Book Reviews

ANCIENT AND MEDIEVAL

The Carolingian Economy. By Adriaan Verhulst. Cambridge Medieval Textbooks series. Cambridge: Cambridge University Press, 2002. Pp. x, 160. $50.00, cloth; $18.00, paper.

Adriaan Verhulst, Professor Emeritus of History at the University of Ghent, is best known to English readers for his work on urban origins, notably *The Rise of Cities in North-West Europe* (Cambridge: Cambridge University Press, 1999), but his Dutch and French work was more extensive in rural history and historical geography. This survey of the early Carolingian period (753–877) reflects each of his areas of interest. Verhulst saw this as a period of economic expansion that paralleled the cultural "Carolingian Renaissance." He provides a wealth of detail in separate chapters devoted to landscape and settlement, demography, agricultural production, agricultural technique, craft and industrial production, commercial organization, directions of trade, money and price movements, and the economy and the state. Much of this book, particularly in the early chapters, consists of historiographical discussions, because the literature on these topics is immense and the documentation fragmentary.

Verhulst concludes that the most dynamic novelty of the Carolingian economy was the greater efficiency of agrarian institutions, as the manorial village evolved from the hamlet and generated an exchange economy. Production increase, however, was the result of reclamation of new land and extension of the three-course rotation; Verhulst rejects the notion of improvement in agricultural technology increasing food production significantly. He attributes the well-known famines of Charlemagne's period to population pressure and thus finds them a sign of economic growth.

Verhulst detects cyclical movements in the Carolingian economy, which are best followed in reference to towns and trade. Some of the most vibrant passages are his discussions of Frisian commerce, the international trade in cloth, and the wine and grain trades. He is forced to concede Henri Pirenne's point that the Roman towns of southern Gaul declined sharply in the early eighth century, although not only because of the activities of the Arabs in the Mediterranean. In an argument based on broad notions of "urban," he further admits that in the Merovingian period artisanry was done "mostly in an urban context" (p. 72), but in the Carolingian Empire it was mainly rural and manorial. Thus despite the evidence of commercial activity outside the towns, he argues that "towns were even in the Carolingian period . . . the favourite location of commerce. . . Most merchants . . . lived in towns" (p. 91). Verhulst argues that the coastal emporia of the early Carolingian period, which handled mainly gift exchanges for the elite, evolved on the same sites in Charlemagne's time into genuine towns with a grid street layout, artisan production, and both regional and long-distance trade. They reached their height between 775–780 and 825. During this period the kings upgraded the silver penny. The great estates reached their greatest territorial extent at the turn of the ninth century. This coincidence of rural and urban–commercial indices suggests prosperity. The Frankish monarchy, its coin, and the towns declined after about 825 in the wake of Scandinavian attacks. Verhulst argues, however, that in the early 860s the decayed coastal emporia were succeeded by commercial *portus* in new or revived riverine towns of the interior. This new prosperity lasted until a new wave of Scandinavian attacks began in 879, but this time most of the interior towns survived, as the coastal places had not; albeit with a gap in settlement of 20 to 30 years.

As elsewhere in his work, Verhulst emphasizes the impact of government and politics on economic activity, but his arguments have a hypothetical quality. For example, he suggests that given the extent to which aristocratic and church estates originated as royal gifts, "it may be" (p. 34) that the system was of Frankish royal origin, although studies have shown that elements of it existed before the Carolingian period. He says (p. 126) that the kings had no economic policy as such, but rather a series of *ad hoc* responses to situations; even the royal "concern for infrastructure" (p. 131) such as roads and bridges was more military than economic, although it benefited trade. This is a far cry from his argument that the Carolingian kings were involved in establishing a "manorial system" that "may even have been a component of a 'Carolingian agrarian policy'" whose "fundamental aim was an increase of production through an optimisation of efficiency" (p. 59).

This book is the final work of scholarship on an important topic by a profound and original scholar. Unfortunately, it is unsuited to the series in which it appears. The book presupposes a level of sophistication about sources, criticism, and historiography that few British, let alone American, undergraduates possess. There is not a single map or diagram in a book that requires considerable knowledge of geography. The style is extremely turgid, which is not surprising from an author whose native language was Flemish, but the book shows no trace of copy-editing. Some words are misused, and others appear in a technical sense with which most of the intended readers will be unfamiliar. A glossary of terms should have been provided.

DAVID NICHOLAS, *Clemson University*

A History of Brewing in Holland, 900–1900: Economy, Technology and the State. By Richard W. Unger. Leiden: Brill, 2001. Pp. xxii, 428. €140, $154.00.

Within the economic history of the Netherlands in the early modern period the brewing industry has been a suppositious child for a long time. Only in the last couple of years has this started to change. In addition to that in textile production (one of the few branches that was already relatively well studied), historical interest in other industries is now rising. One of the most recent examples of this trend is the new book by Richard Unger. Although brewing in Holland was not really unknown to historians (see particularly: Richard Yntema, *The Brewing Industry in Holland, 1300–1800: A Study in Industrial Development* Unpublished doctoral dissertation, University of Chicago, 1992), this is the first time an attempt is made to write a both geographically and chronologically wide-ranging analysis of the history of the brewing industry in Holland. Unfortunately the book does not contain a critical discussion of the historiography on brewing. Therefore it remains largely unclear at what points Unger's view differs from the already existing picture and in what way his analysis is new or innovative.

Unger states that it is the purpose of his book to make clear "how the interplay of technical change and government action along with the evolution of the economy made beer so important and then unimportant and then important again to the Dutch" (p. xvii). Of these, Unger brings the economic development of brewing and the role of government to the fore. How municipal and regional authorities tried to get a grip on beer production through taxation and how the brewers reacted prove to be the most important aspects of Unger's story.

Unger chooses a more-or-less chronological approach in his book. He describes the history of brewing in Holland beginning from about the twelfth century (and actually not since 900, as the title indicates) in 12 chapters, apart from an introduction and an epilogue. First of all he brings up the medieval transformation in beer production from *gruit* to hops-beer around the year 1300. With it came a professionalization of the brewing industry. By using hops in the production process the taste of beer was changed, but it also became less perishable. This was of prime importance to the various Holland brewers that exported their beer.

The middle of the fifteenth century marked the beginning of the hey-day of Holland beer. By using and presenting a huge amount of numerical data in several tables and graphs, Unger shows how production and consumption levels of beer in the cities of Holland increased. From 1450 up to about 1650 Holland experienced what Unger calls the golden age of brewing. Yet in the course of the seventeenth century the decline of the brewing industry set in. Brewers had to deal with rising costs for raw materials and tax pressure that began to weigh more heavily on them. Furthermore, beer as a popular drink had to face competition from other (new) drinks such as wine, jenever, coffee, tea, and chocolate. A combination of these influences made the Holland brewing industry fall into decline after 1650.

Unger ends his book with a description of the revival of beer production in the Netherlands from the 1860s onwards. By adopting and applying technical innovations in the production process (such as steam engines and mechanical refrigeration systems that had their origins in England and especially Bavaria), Dutch beer producers such as *Heineken* and *Amstel* made the industry flourish again. By combining primary sources and literature, Unger succeeds in painting a beautiful picture of the long-term development of the brewing industry in Holland from the Middle Ages through to 1900.

Although Unger points out in his introduction and his epilogue that the history of brewing has many aspects, his emphasis is explicitly on the relationship between the brewing industry and the government. With this focus, his research becomes fairly traditional economic history. There is nothing wrong with that, on the contrary I would say, but Unger regularly gives the idea that his book wants to be more. Yet he is at his best on tax and excise policy and when he analyses the causes for the decline of the brewing industry in the second half of the seventeenth and the eighteenth century. What he shows very clearly is the constant tension between the authorities that wanted to exercise control over beer production and were eager to get funds out of it by taxes and levies on the one hand and the brewers who wanted to limit the interference of government on the production process as much as possible on the other.

However, the book as a whole gives a somewhat unstructured impression. It frequently jumps from one subject to another, or from one city to another, and it is not always instantly clear which way the argument will go. This is not in the least caused by the fact that the chapters are not subdivided into paragraphs. Chapter titles are not very descriptive and are sometimes misleading.

Finally a general remark. The book gives the impression of being edited rather carelessly, especially concerning the tables and figures. The figures are not always very clear in their layout. The tables have plenty of space, but their treatment is quite sloppy. Dates and numbers frequently end up in the wrong place. Besides, the positioning of the figures and tables vis-à-vis the text that they are intended to illustrate is awkward most of the time, so one has to thumb back and forth a lot. It is a great pity that a rich and beautifully illustrated book such as this one is edited with such negligence.

ARJAN POELWIJK, *University of Amsterdam*

MODERN EUROPE

Italian Industrialists from Liberalism to Fascism: The Political Development of the Industrial Bourgeoisie. By Franklin Hugh Adler. Cambridge: Cambridge University Press, 1995. Pp. ix, 458. $90.00.

The author, in this book, focuses on the evolution of industrial associations in Italy from the creation of the Industrial League of Turin (LIT) in 1906 to the establishment of fascist industrial corporations in 1934. He concentrates on the activities of industrial associations because they permit him to view industrialists, as he puts it, acting and expressing themselves self-consciously as a class. The question, of course, is why does he (and why should we) care about the opinions and activities of industrialists as a class? An obvious response is that, as historians, we seek to understand all aspects of the past—and certainly Franklin Adler's effort to chronicle the ideological coming of age of industrialists in Italy makes up an important part of his study. But there is more, especially during this period, because the answer links directly with the controversy over the role industrialists played in the demise of liberal democracy and the rise of fascism. This leads to two additional questions. First, does a study of industrial associations actually reveal industrials acting as a class and, second, does it shed new light on the industrialists' commitment to democracy and their involvement with fascism? The answer to the first question is yes and no, to the, second, an unqualified yes. The more dynamic and efficient firms in Italy—Pirelli, Fiat, Edison and so on—were mainstays and strong supporters of the associations whereas the large, state-dependent and less efficient ones, Ansaldo and Ilva for example, were not. Adler is well aware of these distinctions but seems to feel that they do not compromise his conclusions. Perhaps, but as Roland Sarti (*Fascism and the Industrial Leadership of Italy, 1919–1940: A Study in the Expansion of Private Power Under Fascism*. Berkeley: University of California Press, 1971) and Douglas Forsyth (*The Crisis of Liberal Italy: Monetary and Financial Policy, 1914–1922*. Cambridge: Cambridge University Press, 1993) indicate, the two groups of firms had very different views about politicians and policies and thus very different political loyalties. On the other hand, Adler argues persuasively that the class of industrialists closely tied to the associations never wavered in their support of democracy and never lost their distrust of fascism. His findings may not break new ground but much of his analysis is original and exciting. In short, for those with an interest in Europe between the wars, the book is well worth reading. I turn briefly to a few highlights of his argument then touch on a couple of problems.

Adler shows that Italian industrialists—excluding, of course, the directors of the state-dependent industrial complexes—developed very early in their organizational life a sophisticated set of liberal-technocratic principles to which they remained remarkably faithful throughout the turmoil of the war and postwar period. The principles included hierarchy in the factory, minimal state involvement in the economy, particularly in labor relations, and endorsement of industrial growth as the basis for national development. The key operational feature for these industrialists was autonomy in their investment, production, and employment decisions. As Adler demonstrates, conflicts between industrialists and liberal politicians—over trade policy, over mobilization efforts, over the slow dismantling of controls at the war's end—all boiled down to the same issue, the right of industrialists to run their affairs without state interference. As a result of this commitment to liberal economic values, these industrialists were highly suspicious of Mussolini, an ex-socialist, and were positively hostile to many of his ideological associates such as Alfredo Rocco and Edmondo Rossoni, both of whom actively promoted state involvement in the private sector. Their misgivings were well founded. Once Mussolini took control of the state, the assault on their autonomy began in earnest and, much like high tides in Venice, with predictable results.

In spite of the over-all success of Adler's study, two shortcomings need to be highlighted. First, the book lacks a bibliography, a surprising omission in a heavily referenced scholarly work such as this. Second, the author seems to be unacquainted with much of the economic-history literature published after roughly the mid-1970s. Although his conclusions are, I think, unaffected by much of this more recent research, some of his observations about the economy are in serious need of updating.

JON COHEN, *University of Toronto*

Les aides americaines economiques et militaires a la France 1938–1960: Une nouvelle image des rapports de puissance. By Gerard Bossuat. Paris: Comite pour l'histoire economique et financiere de la France, 2001. Pp. 406.

This book adds to Gerard Bossuat's stature as the leading world authority on American aid to postwar Europe. The commencement date in the title, 1938, is not a typo but underscores one of the fundamental points Bossuat tries to drives home to his largely French readership. In that year, Jean Monnet, the central figure in this account, led an aircraft purchasing mission, created largely at his own behest, in a last-ditch attempt to prepare for the impending war with Germany. Its destination was the United States—a trans-Atlantic, officially neutral nation with far less experience in the manufacture of warplanes than his own. France's astonishing failure to mobilize economically in order to defend itself from the Nazi invaders not only exposed the hollowness of French claims to great power status, the author argues, but foreshadowed the nation's future dependence upon American material and financial assistance.

Over the next 20 years it would, he demonstrates with virtuoso flair, come in many sizes and shapes, flow in an almost unbroken stream, and carry a variety of significant long- and short-term consequences. Bossuat launches his account of the war with the telling reminder that the United States both provided food to Algeria and negotiated with Britain for a partial breech in the wartime blockade. The objects of the effort were to provision unoccupied France, reinforce Vichy neutrality, and perhaps even win the Petain government over to the allied cause. The episode provided a symbolic beginning for the critical future American linkage between France and its overseas territories.

Through Lend-Lease, the author proceeds, the United States later not only armed de Gaulle's Free French—while also subjecting them to American military leadership and training—but also supplied raw material, food, and machinery needed for reconstruction, a critical precedent. The subsequent 1946 Blum-Byrnes loan provided by Washington may have been disappointingly small compared to what Britain received, but, according to Bossuat, had a consequential result: negotiated by Monnet (who had also served as Lend-Lease broker between the United States and the Provisional French Government), it bound American assistance to France's economic modernization, as well as to the French *Plan de Modernisation et d'Equippement*, which he headed.

