 17 Congress street, Boston” (p. 29, quoting rules of the Boston and Providence Railroad). From 1851 to 1862 time signals were sent along telegraph wires from the Harvard Observatory to participating railroads as well as to 17 Congress Street, twice a week. Bond did not charge for this service, but other observatories across the expanding nation later did so.

Other historians have emphasized the role of railroads and railroad travel in establishing the need for, and consensus by 1883 on, standardized time zones across the expanse of the United States. Instead of this demand-side explanation, Bartky prefers a supply-side explanation: the active promotion by astronomers of telegraphic time distribution from their observatories, charging annual fees for their regional services. They joined new companies based on burgeoning patented electro-mechanical devices such as clock self-regulators and self-winders. These were for use not only by railroads but in other contexts where synchronized bell-ringing and time-keeping was important, such as schools, fire-alarm systems, government agencies, as well as, traditionally, local jewelers.

Prominent astronomers on interlocking boards of time-system companies pleaded for support of “capital-S” science via time-selling when they lobbied in Congress for preferences over competitors. Nevertheless, Western Union’s expanding network plus free time signals from the Naval Observatory in Washington eventually outgrew and underpriced the systems centered on regional observatories, which gradually went out of the time-distribution business.

It is an intriguing episode, but unfortunately Bartky provides, in 207 pages, more details—who did, wrote, or said what; with, for, or to whom—than analysis or insight into a sustained theme, such as the market supply of public goods, the mutual stimulation of science, business, and technology, or the dynamics of national expansion and integration. Fortunately, a further hundred pages of notes, bibliography, and index supply abundant leads for possible broader-based research next time.

CAROLYN C. COOPER, *Yale University*

It's in the Cards: Consumer Credit and the American Experience. By Lloyd Klein. Westport, CT: Praeger Publishers. 1999. Pp. xii, 155. \$55.00.

It is not every day that I am asked to review a book that discusses telephone sex.

Lloyd Klein, a sociologist at Medgar Evers College, CUNY, provides a sociological interpretation of the twentieth-century rise of consumer credit. For historians seeking a

cultural analysis of American consumer debt, Klein's book offers not only his analysis but also an overview of the existing literature on the subject. Those seeking an economic analysis of American consumer debt will need to look elsewhere.

The language of sociology is unfamiliar to most economists. Klein writes, "This study considers consumer credit within a framework of creating social control through implicit and explicit controls on the consumption of material and experiential social products" (p. 2). Klein offers, that is, an interpretation of consumer credit that asserts that individuals within society are being controlled and manipulated by those offering consumer credit. Our desire for access to credit is manipulated through advertising as is our desire to buy household items on credit. Our desire to use credit to indulge in visits to Disney World is created through advertising. "Historical, ideological, and technological developments were instrumental in paving the way for the dissemination of consumer culture" (p. 3).

The book is divided into seven chapters. Chapter 1 provides the overview of Klein's thesis that consumer credit can be interpreted as a social control mechanism. Chapter 2, "Advent of Post-Fordist Cultural Developments," looks at the development of consumerism in the United States as production became increasingly focused on consumer goods and services rather than on industrial goods, and its connection to consumer credit. Chapter 3 focuses particularly upon the service sector by examining the relationship between credit and "the experiential realm" of vacation and leisure: "Credit card utilization facilitates leisure and the acquisition of cultural capital . . . [T]he images and experiences offered in the travel experience encompass a staged authenticity transferring sightseeing into a cultural pursuit" (p. 39). Chapter 4 focuses on the advertising industry and its marketing of consumer credit. Chapter 5 examines the retail sector: the advent of the department store, the development of "mall culture," the more recent rise of discount warehouse stores, and the Home Shopping Network. Klein concludes, "Economic resources (such as credit cards) permit the consumption of cultural artifacts in the form of symbolic capital (or commodities). Acquisition of cultural objects implies the achievement of particular social statuses" (p. 118). Chapter 6 considers changes in bankruptcy laws and incidence of bankruptcy. Change in attitudes toward the propriety of indebtedness and more recently, of declaring bankruptcy, are essential to the expansion of consumer credit. The concluding chapter offers conclusions and implications.

As an economist and a specialist in the history of consumer credit, I find Klein's book disappointing. I readily admit, however, that Klein no doubt found my book on consumer credit (*Buy Now, Pay Later*. Chapel Hill: University of North Carolina Press, 1991) equally disappointing. He includes no data, no testable hypotheses. I included no cultural analysis, no discussion of consumerism or consumer culture. What we need, I think, is a history of consumer credit that covers all the bases.

Oh, and the phone sex? It "is a perfect example of postmodern experience dependent on cultural acceptance and technological advances."

MARTHA L. OLNEY, *University of California, Berkeley*

Coast to Coast by Automobile: The Pioneering Trips, 1899–1908. By Curt McConnell. Stanford, CA: Stanford University Press, 2000. Pp. xiv, 349. \$45.00.

Coast to Coast by Automobile is a detailed account of eight early efforts to drive across the North American continent. The first two expeditions, undertaken in 1899 and 1901, met with failure; but from 1903 to 1908, at least six groups successfully crossed from west to east. Dr. H. Nelson Jackson completed the first transcontinental journey on 26 July 1903,

arriving in New York City in a stock 1903 Winton touring car approximately 63.5 days after he and his mechanic Sewall K. Crocker left San Francisco. Chapter 2 reports on the Jackson-Crocker venture, and subsequent chapters describe how later groups using teams of drivers managed to reduce total travel time to a little more than 15 days by 1906 and to five days by 1916.

Where previous accounts of these events have drawn upon reports in national automotive journals such as *Horseless Age* and *Automobile*, McConnell's narrative is based upon articles published in dozens of local newspapers from the cities through which the transcontinental travelers passed. These multiple sources allow McConnell to clear up contradictions and inconsistencies that have plagued previous histories of these ventures, and in this sense, scholars may accept McConnell's factual accounts as definitive. The volume is handsomely produced and extensively illustrated, containing numerous photographs of dusty, road-weary travelers extricating their vehicles from ditches, streams, and flooded roadbeds. Although minutia on the ultimate fate of actual vehicles and the proper spelling of various people's names could have been relegated to footnotes and appendices, McConnell's writing style suits the historical material well, and some of the specific incidents he reports are very entertaining. Inexplicably for a travelogue, *Coast to Coast* does not contain a map of the various routes followed by the transcontinental motorists. Nevertheless, the punch line is clear: Cross-country touring was not for the faint of heart. Why, then, did early motorists bother to try to drive across the continent? McConnell's narrative offers several possible explanations.

First, because driving across the country was the most difficult test that could be imagined for a new transportation technology, long-distance touring "on a grand scale" (p. 3), it was hoped, would validate the capabilities of early American automobiles. Such concerns were especially important during the early years of the industry, as domestic vehicle makers sought to demonstrate that the quality of American cars was equal to that of more-established European manufacturers; successful transcontinental touring was thus "an object lesson in the character of American cars" (p. 92). To this end, individual manufacturers promoted their brands on the basis of touring success, and many sponsored long-distance runs with such aims clearly in mind. McConnell thoroughly documents the extent of corporate involvement in each of the various crossings and reveals how such involvement shaded reporting about mechanical reliability. Following the success of the independent Jackson-Crocker venture in 1903, cross-country travel was rapidly transformed from "an experiment designed to test the capabilities of the automobile—with results generally reported openly and honestly to anyone interested in them—to a promotional and advertising stunt, where any weaknesses of the machine were de-emphasized or masked" (p. 223).

Early cross-country motorists also sought to evaluate and encourage the development of infrastructure to support the continued expansion of the automobile system. Here, the "real contest" was "between the enduring quality of the American-built machinery and the destructive energy of the awful American highways" (p. 28), and *Coast to Coast* exemplifies the range of interactions between road and vehicle. Journalist Charles B. Shanks, who accompanied Alexander Winton on his ill-fated expedition in 1901, concluded "neither was the failure due to roads. The utter *absence* of roads was the direct and only cause" (p. 51). Indeed, the successful motorists soon adopted techniques that allowed them to bring their roads with them. The Jackson-Crocker team placed ten-yard-long canvas strips over particularly slippery and sandy patches, while later in 1903, Lester Whitman and Eugene Hammond wrapped the tires of their Olds runabout with cotton padding—so-called sand tires (p. 158)—in order to distribute the vehicle weight over a greater surface area. At the same time, the pioneers also took advantage of existing systems. Each of the successful transcontinental motorists, for instance, used railroad infrastructure: regularly-spaced

section houses doubled in extremis as travel inns and restaurants, and occasionally motorists would simply drive on the rail bed rather than fight through poorly maintained horse trails. Obtaining gasoline also proved challenging: Although the motorists added supplementary gas tanks to their vehicles and used the railroad to pre-position additional supplies, jobbers in remote locales could still extract up to triple the market price for gasoline, an indirect measure of the poor reach of prevailing distribution systems. Even as the drivers developed innovative transport technology, they continued to depend on complementary pieces of the transport infrastructure.

Neither technology validation and public relations nor infrastructure development can fully explain the urge to drive from coast to coast. Although McConnell unquestioningly accepts the traditional answer to the children's joke, "Why did the chicken cross the road?," not every motorist wanted "to get to the other side," and McConnell makes no attempt to plumb the differences that set his transcontinental motorists apart. Biographical sketches of the drivers and other participants are surprisingly thin given the richness of McConnell's sources and the evident importance of character to accomplishing the tasks in question. The quenching of this thirst for novelty also explains a peculiar weakness of *Coast to Coast*. As the journeys become more mundane, the narrative becomes less interesting and the outstanding issues more arcane and antiquarian. By 1910 the national media had lost interest in reporting on cross-country runs; unfortunately, by the end of *Coast to Coast* so too may the reader. Ultimately, McConnell claims that the crossings "served to underscore the emerging notion that the automobile, despite occasional breakdowns, could go virtually anywhere (p. 96)." It would be more accurate, however, to say that the journeys themselves, contemporary reporting on the trips, and even McConnell's year 2000 accounts of these events are the bricks out of which that notion was constructed and maintained.

From a practical perspective, readers of this JOURNAL may wish to use *Coast to Coast* to teach about the early history of the automobile and the emergence of new technology more generally. One or two chapters from the book could be paired with traditional readings on the history of auto manufacturing to give students a palpable feeling for what it was like to be a motorist in the early days of the industry. In this respect, McConnell's transcontinental pioneers differed from the typical early driver in degree, not in kind.

DAVID A. KIRSCH, *University of California, Los Angeles*

My Sisters Telegraphic. By Thomas C. Jepsen. Athens: Ohio University Press, 2000. Pp. x, 231. \$49.96, cloth; \$21.95, paper.

In what he terms a pursuit of "unwritten history" (p. 197), Thomas Jepsen has penned a social history of women in telegraphy during the nineteenth and early twentieth centuries. Jepsen is well versed in the technology of the industry and has written several articles on women telegraphers. Here in seven chapters he describes the extent of female employment, both in the United States and other countries, and then ranges widely over a host of issues. These include wages, working conditions, personal characteristics of women telegraphers, social class, ethnicity, mechanization, love in the office, and even women telegraphers in literature and the cinema. There is also a considerable discussion of the role women played in the many, mostly unsuccessful attempts of telegraphers to unionize. The book is based on a wide reading of primary sources including the Western Union Archives, as well as much secondary literature.

Readers looking for a systematic analysis of the labor-market experience of women telegraphers will be disappointed. The book is not informed by economic theory, and

although the Western Union Collection contains payroll data for one office from 1861 through 1879, Jepsen makes little use of them. Hence, the book is likely to appeal to social and labor historians more than to readers of this JOURNAL. Still, the discussion contains much suggestive information on women telegraphers' work, confirming the discussion by Edwin Gabler (*The American Telegrapher, A Social History, 1860–1900*. New Brunswick, NJ: Rutgers University Press, 1988, chapter 4.)