Monnet would be closely involved in almost every subsequent grant of American assistance, military as well as civilian. The importance of his personal role is hard to exaggerate. The author notes for example that "In 1958 Monnet obtained what no premier, minister of finance or defense, or ambassador had obtained since the war in Indochina . . . a borrowing facility of $655 million," that could (though indirectly) be applied to covering the costs of the Algerian war (p. 354).

Aid to Vichy, Lend-Lease equipment, and the Blum-Byrnes loan provided a mere warm-up for the Marshall Plan, which Bossuat characterizes as an unequivocally sound and essential bargain for France: for without American aid in modernizing, France would have remained hopelessly dependent on Germany, the United Kingdom, and of course the United States itself. Though he finds evidence of both American heavy-handedness and excessive French *amour-propre*, Bossuat reckons the Marshall Plan a diplomatic, as well as an economic success for France. Monnet failed, to be sure, in a bid to introduce indicative planning at the European level through the Organization of European Economic Cooperation. Yet, according to the author, France won acceptance of its overall strategy: sponsoring state-supported *grands projets* in order to shape-up the economy for future liberalization, at both the European and international levels. Bossuat might have strengthened the argument by including a discussion of the impact of the Marshall Plan on French participation in NATO, the OEEC, and the Schuman Plan.

Military aid began on a large scale with the premature termination of the Marshall Plan and its conversion into the mutual security program in early 1951. This kind of assistance held long-term implications of a more ambiguous sort. Without it, France could not have waged the war in Indochina except by handing over responsibility for defending the West to foreign armies. The French kept fighting only because the United States paid most of the bill. Yet during the very years of defeat and demoralization overseas, American offshore procurement (for NATO) provided the juicy contracts needed to advance the French military and civilian aviation industry to standards of international competitiveness. Bossuat might, by examining the influence of American nuclear technology, have nicely rounded off his main argument that U.S. aid enabled France to resurrect itself from Malthusian defeatism to the semblance of great power status.

Between 1938 and 1960, the French received about $15.3 billion worth of American aid at current values, an amount equal to a quarter of the total Marshall Plan package. Americanization, Bossuat concludes, has been the inescapable consequence of dependence, yet thanks to U.S. generosity and goodwill France experienced a "fabulous regeneration . . . with economic and demographic growth, industrial and agricultural modernization, competition and openness to (a) Europe . . . in which bilateral dependence has become trans-Atlantic interdependence" (p. 373).

JOHN GILLINGHAM, *University of Missouri at St. Louis*

The Allotment Movement in England, 1793–1873. By Jeremy Burchardt. Rochester, NY: Boydell & Brewer, 2002. Pp. x, 287. $75.00.

To the native Britain the observable landscape on rail and road journeys is punctuated by land uses that have become so familiar that they are ignored. One of these is the allotment; those regular rectangles of land of not many square yards whose patchwork of vegetables and fruits adds so much variation in color to an otherwise relatively unchanging landscape of cultivated fields and buildings. Not many of us stop to think of the origin of these havens of peace, pride, and industry, nor indeed of the contribution they make to the national economy. We take them for granted and naturally assume that they have a history which can be summoned from the library catalogue should we so need. Yet when the question is put and we search the indexes we find very little. Not any more, Jeremy Burchardt has provided a first port of reference.

This history developed in phases, beginning with a food supply crisis in the 1790s and the conjunction of the twin enemies of the time, the French externally and harvest failure internally. This first phase had limited impact. The second phase was a response to a wholly internal rural crisis in the late 1820s and early 1830s, characterized by unemployment and accusations of labor displacement by mechanization, a crisis reinforced by the threats from Captain Swing. Yet the crude geographical correlation between the intensity of allotment provision and the Swing riots is weak. This period also merges with the general age of reform when the limitations of democracy in land ownership and land access were particularly to the fore, not least in the rise of Chartism. Burchardt's story ends in the early 1870s when the first detailed national statistics were constructed, and on the eve of the next phase of internal rural crisis, the Great Agricultural Depression of the last quarter of the nineteenth century.

The sense of ownership that comes with working a patch of God's little acre induces extra effort measured in improved productivity. The size of the task is manageable with family labor and the resulting fruits are for personal consumption. The pay off in self esteem alone can be imagined, but what Burchardt also claims is that the productivity of that labor extended beyond marginal supplementation for the rural poor. He claims that the provision was crucial in maintaining living standards, to a degree that perhaps has not been fully recognized. This is a large claim. Does it stand up to the weight of evidence? Burchardt's calculations of the rate of return from renting allotments compared with rates of return on commercial property does not quite answer this question. More importantly, the allotments were relatively slow to develop, not everybody had access to them, and there were queues to acquire them. For those at the head of the queue they probably were a crucial supplementation, but how long was the queue? By 1873 there was one allotment for every three male agricultural laborers over the age of ten, with a greater density in the worst affected counties in the forthcoming agricultural depression. That is an impressive first measure of supplementation.

Yet the compromising posture of landed society in allowing the allotment movement to grow and develop should not be seen as benevolence or paternalism. It was a commercial deal, not a gift, even if the market price was heavily subsidized. This was no customary right of access, but a market contract. The attributes the allotment tenants substituted for ownership were measured in terms of respectability, independence, and self-help, but certainly not charity. Indeed the greater correlation of incidence in the 1830s seems to be with a reduction in poor rates, which questions paternalistic motivation and widening democracy, and points more to a ruling class selfishness. Other recent research, particularly by Leigh Shaw Taylor, has questioned the extent of common rights lost in the eighteenth and nineteenth century enclosure movement. If psychologically the provision of allotments was seen as a substitute for those imagined common rights then it does no harm to regard them in this way, but as with the common-right literature it does reinforce the undemocratic nature of past society, and the ease with which the underclass was easily held in check. Therefore if Burchardt's more-or-less concluding remark that "allotments restored land to labourers, but in a marketised form" (p. 236) seems too easily to ignore Shaw Taylor's emphasis on the narrowness of former common-right ownership, then perhaps it will serve the purpose of promoting further investigation into the access to land. A more telling conclusion might be that allotments provided laboring families with "a source of hope" (p. 165).

MICHAEL TURNER, *University of Hull*

Hobson and Imperialism: Radicalism, New Liberalism, and Finance 1887–1938. By Peter Cain. Oxford: Oxford University Press, 2002. Pp. ix, 320. £45.00.

In this book Peter Cain traces the shifting and sometimes contradictory views on imperialism held by J. H. Hobson from his earliest comments in 1887 to the publication of the third edition of *Imperialism: A Study* in 1938, two years before Hobson's death. Cain contrasts the polar extremes found in *Imperialism: A Study* (1902), where Hobson viewed international transactions as a source of exploitation, and *An Economic Interpretation of Investment* (1911), in which he exhibited more "Cobdenite optimism" and regarded international investment as a positive force for both sending and receiving nations. In the intervening years his thoughts on major issues such as the role of financiers and the importance of international trade changed substantially and his writings were especially contradictory.

Readers of Cain's book are likely to be more familiar with the first pole, but Cain argues that the views expressed in *An Economic Interpretation of Investment* were an important and persistent component of Hobson's thought. During World War I and the depression

Hobson shifted back towards *Imperialism*, but after the war to the mid-1930s his writings were closer in spirit to *An Economic Interpretation of Investment*. Similarly, Hobson is well known for his proposals to redistribute income within advanced capitalist nations. Cain argues that he offered international as well as domestic remedies for imperialism, depending on the political and economic circumstances of the time.

After the introduction, chapters 2 and 3 describe Hobson's evolution as a New Liberal thinker during 1887–1898 and his more gradual emergence as an anti-imperialist. Chapters 4 and 5 focus on the period of the Boer War and *Imperialism*, discussing the book's origins and providing a close textual analysis. Chapter 6 explores contradictions in Hobson's thinking between 1903 and 1914. Chapter 7 completes the chronology, covering Hobson's views from World War I through 1938, when he had returned to a more pessimistic outlook. The final chapter discusses Hobson's work in relation to more recent literature on foreign investment and the British economy and on the costs and benefits of imperialism. Drawing upon his extensive historical research with A. G. Hopkins on imperialism and the work of other scholars, Cain evaluates Hobson's arguments in relation to the cases of tropical Africa, Egypt, South Africa, and China.

Hobson often is viewed primarily as a precursor to Lenin and in relation to the Marxian tradition, with emphasis on his arguments concerning underconsumption and the "financial conspiracy" behind imperialist ventures. One of the important contributions of Cain's book is its richly detailed account of Hobson's relationship to radical and New Liberal thought. Hobson's commitment to free trade, for example, is explained in the context of a radical tradition that associated protection with privilege, "unearned increments," and autocracy. These ideas were prominent in the era of British landowner opposition to repeal of the Corn Laws. Hobson vigorously defended free trade after the launch of Chamberlain's tariff campaign in 1903, although this sometimes resulted in positions opposed to those advanced in *Imperialism.* Hobson's concerns about international investment have resonance today, as Cain points out in his closing footnote. However, many current critics of globalization do not share Hobson's faith in free trade.

In his closing chapter, Cain argues convincingly that Hobson was right that finance often was crucial to imperial expansion, but that he misunderstood the relationship between finance and policy, particularly in the South African case. He criticizes the financial conspiracy strand in Hobson's thought but claims Hobson did offer more subtle approaches, forecasting the emergence of a new kind of financial capitalism in Britain, more along German or American lines. Cain notes the resemblance of these ideas to Hilferding's; a more detailed comparison would have been welcome. This book will be of considerable interest both to historians of economic thought and to economic historians. It is a careful and thorough intellectual biography of an important figure and a valuable addition to the literature on imperialism.

CAROL E. HEIM, *University of Massachusetts, Amherst*

Hosiery and Knitwear: Four Centuries of Small-Scale Industry in Britain c.1589–2000. By Stanley Chapman. Oxford: Oxford University Press, 2002. Pp. xxiv, 328. £55.00.

The hosiery and knitwear industry of Great Britain has a long and fascinating history, despite the often prosaic nature of its products. The starting date of Chapman's book is that of the invention of the stocking frame, the most advanced textile machine of its time. Yet this was the last of the textile industries to embrace the factory system, much of its production remaining on a domestic basis until the late nineteenth century. In the twentieth century, the vagaries of fashion came to dominate the industry, as skirts became shorter, stockings became more visible and underwear ceased to be an unmentionable subject. The production of knitwear also overtook that of woven fabrics as casual clothing became the norm. Stanley Chapman has very successfully told this complex story in a book full of household names—Pretty Polly, Wolsey, Vedonis—and Marks and Spencer, whose domination of the retail aspect of the industry occupies many of the later pages of the book.

As Chapman says, this book has been over 30 years in the making, particularly the first chapter, which does not really take account of more recent research into the origins of the industry: the story of William Lee is based more on M. and A. Grass, *Stockings for a Queen* (London: Heinemann, 1967) than the articles published in *Knitting International* and *Textile History* by Eric Pasold and Negley Harte in the 1980s and 1990s. Nevertheless, because Chapman pioneered the use of insurance policies for industrial history, he has been able to add much more detail about early hosiers and knitters from these and probate inventories than other writers have succeeded in doing. The second chapter, "Enterprise and Innovation c.1750–1810," deals with significant technological developments such as the Jedediah Strutt's Derby rib frame, William Horton's warp knitting "which unites the flexibility of knitted fabrics with run-resist properties" (p. 70) and, of course, the beginnings of machine-made lace which came to dominate the town of Nottingham in the nineteenth century. By this time, over 90 percent of knitted goods were produced in the East Midlands. Hosiers, though, were still relying on banks for capital, outworkers for production, and small-scale marketing in a period that saw the development of powered mills in the cotton and woollen industries.

In the early nineteenth century, the change in men's fashion from knee breeches to ankle-length trousers spelled disaster for an industry where 90 percent of its output was still hosiery (p. 79). At least until the twentieth century, when fully-fashioned hosiery again became important, its salvation lay in the development of the "cut-and-sew" branch using wide frames, an innovation which the workforce were reluctant to accept. Marketing came to be dominated by the London wholesalers, who preferred to deal with large firms rather than a multiplicity of small ones, leading to the emergence of successful partnerships such as Ward, Brettle and Ward of Belper and I. & R. Morley of Nottingham. Nevertheless, even these large firms preferred to keep their workforce as outworkers and did not invest capital in powered factories. The latter only emerged with the invention of circular knitting machines in the 1850s, whose products had to fight for a place in a tradition still resistant to "cut-and-sew" products. Chapman's lengthy chapter on the transition to the factory system contains much new material based on such company archives as exist and charts the growth of both major and minor producers, the latter often making use of the marketing skills of the former: by 1907, I. & R. Morley were selling over 36 percent of all hosiery and knitwear produced in the United Kingdom through their Wood Street warehouse in the City of London (p. 158).

The period from the 1920s to the 1960s is described by Chapman as "boom time." Fully-fashioned hosiery once again became as popular as it had been in the eighteenth century, only this time worn by women rather than men. Knitwear, too, in the forms of both undergarments and casual jumpers and jerseys, was in great demand by both sexes. The period from the 1960s until the end of the century is dominated by changes in marketing rather than further technological innovation. Textile conglomerates such as Courtaulds and Coats Paton attempted to buy out as many firms as they could, but—as Chapman points out on p. 224—they were bulk producers of yarn and did not really understand the vagaries of the fashion end of the textile industry. Many firms disappeared, while others protected themselves by devoting the bulk of their output to retail chains such as Marks and Spencer. The latter is given a separate chapter, which almost reads like an article in its own right. A secretive firm, their archives are extremely thin and much of Chapman's penetrating analysis of the crisis caused by the dominance of M&S is based on oral testimony derived from his many contacts in the East Midlands hosiery trade. The firm forced their suppliers to abandon their own brands (Corah's "St. Margaret" had to give way to the M&S "St. Michael," for example), to neglect haute couture in favor of mass production and to sell to them rather than export their products. In the end, like most other firms, M&S was forced to seek offshore sourcing to cut costs, and many of their over-reliant suppliers collapsed. As Chapman says (p. 301), "a strategy that had bound the industry to major retailers for a couple of generations had suddenly fallen apart."

As his title suggests, Chapman attributes most of the problems experienced by the hosiery and knitwear industry to its concentration on small-scale production for the four centuries of its existence and to a largely hereditary leadership lacking in enterprise. Although economic and industrial historians have previously taken an interest in the first three centuries of the hosiery industry because of its long existence outside the factory system, Chapman's book is the only one to bring its study up to the present day and to analyze marketing as well as production strategies. It is a very worthy addition to the important Pasold Studies in Textile History.

MARILYN PALMER, *University of Leicester*

Africans and the Industrial Revolution in England: A Study in International Trade and Economic Development. By Joseph E. Inikori. Cambridge: Cambridge University Press, 2002, Pp. xxi, 576. £55.00, cloth; £19.95, paper.

In the book under review, Joseph Inikori argues that "the Industrial Revolution in England was a product of overseas trade—the first case of export-led industrialization in history" (p. 479). This bold assertion concludes a study that emphasizes the role played by slave labor and the commodities generated by transatlantic trade in the creation of the first industrial nation. The geographical scope of the book is broad: the entire Atlantic trading basin is covered, not just Britain and her transatlantic empire. The methodological approach is interesting, drawing on recent developments in international trade theory as well as much archival research. Detailed findings are prefaced by chapters presenting contextual material on the development of the English economy in the longue durée from the Domesday book up to the seventeenth century and on the historiographical trends in the interpretation of England's industrial revolution. In the latter discussion Inikori argues that since the mid-1980s historians have swung back towards greater appreciation of the role of foreign trade in England's industrialization. But this statement ignores some important recent works such as M. J. Daunton's *Progress and Poverty: An Economic and Social History of Britain 1700–1850* (Oxford: Oxford University Press, 1995), which takes a different view.

Much of the material Inikori presents about the role of slave labor and slave-produced commodities, and the trades and businesses associated with them, is familiar from the work of generations of historians. Indeed, he himself has written about these themes in many previous publications, though to what extent those discussions are embedded in *Africans and the Industrial Revolution* is not made clear. Inikori traces the volume of the British slave trade to the Americas over time, showing that Britain became the world's largest supplier of slaves to American markets in the eighteenth century. He charts the growth of English shipping associated with the slave trade and other branches of transatlantic commerce. He shows that financial institutions in England were boosted by the use of credit in transatlantic trade. He also discusses the significance of African-based raw materials for

English industrial production and synthesizes the literature on the commodity outputs achieved by slaves in the Americas. Many of these themes are summarized in my short book *Slavery, Atlantic Trade and the British Economy, 1660–1800* (Cambridge: Cambridge University Press, 2000), which is not cited by Inikori, but it is helpful to have a fuller treatment. One curious feature of *Africans in the Industrial Revolution* is that Inikori has avoided using the comprehensive statistical data found in David Eltis et al., *The Trans-Atlantic Slave Trade: A Database on CD-ROM* (Cambridge: Cambridge University Press, 1999). No reason is given for this significant omission.

The most contentious arguments appear in chapter 9, though they are prefaced in chapter 3. These chapters present the core arguments. Viewing the industrial revolution in England as a regional phenomenon based on the west Midlands, south Lancashire, and the West Riding of Yorkshire, Inikori argues that the chief characteristic of industrialization in those areas was technological innovation in the metal and textile industries. He further shows, as other historians have done before him, that each of those areas had a high proportion of its manufactures geared towards exports and specifically towards Atlantic trade. He suggests that these external demands caused the technological breakthrough in the metal and textile industries in England rather than these changes occurring in response either to spontaneous technological innovation or the pull of rising domestic demand. This is therefore an explicit denial of England's industrial revolution occurring as a result of supply-side factors or the diffusion of a wave of gadgets. Previous scholars have offered a favorable assessment of foreign trade's role at the beginnings of England's industrialization, but no historian has so emphatically argued for the supremacy of overseas trade in that process.

Economic historians will enjoy picking over the material presented in *Africans and the Industrial Revolution*. To my mind Inikori's arguments would have gained greater force if he had demonstrated how external trade impacted on domestic industrial production and technological change: this is asserted, not demonstrated with examples. This would involve looking at the timing of the growth in exports in the Atlantic trades in relation to the diffusion of new technologies in industry. Only by doing so could Inikori prove that external trade associated with Africa and the Atlantic world was the stimulus to technological change that he insists it was. What is needed in the future is a more sophisticated linking of the impact of external trade to the three regions mentioned, with reference to the diffusion of inventions in relation to the growth of exports; and to the interaction of domestic and external forces in creating rising aggregate demand in the later eighteenth century. As the analysis moves forward, *Africans and the Industrial Revolution* and its ideas will have to be taken account of seriously and, given the vast historiography on the creation of the first industrial nation, that is a worthy achievement.

KENNETH MORGAN, *Brunel University*

The Gold Standard Illusion: France, the Bank of France, and the International Gold Standard, 1914–1939. By Kenneth Mouré. Oxford: Oxford University Press, 2002. Pp. xiv, 297. $72.00.

Ken Mouré's book on French interwar monetary history is an interesting contribution to the historiography on monetary regimes. Based on a detailed reading of recently available archives at the Banque de France (as well as more commonly used central banks and Treasuries archives in France, England, and the United States), the book does not focus on the macroeconomics of the period but on the understandings and decision processes of the main actors of monetary policy, mostly in France. The author actually accepts the broad lines of the macroeconomic interpretations presented by what he calls the "new orthodoxy" on the interwar gold standard (e.g., Barry Eichengreen, *Golden Fetters: The Gold Standard and the Great Depression, 1919–1939*, Oxford : Oxford University Press, 1992; or Peter Temin, *Lessons from the Great Depression*, Cambridge, MA: MIT Press, 1989), but suggests that judging a monetary regime independently from the precise intellectual, political, and social environment of the actors is not a historian's job and can lead to simplistic interpretations. As stated in the conclusion: "The agency of the players must not be sacrificed in order to argue the determining force of regime principles or 'rules of the game'" (p. 260). This makes the book an interesting attempt at giving new weight to historical accounting of how history happened on a topic largely invaded in recent years by economists and political scientists. In order to fulfill this promise, Mouré gives much importance to individual opinions, interactions, and decisions, and much less to economic constraints. That rehabilitation of politics and individuals proves largely convincing in the interwar period.

The book begins with 5 chapters in chronological order, which deal in detail with the period not covered by Mouré's previous book (*Managing the Franc Poincaré: Economic Understanding and Political Constraint in French Monetary Policy, 1926–1936*, Cambridge: Cambridge University Press, 1991). It starts with a synthetic presentation of the classical gold standard, which the author presents as largely accidental and much less stable than what idealized retrospective visions argue during the interwar (and after). Chapter 2 describes the action of both the Banque de France and the Trésor during the war, when debt and money creation went side by side with the Banque's accumulation of gold previously held by the public. Chapter 3 analyzes the situation at the end of the war, showing that the understanding of the stakes of monetary policy was quite good both in Academia (Charles Rist), at the Treasury (Pierre de Moüy) and at the Banque (Jules Décamps). This contrasts with many remarks in J. M. Keynes's tradition that suggesting, until today, that a low understanding in monetary policy among French leaders was the reason for many "policy errors."

Chapter 4 confirms the political character of the conflict between the Banque and the government during the Cartel des gauches, and gives details on the scandal of the falsified balance sheets of the Banque, which deprived Premier Herriot of financial credibility without his being responsible for any wrongdoing.

Chapter 5 analyzes the stabilization process, detailing how reporting legal stabilization allowed Poincaré to stay in power until 1928 and to escape the constraints of both the Banque and foreign lenders. As concerns the level of the stabilization of the franc, Mouré closes an ongoing debate by recognizing that the level had not been set voluntarily low (125 francs per pound was much higher than the 150–160 suggested by Quesnay or Rist, and did not receive British criticism at the beginning).

Chapter 6 deals with central banks cooperation. Mouré shows that the interwar gold standard did not suffer from a lack of cooperative behavior among central bankers, nor from their theoretical disagreements, but mostly from political rivalries among nations. For example, the Banque of France wanted to obtain a status equal to that of the Bank of England, which had been at the center of all stabilizations in the 1920s and occupied a superior position in a gold-exchange standard which could be interpreted as a hierarchical system. All this had clear diplomatic implications which the British and Americans tended to neglect but the Banque de France insisted on, as Mouré shows clearly.

One regret is that the discussion on international matters is treated separately and appears as independent from the French policy studied earlier, because many of the determinants of French policy were international (such as reparations and war-debt settlement in the 1920s and the democratic alliance against fascism in the 1930s).

In chapter 7, Mouré presents a synthesis of the main debates on the 1928–1936 period, without repeating the contents of its previous book, but answering some critics. This is the chapter where the methodological divergence appears most clearly between the regime-wide judgments of the "new orthodoxy" on one side, and both the historian's emphasis on the representations of the gold standard in the french elite and the irreversible impact of historical accidents and decisions on the other. The gold standard appears then as a rigid system, the destruction of which resulted from a succession of accidents and inadequate decisions in a conflictual world, more than from its causal role in the onset of the Great Depression (a typical American preoccupation that forgets the realities of the European interwar).

Chapter 8 deals with the post-1936 period. It is less clear on the macroeconomics of the period, giving, for example, no satisfactory explanation for the failure of the devaluation. The evolution of ideas on monetary policy is studied in particular in relation to the transformations of the functioning of the Banque de France and the debates on open-market policy, which is a somewhat restricted point of view. Mouré insists that the French were exceptionally devoted to the gold standard. One should nevertheless notice that the survival of the term "gold standard" as a metaphor for perfection until today, as shown in the book's last pages, is an American and not a French phenomena.

The book's main novelty is the discussion of the economic "orthodoxy" which considers the interwar gold standard as a monetary regime that intrinsically could not work. This is not replaced by a defense of the gold standard but only by the suggestion that only the path of events and the interaction of representations with new situations led to the gold-standard failure, which should then be considered as a historical process more than from a theoretical point of view. Many historians will agree and economists disagree.

They should instead regret the lack of some micro and macroeconomic analysis, especially in the last chapter for which the existing literature does not provide satisfactory background. More generally, a presentation of the functioning of the French financial system, which was an essential constraint on monetary policy during all the period, is clearly lacking. For example, a discussion of the exchange controls both in the 1920s and the late 1930s would have been useful. More profoundly, Mouré debunks convincingly the "gold standard illusion," but sometimes seems to be falling into another one, the Greenspan illusion that "fine tuning" by central banks can remedy any disorder.

Even considering these small shortcomings, Mouré's book makes an interesting read and a convincing presentation of the interwar gold standard's difficult birth and inevitable fall.

PIERRE-CYRILLE HAUTCOEUR, *Université de Paris 1 Panthéon-Sorbonne*

The Political Economy of British Historical Experience, 1688–1914. Edited by Donald Winch and Patrick K. O'Brien. Oxford: Published for The British Academy by Oxford University Press, 2002. Pp. x, 453. £40.

This thick volume is part of a series published for the centenary celebrations of the British Academy whose aim is "to demonstrate the vitality of British scholarship at the start of a new millennium." As such, one might expect it to be a mainstream tribute rather than a cutting-edge innovative project. But it proves to be much more than the first. The papers were presented at two colloquia to provide authors with ample discussion and interaction. The editors, Donald Winch and Patrick O'Brien, initially intended to base the collection on the theme of "peculiarities," but had the flexibility to drop it from the title when the theme turned out to be conceptually problematic and not shared by all contributors. Winch wrote an introduction that does much more than just interweave the individual abstracts. The end product is a coherent collection. The interaction among the authors and the chapters is well felt. Common themes do emerge. The coverage of the various topics is impressive.

The 16 essays are grouped in five parts: The view from abroad (Emma Rothschild, Gareth Stedman Jones, James Thompson); Land tenure (F. M. L. Thompson, Peter Grey); Empire, free trade and protection (Kenneth Morgan, Anthony Howe, Frank Trentmann); Fiscal and monetary regimes (Obrien, Julian Hoppit, Forrest Capie, Martin Daunton, G. C. Peden); and Poor law and welfare (Joanna Innes, Jose Harris). Because, in a short review of a lengthy collection of essays, there is not way to do justice to each of them, my compromise will be to discuss four of the articles and three general themes.

O'Brien's essay criticizes as too narrow the "New Whig" interpretation that places the Glorious Revolution and the shaping and protection of property rights at the center of Britain's rise to political and economic power. He views the Civil War, rather than 1688, as a crucial turning point in public finance after which the English, partly out of necessity, became innovative. Britain's own low starting point, and the sclerosis experienced by her rivals in the seventeenth and eighteenth centuries, enabled Britain to finance her rise to world power in the eighteenth century. When she exhausted her ability to finance more wars, in the aftermath of the Napoleonic Wars, she had already established a hegemony that lasted for nearly another century.

Hoppit underlines the fact that at a time when the government and its expenditure grew, the attempts, led by Parliament, to check the executive also, dialectically, intensified. These attempts took the form of itemizing and limiting in time expenditure legislation, insisting on more detailed accounts and more strict accounting procedures and appointing committees of inquiry into various aspects of government activity. Nonexistent in 1688, a significant degree of supervision and accountability had been achieved by 1832. But even then, making foreign policy and wars remained the exclusive prerogative of the executive. As long as this was the case Britain remained a profligate warfare state, whose expenses were predominantly military; their level depended on executive policy and on foreign powers.

Rothschild investigates the view of foreign observers with respect to the revolution they believed Britain was experiencing in the second half of the eighteenth century. Less impressed by technological inventions and industrial production than some later historians, they were fascinated by advances in communication, information, correspondence, and intelligence. They were impressed by the system of public, but also of private, credit. They considered the economic press exceptional in quantity and quality and attributed its continuous flourishing to the political guaranty of freedom of expression and information.

Innes expands upon one of the systems that fascinated Rothschild's foreign observers: the Poor laws. She examines the comparative element in the contemporary debate. Only when the English became aware of Scottish, Irish, and European observations on the nature of the difference between the various systems, did they realize that their system was distinctive: it was tax-based and granted legal entitlements.

Information emerges as one of the common themes of this collection. It is not the talents of the entrepreneurs and the statesmen, nor the ingenuity of technological inventors that makes the difference. What really changes in eighteenth century Britain, and makes it "peculiar," is the flow of information made possible by the political and institutional infrastructure. This flow is essential for taxation and borrowing, for the stock market, for overseas commerce, for innovations and inventions, and for an enlightened political discourse.