Jepsen argues that as a new occupation telegraphy was gender-neutral and therefore open to women but economists may be more persuaded by Ezra Cornell's observation that they were "abundantly qualified to do the business better than any boy or man that we can afford to pay" (p. 4). By 1910 about 12 percent of telegraphers were women, and as the teletype reduced skill requirements the figure rose to 21 percent by 1920. Jepsen portrays women telegraphers as path breakers; workers whose characteristics and labor-market experiences differentiate them from most nineteenth-century women workers. While this is partly true—telegraphy, for example, was skilled labor, and these women seem to have been quite mobile—in many ways female telegraphers' lives were quite typical. Most were single and left work at marriage, and most were "second class" operators—the lowest rung on the skill ladder. A relative few pursued careers in telegraphy, becoming dispatchers in railroad telegraph offices, or office managers or chief operators, and most who did remained single.

Telegraphy was a general skill and as a result, women typically paid for their own training, sometimes by taking courses, sometimes through informal mentoring and sometimes by working as a trainee without pay. Initially offices sometimes erected partitions to segregate operators by sex, but these disappeared by the late nineteenth century. Nor was there evidence of firm-level segregation. Telegraphy paid better than being a seamstress, but women typically earned less than men. The author characterizes this as wage discrimination, although he admits that it cannot be disentangled from the effects of experience. Companies may also have used wages to motivate males who were expected to stay longer, for Jepsen informs us that "starting pay was about the same for men and women, but men moved up more rapidly" (p. 62). He also argues that discrimination was more common in Europe at state-run companies than in the United States. Yet rates of absenteeism were far higher for female than male telegraphers in Europe.

All in all, this book offers much of interest to social and labor historians, while historically minded economists may find it a useful source for students' papers and information to liven up lectures.

MARK ALDRICH, *Smith College*

David Brian Robertson. *Capital, Labor, and State: The Battle for American Labor Markets from the Civil War to the New Deal*. Lanham, MD: Rowman & Littlefield, 2000. Pp. xxii, 297. \$22.95, paper.

American employers today enjoy considerably greater latitude in the labor market than do employers in other industrialized economies. Laws protecting unions are weaker, employers can more easily hire and fire workers, minimum-wage laws are less binding, the government plays a smaller role in managing the labor market through public employment offices, and work and unemployment insurance programs are smaller and less costly to employers in the United States than elsewhere. In this book David Brian Robertson, Associate Professor of Political Science at the University of Missouri, St. Louis, offers an explanation for the unique pattern of labor-market governance that has emerged in the United States.

The distinctive features of American labor markets first emerged, Robertson argues,

around 1900. Prior to this date similar efforts to regulate labor markets were undertaken in both Europe and the United States. Indeed, Americans were early leaders in the passage of laws requiring factory inspection, minimum wages, and maximum hours, establishing public labor-statistics bureaus and public labor exchanges, and creating government mediation and conciliation services (though most of this legislation was passed at the state not federal level). Moreover, unionization rates in the United States were comparable to those in Germany and close to those in Britain. After 1900, however, European labor markets began to become more regulated, and union membership started to increase.

According to Robertson, the key to understanding the unique history of American labor-market governance lies in the country's "singular policymaking institutions [which] forced labor leaders, employers, reformers, and politicians to invent distinctive strategies for labor market control" (p. 258). In particular, he identifies three aspects of the nation's policy making institutions that were especially important. First, the federal structure of political power made it impossible for the national government to enact uniform labor-market rules or regulations. Although state governments possessed wider authority to enact prolabor legislation, they were constrained by the strictures of interstate competition. If any one state adopted especially strong prolabor laws or regulation it risked substantially disadvantaging its employers. Second is the division of governmental power between legislative, executive, and judicial branches. Even when state legislatures passed prolabor legislation, the intent of these laws was often stymied by ineffective enforcement. In other cases court decisions effectively voided these laws. Third, the antimonopoly tradition in the American legal system both inhibited the growth of organized labor and created a business climate especially hostile to unionization. Anticollusion laws directly affected unions, because they could be used to prosecute them as conspiracies in restraint of trade. At the same time antitrust laws encouraged virulent and effective anti-unionism among American employers. In some industries prohibitions on cartels encouraged the emergence of big businesses that could effectively combat union organizing. In others antitrust laws blocked efforts by small businesses to collude that might have made them more receptive to reaching industry-wide agreements with organized labor.

Drawing on an extensive analysis of public and private documents as well as existing histories, Robertson shows how American policymaking institutions shaped the emerging "policy strategies" followed by union leaders, employers, government officials, and reformers. Although unions initially pursued government involvement in labor-market regulation and management to achieve their goals, their experiences in the late nineteenth century were almost uniformly discouraging. The failure of eight-hour laws and protective legislation limiting the use of women and children discouraged union leaders from relying on government regulation to achieve their goals. Their frustration was compounded by the limited benefits that they derived from the establishment of state labor-statistics bureaus, and the passage of laws legalizing their incorporation. The lesson that Samuel Gompers and other labor leaders drew from these experiences was that the American Federation of Labor (AFL) should pursue a strategy largely independent of government, relying on its control of labor supply to bargain with employers. The AFL did not, however, entirely abandon government regulation, but advocated such efforts only in those parts of the labor market that were difficult to organize, such as industries relying on child and female labor.

After 1900 the AFL's efforts provoked a strong open-shop counterattack by American employers, which resulted in the defeat of unionization efforts among miners and machinists—groups that might have advocated an industrial rather than craft-based approach to labor organization. These defeats left the AFL dominated by unions in the construction trades, printing, and railroads, a development that hardened the AFL's commitment to craft-based unionism and a narrow strategy based on winning benefits through the use of

union bargaining power. Although the AFL eventually came to support workers' compensation laws it opposed efforts to introduce public health or unemployment insurance schemes, resulting in a widening gap between organized labor and reformers. Finding themselves at odds with organized labor reform advocates allied themselves with employers, coming increasingly to advocate employer-based solutions to the provision of social insurance and labor-market regulation that gave employers much greater control over the market.

Although the economic crisis of the Great Depression provided the opportunity for a substantial expansion of government regulation and management of the labor market, the precedents of the past three decades encouraged the adoption of policies that preserved a much greater role for employers. Similarly, although unions finally won the right to organize and bargain collectively, the rules under which representation elections were conducted promoted a narrow, establishment-based system of representation that limited the scope of union concerns.

The argument that Robertson advances here for the importance of political institutions as a determining force in the development of American labor markets is provocative and should be of interest to many economic historians. Robertson is an effective advocate of this interpretation, and goes a long way toward documenting the way in which American policymaking institutions shaped this country's labor-market policies. His case would be stronger, however, if he could show a similar connection between institutions and labor-market policies in at least a few representative European countries.

JOSHUA ROSENBLOOM, *University of Kansas*

Banking Panics of the Gilded Age. By Elmus Wicker. New York: Cambridge University Press, 2000. Pp. xvii, 160. \$49.95.

Feeling panicky? Elmus Wicker might be able to explain why. In 1996, he authored *The Banking Panics of the Great Depression* (New York: Cambridge University Press); now, pushing the frontiers of our knowledge forward (and chronologically backward!), he has tackled the late nineteenth century in the United States. In this new book Wicker asks an interesting and important question: Were banking panics different under the National Banking System than they were under the Federal Reserve System?

To answer this question Wicker collects data from a wide array of government reports and financial periodicals, including one data-rich *Bradstreet's* volume from 1893. Using these data and accounts from contemporary newspapers, he constructs a blow-by-blow narrative of financial upset in the post-Civil War period, accompanied by detailed tables of bank statistics. Wicker's first conclusion is that the Gilded Age was a relatively stable one: whereas there were five major panics during the early years of the Great Depression, he can identify only three panics (1873, 1893, and 1907) and two "incipient" panics (1884 and 1890) during the entire National Banking era. He thus discredits the view that the structure of national banking in the postbellum United States was inherently unstable.

Wicker then quickly zeroes in on what he believes to be the real culprit in these panics: the New York Clearing House (NYCH). According to the author, this private organization "had the knowledge, the power, and the instruments to forestall banking panics" (p. 15). We are thus presented with a very simple counterfactual: if the NYCH had acted like a central bank, the panics of the Gilded Age could have been dampened or even prevented. Wicker's test of that proposition lies in an investigation of the "incipient" panics of 1884 and 1890. The logic goes something like this: In 1884 and in 1890 financial instability did

not grow into major panics. In 1884 and in 1890 the NYCH authorized loan certificates to troubled banks and did not suspend cash payments. Ergo, the reason there were no panics in 1884 and in 1890 was the action of the NYCH. It follows, then, that the panics of 1873, 1893, and 1907 were wide and deep because the NYCH did *not* act to stop them.

Of course, this conclusion relies on a fair bit of *ceteris paribus*; all economic historians have to deal with that. Even more worrying, though, are the two questions begged by this conclusion. First, if, as Wicker suggests, the NYCH *did* take on the role of financial stabilizer in 1884 and in 1890, why did it fail to assume the role in later years? His answer is somewhat unsatisfying: "Its responsibility for preventing banking panics, well understood before 1873, had by now been mostly forgotten" (p. 83). While it is true that institutions may be composed more of men than of rules, I doubt that any *profitable* strategy would have disappeared from the collective consciousness of the NYCH. Wicker does note that in 1873, NYCH member George S. Coe headed a committee that "made the case that banking panics could be averted if the NYCH exercised bold leadership and was fully prepared to use its power and instruments to achieve that objective" (p. 13). But how did Coe get so smart so early? And why were not the NYCH members who succeeded him as smart?

The second question is philosophically much broader: Why should the clearinghouse even have had the responsibility to act for the financial good of the public? Wicker criticizes the NYCH for "an unduly narrow conception of [its] responsibilities" (p. 14), and for lacking "a resolute and persuasive leadership to surmount the natural tendency of the member banks to give rent-seeking a higher priority than the pursuit of the public interest" (p. 115). I, on the other hand, would have been surprised if the NYCH *had* acted in the public interest, particularly in light of the fact that a governmental body—the Federal Reserve—itself shunned the role of stabilizer and lender-of-last-resort in the 1930s.

None of these criticisms diminish the important contribution this book makes. Elmus Wicker has done the difficult work of mapping out the landscape of American panics. Although the text is a bit short on answers to "how" and "why," it is replete with information on "what," "where," and "when," and this will be of enormous value to other researchers. In addition, the book raises a number of fascinating conjectures, such as the one that Wicker saves for the very last page: "The NYCH bungled a once-for-all opportunity for effective voluntary collective action to forestall banking panics *and thereby ward off the establishment of a government central bank*" (p. 147; my emphasis). Some political economist could make a living off that one idea.

KERRY ODELL, *Scripps College, Claremont*

The Way It Worked and Why It Won't: Structural Change and the Slowdown of U.S. Economic Growth. By Gordon C. Bjork. Westport, CT: Praeger, 1999. Pp. xviii, 299. \$69.50, cloth; \$27.95, paper.

At a time when serious questions in economic history are often delivered by publishers into the hands of either misinformed polemicists (such as Dinesh D'Souza in his remarkably misleading *The Virtue of Prosperity: Finding Values in an Age of Techno-Affluence*) or facile pundits (such as Jeffrey Madrick in his thoughtful yet under-researched *The End of Affluence*), it is a special pleasure to encounter Gordon Bjork's *The Way It Worked and Why It Won't*. Here is a work, skillfully rendered by the Lovelace Professor of Economics of Claremont McKenna College and the Drucker School of Management (Claremont Graduate School), that carefully assays the recent economic

history of the United States with a view toward understanding the productivity slowdown that has obtained since the 1970s.

Bjork's specific agenda is to analyze three interrelated features of recent economic experience in this country: the deceleration in the growth-rate of gross domestic product (GDP) that has been tied to apparent changes in workforce productivity; the decline in per capita GDP growth rates linked with changes in the labor-force participation rate; and the reduction in disposable income for average working Americans that has been another characteristic of recent economic performance. Studiously avoiding the posture of a pessimist or a cynic, Bjork takes great pains to emphasize that, far from being symptomatic of some deep-seated economic pathology, these features of the contemporary national economy are the signature of a robust, mature, significantly advanced industrialized state. In this respect, his book deploys an impressive historical sensibility, one that is tied to a resolute determination to utilize neoclassical growth theory, and no small amount of empirical data, to make a most compelling case. For all these reasons, the volume is a welcome and important contribution that both colleagues and students will find quite enlightening and useful.