Though "peculiarities" was formally dropped from the title of the collection, comparison still figures high among its common themes. Some of the essays in this collection contrast Britain with Continental Europe, particularly with France and Germany. A few articles compare the Irish, Scottish, and colonial North American experience with the English one. Several of the articles present less conventional comparisons, not of realities but of conceptions. They examine the views of Continental contemporaries as to the nature of the transformation that Britain experienced in the eighteenth and nineteenth centuries with the views of British contemporaries or of later historians.

The concept of political economy is the last common theme to which I wish to refer in this short review. This book is the result of collaboration among historians of ideas, historians of the state, historians of the economy using historical methodology, and historians of the economy using economics methodology. The explicit exchange of ideas among the authors, as well as the overall effect of the collection on readers, exemplify the potential of interaction among various types of historians.

Today, when the information available to individuals, the limits on their rationality, and their process of learning loom high in economic theory; when the policies, rules, and structures of institutions combine; when the borderline between the market and the state is becoming obscure; the insights that can be gained from this book by reflective readers are indispensable.

RON HARRIS, *School of Law, Tel Aviv University*

Population and Society in an East Devon Parish: Reproducing Colyton 1540–1840. By Pamela Sharpe. Exeter: University of Exeter Press, 2002. Pp. xv, 408. £45.00.

Social and economic historians are well versed on the demographic history of the parish of Colyton. In the 1960s Tony Wrigley of the Cambridge Group pioneered the family reconstitution method of nominal record linkage using the impressively intact parish register of Colyton. Subsequently, the demographic history of the parish has attracted much academic attention. Pamela Sharpe builds on this research but aims to go further by situating demographic trends within the wider history of the parish. Thus chapter 2 of the book explores the religious and political background of the community, whilst chapters 3 and 4 focus on economic issues. Demographic trends by sex and class are examined in chapter 5. Finally, the effect local politics and policies had on Colyton families and households are assessed in chapters 6 and 7. Sharpe achieves her aims by using the total reconstitution method, a technique that involves exploiting additional parochial records (such as tax, ecclesiastical court, manorial, and poor relief material) to enhance the demographic details already uncovered.

Although the chapters proceed with the history of Colyton thematically, the interconnections between religion, economics, politics, and society are a dominant motif throughout. Colyton in the seventeenth century was a Puritan stronghold. By the 1640s nonconformist belief was overtly politicized, thereby sometimes setting this anti-authoritarian Devon parish at odds with national policy. The Chamber of Feoffees was the body that provided coherence to this outlook, and its control over parish policy and governance in the seventeenth century led to attempts to control those at the margins of the parish. Although the role of the Colyton middling sort is shown to have played a pivotal part in transmitting godly zeal (they formed the bulk of those who fought for the Duke of Monmouth in 1685 for example), Puritanism also enjoyed support among the gentry and the poor. Seventeenth-century Colyton was also a thriving industrial community, with textiles dominating the production process. The importance of local, national, and overseas markets again suggest this was not a backward, isolated southwest parish. Although the cloth trade remained buoyant despite the ravages of war and plague, it was the lace trade that had a much greater significance on the demographic history of the parish. Periods of prosperity in lace-making coincided with a time of rising age of marriage for women in the seventeenth century. The trade attracted women into the parish, whilst promoting the out-migration of men. This skewed the sex ratio and significantly influenced women's chances of marrying. Poor women were most likely to marry late and had few prospects of remarriage if widowed. During the eighteenth century the industrial prosperity of the parish declined, as did the attraction of Puritanism. Colyton became almost wholly reliant on the agricultural sector, increasing the power of the lord of the manor and the division in the parish between the landed and the landless. Employment in arable farming was male dominated, so balancing the sex ratio and ultimately prompting a fall in the age of marriage for women. These changes also had an impact on relief policies. Whereas elderly women had been the most likely recipients of relief in the seventeenth century, by the late eighteenth century the system became "male orientated," with family allowances the usual method of relief.

How successful is this approach in bringing to life this east Devon parish? This is a dense book but it is a testament to Sharpe's knowledge and handling of the sources that it is ultimately successful in one of its key aims—"injecting" class and gender into the analysis. The extensive employment of women in the domestic industries, for example, emerges as a crucial factor in the history of the parish and warns against dismissing women's work of this type as secondary or unimportant as many economic historians have previously done. Looking in detail at the experience of marriage, mortality rates, childbirth and child rearing, apprenticeship and service, Sharpe also shows that large numbers of women, men and children lived outside a "traditional" family structure, a phenomenon which is all-too-often considered a very modern one by uninformed political commentators. The patchy coverage of surviving sources from Colyton means that there are problems with the total reconstitution method. However Sharpe never shies away from confronting the gaps and weaknesses. Indeed she shows how the process of record collection and survival was itself often influenced by the political and religious conditions prevailing in the parish. Despite the considerable gaps in the historical record, the interweaving of individual life stories with analysis of population trends, economic conditions, politics, and religion makes this a human story too.

NICOLA VERDON, *University of Reading*

West German Industry and the Challenge of the Nazi Past, 1945–1955. By S. Jonathan Wiesen. Chapel Hill and London: University of North Carolina Press, 2001. Pp. 329, viii. $39.95.

This book has a limited aim: to reveal how German big businessmen tried to come to grips with the Nazi past in the ten years following defeat. Left unexamined are the adaptation of producers to new markets and institutions, changes within the structure of industry, and in the relationship between industry and the state, as well as a couple of overriding questions. Was the moral challenge met successfully or not? Might better results otherwise have been obtained, or obtained sooner?

Although Jonathan Wiesen couches this study of "business mentalities" in the language of "historical memory," what he in fact shows are the many ways in which the leadership of German industry tried to sweep the recent past under the carpet. The author supplies welcome detail and description to a story whose outlines have long been well known. Faced during the postwar occupation with threats of trust busting, socialization, confiscation, and dismantlement—not to mention arbitrary imprisonment—the former titans of big business pled innocent to complicity in Hitler's evil-doings and, when that failed, begged for mitigation. Death-bed confession was absent. To a man, the one-time power-wielders felt victimized. This collective reaction may not elicit much sympathy, but it is easily understood:

until the 1948 currency reform shifted the wind, the big industrialists were unable to defend themselves against their Allied accusers. The less they said about the past the better.

Thanks to their belated discovery of public relations, Wiesen argues, the lot of the employers gradually improved. They were indeed slow learners. Only in the mid-1950s, did their paid image-makers make any attempt to shift the discourse to the New Consumerism—the joys of home ownership, the existence of abundance, and the opportunities for upward mobility and the development of the "whole man" (instead of the factory cipher). On the other hand, the smoke-stack barons devoted much energy to revising the bad public image of Albert Voegler (the badly-compromised CEO of Vereinigte Stahlwerke during the 1930s), reviving factory paternalism, and refurbishing the international image of traditional German industrial products. On the cusp of the Age of Elvis, they courted the public by generous donations to museums and symphony orchestras. Allegations of labor exploitation, Jewish or otherwise, were generally overlooked but occasionally met with outraged denial. Employers remained generally suspicious of "mass democracy," the author concludes, and held its excesses responsible for "totalitarianism," in Germany as elsewhere.

Such attitudes do not suggest an "Americanization" of business. Wiesen does, however, find evidence of a growing acceptance of the market as legitimate mediator of change. ORDO—the Freiburg School associated with Roepke, Mueller-Armack, Eucken and, of course, their disciple Ludwig Erhard—was the mainspring of this fresh development. The author appropriately notes that the competition principle central to this approach did not sit well with the revival of the traditional trustified German economy. He might, in concluding, have told the reader whether by the mid-1950s a new entrepreneurship was in evidence and, if so, examined origins and impacts. Such a quibble hardly detracts from the value of this interesting and worthwhile book.

JOHN GILLINGHAM, *University of Missouri at St Louis*

ASIA, PACIFIC, AFRICA, AND LATIN AMERICA

Negotiated Empires: Centers and Peripheries in the Americas, 1500–1820. Edited by Christine Daniels and Michael V. Kennedy. New York: Routledge, 2002. Pp. 328. $80.00, cloth; $19.95, paper.

I have before me an edition in facsimile of *Recopilación de Leyes de los Reynos de las Indias. Mandadas Imprimir, y Publicar por la Magestad Católica del Rey Don Carlos II* (Madrid: Julián de Paredes, 1681). It is four volumes long; runs to some 1,200 pages; and took more than a century to compile. Is it too much to call these volumes the legal backbone of the Spanish Empire? Probably not. When printed, copies were shipped to every corner of the Indies, whose courts took every jot and tittle seriously. Especially Volume 2, Book 6, Title 2, Law One: "That the Indians be free, and not subject to servitude." Funny thing, that directive. As any modern historian would explain, and as an honest contemporary would likely have admitted, the Indians were anything but free. Nor could they have been. Indians worked so that Spaniards did not have to, at least one version of the imperial story insists. You have to tolerate more than a little cognitive dissonance to be a student of the Spanish Empire, and you wonder if the Spaniards themselves did not as well.

The same could be said of most any early modern empire, to judge from the essays in this thoughtful collection. One of the things I always supposed, good Hispanist that I am, is that the British were more efficient at administering their colonies than were the Spaniards or the Portuguese, to say nothing of the French and Dutch, about whom, unfortunately, I remain largely uninformed. But many of the essays here, including those by Amy Turner Bushnell, Leslie Choquette, Jack Greene, Elizabeth Mancke, and Wim Klooster, imply that this is a peculiarly Hispanic version of the "grass is always greener" beyond the line story, so to speak. Imperial undertakings everywhere were, as the title *Negotiated Empires* holds, a negotiation, an act of measured compromise between the ostensibly sovereign and the ostensibly subject. If there were no absolutist states (were there?), it follows that absolutist empires were similarly chimerical. Authority flowed not only from top to bottom, but also from bottom to top. The reasons are not far to seek. As Fernand Braudel recognized years ago, geography, time and distance were every monarch's enemy, regardless of nationality. Regulations, laws, administrative procedures, even language, and social and cultural practice were garbled in their translation from the Old World to the New. Before the eighteenth century, the Spaniards, perhaps, attempted to control more than did the other powers, but this obviously reflected the challenge of trying to govern millions of native Americans on terms other than their own. In general, the Iberians claimed to have God, or at least Pope Alexander VI, on their side, but alas, the Pope had no legions, and violations of the Treaty of Tordesillas (1494) were apparently considered less than mortally sinful by their co-religionists, the French. And in those outlying areas, the peripheries or frontiers in which virtually everything was a state of flux, the locals might take it into their heads that the authority of Mexico City, Rio de Janeiro, or Lima, let alone London, was too remote to bother about, let alone to pay for. Although frontiersmen were considered obstinate at best and positively rebellious at worst, the future belonged to them, and for the Spaniards at least, Humpty Dumpty never got put together again after 1825. The British, of course, looked elsewhere, and to the Orient, from which their historiography may never recover, but not before altering the subsequent history of Brazil by escorting the Portuguese Court to Rio de Janeiro in 1808. So much for the "periphery" of empire.

Finally, what of those "free" Indians who cost their erstwhile masters and themselves so much blood and treasure in Mesoamerica, the Andes, and elsewhere? Well, Evsey Domar got it right, at least to a first approximation. The distinct contribution of this volume, justly described as "richly comparative" in the blurb, is to show the great variety of circumstances under which material constraints (and principal–agent problems) intervened to change the face of the Americas forever, but only sometimes, for good. Even Alexander Gerschenkron, an economic historian who reputedly knew the most, would have found something of interest here. May lesser mortals among us take note.

RICHARD SALVUCCI, *Trinity University*

The Power and the Money: The Mexican Financial System, 1876–1932. By Noel Maurer. Stanford, CA: Stanford University Press, 2002. Pp. xiv, 250. $60.00.

One of the most important and enduring features of the Mexican banking system is its high degree of concentration. In 1909 there were 41 formally incorporated financial institutions in Mexico (in contrast to 18,723 banks and trust companies in the United States). Of those 41 institutions, two banks—Banco Nacional de México (Banamex) and Banco de Londres y México (BLM)—controlled more than half of the assets in the banking system. Ninety years later, the number of financial institutions in Mexico has decreased, with the largest two still controlling approximately half of the banking system's assets.

What explains the emergence and persistence of this concentration? What impact has it had on the Mexican economy? These are the questions that Noel Maurer addresses in his well-researched and marvelously written book that focuses on two periods of Mexican history: "Porfirian Mexico," which ran from 1876 to 1909 and was named for President Porfirio Díaz; and the 1910–1932 period that encompasses the Mexican Revolution and various episodes of institution rebuilding.

For Maurer, the story of Mexico's financial development is "fundamentally political." That is, the highly concentrated banking system is largely explained by the existence of an undemocratic political system and selective enforcement of property rights.

Maurer uses the logic of game theory to explain the emergence of the concentrated banking system. Limited in its ability to raise taxes to finance infrastructure projects and fend off political rivals, the government relied on banks as a source of credit. Banks, in turn, relied on the government to enforce property rights so that financial intermediation was profitable. The potential for coordination failure existed because the government, unhindered by the rule of law, had the incentive to act opportunistically by defaulting on debt, selectively enforcing contracts, debasing the currency, and expropriating bank assets. Knowing this, banks had less incentive to serve as intermediaries. Sub-optimal equilibria could arise where the financial system remained underdeveloped and the government, without access to credit, risked being overthrown. To avoid this outcome, governments needed to tie their own hands and make credible commitments to protect property rights.

The Porfirian government did so by creating Banamex in 1884 and making it the sole lender to the government. In theory, a single lender could easily impose a boycott on future lending if the government behaved opportunistically. This made the government less likely to default and raised its borrowing capacity. In return for funneling credit to the government, Banamex was given lucrative privileges, such as the ability to branch nationally. Nevertheless, political constraints prevented the complete monopolization of banking. For instance, the government was forced to extend many of the privileges enjoyed by Banamex to the politically connected BLM. In addition, to maintain political support of the state governors, the federal government gave each "the right to distribute a single federal banking charter to selected cronies" (p. 34).

The fragile Porfirian institutional arrangement was shattered by the revolution of 1910. Driven by "fiscal desperation" and the "exigencies of defending themselves against their rivals" (p. 135), various revolutionary and counterrevolutionary regimes began preying on bank resources. They did so by pressuring banks to make loans, using government printing presses to collect seigniorage revenue and expropriating bank specie reserves. Lending and check-clearing operations collapsed, and by 1925 the real assets of the banking sector were 34 percent of their 1912 level. Maurer argues that the competing political factions did not deliberately set out to destroy the financial system. Rather, they knew that if they did not plunder the banks "they would be defeated in the battlefield by someone who did" (p. 135).

Recognizing the importance of the banking system to its survival, the government attempted to reestablish the Porfirian rules of the game. Bankers were allowed to write new banking laws that erected extremely high entry barriers. To prevent itself from behaving opportunistically, the government created Banco de México (Banxico) in 1925. Banxico coordinated private lending to the government and was capitalized with tax revenues, much of which was stashed outside the country. If the government changed the rules of the game, it lost the gold. In addition, Banxico shared its monopoly rents with powerful government officials who were, as a consequence, less likely to kill the goose that laid the golden eggs.

How did the concentrated banking system affect the economy? According to Maurer, each bank "grew fat in its protected niche, engaging in monopolistic behavior and dragging down the rest of the economy" (p. 87). To overcome the problems associated with information asymmetry, banks lent largely to their own shareholders and other "insiders." Insider lending contributed to high levels of industrial concentration and reduced productivity growth. For instance, Maurer's detailed analysis of the textiles industry shows that the "fastest-growing firms were not the *best* firms; they were the *best-connected* firms" (p. 115).

Despite these findings, Maurer takes issue with scholars who point to kinship-based business practices as an impediment to economic development in Latin America. He argues that banks served as the engines of economic development "not merely in spite of insider lending but because of it" (p. 94). Instead, the fundamental problems afflicting Mexico were: limits on entry into banking that reduced the number of firms with access to insider lending, and poorly defined property rights that prevented those excluded from the insider networks from pledging collateral and contracting around these constraints. Had property rights been well enforced, impersonal mechanisms could have substituted for personal networks in Mexico. However, historical circumstances made this impossible and the "second-best theoretical option was the first-best *available* option" (p. 11).

The Power and the Money provides an excellent case study of the role that political and property right systems play in financial development.

J. PETER FERDERER, *Macalester College*

Ensayos de Historia Económica: Uruguay y la Región en la Economía Mundial 1870–1990. By Luis Bértola. Montevideo: Ediciones Trilce, 2000. Pp. 199.

Ensayos de Historia Económica offers an outstanding economic history of a Latin American country often neglected in the literature but fascinating as a comparative case. Yet this volume offers much more as well, including the author's experienced commentary on economic theories of growth and development and, most importantly, an examination of the economic history of Argentina, Brazil, and Uruguay set in relation to international trends. Although the book's audience in the United States will likely be limited to Spanish-reading Latin Americanists, this volume makes a strong contribution to broader comparative studies of economic growth and especially the literature on international convergence. Luis Bértola, Professor of Economic History at the Universidad de la República in Uruguay, is that country's leading economic historian and has published widely on long-term development and convergence themes in the Southern Cone and on related comparative themes. For non-Spanish readers, his dissertation and a comparative study have been published in English (*The Manufacturing Industry of Uruguay, 1913–1961: A Sectoral Approach to Growth, Fluctuations and Crisis*. Institute of Latin American Studies of Stockholm University, no. 20, 1990; *Argentina, Brazil, Uruguay and the World Economy: An Approach to Different Convergence and Divergence Regimes*. Facultad de Ciencias Sociales, Universidad de la República, Montevideo, 1998). This volume collects five previous essays, prefaced by two overview chapters on the state of economic history. Three comparative essays are all coauthored, with Gabriel Porcile (chapter 3), with Reto Bertoni and María Camou (chapter 4), and with Fernando Lorenzo (chapter 5). The strengths of the scholarship presented in this volume are multiple and run through all of Bértola's work: the reconstruction of long-term statistical series on the national economies of Uruguay, Argentina, and Brazil, close attention to the social and particularly the institutional contexts of economic growth (informed by the new institutional economics), and a persistent and insightful comparative vision.

Part 1 of this volume (chapters one and two) presents Bértola's lamentful commentary on the divergent paths of mainstream economics and economic history and his goal of seeking "reconciliation." In Uruguay, the small subfield of economic history has arrived at a place not entirely unlike that of the field in the United States, but one that was long influenced by a different set of factors, especially through the 1960s and 1970s: *Annales*-school history, Marxist (*más ó menos*) explanations for underdevelopment and dependence, and the challenges of scholarship under 30-odd years of dictatorship. Part 2 presents the heart of the volume in three comparative and empirical chapters on long-term economic growth (1870–1990) in the three Southern Cone countries. The first uses reconstructed series of national product for Argentina, Brazil, and Uruguay to set against the work of Maddison, Williamson, and others on convergence and divergence in the international economy (and Bértola usefully includes the full series on national GDP and export volumes for the three countries here, in an appendix to chapter 3). Why have rates of growth differed over the long term? Although Bértola's theoretical approach to convergence draws heavily on the work of Anthony Thirlwall and the potential for technological learning, the conclusions are broadly stated and suggest a complex and contingent story. Of particular note here is the divergent pattern of Argentina and Brazil: in the post-1930 period both presented a less "open" economy (measured as the relation between export volume and real GDP) under import-substituting regimes, yet where Argentine growth slowed dramatically and diverged from North Atlantic growth, Brazil experienced significant catch-up until the 1980s. The evidence indicates that explanations for the variety of growth experiences within the Southern Cone and the world more broadly need to be sought in a variety of factors: the so-called commodity lottery, institutional regimes, and the distribution of economic activity across economic sectors. Indeed, it is the particular relation between technological learning, institutions, and structural change that the authors highlight as central in explaining convergence patterns.

The second comparative chapter examines the relationship between patterns of Southern Cone convergence and divergence and the behavior of wages, income distribution, and technological learning. What results are insights into the persistence (and sometimes deepening) of wage disparities and income inequalities—mostly pessimistic concerning the late-century era of liberalization. Finally, the third examines cyclical components of long-term growth in the Southern Cone over the long term. The final two chapters in this book examine two phases of Uruguayan economic history: early industrialization—placing Uruguay firmly within broader Latin American trends between 1870 and 1930—and, more narrowly, the impact of the First World War. It comes as little surprise that the rather extensive range of new industries that appeared just before and after the turn of the century did so under a protectionist umbrella, although whether tariff policy was primarily a fiscal tool or a development tool is left unresolved.

Although among the South American countries, Argentina has attracted the most concerted attention of economic historians over the past several decades (from Roberto Cortés Conde and Carlos Díaz Alejandro to Alan Taylor), this volume demonstrates that the field is thriving throughout the Southern Cone. The scholarship presented here, together with the broader body of work produced by Luis Bértola and his collaborators, adds substantially to a foundation for future efforts to produce an economic history of the region.

EDWARD BEATTY, *University of Notre Dame*

Straining at the Anchor: The Argentine Currency Board and the Search for Macroeconomic Stability, 1880–1935. By Gerardo della Paolera and Alan M. Taylor. Chicago: The University of Chicago Press, 2001. Pp. xviii, 275. $35.00.

With perceptiveness and scholarship, Gerardo della Paolera and Alan Taylor explain Argentina's extraordinary rise to Western Europe's economic standards by 1913 and its equally dramatic fall from grace. In doing so, they answer the following questions: During the 1880–1935 period, why were the regimes governing Argentina's money and banking system established and discarded? And how did the regimes function and perform? In the end, *Straining at the Anchor* is yet another testimonial that confirms Karl Schiller's pithy remark: "stability might not be everything, but without stability, everything is nothing." (Quoted in David Marsh, *The Bundesbank: The Bank that Rules Europe*. London: Mandrin, 1992: 30.) Argentina boomed when it embraced rule-bound regimes that delivered price and exchange-rate stability and floundered when it flouted the rules or changed them to allow for discretion and, yes, monetary mischief.

Argentina's *Caja de Conversión*—the book's institutional centerpiece—was established in 1890, operated for brief periods of its life as an orthodox currency board (not from 1880–1935, as the book's subtitle would have us believe) and was liquidated and replaced by a central bank in 1935. The "convertibility" system Argentina had from April 1991 to January 2002 was, therefore, nothing new. Every misstep under convertibility had an echo during the 1890–1935 period.

Like the convertibility system, the *Caja* was for most of its existence not an orthodox currency board system. Gold outflows were offset by a rapid rise in net domestic assets and by 1932 the gold backing of outstanding peso notes was 43 percent (p. 202). In 1914 the *Caja* was empowered to rediscount commercial and government paper, and it did so after 1931 (pp. 200–02). Internal convertibility was suspended from 1914–1927 (pp. 165–77, 186–87). State-owned banks lent to the government (see pp. 70–74, 76–79). Provincial quasi-monies, widely in circulation today, also have a rich history in Argentina, although they were not present during the existence of the *Caja* (pp. 12–15, 23).

My dissatisfaction with *Straining at the Anchor* is not with the authors' story of the 1880–1935 rise and fall of Argentina; it is with their rather superficial treatment of currency boards and Argentina's contemporary monetary history. The authors assert "this book speaks directly to present-day debates on the merits of opening capital markets, managing exchange rate regimes, and coping with the tradeoffs and constraints that are the occupational hazards of an small open economy in a globalized world" (p. 15). Contrary to the authors' claims, though, they only speak indirectly to present-day debates. Their historical study risks engulfing the current debate about currency boards in even more confusion.

The authors imply that the *Caja* always operated as a currency board. Had the authors delved into the operation of currency boards, their graphs of the changes in base money versus changes in gold (p. 198) alone would have led them to the conclusion that the *Caja* did not operate as an orthodox currency board from 1914 to 1927 and 1929 to 1935. The authors fail to identify and diagnose the *Caja*'s other deviations from orthodoxy because they have failed to define carefully what a currency board is and how it operates.

They further wrongly assert that the convertibility system was a currency board, whereas in reality it was more like central banking than an orthodox currency-board system in many important respects. An orthodox currency board holds foreign reserves of at least 100 percent but typically no more than 110 percent of its monetary liabilities; it holds no domestic assets. Accordingly, it has no latitude for sterilized intervention and cannot engage in discretionary monetary policies. The convertibility system had a floor of 67 percent, but no ceiling, and the central bank held domestic assets. Consequently, the central bank could engage in sterilized intervention, and it used those powers extensively. After 1994, its net domestic asset position was over six times more volatile than that of Chile's central bank, which operates a floating rate.

In broad-brush strokes, Argentina's past has become its present. However, *Straining at the Anchor* contains a methodological shortcoming: it lacks an anchor. Absent an intelligible explanation of how orthodox currency boards are supposed to work, readers are left adrift, without an understanding of how deviations from orthodoxy can cause currency boards to malfunction. In consequence, the wealth of historical facts presented cannot be interpreted lucidly and Argentina's past and present travails with a currency-board system cannot be understood. That said, the authors admirably tell and support their story about Argentina's rise and fall during the 1880–1935 period.

STEVE H. HANKE, *The Johns Hopkins University*

Encumbered Cuba: Capital Markets and Revolt, 1878–1895. By Susan Fernández. Gainesville: University of Florida Press, 2002. Pp. xii, 203. $59.95.

Encumbered Cuba has a provocative thesis: poor colonial institutions led Cuban sugar planters to increasingly rely on American credit, creating political pressures in favor of independence or association with the United States. Susan Fernández argues that Spanish rule failed Cuba for two interrelated reasons. First, financial institutions and laws designed for Spain failed to promote economic growth in Cuba. Second, the institutions governing finance in Cuba, like their metropolitan predecessors, were designed to meet the instrumental needs of the government, not the private sector. Spain, therefore, trapped itself in a vicious circle. Deficiencies in domestic credit institutions caused economic actors to turn towards American substitutes. These same deficiencies prompted Cuban opposition to Madrid. Suppressing that opposition required resources, which in turn made it increasingly difficult to reform the offending institutions. The end result was Cuban "independence" as a protectorate of the United States.

Fernández's book explores a fascinating topic for economists, political scientists, and historians. Economists will be interested in what the Cuban experience shows about the role of financial systems in economic growth. Political scientists will be interested in the question of how the instrumental needs of the government determine economic policies. Historians will be interested in the thesis that Spain's economic mismanagement was a key factor in prompting Cuban independence.

Fernández opens her analysis with an overview of the state of the Spanish economy and financial system in the nineteenth century. From 1864 onwards, the financial system "became more firmly tied to the needs of the state." The privately owned Banco de España received a range of special privileges from the government in return for cheap credit. In fact, by 1888 the Banco de España collected all direct taxes and became, in effect, a tax farmer. These arrangements were designed to meet the Spanish central government's appetite for credit.

Chapter 3 recounts how these arrangements were replicated in Cuba. The Banco Español de la Isla de Cuba (BEIC) and Banco Hispano-Colonial (BHC) functioned much like the Banco de España. The BEIC began to finance the government during the Ten Years War (1868–1878). Many of these loans were, in fact, interest free! In return, the BEIC was given lower reserve requirements than its competitors. Later, it received responsibility for all internal tax collection, on which it earned a 5 percent commission. This massive giveaway to the BEIC's owners benefited the colonial government in two ways. First, they enjoyed cheap (or even free!) credit from the bank. Second, the percentage of owed taxes that were actually collected rose from 65 to 98 percent after the BEIC took over tax collection. The BHC essentially replicated these arrangements, after it entered the market, although its tax-farming arrangements covered customs duties. (Interestingly, these arrangements in Spain's empire parallel the deal struck between the Mexican government and the Banco Nacional de México in the 1880s.)

By repressing the domestic credit system and gobbling up available credit, the government forced the sugar planters to turn to American merchants. Chapters 2 and 4 recount the problems faced by Cuban creditors and debtors. In 1880 the Spanish finally extended an 1869 law allowing land to be used as collateral to the island, but the law proved ineffective. American creditors slowly took over storing, packaging, and transporting the crop to the U.S. market.