It is testimony to the lucid quality of his prose that Bjork's core arguments may be straightforwardly summarized. He claims that structural changes in the size of industrial sectors and in labor-force participation rates have played major rôles in lowering the rate of growth of per-capita real incomes. On the one side, as final demand has shifted, in recent decades, from traditional manufacturing markets to services, the consequences for productivity change have been unfortunate. The notoriously slower gains in productivity that are characteristic of the services sector are the major factor in Bjork's mind. On the other, changes in the age-composition of the American population and in the labor-force participation rate of women that once combined to generate high GDP growth rates (through the concentration of people in the "high-employment" age brackets—ages 16 to 65—and a "one-time" dramatic shift of women from unremunerated household production to wage labor employment) have now become ever-more-attenuated in their growth-enhancing potential.

Such broadly based historical conceptions allow Bjork to address directly the more recent slowdown in productivity growth. Over a century ago, in his view, American growth accelerated as per-worker output increased through technical change and as larger segments of the labor force moved out of agriculture into manufacturing. Similarly, demographic change (driven both by internal growth and large waves of immigration) increased the labor force both extensively (in the "prime age" groups) and intensively (as women moved from home to work in increasing proportions). This is, in broad outline, "the way it worked" for the American economy through many decades of the twentieth century. For Bjork, "why it won't work" further has simply to do with the fact that the large, intramarginal gains to be won from the structural shifts in question have now been played out. Productivity gains from intersectoral shifts in employment and from changes in the sex composition of the labor force can no longer be as large as they once were. At the margin, the "bang per buck" is necessarily smaller. This is, to a large extent, a familiar story for economists trained in, what we might call, the Abramovitz-Kuznets tradition; it is the essence of the view that leaders eventually slow down while stragglers catch up in the ever-changing terrain of the global economy.

In what is a consistently clear and often compelling exposition, Bjork makes his points well. What weaknesses emerge within the argument are most clearly discerned in his discussions of the reasons for changes in labor-force participation, the impact of immigration and human-capital formation on macroeconomic performance, and in the treatment of seemingly "exogenous" forces. Noting what he terms the "popular explanation" for increasing female labor-force participation, that married women have chosen to work in increasing

proportions to maintain household consumption standards, Bjork somewhat woodenly substitutes the notion that women have, over time, simply expressed “a preference for more income.” While neither of these viewpoints are powerfully objectionable, they are nonetheless unpersuasive and strikingly clumsy. There is now an impressive and well-researched literature on the historical trends accounting for changes in the position of women in American society (in the labor force, in politics, in cultural activities, and so forth) that portrays a far more complicated and interesting reality, one not well understood by resort to simple neoclassical postulates. The effects of changing conceptions of gender rôles, the separation of sexuality from reproduction, the impact of advertising and consumer-credit practices are just a few of the mechanisms by which the economic activities of women have changed in modern America. Although it is quite understandable that Bjork would not spend much time on these matters in a volume devoted to a discussion of movements in macroeconomic aggregates, some acknowledgment of this literature and its importance for economists would have been both appropriate and informative. Even more to the point, it would have been a most effective reminder to economists that, when dealing with complicated issues of demographic and social change, the rigorous yet often crude first-approximations afforded by mainstream price theory can be, and often are, misleading.

On the matter of immigration Bjork unfortunately indulges in a variety of generalizations that ill serve the broader, and far more interesting, themes of his book. He claims that recent waves of immigration, during the 1980s and 1990s, “ha[ve] been totally different than . . . a century ago” (p. 236). Lower human-capital endowments have made these new arrivals less productive, and potentially more a drain (in the form of welfare dependency), than immigrants in the late nineteenth and early twentieth centuries. As a consequence, “structural” changes in immigration demography have also contributed to the slowdown in the American productivity growth rate in more recent years. This is a surprisingly unpersuasive portion of Bjork’s work, one that indulges in assertions not only about the “quality” of recent immigrant populations that overlooks their productivity-enhancing contributions, but also concerning the early-twentieth-century experience in ways that belie the record. Wave after wave of eastern and southern European immigrants beginning roughly a century ago ultimately brought millions of individuals to the nation’s borders who were (in English) functionally illiterate, bereft of industrial “time-discipline” and requisite skills, and ignorant of (and often quite hostile to) “modernized” economic practices that, more often than not, violated deeply held social convictions (for example, about the Sabbath or about the public mingling of men and women). In short, Bjork fails to make clear why and how the recent hostility towards immigration in the United States should not be subjected to the criticism that it violates not only notions of social justice and humanity but also contradicts, in the American case, historical experience itself.

Similar weaknesses emerge in Bjork’s treatment of education and the civil justice system, two venues he singles out for a recent tendency to drain more resources away from productive economic activity into sometimes wasteful or inefficient uses. Although Bjork manages to deploy several interesting, even arresting insights on these matters, in the end he relies on a received wisdom that is quite unsatisfying; to wit, that rising expenditures per student in the public education sector have failed to yield impressive results, suggesting that the “monopoly of public education” should be challenged, and that excessive tort settlements have driven transactions costs up to levels that threaten continued growth and productivity gains. In both cases, Bjork’s seeming reliance on arguments drawn from anecdotal observations by the press and other contemporary observers obscures more compelling issues—such as the overwhelming social and regional inequality in the distribution of public educational resources (usually refracted along racial lines), and the tendency of proponents of “tort reform” to seek less a reduction in enterprise risk than a

curtailment of individuals' rights to pursue legal remedies for demonstrated injury in the courts—that clearly must be addressed before a firm position on these matters may be taken.

Finally, there is the matter of “exogeneity” as a whole. Bjork has little to say, although he acknowledges the decisive impact the military-industrial “complex” has had on the economic history of the United States since World War II. All honor of course should be given to his determination to focus his discussion on the mechanics of macroeconomic growth and of long-term structural change in advanced industrialized states. But the interconnection of American enterprise, both domestic and export oriented, with the dramatic elaboration of an enormous military establishment since the defeat of the Axis Powers has been both significant and, most interestingly in light of the recent “end” of the Cold War itself, enduring. To pretend that contemporary growth performance may be understood in abstraction from this reality, to the point of treating the interdependence of the American economy with the instrumentalities of the state as “exogenous” and intermittent, is entirely unwarranted. If economists will not take up this matter in substantive detail, determined to show the endogenous nature of such activities and spending processes, then who will? Such criticisms and speculations as these, far from detracting from the quality and impact of Bjork's book, only show the thought-provoking and stimulating nature of the discussion within it.

MICHAEL A. BERNSTEIN, *University of California-San Diego*

The Kennedy-Johnson Tax Cut: A Revisionist History. By Martin F. J. Prachowny. Cheltenham, UK, Northampton, MA: Edward Elgar, 2000. Pp. ix, 227. \$80.00.

Was the Kennedy-Johnson tax cut and the tax surcharge of 1968 a triumph of “new economics” and “fine tuning” as the architects of these policies claimed? Not exactly, according to Martin Prachowny. He concludes that the chairmen of the Council of Economic Advisers (CEA) excessively trumpeted their self-described stellar performance and this self-serving propaganda led to the demise of activist stabilization policy.

The roles of Walter Heller, Gardner Ackley, and Arthur Okun, who were chairmen of the CEA during the Kennedy-Johnson administrations, are center stage in this macroeconomic history. The chairmen's perspectives, analyses, and questionable claims are gleaned from their publications and private papers, and standard public documents. There is a particular focus on the macroeconomic analysis and models that were used to justify tax-policy changes. In spite of Walter Heller's claims that the 1964 tax cut was an application based on postwar economics textbooks, Prachowny says “. . . it is virtually impossible to connect the numerical predictions made in 1963 with the outcomes in 1964–5” (p. 4). The lack of specificity is troubling because sufficiently refined macroeconomic models were available at that time.

With the appointment of Walter Heller as Chairman, the CEA's policy orientation changed from countercyclical economics to the more relevant Keynesian-based notion of output gaps. The development of output gaps was a superb innovation because it quantified the social cost of idle resources. However, the CEA's application of the concept in the form of Okun's Law was flawed by measurement problems. To convince President Kennedy of the need for a tax cut, Walter Heller took considerable license with estimates of potential output, and as a consequence he picked the largest possible output gap from the range of estimates (p. 46).

The CEA toyed with various macroeconomic approaches to connect tax changes with aggregate demand and the output gap. There are references to a Keynesian multiplier, a

gross accumulator, and a multiplier-accelerator relationship. By not committing themselves to an explicit economic model, Heller and Ackley were unduly vague and inconsistent in their statements about the numerical values for the multiplier/accumulator. According to Prachowny, this is a fundamental failure of application because the CEA should have incorporated postwar advances in consumption theory (permanent income), recognized the instability of estimated values of the marginal propensities to consume and save; and utilized a far more serviceable Keynesian-based model of analysis. If the CEA had used an explicit IS-LM model it could “. . . have shown that the required tax cut was not a single figure, but range of values, which in turn depended on assumptions about parameter sizes” (p. 66).

Prachowny simulates an IS-LM-AS model to ascertain the impact of the 1964 tax cut on macroeconomic performance. He finds that the tax cut expanded what would have been a stagnant economy and it nearly eliminated the output gap by 1966. All-in-all, the 1964 tax cut was a “stunning success.” However, not all is well that ends well because the CEA’s predictions about macroeconomic performance were not close to the actual outcomes. And the outcomes reflected a number of lucky circumstances and thus “. . . Walter Heller’s claims that the results followed the Council’s predictions closely are exorbitant” (p. 94).

Although the tax cut had a favorable impact on the economy between 1964 and 1966, the overall evaluation of activist fiscal policy must include a test of symmetry. If the elimination of output gaps is the primary target, it is reasonable to ask whether fiscal policy responded with equal vigor to negative output gaps that occurred after 1966. The CEA’s analysis and debate that led to the 1968 tax surcharge reveals a lack of symmetry in its response to unemployment and inflation. The CEA was not nearly as persuasive in recommending a tax increase to fight inflation as it had been in promoting a tax cut to eliminate an output gap. The CEA appeared to be less certain of its own position and it waffled on the issue of tax increases. These problems were compounded when President Johnson early on (1966) was worried about the impact of a tax increase on the public’s continued support for the war in Vietnam and Great Society programs. One of the consequences is that the CEA inappropriately increased the estimate of potential GNP and then it relied on a model of monopoly pricing in strategic markets to explain inflation. Consequently, the CEA focused on Keynesian demand-management policy to achieve full employment and wage-price guideposts to promote price stability.

Prachowny achieves his primary objective which is a thorough qualitative and quantitative analysis of the Kennedy-Johnson fiscal experiments. The simulation of an IS-LM model to ascertain the impact of the 1964 tax cut is well done. And abundant evidence is presented to show that the CEA failed to justify tax-policy changes with explicit models that yielded precise numerical predictions in spite of claims to the contrary by chairmen, especially Walter Heller.

There are several instances where Prachowny’s interpretations are troubling. For example, he states that the CEA’s extravagant claims and lack of symmetry in dealing with positive and negative output gaps were the primary reasons for the demise of activist stabilization policy that “would have been used more effectively in the recessions of 1982 and 1990” (p. 11). This is a far-reaching statement that requires an analysis of theoretical, empirical, and political developments after 1968, and an evaluation of the likelihood that two Republican presidents would have selected activist policy advisers and policies. Another illustration occurs when Prachowny criticizes CEA chairmen for failing to utilize explicit models that yield precise numerical predictions. The criticism occurs because he prefers economists as policy advisers to be apolitical promoters of economic efficiency. On the other hand, he is well aware of advisers’ other mandates such as educating and persuading the public and politicians (p. 40), describing the impact of policy actions with certainty

when in fact they are shrouded with uncertainty (p. 68), and noting that political requirements may trump economic reasoning (p. 77). For these mandates, vagueness and imprecision in models and numerical predictions may reflect advisers' conscious exercise of the politics of economics.

ROBERT R. KELLER, *Colorado State University*

GENERAL AND MISCELLANEOUS

Research in the History of Economic Thought and Methodology, Archival Supplement 8, 1999, Selig Perlman and Wisconsin Institutionalism. Edited by Warren J. Samuels. Stamford, CT: JAI Press, 1999. Pp. vi, 262. \$82.50.