Chapter 5 ties dissatisfaction with the credit system, and Spain's economic management in general, to the independence movement. In some ways, this chapter is the least satisfying of the book. Fernández shows that the revolutionaries had grievances that went far beyond money and banking. It is not clear, therefore, that Spain's clumsy handling of the financial system was a key factor in galvanizing the independence movement.

Readers should understand that *Encumbered Cuba* is not a new economic history. Much of the book is informed by dependency theory, which certain readers may find problematic. The hypothesized links between the special privileges granted the BEIC and BHC and the slow growth of the rest of the financial system make intuitive sense, but they are not clearly specified or tested. The special privileges of the two major banks are listed, but their profits are not shown. The chaotic monetary situation on the island is mentioned, but never quantified. Anecdotal evidence is brought to bear that Cuban banks acted like "early nineteenth-century New England banks" and engaged in insider lending. Fernández contends that this behavior was bad for the Cuban economy, but it is not clear that insider lending was necessarily bad for economic growth in an environment characterized by poorly defined property rights.

Encumbered Cuba is a thoroughly researched book that explores two very important topics: the role of financial systems in growth and the role of history in constraining economic policy. It presents a number of interesting and clearly stated hypotheses that should stimulate future research.

NOEL MAURER, *Yale University* and
Instituto Tecnológico Autónomo de México (*ITAM*)

Population Matters: Demographic Change, Economic Growth and Poverty in the Developing World. Edited by Nancy Birdsall, Allen C. Kelley, and Steven W. Sinding. Oxford: Oxford University Press, 2001. £57.50, cloth; £19.99, paper.

This book includes 14 papers on linkages between population growth, age composition of the population, and various features of the economic development process. The reason for this new collection is the editor's belief that the consensus on the consequences of population growth for economic development has evolved and needs to be restated. More specifically, the book seeks to reopen debate on the views advanced in a 1986 National Academy of Sciences report on *Population Growth and Economic Development: Policy Issues*. The papers by Jeffery Williamson, David Bloom, and David Canning describe the key hypotheses around which the new consensus is centered.

The declines in child mortality in the developing countries after the Second World War, followed by the decline in fertility, gave rise to a shift in the age composition of the population from 1965 to 1990, in which the ratio of the number of persons in the nonworking or dependent ages of 0–15 and over 65, to the number in the working ages of 16–65, decreased substantially. This drop in the dependency ratio is credited with increasing household savings and investment rates, and raising national economic growth rates. This set of connections from the timing of the demographic transition, to a rise in national savings and investment rates, is offered as an explanation for the growth among countries from 1960 to 1997, and in particular those in East and South-East Asia. Other regions conform less well to these stylized facts. Latin America, which also experienced an early demographic transition and change in its age composition, did not reap comparable gains in savings or domestically financed investment rates and grew more slowly. Williamson and his coauthors argue in a series of papers that the decline in a country's dependency rate a decade or two after their crude birth rates falls provides a "window of opportunity" for a country to save and invest in the human and physical capital required for modern economic growth. This "window" may benefit a country's growth for about four decades, or from the time of entry into the labor force of the largest birth cohort, until this cohort starts to retire. These sharp fertility declines cast a long shadow and eventually society needs to support its growing dependent population. In this final phase of the demographic transition a society experiences a rise in the dependency ratio, leading to familiar problems of funding old-age retirement benefit plans. This problem is demographically dramatized by Japan, but is evident to some degree in most high income countries today. Only a few decades from now such low-income countries as China, Taiwan, Korea, and Singapore will be facing this final challenging phase of their demographic transition. Although changes in the age composition of the population of a nation is a covariate associated with national savings and growth rates, the new consensus views the timing of the demographic transition as exogenous or, in other words, unrelated with other parental lifetime choices such as savings. The critical forcing variable that has caused the age composition changes is fertility, which economists often view as a choice variable, and one which could substitute or complement for the level of parental savings and investment, offering alternative explanations for growth patterns across countries since 1960. The model of growth endorsed by the editors of this volume requires for identification some insight into the causes for the decline in fertility. The other challenge to the new consensus is that household-level evidence of the magnitude of the life-cycle humped pattern of savings, as originally hypothesized in the work by Modigliani and Ando, does not appear large enough to explain the key intercountry correlations of age composition and savings.

Not all of the papers in the book are aligned with the new consensus. The chapter by John Pender on agricultural and natural resources argues that the Malthusian limit to growth has not been a dominant constraint recently, with the production of food outstripping population growth in the world. Many problems of distribution and management of natural and agricultural resources remain serious today, often because of inadequate institutions and market regulations, but not because of population growth. In his paper entitled "why micro matters," Jere Behrman argues that it is essential for policy formulations to understand and document behavioral and technical relationships as observed at the micro level: individual behavior, family coordination of production and consumption, and firms and government units. Aggregate comparisons across countries constitutes the bulk of the evidence in support of the new consensus, and it has not been supported by behavioral and technical studies needed to transform associations into plausible causal connections on which to base policy discussions. Household studies are reviewed by Thomas Merrick, but without clarifying how evidence drawn from different conceptual and statistical traditions can be reconciled. Ricardo Hausman and Miguel Szekely explore covariates of inequality and the distribution of family welfare in Latin America. There is much in this volume that should engage the reader interested in the contemporary evolution of the economic-demographic debate initiated by Malthus some two centuries ago. Whether a consensus has shifted to accept the evidence of cross-country regressions instead of micro-level studies of behavior and technology, I will leave the reader to assess.

T. PAUL SCHULTZ, *Yale University*

Surabaya, City of Work: A Socioeconomic History, 1900–2000. By H. W. Dick. Athens: Ohio University Press, 2002. Pp. xxx, 541. $30.00, paper.

This book is Howard Dick's long awaited socioeconomic history of Surabaya. In the late nineteenth century Surabaya was the biggest city in Indonesia and ranked alongside Calcutta and Singapore. It formed the center of the expanding sugar plantations in East Java. When the Javanese sugar export industry collapsed with the depression of the 1930s, Surabaya entered a protracted period of stagnation and decline. In November 1945, at the beginning of the Indonesian Revolution, large sections of the city were destroyed by fighting. Not until the 1970s did Surabaya regain its dynamism through the emergence of an industrial economy. The development culminated in a real estate boom of the 1990s, which ended with the economic and political crisis around the resignation of the dictatorial president Suharto (in 1998).

The author introduces the reader to the city through a fictive entry by sea and air, and through a description of the rhythms of urban life, throughout the day and the year. In the second chapter the introduction continues with a chronological account of the main episodes of the city's history. The third chapter gives a demographic profile, based on census data, with details on ethnic composition, the composition of the labor force, education, and living conditions.

The next chapter deals with administration. Both the colonial and postcolonial governments were concerned about issues that mattered to the elite, directly or indirectly, such as security, public health, prostitution and venereal disease, slum improvement, public housing, and street trade.

In the fifth chapter Dick raises the question why the colonial economy failed to capitalize on the early presence of steam power (for the sugar industry). Massive capital investment in industry began again in the 1970s, but in adjacent towns such as Gresik rather than in Surabaya itself. Surabaya developed as a service center for the industrializing hinterland.

The next topic is land. The most important colonial urban development took place on private estates at the fringe of the built-up area. Land rights were a continuous source of conflicts between developers and the urban poor, especially during the economic booms of the early twentieth century and the 1990s. During both periods the urban poor had to clear their houses in order to make room for elite housing, but the tension subsided after the boom was over because the demand for elite housing collapsed. Squatting solved the shortage of affordable housing, but added to the land-rights issue.

In the seventh chapter, shifts in trade patterns explain the stagnation between the 1930s and 1970s. By the 1920s the frontier of sugar cultivation was closed. With the wisdom of hindsight, it becomes clear that, even before the world depression set in, the decline of the Surabaya economy was inevitable. In the 1980s the balance of trade showed improvement at last, when a broad spectrum of manufactured goods was exported. In the conclusion the main arguments are reiterated and the prospects for Surabaya in the present *Reformasi* period are assessed.

The book is a comprehensive study of Surabaya in the twentieth century. Dick is at his best in the economic-historical chapters on industry and trade, but his attention to land issues is also laudable. More attention to the sociological question of how certain change agents (planners, real estate developers, and squatters) tried to attain their goals would have enriched the analysis. But perhaps this is asking for too much.

After three decades of recurrent visits to Surabaya, the author has become something of a Surabayan himself. This may explain the sometimes almost obsessive inclination to make comparisons with the national capital, Jakarta, the rival city that has eclipsed Surabaya. A comparative analysis with other provincial capitals, instead of the national capital, might have been more instructive.

This urban biography book is well written and offers many engaging details; for example, "until 1864 'natives' were obliged to carry a lamp at night, a marvelously unenforceable regulation" (p. 163). The book has been carefully edited, with numerous notes, photographs, maps, tables, a glossary, an index, and a very useful bibliography, which includes archival government and corporate documents. Yet, there are two serious editorial flaws. The first imperfection is that the first map of Surabaya appears only on page 309. Moreover, although there are interesting maps showing, for example, rail and steam tram routes in 1905 or the land use plan for 2005, there are no maps with the important toponyms for either the colonial period or the present. I therefore often found it difficult to find my way around in the city that has been evoked by Dick's words. The second weakness is that the notes and references are too general to permit another scholar to pursue a point of particular interest to that scholar. I, at least, was frustrated when I could not trace on exactly which sources Dick's engaging analysis of colonial public housing was based.

In conclusion, this is a rare, and therefore most welcome, history of an Indonesian city. The focus on a limited geographical area instead of an analysis of gross economic figures, the attention to events outside the national capital Jakarta, and the long-term view spanning several periods are all refreshing.

FREEK COLOMBIJN, *Leiden University*

The Arabian Seas: The Indian Ocean World of the Seventeenth Century. By R. J. Barendse. Armonk, NY: M. E. Sharpe, 2002. Pp. xvi, 588. $85.00.

This is a revised edition of *The Arabian Seas, 1640–1700* published by Leiden University in 1998. This is the first time that the entire region of the Western Indian Ocean is treated as a distinct economic and commercial unit with linkages across different segments being brought out clearly. The inclusion of the East African littoral and hinterland as one of the segments enhances the richness of the analysis quite considerably. The issues discussed in the book cover a truly wide range encompassing both theoretical as well as empirical questions.

In addition to a short and useful introduction seeking to situate the study in a global framework, the book consists of ten chapters. The first of these on "Ports and the Hinterland" is among the more useful ones. It contains a good deal of new material on a whole range of little known ports in the Persian Gulf, the Red Sea, and the west coast of India with special reference to the merchandise handled there and the kinds of vessels operating from there. Chapter 3 looks at the Europeans in the Arabian Seas using the concept of *natios* to denote "enclaves of separate jurisdiction." Barendse distinguishes it from the sociological concept of diaspora but it is not quite clear what precise purpose the concept serves. The chapter is a hotchpotch of many topics dealing with women, settlement colonies, the social life of Europeans, deserters, and so on. The relationship between the trading companies and the Asian states is analyzed in the following chapter. It goes over a good deal of well-known abstract ground pertaining to the rights of the Europeans vis-à-vis those of the Asian states. There is also an interesting discussion of royal shipping and trade in Mughal India.

Chapter 5 entitled "The Merchants' World" begins with the Van Leur hypothesis. A discussion of the cost of transporting textiles from Agra to Surat follows. There is a similar discussion for the route between Bandar Abbas and Isfahan. Barendse unfortunately does not distinguish between customs duties and transit duties and clubs them together under the former head. Much discussed issues such as protection cost and opaqueness of markets are also taken up in some detail. The following chapter deals with issues such as the Dutch East India Company's Euro-Asian and intra-Asian trade from the region, the import of bullion and its impact on the economies of the region, and so on. Unfortunately, the section on bullion imports is rather muddled. Barendse's assertion, following many western scholars, that "an increase in the imports of silver did not have an immediate impact upon the amount of money in circulation" is demonstrably false. Also his suggestion that the issue of the impact of the import of bullion on the Indian economy should be analyzed in a political context rather than in terms of price history or the growth in output is certainly innovative but not particularly meaningful, even in terms of the attributes and variables proposed by him.

Chapters 7 through 9 deal respectively with the Portuguese, the Dutch, and the English. All three chapters deal with very general and well debated issues such as the nature of the Portuguese enterprise as opposed to that of the Dutch, the interpretation by the VOC of the laws governing freedom of navigation on the high seas, and so on. An important achievement of the book is the discussion in chapter 10 of piracy and smuggling in both the Indian and the Atlantic Oceans serving to tie the two oceans together and bringing home the fact that the trade of the Atlantic was inseparably linked to that of Asia.

In his "concluding thoughts," Barendse addresses the important issue of the role of the Europeans in integrating the Arabian Seas with the rest of the world economy and concludes that this role was indeed of key importance. However, on the more general question of the implications of the European trade for the economies of the region, Barendse does not have very much to add to what is already available in the literature.

Partly because of the extremely large number of rather disparate issues discussed, the book unfortunately lacks a focus or even a structure. The problem is further aggravated by the manner in which stray pieces of evidence separated in time and space by huge margins and relating to very different contexts are frequently put together to support the statements made. Let us look at some of the issues discussed on pp. 208–10 as an example. After talking about the great Gujarat famine of 1630–1634, Barendse refers to an epidemic in the Arabian Seas in the 1680s. "This may well have been the bubonic plague, which also ravaged southern Spain in this period, and which is, of course, still endemic in Surat." In this context, the ghost of the seventeenth century crisis is resurrected. The burden imposed by the high fiscal demands of the Mughal state contributing to the crisis "was made even more unbearable by disturbances due to Aurangzeb's wars." That leads Barendse to a discussion of the disruptive effects of wars. The point made in the recent revisionist historiography of eighteenth century India that these effects were of limited consequence seeks support in a statement made by a Dutch factor in 1665 at Vengurla to the effect that "wars here are in general not fought this bitterly as to totally destroy trade though they do impede it." The beneficial effects of wars as generators of new demand are then analyzed. The point is that in this unending process of one issue leading to another, it becomes impossible to keep track of where the discussion is going and what the author's argument really is.

These criticisms should, however, not detract from the fact that *The Arabian Seas* is indeed a major book based solidly on archival and secondary sources across many languages and which will be used by scholars with profit for years to come.

OM PRAKASH, *University of Delhi*

UNITED STATES AND CANADA

Within Her Power: Propertied Women in Colonial Virginia. By Linda L. Sturtz. New York: Routledge, 2002. Pp. xv, 278, $85.00.

Linda Sturtz has written an engaging and creative book about women as economic agents, particularly as property owners, in colonial York County, Virginia. The book's primary strength resides in Sturtz's careful archival work, resulting in a great number of revealing anecdotes about women and wealth that, taken together, weave a pattern of changing women's agency in the economic sphere over the course of the colonial period. From a disciplinary perspective, owing to the descriptive methodology employed throughout the text, the end result will please traditional historians more than economic historians. Nevertheless, there are several major themes that emerge in the book that will be of interest to readers of this JOURNAL.

Foremost is Sturtz's compelling description of the way women's economic agency depended upon a combination of local demographic and economic conditions as they interacted with legal structures in the colony and metropole. Throughout the book, readers are treated to variations on this theme regarding specific aspects of propertied women's activity as wives or widows, small business owners, and members of extended transatlantic trading networks. When women were rare in the colony and the legal framework of property rights was at its most nebulous, widows experienced relatively greater economic power and autonomy; when the sex ratio came into balance and institutions regarding property became more anglicized, women lost some of their power and freedom. Sturtz is also good at explaining the varying reasons women held or administered property. Among widows, property was often settled on women in order to protect the children of the first marriage in the case of remarriage. Women married to traders, especially those who traveled widely, often obtained power of attorney to administer their family business while their husbands were away. Finally, Sturtz presents a wealth of descriptive evidence showing how women's economic agency extended beyond family wealth strategies to encompass that of businesswomen (usually tavern or innkeepers) and consumers. A particularly stunning case examined by Sturtz involved a tavern keeper named Susanna Allen, who appeared in court in scores of cases over the period 1711 to 1718. In 1715 alone, Allen appeared in 28 personal cases, 6 cases as an estate administrator, and 2 cases as a witness (p. 103, table 4.4). Painstaking archival research of this kind imbues the author's argument about businesswomen and the courts with great force and color. Throughout the book, the author uses a wide range of sources from court records, personal account books, store ledgers, wills and testaments, and literary and travelers' accounts.

The method used to analyze these sources, and the manner in which they are linked to other studies of wealthholding in colonial America, are the main weaknesses in an otherwise excellent book. First, with respect to methodology, the sources are deployed in an almost purely descriptive and anecdotal manner. Many of the primary documents used in the study are amenable to quantitative analysis, but there is very little counting in this study of women and wealth, notwithstanding the exceptional case of Susanna Allen cited previously. We never know how many widows there are, how many hold wealth, or how much wealth they hold. Second, the author is aware of the existence of a large body of quantitative work on wealthholding (p. 179) but she chooses not to address it directly. As a result, many of the plausible claims made in the book cannot be substantiated on the basis of the author's data and, likewise, cannot be easily judged against the broader literature. At several key points, the author makes claims regarding broad tendencies, that could be tested against data, only to provide a footnote referring to a single case. An example is a rise in grants of power of attorney to third-party males rather than wives (p. 86). Thus, the book's arguments come across as suggestive, often compelling, but difficult to evaluate because the evidence is thin or presented without systematic quantification.

Notwithstanding these problems, the author's research illuminates women's wealthholding across a spectrum of economic, social, institutional, and cultural categories. This, in itself, is a major accomplishment. There is much more, as Sturtz emphatically shows, to women's wealth than counting the number and fortunes of widows. It is a pity, therefore, that she did not count and quantify more along the way.

ZEPHYR FRANK, *Stanford University*

Republic of Debtors: Bankruptcy in the Age of American Independence. By Bruce H. Mann. Cambridge, MA and London: Harvard University Press, 2002. Pp. viii, 344. $29.95.

Republic of Debtors is a book about changing attitudes toward and legal treatment of debtors in America from the beginning of the eighteenth century to the beginning of the nineteenth. The book concludes with the congressional passage in 1800 of the first United States Bankruptcy Act and its subsequent repeal only three years later. It was not until 1841 that Congress again enacted, if only temporarily, a second Bankruptcy Act, and not until 1898 that it finally made bankruptcy a permanent feature of the law of the United States.

The thrust of Mann's argument is that "[e]arlier in the [eighteenth] century, bankruptcy relief was not so much controversial as unthinkable. By 1800 debtors and creditors alike desired it" (p. 2). Bankruptcy relief offered debtors a discharge from their debts once they had produced what property they still owned for distribution to their creditors. Mann argues that at the beginning of the eighteenth century attitudes towards debt and debtors were still largely based on a religious morality that viewed both as morally suspect. The idea that debtors should ever be forgiven their debts was completely anathema. Mann is of the historical school that views the early-eighteenth-century American economy as a species of moral economy, one in which economic activity has not yet been liberated from the constraints of traditional morality and religion. Debtors who were not able to meet their obligations could be imprisoned by their creditors and in some cases held indefinitely. In a number of colonies debtors who failed to repay debts could be indented to their creditors and compelled to work off their debts. Above all, unlike England, there was no general bankruptcy law under which debtors might, under certain circumstances, be released entirely from their obligations to repay debts.

For Mann the moral economy of debt and the legal treatment of debtors that was an expression of it were part and parcel of a traditional society and economy in which production for the market and economic specialization were still quite limited and in which most internal trade was highly localized, limited to neighbors in the same or nearby towns. The driving force for change in eighteenth-century America came from the growing role that commerce began to play in American life. More products were exchanged over longer distances, increasingly now between strangers brought together by faceless markets that were governed less and less by traditional morality. Mann views the economic boom followed by the economic bust brought on by the Seven Years' War as a crucial turning point in American attitudes towards and legal treatment of debtors. "War made everyone familiar with risk, economic risk included" (p. 55). Despite their best intentions and best efforts, numerous people found themselves in economic difficulty. It became increasingly apparent that economic failure was often the result of market forces rather than a consequence of moral improvidence.

"The legal landscape changed dramatically after about 1755, coincident with the Seven Years' War" (p. 53). A number of colonies, New York, Rhode Island, and Massachusetts among them, enacted true bankruptcy legislation that allowed debtors to escape their debts entirely under specified conditions. These legal developments reflected fundamental changes in attitudes. In the 1750s, Mann shows, Americans first began to produce a pamphlet literature that both criticized the practice of imprisonment for debt and advocated bankruptcy relief. Throughout the period from the 1750s to the Revolution, however, debt relief by the individual colonies remained a patchwork of provisions often enacted temporarily, frequently amended, and almost as often subsequently repealed.

After the Revolution the story of debt relief became entwined with the story of the adoption of the Constitution and the establishment of a Federal Government. During the difficult times of the 1780s, a number of state legislatures took steps to try to limit the economic impact on their ordinary citizens by adopting paper currency, and passing a variety of debtor relief measures such as stay laws (which delayed collection of debts) and legal tender laws (which compelled creditors to accept repayment of debts in often depreciated paper currency). A group of Revolutionary leaders came to believe that in creating a stronger central government, the power of the states to interfere with creditor rights had to be restricted. Not only did the new Constitution authorize the Federal Government to regulate commerce, it also prohibited the states from "mak[ing] any thing but gold and silver Coin a Tender in Payment of Debts; . . . [or] pass[ing] any . . . Law impairing the Obligation of Contracts . . . ." (U.S. Const., Art. I, Sec. 10). The clause that gave the Federal Government authority to pass uniform bankruptcy laws was added to the Constitution with little controversy or discussion, according to Mann.

Although he does not say so, the easy adoption of the bankruptcy clause poses a problem. Why was the grant of power to the Federal Government to discharge debtors entirely from their contractual obligations not more controversial? Mann says that the bankruptcy clause was seen merely as an adjunct to the power to regulate commerce and this is undoubtedly true. But it is only part of the answer. The rest of the answer is to be found in the debates that took place throughout the 1790s as bankruptcy bill after bankruptcy bill was introduced and failed to be enacted by Congress. Finally in 1800 the Federalists, still in control of Congress and the Presidency, managed to pass the first Federal Bankruptcy Law. Like the English bankruptcy acts, the American law limited bankruptcy relief to commercial traders who owed very large sums of money. Farmers and other small debtors did not qualify. Although they had shown great solicitude for creditor rights so far as popularly elected state legislatures were concerned, proponents of the new federal government had also shown great solicitude for commerce and the moneyed interest. In commerce where creditors were also, and frequently simultaneously, debtors, the system might operate more smoothly and efficiently under a law that discharged debts so long as it was enacted by a responsible political body such as the Federal Congress and discharged only the right kind of debtor.

This is a beautifully written book about an important subject, but it suffers from a serious weakness. The larger explanatory framework (from moral economy to market society) does not always account well for many of the details of the story. Put another way, many of the details raise questions for which the larger explanatory framework simply does not offer answers. If discharge of debts was unthinkable early in the eighteenth century, how could Massachusetts and New Hampshire ever have passed bankruptcy acts (even temporary ones) in 1714 and 1715? (pp. 52–53). Decades before the 1750s a number of colonies showed surprising solicitude for debtors by enacting insolvency legislation. Although this legislation did not formally discharge debts it did discharge some debtors from imprisonment for those debts, making ultimate repayment of the debts seemingly much less likely. How does this apparent solicitude for some debtors square with the morally suspect character of debt and debtors in the early eighteenth century? Moreover, a number of these insolvency acts provided for the release of the wrong kind of debtor, it would seem, poor debtors and debtors who only owed relatively small sums of money. Larger debtors often did not qualify for release under these insolvency acts. Just the opposite held true of the Bankruptcy Act of 1800. What are we to make of these differences? And what are we to make, for example, of a legal tender law enacted in Massachusetts as early as 1712 and titled "An Act To Prevent the Oppression of Debtors"? (*The Acts and Resolves Public and Private of the Province of Massachusetts Bay*. Boston: Wright & Potter, 1869: Vol. 1, pp. 700–01). Might an older political and economic conflict between farmers and other small debtors on the one hand and commercial interests on the other running all the way back through the eighteenth century help to explain some of this evidence? Although Mann's book has brought together a great deal of material and fashioned it into a large narrative, one comes away with a sense that a good deal more of the political and economic context of debt in the eighteenth century remains to be explored.

ROBERT J. STEINFELD, *State University of New York at Buffalo*

Hamilton Unbound: Finance and the Creation of the American Republic. By Robert E. Wright. Westport, CT: Greenwood Press, 2002. Pp. xii, 230. $62.95.

This book, unfortunately, has some very serious problems. I say "unfortunately" because the author, Robert Wright, has recently published a great deal of enormously productive scholarship on the emergence of the U.S. financial services sector in the early national period. But this volume proves that "you-can't-win-them-all."

First, the title is deceptive. The inducement suggests that Alexander Hamilton is a crucial player in the body of the text, but the famous Secretary of the Treasury is, in truth, little more than a tangential figure in one or two chapters. The volume's organization can be described, at best, as severely disjointed. The chapters consist of a seemingly random collection of essays, some of which have already appeared in one form or another in other scholarly publications.

These chapters do explore novel topics. I found some of the author's interpretations interesting and persuasive, whereas others bordered on the preposterous. In the latter category were separate chapters on how concerns about interest rates helped to foster the break with Great Britain in the 1770s and, later, on how personal credit markets—as opposed to impersonal transactions through intermediaries—promoted "dueling," especially in the antebellum South. In a more constructive mode were the following: a chapter that applies "agency" theory to help explain the formulation of the U.S. Constitution; a chapter that argues the importance of financial markets in promoting the growth of the U.S. economy from 1790 to 1860; and finally a chapter that reveals how the disenchantment of urban artisans with the Federalists' monopolistic attitude toward the chartering of commercial banks turned the small business community into Republicans and thereby led to the election of Thomas Jefferson to the presidency in 1800. In that context, I was disappointed that Wright failed to cite the previous work of historian Joyce Appleby, a former president of the AHA, who stressed, long ago, the commercial outlook of many Jeffersonian Republicans.

Wright is angry that mainstream historians have paid little attention to the work of economic history specialists over the last quarter century, and he persistently chastises them for their gross oversights. His complaints have validity, but can these types of denunciations turn the tide? I doubt it. Most of us realize that the intellectual interests of non-economic historians follow the most fashionable trends, and we can only be patient, cross our fingers, and hope that one of these days, in the not too distant future, we and all our peers will receive the recognition we so richly deserve. Even if posthumously.

More economic historians should become familiar with the scholarship of Robert Wright. I recommend the author's *The Wealth of Nations Rediscovered: Integration and*

Expansion in American Financial Markets, 1780–1850 (Cambridge) also published in 2002. It is the genuine article.

EDWIN J. PERKINS, *University of Southern California*

Recreating the American Republic: Rules of Apportionment, Constitutional Change, and American Political Development, 1700–1870. By Charles A. Kromkowski. Cambridge and New York: Cambridge University Press, 2002. Pp. v, 451. $70.00.

In *Recreating the American Republic*, Charles Kromkowski, a political scientist, tells a complex historical story of three changes in our nation's constitutional order. The book chronicles American political development from the break with Britain through the aftermath of the American Civil War. The specific constitutional changes chronicled are the changes in the rules of apportionment embodied in our constitutional order—the rules determining the intragovernmental division of decision-making authority and power. Kromkowski argues that these were important constitutional developments because apportionment rules determine the fundamental division of collective decision-making within the nation.

Under British rule, the American colonies had colonial assemblies that in most cases were based initially on various forms of limited proportional apportionment rules; these became less proportional as populations grew and colonies did not reapportion on a regular basis. What is more, "even at mid-century, many colonials simply did not equate the apportionment of assembly representation with standards like population or the amount of taxes paid" (p. 93). The colonies, of course, had no formal decision-making authority, capacity, or representation within the British government. So, the first change in the American constitutional order that Kromkowski chronicles is the break with Britain and adoption of the Articles of Confederation and Perpetual Union, which contained a new rule of apportionment—equal state representation for each state in the federal Congress of the United States. The second change was the abandonment of the Articles of Confederation in 1787 at the Constitutional Convention in Philadelphia, leading to the new constitution and its ratification on 21 June 1788. Adoption of the constitution meant the nation now had new rules of apportionment—proportional state representation for each state in the national House of Representatives based on a state's free white population and three-fifths of its enslaved population, and equal state representation in the national Senate. The third change began with the secession of southern states in 1861 and the American Civil War, culminating in the Thirteenth, Fourteenth, and Fifteenth Amendments adopted in the late 1860s. These amendments meant that the nation again had new rules of apportionment—proportional state representation in the national House of Representatives based on each state's total population. The three-fifths rule was nullified with the abolition of slavery, the reduction in a state's representation if it denied the right to vote to former slaves, and the prohibition against denying a citizen the right to vote "on account of race, color, or previous condition of servitude."