This book is one of a series in the history of economic thought published by JAI Press. It may be of more interest to historians of economic thought than to economic historians. Nevertheless, because history has shaped what economists have said, especially the American Institutionalists, there is much history in this volume about Selig Perlman.

Selig Perlman was born in Poland in 1888 and attended school there until 1906, and in Italy in 1906 and 1907. He came to the United States in 1908, and to the University of Wisconsin in Madison in 1909, where he earned an A.B. degree in 1910 and a Ph.D. in 1915. He became a naturalized citizen in 1913, worked as a special investigator for the U.S. Commission on Industrial Relations from 1913 to 1915, returned to Madison to do research in 1916, and joined the faculty there in 1919. Perlman was a student of John R. Commons, who had been a student of Thorstein Veblen. Perlman, who spent his career at Madison, was principally an American labor historian (according to the editor, he was the foremost interpreter of this field next to Commons). He also taught courses on capitalism and socialism (perhaps what we would call comparative economic systems today, but with a significant historical dimension). Perlman died at the age of 70 in 1959. This biographical information about Perlman was not included in the editor's introduction to the volume, but it would have been helpful to the reader if it had been.

This volume contains previously unpublished student notes from two of Perlman's classes; unpublished chapters that were intended to be a revision and updating to the 1950s of his book, *A History of Trade Unionism in the United States*, originally published in 1922; correspondence between Commons and Perlman, 1928–1945; and three miscellaneous documents and correspondence of Perlman's. Part 1 (over half the book) consists of two sets class notes from Perlman's class in capitalism and socialism given and taken one and one-half years apart (one set is by Warren Samuels, taken in the spring of 1955, and the other by Allan Schmid, taken in the fall of 1956); and notes from Perlman's class in American labor history (taken by Warren Samuels in the fall of 1956). These are student notes, and not transcripts of Perlman's lectures. As such, they are not a verbatim record of the lectures, but they nevertheless represent Perlman's ideas in these areas.

Part 2 (about a third of the volume) consists of Perlman's additional six chapters for his *History of Trade Unionism in the United States*, which concluded with the end of World War I. He never published a revised edition, but he disseminated to students these chapters with a view to updating the *History* to the 1950s. These chapters continue the Commons–Perlman theory of the American labor movement in which U.S. unions are, relative to the European experience, “exceptional.” “American exceptionalism resides in the non-revolutionary nature of U.S. unions, their acceptance of private property and private enterprise in a business system, and their efforts to enhance the social and, especially, economic

position of working people" (Samuels's "Introduction," p. 154). Accordingly, political labor parties have not emerged in the United States. Their political activity, like that of other economic interest groups, has taken place through existing political parties.

Parts 3 and 4 of the volume are relatively short. Part 3 publishes correspondence between Commons and Perlman. Letters from 1928 were written when Commons was in Washington, DC at the Institute of Economics. Commons retired from the University of Wisconsin in 1933 and moved to Florida. Correspondence (mostly from the early 1940s) is from there (Commons died in 1945). Finally, Part 4 contains reprints of three documents. The first is a sympathetic letter to James Griffiths, Minister of National Insurance in the British government regarding the Labor Party's policies. The second is entitled "My Faith as an American Jew." This was apparently written for a volume intended to collect some one hundred such statements. (The editor was unable to locate such a publication, so it may never have been published.) In this document, Perlman acknowledges the influence of his teachers. His greatest debt is to John R. Commons, but he also mentions Frederick Jackson Turner and Edward A. Ross. The third was a letter to Merle Curti dated 6 May 1950, in which Perlman expressed his displeasure at the Department of History at Madison for not hiring any Jewish members. In the letter Perlman states that for this reason he no longer is willing to serve as a member of committees examining history Ph.D. students.

In summary, this is a highly specialized book, but of interest generally to those concerned with the history of economic thought, and especially to those with an interest in Wisconsin Institutionalism.

JAMES F. SHEPHERD, *Whitman College*

The New Imperial Economy: The British Army and the American Frontier, 1764–1768. By Walter S. Dunn, Jr. Westport, CT: Greenwood, 2001. Pp. viii, 208. \$62.50.

Retired public historian Walter Dunn has given us a good, old-fashioned narrative economic history that describes the importance of the frontier during "a crucial time in American history filled with turmoil that ruptured a once amicable relationship between the thirteen colonies and Great Britain" (p. 1). Dunn is no econometrician, but his work shows a solid understanding of neoclassical economic principles. His writing is sharp and clear, if generally unexciting due to the overwhelming number of detailed examples that he often supplies.

In the book's eight main chapters, Dunn uses words and a handful of tables to describe the British economy, the transatlantic trade, and the mainland colonial economies, with a focus on the economic conditions on the frontier in the 1760s. His description of the colonial economies, which includes summaries of their products, exports, transportation networks, and mercantile activities, is the most detailed. Although those chapters offer little new to those already conversant with the works of John J. McCusker, Russell Menard, and Edwin Perkins, nonspecialists and undergraduates may find them a useful overview of the economic history of British North America in the mid-eighteenth century.

The overview chapters provide the background for the study's major conclusion, that "not the fur trade, but the intercourse among the people to satisfy the needs of the Indians, led to the development of the frontier and the emergence of a new nation" (p. 191). In other words, the frontier "developed" and did so because of trade with the indigents; also, frontier development and economic development helped to lead to the Revolution. Frontier issues clearly played a role in the coming of the American Revolution, but Dunn's exposition, which stops in 1768, is less than satisfying. Additionally, it is not clear that the fron-

tier “developed” at all, and if it did, the role of the Indian trade in that development remains unclear.

Peter Mancall and Thomas Weiss (“Was Economic Growth Likely in Colonial British North America?” This JOURNAL 59, no. 1 [1999]: 17–40) recently argued that Indians were an important variable for explaining what little economic growth (increased real per capita aggregate output) the colonies experienced. Mancall and Weiss, however, posit that the gradual replacement of Indians with more productive Europeans led to the increased efficiency, while Dunn, clearly enamored with Adam Smith’s notion of “the extent of the market,” suggests that frontier (Indian, settler, and Army) consumption fueled colonial growth (pp. 6, 37, 186). The growth stories set forth by Stanley Engerman and Kenneth Sokoloff (“Factor Endowments, Institutions, and Differential Paths of Growth Among New World Economies: A View from Economic Historians of the United States.” In *How Latin America Fell Behind*, edited by Stephen Haber. Stanford, CA: Stanford University Press, 1997) and Richard Sylla (“U.S. Securities Markets and the Banking System, 1790–1840.” *Federal Reserve Bank of St. Louis Review* 80 [1998]: 83–104) are more compelling than Dunn’s narrative.

The second part of Dunn’s conclusion, that structural economic changes helped to cause the Revolution (p. 1), is undoubtedly true, though hardly a new notion. (See, for example, Marc Egnal and Joseph Ernst, “An Economic Interpretation of the American Revolution,” *William & Mary Quarterly* 29 [1972]: 3–32). Reductions in British military spending in America helped to foment the imperial crisis by reducing colonists’ ability “to balance trade between the colonies and Britain” (pp. 187, 6–7, 191). Furthermore, the redeployment of the British army from the frontier to the seaboard in 1768, a successful attempt to reduce imperial defense expenditures, infuriated frontier merchants because it cost them their lucrative frontier provisioning business (pp. 6, 188). In addition, frontier colonists found imperial restrictions on the fur trade irksome (p. 7). Clearly, the changing frontier economic scene helped to lead some colonists to believe that they would be better off without British rule, but Dunn does not help readers to assess the relative importance of those changes.

By far the biggest weakness of this book is that it was published about three decades too late. Dunn, who worked under Merrill Jensen, admits that he conducted much of the research for the book between 1952 and 1970 and that his coverage of recent historiography is limited (p. vii). For instance, Dunn ignores Cathy Matson’s *Merchants and Empire: Trading in Colonial New York* (Baltimore: Johns Hopkins University Press, 1998) and Craig Horle, Joseph S. Foster, and Jeffrey L. Scheib’s pathbreaking *Lawmaking and Legislators in Pennsylvania* series (Philadelphia: University of Pennsylvania Press, 1997). *The New Imperial Economy*, however, is still valuable because of Dunn’s close attention to primary sources, like the papers of important mercantile firms such as Baynton, Wharton, & Morgan and Phyn & Ellice, and the narrative’s solid grounding in economic principles.

ROBERT E. WRIGHT, *University of Virginia*

Insatiable Appetite: The United States and the Ecological Degradation of the Tropical World. By Richard P. Tucker. Berkeley: University of California Press, 2000. Pp. xiii, 551. \$45.

Why *les tristes tropiques* have become the world’s major sump of poverty has been analyzed from various perspectives, producing varying judgments on the complicity of the foreign powers who had “opened up” the tropics to foreign settlement, trade and investment. This volume’s basic theses are that ecological degradation from the expansion of

tropical agricultural and silvicultural exports have been a major source of socioeconomic damage to the tropics and that, especially in the past century, U.S. consumer demand and enterprise have dominated the external forces propelling this adverse dynamic.

Chapters on cane sugar, bananas, coffee, rubber, meat, and tropical-wood products intended to validate the two theses make up the bulk of the volume. The general format is selective narrative history, with the chief selection criterion geographic. For each product the chapter's primary focus is on the tropical regions in which U.S. firms came to be important planters, processors, or export marketers of the product. Indeed the volume according to the preface is intended to fill a remaining gap—the U.S. environmental impact on the tropics—the European impact having been already well covered. Thus the main focus of the chapters is on Central America, the Antilles, Mexico, Hawaii, and the Philippines. Of Africa only Liberia, and of South Asia only Indonesia, get detailed treatment, and only in the chapter on rubber. The partitioning essentially isolates for detailed treatment the tropical regions that had become part of the *de jure* and *de facto* American empire by the beginning of the twentieth century. The chapters therefore also sketch the political aspects of the “opening up” of these regions to U.S. enterprise as well as the socioeconomic and political entities with which they had to interact.

The author has drawn on a wide array of secondary, mostly English-language, literature plus consultations with other historians and ecologists. The chapters and the extensive bibliography thus provide a readable summary of the history and ecological evolution of the respective crops, albeit of uneven quality. The forestry chapter is the most detailed and coherent on ecological matters, whereas the two chapters on sugar do not go much beyond stating and restating that monocropping reduces biodiversity. In cutting its wide swathe through the diverse historical literature some minor errors of fact and emphasis show up. Thus the chapter on ranching badly overemphasizes the importance of the United States in the early evolution of Argentine beef exporting. The British diplomats who toasted Argentina as the jewel of the British Empire were not merely joking.

Where the book is fuzziest is in its normative analysis. Its normative yardstick seems to be neo-Jeffersonian: the self-sustaining society of smallholders pursuing mixed agriculture and silviculture on periodically shifting clearings so as to preserve biodiversity, the regeneration of forests, and their soil and watershed maintenance function. This will not do. Such societies have been very vulnerable to demographic surges, and in our world of six billion people and counting, the yardstick implies mass starvation. But while the book also shows that free trade and corporate agriculture have usually been part of the problem, not the solution, it also describes favorable hybrids. Export cooperatives of small banana farmers in Jamaica, and Hawaiian “Big Five” sugar firms effectively exploiting rent-seeking quotas and technological advances to absorb the higher wages that unionization of their field workers during the New Deal forced on them, demonstrate that loss of biodiversity is not as critical a determinant of tropical poverty and social disintegration as the book's yardstick implies. The contrast documented by the book between the Hawaiian and Philippine sugar experience is also instructive. Both benefited from the pre-1974 U.S. sugar quotas, but the technologically torpid Philippine sugar barons exploited the quota rents by expanding through land grabbing, forcing the dispossessed to subsist by deforesting the mountain slopes. When the United States repealed sugar quotas in 1974, the “Big Five” shifted to pineapples, and the field workers to urban jobs and small businesses; sugar had been a developmental stepping stone. Not so in the Philippines where the sugar regions were left with deepened poverty as well as degraded land. The distribution of land and political power, not ecological factors as such, determined the welfare effects in these hybrid examples.

Criticism of development economists for worshipping GDP growth without regard for the ecological costs of that growth has been leading them to introduce ecological elements into

their welfare assessments. Ecologists need to incorporate growth dynamics into their welfare criteria.

DAVID FELIX, *Washington University in St. Louis*

R&D, Education, and Productivity: A Retrospective. By Zvi Griliches. Cambridge, MA: Harvard University Press, 2000. Pp. ix, 127. \$39.95.

Until his untimely death Zvi Griliches was an important contributor to the measurement and interpretation of productivity growth in the United States and elsewhere. Economists working in this area have been principally concerned with trends in two key measures of productivity: labor productivity, or output per hour, and multifactor or total factor productivity (MFP). Growth in labor productivity is the growth in real output less growth in hours, and multifactor productivity growth is the growth in real output less a weighted average of growth of inputs conventionally measured, with the weights corresponding to shares of factors in national income. The former measure is the most important determinant of growth in our material standard of living as measured by growth in per capita real output. The latter variable, subject to some qualifications, gives us some measure of the impact on economic growth of scientific and technical progress in different periods. The two are closely related, since growth in labor productivity can be decomposed into a portion accounted for by capital deepening (rise in the ratio of capital services per labor hour) and the “residual” contributed by growth in MFP.

This short book, completed shortly before the author died, is a retrospective on what he and we have learned in this area. Based on the Simon Kuznets lectures given at Yale in 1997, it consists of five main chapters, two of which have appeared in slightly altered form elsewhere. In chapter 1, a version of which was published in the *Journal of Economic Literature*, Griliches provides a historical perspective on the origin of the concept of the residual. Chapter 2 covers work in the 1960s and 1970s, some coauthored with Dale Jorgenson, and provides a retrospective on the search for explanations of the large residual identified in midcentury data by Moses Abramovitz, John Kendrick, Robert Solow, and others. Chapter 3 explores the impact of rising educational attainment on productivity, including discussion of how much to be concerned about the association of ability with achieved educational level and the role of the increased public-sector employment of educated personnel in accounting for the weak cross-national association between increases in schooling and productivity growth.

Chapter 4, the most technical, and a revised version of work published elsewhere, examines the conceptual and econometric issues associated with constructing estimates of stocks of R&D capital. One of the most interesting points is that much of the measured depreciation of such capital represents the dissipation of monopoly rents, a private but not a social loss (p. 56). Chapter 4 and chapter 5 consider the use of R&D capital-stock estimates on the input side to reduce the residual, chapter 5 focusing specifically on the role of this factor in accounting for the decline in productivity growth rates. Chapter 6 is a five-page “Reminders for Traveling the Research Road Ahead.”

Reading this retrospective one cannot help but be struck by how much the intellectual terrain in this area has changed with the unfolding of the last 30 years of economic history. In the 1950s Solow and others examined data through midcentury and distinguished between the late nineteenth century, when most real growth could be swept back to growth in inputs as conventionally measured, and the twentieth century, particularly its second quarter, in which there was a very large unexplained “residual.” This was understood by

many as reflecting a secular change involving the greater importance of science-based economic growth in the modern epoch as compared with that which preceded.

The last 30 years of the twentieth century, however, with the exception of the years after 1995, turned out surprisingly to have been very much like the last third of the nineteenth century in this sense: most of the (reduced) growth in labor productivity can be attributed to (physical) capital deepening. It is thus possible that from the standpoint of MFP growth it is the mid-century experience that will eventually prove to have been anomalous.

Throughout the 1950s and 1960s the central intellectual challenge was perceived to be understanding the apparent growth in the importance of MFP over the course of the twentieth century, which Abramovitz labeled a measure of our ignorance. The fall in growth rates of both types of productivity in the last third of the twentieth century has altered the terms of the discourse. Griliches's work can be seen as spanning both chapters of this intellectual history: the initial efforts to address why the residual was so large, and the efforts subsequently to explain why it has more recently been so small.

Griliches's dissertation and early articles explored the contribution of technological change and research and development to productivity in the production of hybrid corn. He went on to address productivity growth in manufacturing and the rest of the economy, where he focused initially on eliminating the "residual" by adding factors reflecting growth in the measured contribution of human capital accumulation and expenditures on research and development. Towards the end of his career, he tackled the slowdown in a residual he had earlier tried to explain away, exploring measurement issues as a possible explanation for the deterioration of productivity growth in the last third of the twentieth century. He argued that the shift of GDP out of relatively easy-to-measure goods-producing sectors such as agriculture and manufacturing, into what he viewed as the inherently more difficult-to-measure sectors such as wholesale and retail trade, financial and insurance services, and the health sector could be responsible. But he vacillated, also acknowledging, as have most interpreters, that the phenomenon was likely real. In particular, as he noted, rates of productivity growth fell even within manufacturing, and quite dramatically in some subsectors, because the growth that did occur was concentrated within durable manufacturing and within durable manufacturing within computers and telecommunications equipment (p. 80).

Those familiar with Griliches's many publications will find relatively little that is new here. But those interested in an overview of this important area of inquiry and the author's contribution to it will be well rewarded. This book is a fitting epitaph for his career.

ALEXANDER J. FIELD, *Santa Clara University*

Probability, Econometrics, and Truth: The Methodology of Econometrics. By Hugo A. Keuzenkamp. Cambridge: Cambridge University Press, 2000. Pp. ix, 312. \$69.95.

Richard von Mises (1883–1953), the younger brother of Ludwig, had an Apollonian genius. He was a leading probabilist and statistician of the world, a leading engineer and designer of airplanes for the Austro-Hungarian Empire, and a leading philosopher of "positivism." Von Mises was rational and measured, a man of observations and proportions. Though too modest to say so, he was a hero in his own story of scientific philosophy. A Harvard professor, von Mises was also learned in the humanities: his *Positivism: A Study in Human Understanding* (Cambridge, MA: Harvard University Press, 1939 [1951]) is painted with images from Nietzsche, Schopenhauer, and Goethe, and in equal proportions with the positivist icons of Mach and Comte and Kant (pp. 401–04). Von Mises owned and loved the world's largest collection of Rilke poetry before bequeathing the collection to

Harvard's Houghton library. But von Mises believed his Dionysian self to be rhetorically separable from the positivist-scientist self, and lower. He tried to peel rhetoric away from science and gaze at its logical and empirical clarity. Abraham Wald was his student. His theory of collective probability influenced the econometrics of Trygve Haavelmo. Yet econometricians and philosophers younger than Arnold Zellner have never heard of Richard von Mises.

In *Probability, Econometrics, and Truth: The Methodology of Econometrics*, Hugo A. Keuzenkamp introduces von Mises to the new generation (the very title echoes a 1928 title by the master). Keuzenkamp's ambitions are in fact larger, and largely reformist. Keuzenkamp wants to delete from econometric software the programs of Jerzy Neyman and Egon Pearson and codify instead the ideas of von Mises and Harold Jeffreys. He would prefer that philosophers stop reading Karl Popper, Mark Blaug, Bruce Caldwell, and Deirdre McCloskey, and begin reading von Mises, Jeffreys, Karl Pearson, and R. A. Fisher (pp. 265–66). Keuzenkamp believes that econometrics has hit upon hard times. Keuzenkamp laments econometric epistemology, a site, he says, where “falsificationism, apriorism, and ‘post-modernism’ dominate the scene” (p. 263). He believes that “the only useful interpretation of econometrics is a positivist one” (p. 213), and that consensus (he will not say “truth” [pp. viii, 266–75]) in an econometrics with warrantable claims is going to require a revival of positivism of a von Mises-Jeffreys type.

The rarity of books that try to speak to both the practicing econometrician and the economic philosopher is I think evidence of their high cost of production and persuasion. Institutional incentives to specialize in a remote equation of the contemporary academy has partitioned thinkers and doers, and pretends there is a significant difference. The practicing econometrician is busy trying to solve her sample attrition problem with the welfare data while the philosopher of econometrics is defending or refuting the fact-value split, and the two vocabularies rarely overlap. Keuzenkamp's effort will therefore be seen as unusual. Keuzenkamp is both practitioner and philosopher, and is able to speak from inside the overlaps. And yet a necessary condition of truth is that one person speaks and another listens. Whether, and how much, people on each side of the overlaps *will* listen are important questions. I suspect not very much. Forget about whether or not one *should* listen—or subscribe—to Keuzenkamp's program. It is that the Neyman-Pearson program and the accompanying philosophy of methodological falsificationism that he wants to obliterate (p. viii) are entrenched in the wealth and authority of three or four living generations of contributing scholars. Soon after von Mises completed his last book at Harvard, a white sheriff in Montgomery and then in Birmingham wanted to save his job and cling to Jim Crow segregation, even though a black minister for nonviolent change explained patiently from behind bars that the sheriff was oppressed by his own methods. Sadly, the sheriff's behavior is something like a constant in economics, and likewise with the guardians of economic methodology.

Yet Keuzenkamp does supply reasons to doubt his vision of a useful and positivistic econometrics in the von Mises vein: “[i]f the theory of von Mises is at all applicable to economics, it must be in cross-section econometrics where data are relatively abundant” (p. 40), he says. “[T]he strength of [von Mises'] theory is also a weakness,” says Keuzenkamp, “it is impossible to analyse probabilities of individual events with a theory based on collectives, or to perform small sample analysis without exact knowledge of prior probabilities” (p. 40). These reasons alone seem to reduce the implied audience to not much more than the rural development economists, who suffer from fallacies of composition. (Von Mises' hallmark notion of collective probability, having faded from the mainstream, is nicely described by Keuzenkamp [pp. 34–40].)

Other reasons to turn from the vision hail from material that Keuzenkamp does not supply. Practicing econometricians may not readily perceive that *Probability, Econometrics, and Truth*

is more manifesto than philosophical treatise; and as a positivist tract, it rarely allows the resistant voice to speak. (Censorship was characteristic of 1920s positivism, especially of the breed that A. J. Ayer took into the 1950s.) Given the complex of audience considerations Keuzenkamp may be forgiven for letting Deirdre McCloskey's *The Rhetoric of Economics* (Madison: University of Wisconsin Press, 1985 [1998]) stand for all postmodern research on econometrics (pp. 264–66 and throughout). Yet Keuzenkamp fails to engage with McCloskey's critique of positivism. He does not refute the fundamental finding of McCloskey's research (a finding first made by Aristotle): rhetoric is not an alternative to some other way or path; it is the only path, even in the normal and deviant assumptions of econometrics. Still, Keuzenkamp's weak "rejection" of rhetoric is enough to make a positivist blush:

I concur with McCloskey's view [*Huh?*] and share the critique on the use of significance tests [*Of course*]. However, I disagree with the 'rhetorical' cure [*McCloskey never said there is a rhetorical cure: the idea of a cure for science, now and forever, is the positivist's fantasy for a unified science*]. The fact that few writers are so well versed in rhetorics as McCloskey herself [*McCloskey's literacy is a fact, but she did not begin her study of rhetoric and postmodernism until age 38. By the way, how many practicing econometricians are as well versed as Neyman and Pearson in hypothesis testing?*] combined with my feeling that she does not persuade (me, personally, nor the majority of the econom(etr)ics profession) [*Your feeling? That is great! I am sure that Rilke, Albert Hirschman, Nancy Folbre, and Michael Polanyi would praise you for the self-knowledge that came to you through introspection. Though without a doubt they would expect you to show them some empirical measurements, too, as McCloskey and Stephen Ziliak do: "The Standard Error of Regressions," Journal of Economic Literature (March 1996); by the way, are the majority of statisticians persuaded by von Mises's aeronautical rhetoric of the "Grossflugzeug" and the Austrian-Empire in its own terms?*], should be sufficient reason [*That is an example of the praxeology of Ludwig von Mises, and the Kantian a priori, to which you object (pp. 7, 40)*] to reject [*That is Neyman-Pearson hypothesis testing, without a loss function, to which you object (pp. 54–55)*] the rhetorical approach to methodology on its own terms [*The rhetorical terms can be found in McCloskey's six books and hundred-some articles but they do not appear anywhere in this rejection of the terms*] (p. 266).

Keuzenkamp ought to claim rhetoricians as comrades in his project of counseling econometricians out of their defective Neyman-Pearson and falsification fetishes, because we are. Econometrics has fallen upon hard times. But on a scale of harms, most of us would consider the frequentist debate of lesser importance. Good rhetoricians, like Arnold Zellner, get close to the text. They have found the positivist gaze to be illusory: rhetoric is not separable from science. They have found that metaphor is not "lower" than fact (p. 63). And they have found that the bigger tragedy in the practice of econometrics is that policy borne of a false and forced separation of the Apollonian and the Dionysian, the Pearsons and the Rilkes, the statistics and the ethics, the measurements and the meanings.

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

The United States and GATT: A Relational Study. By Donald G. Beane. New York: Pergamon Press, 2000. Pp. ix, 296. \$75.00.

The ultimate success of the General Agreement on Tariffs and Trade (GATT), negotiated by 27 nations in 1947, would have been hard to predict at the outset, given its lack of

enforcement tools and dependence on the United States for leadership. What explains the GATT's achievements in the areas of tariff reduction and trade expansion, as argued by Donald Beane, is the adoption of a more pragmatic rather than ideological approach to problem-solving, along with a growing reliance on multilateral negotiations as the dominant position of the United States eroded. Lurking behind the scenes was the long-running battle between the U.S. Congress and the president over the role each would play in trade policy.

In the current trading system now centered on the World Trade Organization, it is easy to overlook the missteps of American policy that led to the failure to maintain the original dominance of the United States. Beane's viewpoint is that the United States routinely failed to have a consistent policy message, and frequently took actions against the system. These shortcomings opened the way for other nations to assume the leadership role and to push their respective agendas. In an ironic way, it was the very weakness of the United States that made the GATT system stronger.

What Beane terms weakness on the part of the United States was evident early on when Congress was determined to continue to offer tariff protection to a weak agriculture sector, contrary to the commitment to trade liberalization embodied in the GATT. The conflict served as a model for other nations, demonstrating that the system was flexible enough to tolerate both positions. Beane sees this flexibility as the main attraction for developing nations to join the GATT since they often wanted to have it both ways themselves by continuing protection for their "infant industries" while gaining access to export markets for their natural resources.

An overview of U.S. trade policy, considered separately in one of the final chapters, is confined to legislation passed by Congress. Though there are occasional references earlier in the book to the importance of "internal and external" forces, the overview is primarily a legislative history covering the period from the Tariff Act of 1930 (Smoot-Hawley) to the 1998 World Intellectual Property legislation. There is little attempt to evaluate the consequences of these laws for the U.S. economy or the world trading system. Each piece of legislation is assumed to have been perfectly and costlessly enforced and to have had its full intended effect, which makes it difficult for Beane to consider their economic impacts.

There are numerous examples, many well known, of trade policies enacted by Congress that were in direct conflict with the principles originally embodied in the GATT. As Beane convincingly shows, American policy routinely proceeded on two fronts: toward increased protectionism for favored sectors such as agriculture, textiles, and automobiles, and toward trade liberalization through its support of negotiations within the GATT framework. Whether the enactment of trade laws by Congress was ever constrained by the commitment to the GATT is difficult to uncover from this work. For example, several hundred protectionist measures were introduced by members of Congress in the mid-1980s, yet only a handful were ultimately passed. Was this because they were judged to be in serious violation of the GATT principles, or simply because the domestic support base was too slim for passage? The evidence gathered by Beane suggests that the causation was almost exclusively one-way. Actions by the United States, such as voluntary export restraints and "Super 301," which mandated retaliation, challenged the principles underlying the GATT. There are few cases of parallel challenges posed by the GATT system on American legislation.

The protectionist tone set by the United States did not devastate the system. As the relative role of the United States diminished over time, other nations took up more of the burden of maintaining the system, presumably because they saw the GATT as valuable. The GATT, in hindsight, appears to have been able to withstand even severe challenges precisely because of its perceived value, created initially by the early decisions made by the United States in the realm of tariff concessions, rebuilding loans and grants, and political

support. Though he has not fully explained the reasons behind the GATT's resiliency, Beane has successfully shown the ways in which the United States has over the years played the role of both friend and enemy to the system.

COLLEEN M. CALLAHAN, *Occidental College*

The Luddite Rebellion. By Brian Bailey. New York: New York University Press, 1998. Pp. xvii, 182. \$38.00.

The story of the Luddite risings has all the ingredients for an excellent popular history: drama and excitement, poverty and exploitation, youthful resistance, violence and murder, romance and repression. Thanks to legal, Home Office, and newspaper evidence, and thanks especially to the work of earlier historians, Brian Bailey is able to convey a lively narrative of events which includes many telling details about the major protagonists. The book is at its best in the short chapters on the major attacks on properties and persons in Yorkshire and elsewhere, and in sensitively discussing the reaction of the authorities, the trials, and the punishments of offenders. There are however many weaknesses and troublesome aspects to this book. Most historians would wish to challenge the value of a book that aims at a comprehensive account of the sequence of events, free from detailed interpretation of the sort that Bailey sees as clouding other works (p. iv). They will also be troubled by the many assertions that are unsupported by evidence, from rejection of the conventional account of the origin of the term "Luddite" (p. xi), to assessment of George Mellor's motivation (p. 142), to the notion that Midland framework knitters were "dull and unimaginative" (p. 15)! There are also many overblown statements and assumptions, which would immediately be questioned by any historian of the period, such as the development of "a class war" (p. xvii), and the narrow definition of "political" in discussion of communities which were, after all, engaged in a struggle over control of the means of production. Some would argue that this is inherently "political." And if the "small, dark people of Celtic origin" (*sic*, p. 15) who comprised the Midlands workforce were so apolitical, one might ask why news of the prime minister's assassination in May 1812 was greeted in Nottingham with great joy, exultation, bonfires, flags, and drums (E. P. Thompson, *The Making of the English Working Class*. 2d ed. Harmondsworth: Penguin, 1968: 932). It is a pity that Bailey appears to draw so little from Thompson's research (and particularly from his debates with Church and Chapman), except where he is rejecting Thompson's interpretation of Luddite motivations. Others will be unhappy with Bailey's superficial tin-pot psychology in attempting to rehabilitate the role and meaning of "mob behaviour" (pp. 148–51), and with the sparse footnoting and limited bibliography (no journal articles, few books published since 1990), which make it difficult to gauge exactly what has been drawn from primary sources and what has been derived from other secondary literature.

This is not an academic book. It retells an interesting story and has engaging illustrations, but it does not conform to conventional academic standards of primary research, secondary context, referencing, or indexing. It is not easy for a professional historian to judge fairly such a volume. There is a tendency to be hypercritical and to condemn, even though it may have an important role in engaging an audience far beyond the boundaries covered by more conventional, academic literatures. In some respects any book that can attract more people to debates about the past has my vote, providing it is not deliberately misleading or distorting, and providing that it offers something new in terms of synthesizing earlier accounts, adding some new research, or making the results of research more accessible. This book does have something to offer in these respects, but one should also

be aware of its weaknesses and pitfalls—even for the broad popular readership at which it is aimed.

PAT HUDSON, *Cardiff University*

Eight Eurocentric Historians. By J. M. Blaut. New York: Guilford Press, 2000. Pp. xii, 227. \$22.00, paper.

The late J. M. Blaut, a geographer who passed away in November 2000, had a pet theory. Europe's political and economic domination of the rest of the world in the past four centuries or so was due to a single event: the almost happenstance discovery of America's treasures in 1492 by Europeans, who then used these resources to subjugate the rest of the world and to create a capitalist system that produced the Enlightenment, modern science, the Industrial Revolution, and all the rest.

This book consists of eight review essays on books by scholars who at various times have proposed interpretations different from his. The eight scholars under indictment are a distinguished lot: David S. Landes and Eric L. Jones are two of the most influential and respected economic historians of our own time; Max Weber is here, as well as medievalist Lynn White and physiologist Jared Diamond, the author of the celebrated *Guns, Germs, and Steel* (New York: Norton, 1997). Their common denominator is not quite so obvious, and a few of them seem odd choices if the criterion is "eurocentricity": Marxist historian Robert Brenner was really concerned with different issues, and Diamond is more of a "Eurasia-centrist" than a "Eurocentrist." Missing, on the other hand, is William McNeill, author of the classic *The Rise of the West* (Chicago: University of Chicago Press, 1963), who does not even make the bibliography.

Since the historical phenomenon at stake here is nothing less than the rise of Europe, the questions seem complex enough for reasonable scholars to differ respectfully in emphasis and interpretation. Asia, Africa, and Europe, after all, are large and heterogeneous landmasses, and bland statements about "Asia" and "Europe" may be confusing without some kind of specific aggregator that maps the bewildering variety of experiences into some kind of "mean" or "typical" behavior. Nor is the explanandum quite clear. Is it economic development? Political domination? Commercial success? Technological leadership? Superior living standards? The "Rise of Europe" is a large literature, full of fascinating and tantalizing insights and interpretations, but anyone looking for simple, conclusive answers should try elsewhere.

All scholars surveyed in this book have suggested that some of the discrepancies in long-term development among societies may perhaps be due to differences in geography, culture, or political structure. Blaut will have none of this. In his view, up to 1500 Europe was not all that different from the rest of the Old World. Capitalism, commercialism, advancing technology, good economic institutions—all were distributed more or less uniformly across the hemisphere. Christianity, climate, and the classical heritage mattered not one whit. The thesis of course must stand and fall with the proposition that Europe circa 1500 was no more advanced than China, India, or Africa. Some other scholars, including some China specialists, have expressed similar views in recent years. To be sure, no one would deny that medieval Europe was a dynamic society, capable of successful economic growth. Blaut maintains, however, that non-European societies were just as dynamic. This statement is too vague to be tested. Which non-European societies, exactly, and when? How is this dynamism measured? Perhaps the Chinese *could* have reached Europe; but they did not. Was there nothing in Europe for Asians to covet, as he argues? Spectacles, weight-driven clocks, navigational instruments, to say nothing of firearms—could none of those have been put to use in the Orient?

One might wonder if we should look not only at *levels* but also at *rates of change*: it is conceivable that by 1500 Europe was not yet ahead, but had already established a rate of change that was faster than Asia's. In that case, Blaut may have a legitimate argument that the discoveries enhanced this difference in trends, but he cannot demonstrate that *they alone* account for it. After all, among the most interesting flows that were set into motion by the European voyages after 1500 was the flow of *knowledge*. Europeans were eager to observe, interpret, learn, copy, and then adapt things they saw elsewhere, from potatoes to maize to turkeys to chinaware to inoculation. They were not only greedy, pushy, and self-righteous, they were also *curious* and *pragmatic*. Were Asians less so? And if so, why?

At times the critical tone in this book has the overtone of a personal crusade or a settling of accounts. Blaut singles out for special criticism Jones's widely read *European Miracle* (Cambridge: Cambridge University Press, 1981), a pioneering and provocative book which stimulated a great deal of discussion in the 1980s. This anger is rather strange, given that the politics of the issue is by no means obvious. After all, some of the views attacked here as "Eurocentric" can also be found in the writings of Joseph Needham, the much-admired Marxist guru of Eurocentric-bashing scholars. Diamond too is severely criticized, despite the fact that the political implications of his work are obscure. It seems that he is included solely because Blaut objects to his geography; geography, though, seems an odd subject for scholarly passion.

In any case, these essays add little to the existing literature, and the rhetorical tone of many of them is so vehement that they risk not being taken seriously. As McCloskey has noted in *The Writing of Economics*, invective is deeply satisfying for the author, but it arouses suspicion in all but the most uncritical readers that the argument needs a tone of passion to mask its weakness. This is a shame: the debate on the Rise of Europe is endlessly fascinating, and there are times when Blaut makes legitimate points. Yet these will be washed away in the stream of vitriol. Overuse of blunt terms like "wrong," "ignorant," and "false" invariably lessen a critical book's impact. In scholarly disputes, the stiletto is mightier than the meat-axe.

The publication of this odd and angry book stands as testimony that the diversity and pluralism of Western intellectual freedom can sprout some very strange flowers indeed. One cannot help but wonder if such pluralism could have emerged in another cultural tradition.

JOEL MOKYR, *Northwestern University*

Malthus, Medicine and Morality: Malthusianism after 1798. Edited by Brian Dolan. Amsterdam and Atlanta: Rodopi, 2000. Pp. 232. \$53.00.

Malthus, Medicine and Morality is a collection of essays marking the bicentenary of the publication of Thomas Robert Malthus's *Essay on the Principle of Population* in 1798. They vary considerably in subject matter, as is inevitable given the nature of Malthus's work. The *Essay* managed to be both a scientific investigation into demographic history and an intervention into the political debates on the French Revolution. It built its argument on a particular theory of mind and human behavior. Because it linked wealth and poverty to population, the *Essay* became part of the political economy of the time. Malthus gave his name, though not his imprimatur, to the birth-control movement in the nineteenth century. Not only were the *Essay*'s arguments widely and approvingly quoted at the time, they also provoked a resolutely anti-Malthusian current of thought. Though the *Essay*'s argument is apparently simple, this obscures a wealth of ambiguity and dense argumentation in the text, made more so by the constant revisions Malthus carried out for subsequent editions. What this collection does is to begin to deconstruct Malthus and unravel all the various elements that make up the phenomenon of "Malthusianism."

Two pieces deal with Malthus himself. Timothy L. Alborn looks at Malthus's career at Haileybury, the college that prepared candidates for bureaucratic posts in the East Indian Company, and at the influence of his ideas on moral restraint in the disciplining and training of its students. Brian Dolan shows the importance of Malthus's visit to Sweden, which in 1749 had adopted a policy of regularly collecting data on births and deaths. He is able to show Malthus's complex relationship with the material he put together in support of his thesis. The essays by Christopher Hamlin and Kathleen Gallagher-Kamper and Lesley A. Hall examine the more general influence of Malthusian ideas. The first discusses the progress of sanitary reform in Ireland. Hamlin and Gallagher-Kamper see the impact of Malthus as creating a coterie of physicians who regarded the ravages of disease as one of the necessary "natural checks" which Malthus described and, therefore, debated with their colleagues the extent to which sanitary reform would impede, rather than secure, improvement in the health and well-being of society. Hall's essay is on the Malthusian League in Britain; Angus McLaren addresses the development of the French birth-control movement. Antonello Las Vergata discusses wider European reactions to the "politicization" of population and fertility. He ranges from the early nineteenth century socialist reaction to the degenerationist controversies of the first half of the twentieth century.

Three essays (by Roy Porter, Robert M. Young, and Brian Young) deal with the significance of Malthus's work in the early nineteenth century, namely the creation of a symbiotic "world view" encompassing science, religion, and political economy. Porter discusses Malthus's role as a political controversialist, and describes some of the early hostile responses to his work. Robert Young reexamines Darwin's relationship to Malthus. Brian Young discusses Malthus's impact among theologians. These essays remind us there was no simple dislodging of a "religious" world view by a "scientific" one at this time. Malthus, himself an ordained clergyman, expected his propositions on population to be at one and the same time relevant to theodicy, to nature, and to human progress.

The history of the last two hundred years has, probably, disproved the Malthusian axiom in so far as population has risen steadily without necessarily provoking the kind of crisis he predicted would follow. This does not mean to say that Malthusian crises are not occurring in some part of the world at this moment in time. From Malthus's own point of view, however, the history of fertility over the last two hundred years would not vindicate of his basic proposition. But the continuing importance of Malthus's *Essay* is that it is seen as the first modern scientific analysis of the economic costs and benefits of human procreation. This is rather unfair to his contemporaries. There is, in both Condorcet and Godwin, quite a lot of material which, now looks much more "modern" than Malthus's—for example their discussion of the influence of marriage patterns on fertility. Nonetheless, the historical importance and influence of Malthus's *Essay* cannot be denied. This means that, even now, when we think in terms of the relationship between production and reproduction we see ourselves in a tradition of which Malthus was the most influential founding father.

GRETA JONES, *University of Ulster at Jordanstown*

Currency Crises. Edited by Paul Krugman. Chicago: University of Chicago Press, 2000. Pp. 356. \$47.00.

Currency Crises is a collection of papers by leading thinkers on the subject, all presented at a 1998 NBER conference. Paul Krugman edits the volume and asks aloud what the reader can only silently wonder: Why is yet another conference on such a well-established research field necessary? The answer, according to Krugman, is that "despite two decades of research on the causes and consequences of such crises, important issues are either

unresolved or require a fresh look in the face of new experience" (p. 2). This is made immediately clear in the pages that follow. The volume contains applications of new models to old crises, as well as new ways of thinking about recent episodes. It also treats different types of currency crises, including not only those that can and do occur in industrial countries (usually because of policy inconsistencies), but also the breed of animal that rears its head in emerging markets.

The quality of the research is unsurprisingly stellar, as is the depth of thought that has gone into the included "comments." While the volume does achieve some consensus (for example, Guillermo Calvo and Steven Radelet and Jeffrey Sachs agree that the East Asian crises were mostly financial in nature; and many maintain that the 1995–97 crises were inflamed by inappropriate policies), it challenges conventional wisdom and raises as many questions as it offers answers.

For example, Robert Gordon shows that the belief that the ERM "quitters" of 1992–93 performed better than the "stayers" is based on an ill-founded comparison of France with Britain. (According to Gordon, Britain's subsequent success was mostly due to structural rather than monetary factors). Gian Maria Milesi-Ferretti and Assaf Razin challenge the view that large devaluations lead to current-account reversals, and show that the latter do not affect growth whereas the former have an immediate negative impact. Finally, Robert Flood and Peter Garber demonstrate that the euro payment institutions that would begin operating at the outset of the European Monetary Union would preclude all the exchange-rate volatility, indeterminacy, and speculative attacks feared by the markets. Hindsight has proven these authors correct, as the euro was launched without incident.

In addition to the abovementioned contributions, the volume contains work by Barry Eichengreen and Olivier Jeanne on the causes of sterling's collapse in 1931; an original theory of political contagion by Allen Drazen; and a menu of ways to think about recent emerging-market crises by Guillermo Calvo. Steven Radelet and Jeffrey Sachs discuss how the East Asian crises were mostly financial in origin and magnified by a series of policy mishaps; Sebastian Edwards and Miguel Savastano provide a detailed analysis of the Bank of Mexico's behavior during the post-peso-crisis period of 1995–1997.

Far from putting this mature research program to rest, *Currency Crises* adds fuel to the academic debate. It captures the latest thinking of the leading scholars in the field, and in so doing it provides researchers with an endless menu of topics to pursue. Problems such as the ideal set of macro- and microeconomic policies to prevent, circumvent, or resolve a crisis still remain wide-open, as do the exact effects of crises. In addition, alternative ways of modeling emerging-market crises of the sort that are financial in origin but not self-fulfilling seem to be a promising research venue.

Given the quality and timely nature of research contained in *Currency Crises*, I would recommend it to anyone researching in the field, as well as to policy makers dealing with these issues. Its high proportion of nontechnical material makes it accessible even to interested laymen who simply seek a better understanding of the key issues at hand in potential or actual crises.

VICTORIA MILLER, *Université du Québec à Montréal*

Money, Markets, and the State: Social Democratic Economic Policies since 1918. By Ton Notermans. Cambridge: Cambridge University Press, 2000. Pp. xix, 302. \$59.95.

The importance of monetary policy in regime changes lies at the heart of Ton Notermans's study of "social democratic economic policies." Notermans distinguishes

two general regime types, one “social democratic,” emphasizing economic growth and employment as its long-term objectives, the other “liberal,” targeting price stability. Based on the experience of five European countries (Britain, Germany, Sweden, Norway, and the Netherlands), he analyzes three major changes of macroeconomic policy regime in Europe since the First World War. The first is the restoration of the international gold standard in the 1920s, after the years of inflation and exchange-rate instability following the First World War. This regime change reestablished fixed exchange rates as the overriding policy objective, and employed restrictive monetary policy to accomplish this end. The gold-standard regime slowed growth, increased unemployment, and sent world prices into a deflationary spiral. The next regime change was abandonment of the gold standard in the 1930s, in the wake of which individual countries reoriented policy towards economic growth and low unemployment, favoring especially low interest rates and exchange-rate adjustment. The third regime change, beginning in the 1970s and stretching into the 1980s, shifted emphasis from economic growth and employment to disinflation, and returned to restrictive monetary policies as the preferred instrument to achieve this goal.

Notermans is interested in how and why changes in the fundamental objectives of macroeconomic policy took place on a global scale. His approach and argument were first tried in an essay co-authored with Douglas J. Forsyth (“Macroeconomic Policy Regimes and Financial Regulation in Europe, 1931–94.” In *Regime Changes: Macroeconomic Policy and Financial Regulation in Europe from the 1930s to the 1990s*, edited by Forsyth and Notermans, 17–68. Providence, RI: Berghahn Books, 1997). The earlier essay was slightly broader in scope (covering also France, Italy, and Belgium), but this study nevertheless represents a considerable advance: it analyzes policy in much greater detail, paying closer attention to the actual operation of the three regimes, drawing on a richer field of secondary work, providing pithy critiques of conventional arguments about the nature of policy regimes and the supposed market constraints on policy, and arguing his new case for the characterization of policy regimes as either “social democratic” or “liberal.” Monetary policy is absolutely central to both regime types. Social democratic regimes use expansionary monetary policy to promote growth and rely on institutional structures, particularly within the labor market, to restrain inflationary pressures; Notermans’s attention to Swedish and Norwegian experience is particularly illuminating in this regard. In contrast, liberal regimes employ restrictive monetary policy to control inflation, and willingly tolerate slower growth and higher unemployment in the interests of price stability.

The international scale of the regime changes was not a coincidence. Rather, in Notermans’s view, these regime changes were “ultimately driven by the need to break cumulative processes—an inflationary process during the early twenties and the past two decades, and a deflationary process in the 1930s” (p. 32). The changes were forced on governments of varying political coloration, which had to deal with the disintegration of the previous regime and an accumulation of pressures (most evident in price-level changes). Notermans discounts the role of economic ideas and interest-group politics in regime changes. Economic ideas are, in his analysis, adopted to justify policies determined by practical politics: politicians, whether liberal, social democratic, or other, cannot sustain confidence and power in periods of persistent inflation or deflation. Interest-group politics cannot explain why regime changes are eventually adopted by governments of both the right and left in order to restore relative price stability.

Notermans draws on a wealth of secondary studies detailing the Scandinavian experience, where social-democratic policies had a longer and more successful run than in European countries further south. He also provides intriguing evidence for a significant effort

to escape the gold standard in the early 1920s, which ended in a general retreat back to orthodoxy. His study is weighted toward accounting for the first two regime changes, namely the restoration of the gold standard in the 1920s and its rapid abandonment in the 1930s. He explains the successes of European recovery and growth after 1945 by the general adoption of a regime giving priority to growth and employment, and the use of monetary policy to achieve it; “Keynesian” demand management (loosely defined as any use of countercyclical fiscal policy), by contrast, is seen as largely irrelevant to the successes of this era. Cheap money fostered growth without inflation because institutional structures delayed the development of strong wage pressures until the late 1960s. The shift to restrictive monetary policies since then has restored price stability, but at the price of slower growth and higher unemployment in Europe. No doubt many readers will disagree with his definitions, evidence, arguments, and conclusions, and find his choice of countries unduly confining. But Notermans’s emphasis on reassessing the “lessons of the past,” and on rethinking conventional wisdom about the constraints on monetary and fiscal policies intended to promote growth, is timely and salutary.

KENNETH MOURÉ, *University of California, Santa Barbara*

No Ordinary Academics: Economics and Political Science at the University of Saskatchewan, 1910–1960. By Shirley Spafford. Toronto: University of Toronto Press, 2000. Pp. ix, 272. \$45.00.

Who among us journeymen academics, injected with truth serum, would not spill out an elephantine burden of resentments? Who among us, with sufficient lubrication, would not discourse eloquently on the idiocy of referees and editors, on contributions overlooked, colleagues overcompensated, grants rejected, and insights neglected? When the burden becomes too great, I suggest that you turn to this most unpromising of books, and read the story of Mabel Florence Timlin. Mabel Timlin, age 26 in 1917 and without a college degree, was a “good average teacher” in the elementary school in Bounty, Saskatchewan. This same Mabel Timlin finally completed her Ph.D. in economics at the University of Washington in Seattle at age 50, after 24 years of hand-to-mouth existence at the fringes of the economics department at the University of Saskatchewan. In the depression years her compensation was so meager that she, a woman in her middle years, lived in one room in a house shared with students. But her thesis was a path-breaking exposition and development of the New Keynesian economics that she developed from mimeographed notes of a seminar by Keynes in London in 1934, passed to her by a colleague. Her authority on this new economics became so respected that in 1945 she gave seminars at Harvard and Columbia expositing Keynesian economics to such luminaries as Wassily Leontieff.

Unfortunately only a modest share, a mere ten pages or so, of this well-written but very Canadian volume focuses on Timlin. While the story of the manly men who struggled to establish an economics program on the edge of the world is not without its moments—one of the key players ends up in economics only because a hailstorm destroys the crop on his land, preventing him from enrolling in law school—the account is unfortunately focused on their role as builders of an institution rather than as individuals. The economics department at Saskatchewan, however strong it may have been relative to other Canadian universities, was never a big player in the wider economics world. There were no famous scholars, no Saskatchewan School of economics. The institution they were building will be of no great interest to most. It is only Timlin’s story that has that extra Hollywood resonance. But the book here leaves many questions unanswered.

Timlin was actually a native of Wisconsin who, after the death of her parents, headed north in search of who-knows-what with barely enough money to get across the border. She got out of elementary teaching in Bounty quickly, reckoning she could do better as a stenographer in Saskatoon. But there seems to have been some plan in this. She took a reduction in salary to become a secretary at the university, hoping thereby to be able to complete a BA in economics. She found the economics courses less than challenging and so instead completed a degree in languages and literature. She did not manage to complete the BA until 1929, when she was 36. From 1929 to 1943 she was first secretary, then director, of the university's correspondence courses. She herself organized and graded the two economics courses as part of her job.

She enrolled as a master's student at the University of Washington in 1932, planning to work towards the degree in the summers, and by 1934 she had advanced to the PhD program. To meet the residence requirement for the PhD she had to be at Washington for six months. So she took leave without pay, but to retain her position she agreed to set exams and grade assignments gratis while on leave. On her return she was appointed a tutor for students having difficulty with introductory economics, and then an instructor. Finally in 1939 things turned up. The new university president urged her to complete her PhD and gave her \$50 a month to return to Washington. By 1940 she had completed a thesis that was so original by the standards of the time that the Washington economics department had to get assurances from Keynes and Oscar Lange on its quality. By 1943, now aged 52, she was appointed assistant professor in the Saskatchewan department, where she had 13 productive years before retiring as a full professor at age 65. She lived on in Saskatoon to the age of 85, engaged in research and, as far as can be discerned from the book, happy.

So when you shudder at the prospect of your next encounter with the 370 whining ingrates that constitute your Introductory Economics class, when you search desperately for something positive to write in a recommendation letter for a student with a 2.54 GPA whom you don't even recall teaching, when you open the envelope to discover a request for a referee report on a very long paper on cows by a journal that lost the last manuscript you sent them, think of Mabel Timlin, and reflect that life is indeed sweet.

GREGORY CLARK, *University of California, Davis*

The Rise and Fall of State-Owned Enterprise in the Western World. Edited by Pier Angelo Toninelli. Cambridge: Cambridge University Press, 2000. Pp. ix, 320. \$49.95.

This book offers historical perspectives on the origins and purposes of state-owned enterprises in the United States and Western Europe, the performance of these companies, and the growing dissatisfaction with public ownership, culminating in a wave of privatizations over the past twenty years. It combines analytical essays on various aspects of public ownership with a series of country cases (Germany, Italy, Britain, France, Spain, Austria, and the United States). As is often the case with edited volumes, not all the contributors see eye-to-eye. The sharpest contrast is between the paean to the developmental state offered by Erik Reinert on the one hand, and the neoliberal understanding that informs the concluding essay by Louis Galambos and William Baumol on the other. For Galambos and Baumol, the experience of public enterprise teaches us that "there is no substitute for the profit motive and the rigors of fierce competition in eliciting growth of output, productivity, and innovation from the individual firm" (p. 308). The other authors, while closer to Galambos and Baumol's position than to that of Reinert, nonetheless offer a more nuanced perspective on public ownership.

The editor's historical overview and the country chapters point to important economic reasons for the nationalizations of the first half of the twentieth century. Nationalizations were driven by more than the Marxist illusions of socialist and communist parties. Governments were also responding to the problem of private monopolists, particularly in transportation and utilities, who gouged consumers while providing poor service and insufficient coverage. Government suspicion of the private sector was reinforced by the cataclysm of the Great Depression, which seemed to teach the lesson that the profit motive could not be counted upon to secure adequate levels of investment and employment.

The essays show further that during the postwar boom decades public enterprises in most countries delivered entirely respectable economic performances. They were profitable, if not as profitable as their private counterparts, and even this gap was not necessarily a bad thing. As Robert Millward relates in a chapter on Britain (perhaps the best essay in the volume), the low profitability of the public sector was due, in no small part, to the government's desire to provide cheap inputs to the rest of the economy. Private profits were enhanced by the inexpensive gas, coal, water, and transportation furnished by the public sector. In the Italian case, as described by Franco Amatori, public entrepreneurs went a step further, challenging private monopolists and driving down prices, notably for the nitrogen fertilizers used by Italy's long-suffering farmers.

The public-enterprise model began to sour with the end of easy prosperity in the mid-1970s (except in the case of Italy, where patronage politics had set in by the late 1950s). The authors are in broad agreement that the problem was not so much that public enterprises were less prepared for the crisis, or had been less well managed, than their private-sector counterparts. Rather, it was that growing government interference in the 1970s and 1980s prevented public companies from making necessary adjustments. While private companies laid off workers, shuttered plants, and shifted resources to higher-profit usages, public enterprises were forced to retain a bloated payroll and to compete in increasingly unsustainable market segments. To make matters worse, concern for inflation often led government authorities to limit the capacity of public enterprises to raise prices. Thus, in country after country, the competitiveness of public enterprises was sacrificed to short-sighted efforts to hold down unemployment and inflation. It was this shift in public policy, more than the flaws of public ownership per se, that set the stage for the neoliberal backlash of the 1980s and the turn toward privatization.

If this book has one shortcoming, it is the relative lack of attention to the public-private question in our own day. True to its historical orientation, the book has far more to say about the 1940s than the 1990s. The recent wave of privatizations is discussed little, if at all, receiving at most two pages in any given essay. The result is a somewhat misleading comparison between real-world public ownership, with all its warts, and an ideal of private ownership. Yet there are at least three good reasons to be suspicious of such dichotomies. First, in several countries—notably Britain and France—public enterprises registered double-digit rates of productivity growth in the 1980s, once managers were given a free hand to reorganize. In today's more market-oriented environment, the quality of public management may not be as big a problem as in the past. Second, the political objectives that undermined public-sector management do not necessarily disappear just because a company is privatized. The coalmining industry of the German Ruhr is privately held but, as the essay by Ulrich Wengengroth describes, employment concerns have led to huge and sustained subsidies, exceeding total federal investment in science and technology over the past 30 years (pp. 121–22). Third, the performance of privatized companies depends to a considerable extent on the way in which these companies are privatized, and the market and regulatory context in which they operate. As the British have discovered, the transformation

of public monopolies into private ones is not a recipe for dynamic, consumer-friendly enterprise. Unfortunately, these critical issues are not explored in the volume. Still, if the reader is interested in the *history and limits* of public ownership, as opposed to its *current transformation*, then *The Rise and Fall of State-Owned Enterprise* is a not a bad place to start.

JONAH D. LEVY, *University of California, Berkeley*

Americanization and Its Limits: Reworking US Technology and Management in Post-War Europe and Japan. Edited by Jonathan Zeitlin and Gary Herrigel. Oxford: Oxford University Press, 2000. Pp. xviii, 410. \$74.00.

These fascinating essays show why Americanization is both more and less than it seems. On the one hand, the authors argue, the superiority of American production methods remains a widely accepted thesis in the historiography of postwar economic growth. On the other hand, they show that the diffusion of American practices collided at every stage with active efforts by Europeans and Japanese to shape postwar reconstruction according to their own visions. The result was a panoply of hybrid outcomes, rather than any simple process by which superior American models supplanted local practices.

The editors do not insist that other scholars subscribe to the crude concept of Americanization just summarized. Their claim is that even sophisticated skeptics have accepted the existence of an Americanization process, if only to critique it. The introductory essay by Jonathan Zeitlin sets out a more ambitious conceptual agenda. Rather than assessing whether U.S. practices represented a unitary or fragmented model that was either successfully or only partially implanted abroad, Zeitlin argues for viewing the evidence of postwar reconstruction through an entirely different optic. For him, Americanization should be seen as a contested political project rather than a neutral analytic category. A better category is provided by the notion of “hybridization,” which includes such processes as selective adaptation, creative modification, and innovation. In line with this alternative view, the essays in this collection emphasize the “active reworking and transformation of the American model in postwar European and Japanese industry” (p. 11). According to Zeitlin, instances of adapting and modifying U.S. practices should, accordingly, be seen not “as unavoidable compromises . . . , but rather as a positive source of experimentation, innovation, and learning for European and Japanese firms” (p. 11).

In illustrating these effects, the essays range widely. The common empirical thread concerns the Marshall Plan and the productivity commissions it financed for improving industrial capabilities in Europe and Japan. Most of the empirical chapters analyze the metalworking sector and related industries. Some are rather dense business histories of particular firms, while others provide broader doctrinal reviews of reconstruction policies in different contexts. They all focus on the elements of voluntarism and political contingency that shaped the combination—or hybridization—of imported American practices and local strategies for re-industrialization.

Two essays illuminate the ways in which the meaning of Americanization was contested by Americans themselves. Jacqueline McGlade’s chapter on U.S. foreign assistance shows that the liberal developmentalism of the early Marshall Plan generated little enthusiasm among the conservatives who, by the late 1940s, had placed strategic cold-war priorities at the fore. Steven Tolliday’s essay on private-sector efforts to transfer American methods shows that Ford and General Motors elaborated quite distinct approaches to their European subsidiaries from the interwar period through post-1945 reconstruction. His essay also offers a fascinating account of the attempt in the 1950s by Ford-Cologne to develop a light

car with front-wheel drive (the Cardinal), eventually abandoned because Ford's Dagenham unit in Britain obtained more backing from Dearborn for its more conventional lightweight model (the Cortina).

The remaining nine essays treat particular countries. A chapter on Sweden by Henrik Glimstedt shows that Swedish employers initially imposed Taylorist approaches on Swedish engineering workers, but that craft-based principles resurfaced in Volvo's well-known innovations with autonomous work groups in the 1960s. According to Duccio Bigazzi, the Italian auto sector also presents a mixed case, because the fascists' interwar ambivalence toward American management models led to dramatic variation in the way Fiat and Alfa combined "organized craftsmanship" and mass production after 1945. A chapter on the French steel industry shows how the appeal of continuous-casting methods led French officials to promote American practices aggressively, thereby leaving French producers with surprisingly few resources in the 1980s when greater product and process flexibility suddenly became imperative.

The final chapter by Gary Herrigel compares the role of American practices in the two occupied countries, Germany and Japan. Herrigel illustrates how steel executives and labor leaders in both countries highlighted those local precedents for consultative governance that most resonated with the Americans' support for pluralist concepts of market order and democracy. The hybrid results joined scale economies from the American methods with more collaborative work principles from local practices in ways that soon enabled steel firms in both countries to eclipse their American mentors.

The authors employ a range of strategies for linking the firm-level changes they document to larger patterns of postwar growth. In so doing, they stake out important new territory in what political scientists might term "decentered" analysis, or what historians might designate as social-business history "from the bottom up." One hopes that this volume will be read by comparative political economists and management scholars as well. The view that innovation can stem entirely from the hybridization of codified technique with local circumstances is only one of its more striking theoretical insights. This is a most engaging and impressive set of essays.

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