Kromkowski explains the constitutional changes in the apportionment rules with a rather complex macro-level and micro-level analysis of contemporaneous historical events within a political science framework and a game-theoretic foundation. He contends that not only might trends in economic, political, and social conditions matter (the macro-level analysis) but the actions, desires, interests, and motivations of the specific political actors and entrepreneurs involved in the changes also matter (the micro-level analysis). The macro-level analysis is a "[R]econstruction of the developmental patterns associated with . . . four macrostructural conditions prior to each apportionment rule change" (p. 424). The four conditions "reconstructed" for each of the apportionment rule changes are short- and long-term developments in economic conditions, demographic conditions, institutional conditions (concerning governmental structure), and ideological conditions (concerning concepts of political representation). The "reconstructions" are chapter-length syntheses of the historical literature on the structural conditions for each rule change. The micro-level analysis examines within an elementary game-theoretic framework the actions, behaviors, interests, and preferences of the political actors and entrepreneurs involved in the apportionment rule changes, as well as the likely outcomes and strategies involved.

Kromkowski provides two different answers to explain changes in apportionment rules. The first can be found in "the particular and highly detailed accounts of the historical contexts, the principal political actors, and the decision-making motives and negotiations" (p. 425) involved in each rule change. This explanation is based on "the totality of the discrete circumstances, personalities, interests, and choices that affected and effected each particular change" (p. 425). The second explanation can be found in "the generalization that apportionment rule changes are caused by changes in political expectations concerning decision-making capacities and government authority" of the "politically relevant actors" (p. 425). This explanation is actor-centered and based on the actors' "desire to attain sufficient levels of certainty regarding the protection or promotion of their discrete and common interests" (p. 425). In Kromkowski's view, "creating consensual constitutional orders ought not to be conceived as hidden behind a hypothetical veil or the blind hand of *fortuna*" (p. 429). Rather, constitutional order is the result of "the willingness to engage in the diversity of the human condition, the constraints of particular historical circumstances, and the liberty and limits of human imagination" (p. 430). In other words, constitutional order comes from the actions and choices of rationally motivated political actors who face constraints!

This book draws on contemporaneous correspondence, letters, rhetoric, speeches, and other evidence found in primary documents as well as the secondary literature. Although the book is titled *Recreating the American Republic*, it is very much about "creating" the republic during the 1700–1870 period, but it also includes 30 pages of text (about 7 percent) on the development of the British constitution in the seventeenth century. Included are dozens of figures and tables, presenting the game-theoretic models and various interests, preferences, outcomes, and strategies for each rule change. Many of the figures and tables, however, are not described or explained very well or do not contribute much. The book is not a quantitative study nor does it include statistical analysis. Political actors' actual interests and preferences are not directly measured; they are in effect equated with the interests and preferences of the actors' states, regions, or sections. Other than drawing on the deliberations and rhetoric during the drafting of the constitution, the book does not present much "micro-level" analysis of specific political actors and entrepreneurs involved in the other two changes in the constitutional order. In these cases, the analyses are centered on the states, regions, or sections as the unit of analysis.

Notwithstanding the preceding, readers interested in the development of the constitutional order of the American republic should find this an informative and interesting book. But readers looking for a story based upon a clear-cut paradigm might be less than satisfied.

ROBERT A. MCGUIRE, *University of Akron*

Advocate for Free Enterprise: William Buck Dana and the Commercial and Financial Chronicle, 1865–1910. By Douglas Steeples. Westport, CT: Greenwood Press, 2002. Pp. xxvii, 237. $72.95.

The "economic mind" is an elusive beast, whether embodied in a particular historical actor or in the temper of a historical moment. Douglas Steeples, Professor of History

Emeritus at Mercer University, pursues both in this intriguing and intelligently crafted intellectual biography of William Buck Dana (1829–1910), who founded and controlled the most influential business periodical in the United States during the generation after the Civil War.

Steeples covers Dana's life in the first two chapters, and devotes the remaining four to the editor's thinking on four salient economic issues of his day: business cycles, property rights, labor, and the proper role of government in economic affairs. The biographical chapters are crisp and well contextualized, as Steeples examines Dana's upbringing, family life, and entry into publishing. William Buck Dana was raised in a large, devoutly Protestant, prosperous mercantile family in Utica, New York. After attending Yale and becoming an attorney, he returned to Utica. He entered publishing in 1861, when he purchased one of the era's pre-eminent business periodicals, *Hunt's Merchants' Magazine*. After experimenting with special columns and spin-off publications, Dana launched the 32-page *Commercial and Financial Chronicle*—the nation's first business weekly, modeled explicitly on the *London Economist*—in New York City in 1865. Its correspondents relied heavily on the telegraph and the mails as sources of current business information.

As Dana's wealth grew he engaged in real estate development in the Palisades area along the Hudson River (New York) and erected a large home at (present day) Englewood Cliffs, New Jersey. He and his wife, Katherine Floyd Dana (a prolific author of adult and children's fiction), were close but childless. They eventually adopted three children, built a Sunday school, and enjoyed nurturing the young. One of their protégés, William Allison, became a prominent publisher in his own right. William Dana left an estate valued at nearly $2 million.

Dana aimed the *Chronicle* at the nation's business elite, particularly in finance, which he saw as the engine driving the American economic system. He saw economic laws as immutable as the laws governing nature, with employment, wages, prices, and so on tending toward "natural" levels. His was an ethos of self-interest, rationalism, and altruism. But he saw wealth as a means to a larger end: social responsibility. Not surprisingly, given the great cyclicality of his times, Dana devoted considerable attention to business cycles. He usually ascribed wide fluctuations to the uncertainty and lack of confidence ultimately bred by weak monetary policy. Like so many of his elite contemporaries, Dana called for hard money, subscribed to the labor theory of value, staunchly defended property rights, opposed most business regulation, and often spoke the language of Social Darwinism. Even so, Steeples makes a convincing case that Dana's views about human nature and the economy were "considerably more subtle and sophisticated" than those of the vast majority of his contemporaries (p. 113). Steeples argues vigorously, on the one hand, that Dana exercised a strong and consistent editorial hand and, on the other hand, that he was thoroughly "non-partisan." But the author seems to define partisanship in terms of explicit party affiliation. William Dana's social and economic philosophy nevertheless clearly resonated comfortably with—indeed, reinforced strongly—the parties and politicians who stood for the era's "moneyed" interests.

Some of the book's strengths, but also what is perhaps its key weakness, flow from its somewhat unconventional structure. The thematic chapters serve well to illuminate the nuances of Dana's thinking on a handful of key subjects, as well as its evolution. To his credit, Steeples avoids portraying Dana as dogmatic, static, or even wholly consistent. Still, the thematic chapter organization tends to breed repetition and to produce a kind of intellectual silo effect that disguises crosscutting patterns. Shorter thematic chapters on topics such as foreign policy or antitrust might have served better. The book's photographs are well chosen but reproduction quality is very poor. Steeples's discussion of technological change late in the book seems tacked on.

Such shortcomings are minor relative to the book's considerable strengths and contributions. Students of Gilded Age finance and economic thought will find *Advocate for Free Enterprise* indispensable. And for those who venture to plumb the recesses of "the business mind," this book serves as a superb model.

DAVID B. SICILIA, *University of Maryland*

Confederate Industry: Manufacturers and Quartermasters in the Civil War. By Harold S. Wilson. Jackson: University of Mississippi Press, 2002. Pp. xi, 412. $45.00.

Historians have long known that Confederate leaders—despite a long heritage of states' rights rhetoric—built a remarkably vigorous central state during the Civil War. The Confederacy, through the manipulation of conscription exemptions, controlled labor markets and industrial output. To gain even more control of industrial output, the Confederacy built and operated several railroads and numerous factories, and directly impressed an array of agricultural and manufactured goods from private producers. No wonder that more than one scholar has termed the Confederate economy "war socialism." In *Confederate Industry*, Harold C. Wilson adds to the literature on the economic policies of the Confederate state by carefully documenting the often strained and tense relationship between Confederate businessmen and the Confederate government. The book seeks to accomplish two central tasks: documenting the ability of the Confederate quartermasters to keep the army adequately supplied, and chronicling the trials and tribulations of southern manufacturers during the war.

The title of the book is somewhat misleading. Wilson does not examine southern manufacturers as a whole, but instead focuses on the relationship between the textile industry and the quartermasters' department. Wilson initially focuses on Abraham C. Myers, the first Confederate quartermaster general. Myers, according to Wilson, generally failed to make the best use of the limited Confederate resources, yet nevertheless by the fall of 1862 he became "the acknowledged controlling authority of nearly all military supplies within the Confederacy" (p. 42). Perceptions of growing corruption and widespread hostility of field commanders led to Myers's downfall, and he was replaced by Alexander Lawton in the summer of 1863. Wilson gives Lawton credit for standardizing production and increasing the efficiency of Confederate supply operations. Wilson is somewhat ambiguous as to whether the Confederate mobilization was a success or failure, but he mentions the "successful example of the Confederate war department" as an important ingredient in the rise of the New South's textile industry. Many of the most prominent textile manufactures during the war, he notes, would flourish during Reconstruction.

The primary attribute of *Confederate Industry* is its prodigious research base. Wilson has carefully examined the papers of William Gregg, Francis Fries, and several other prominent southern textile manufacturers, as well as the voluminous records of the Confederate government. When combined with evidence from newspapers, diaries, and pamphlets, the result is a richly detailed account, which shows the stresses and strains of Confederate supply operations. Many elements of the story will undoubtedly fascinate economic historians. For instance, Wilson meticulously details the impressive profits that southern manufactures managed to make in the face of widespread devastation and the crucial shortage of raw materials; he shows the widespread emergence of bartering as a response to inflation; he documents the competition between state and central governments over increasingly scarce resources; and he notes the ability of southern manufacturers to rehabilitate themselves both economically and politically during Reconstruction.

Getting to interesting material, though, is not an easy task. Wilson develops rather vague themes in his study but there are no arguments to give the book coherence or organization. The rambling and disjointed narrative is often hard to track, and Wilson shifts to new topics with little sense of transition. Wilson's idiosyncratic citation system—a confusing mix of footnotes and parenthetical citations—makes it time-consuming to see what sources he is using for a particular point. Historiography is almost totally absent, and Wilson does not refer to the rather large economic history literature on the southern economy. When Wilson analyzes "Southern Manufacturing Circa 1860," for example, he completely ignores the substantial secondary literature on southern industry in the antebellum period. As one might suspect, economic analysis or theory of any sort is almost entirely absent.

Those approaching this book should thus consider themselves the academic equivalent to gold prospectors. There are riches to be found here, but one should be prepared to search long and hard to find them.

JOHN MAJEWSKI, *University of California, Santa Barbara*

Freedom's Promise: Ex-Slave Families and Citizenship in the Age of Emancipation. By Elizabeth Regosin. Charlottesville: University Press of Virginia, 2002. Pp xi, 239. $49.50, cloth; $17.50, paper.

Even before the end of the Civil War and the enactment of the Thirteenth Amendment it was clear that the "peculiar institution" that had done so much to bring about the South's bloody and unsuccessful rebellion would not survive the war. The question was what kind of freedom would be granted in the war's wake. Would the former slaves join the ranks of citizens or would they exist in a kind of legal netherworld, not citizens, no longer slaves, but not totally free either? Elizabeth Regosin's *Freedom's Promise: Ex-Slave Families and Citizenship in the Age of Emancipation* draws our attention to the importance of the legal recognition of the African-American family in the development of black citizenship.

Regosin's research centers on the pension records for the familial survivors of deceased soldiers of the United States Colored Troops (USCT). The Civil War pension system was one of the federal government's first attempts at social welfare. It left a body of records that reveal much about both the structure of slave families, at least as they existed in the decade before the Civil War, and the difficulties in reconciling the familial practices and circumstances of slaves with the often rigid bureaucratic demands of the federal pension system.

Slave families, as Regosin properly reminds us, had no status before the law. Slave owners could and did separate families. Sometimes they did so for economic advantage, sometimes out of whim or spite. The frequency of such separations is less important than the fact of the absolute power of slave owners over the families and the absolute inability of slaves to claim redress or to preserve family ties in light of that power. Slaves were left with often thwarted efforts to create and maintain families. Strong family ties did develop during slavery, and the efforts to re-constitute black families after the Civil War gives strong evidence of this. But how were families formed during slavery without the protection of the law to be recognized and regulated after the law proclaimed freedom and demanded conformity with legal and social norms that prevailed in white society? The slave family could be a legally haphazard matter, surviving or not at the master's sufferance. Such could not be the case after emancipation. There were familial responsibilities, care and responsibilities for children, parents and spouses that the law was unconcerned with while blacks were in bondage. Even the Black Codes of 1865–1866 recognized that provision would have to be made for legal recognition of the families of former slaves.

Regosin reminds us that the state and status of the family was even more the concern of the federal government. Officials of the Freedmen's Bureau saw a regularizing of the black family as a continuation of the abolitionist concern with the harm that slavery had done to family life. And there was the stubborn matter of Civil War pensions. Better than 180,000 black men, the vast majority antebellum slaves, enlisted in the federal forces under the banner of the United States Colored Troops. Roughly 37,000 Negroes died in the service of the Union Army. According to Regosin's sample some 6 percent of Union pension applications came from relatives of black veterans.

Regosin uses the pension records to do a composite, qualitative analysis of black family relations in the generation that experienced the transition from slavery to freedom. Using a random sample of 100 pension applications by African Americans, Regosin provides a narrative of individuals and their struggle to be recognized and their right to compensation vindicated. Former slaves often faced formidable difficulties in collecting pensions. Often illiterate, frequently the victims of forced family separations during slavery and sometimes even lacking recognized family names, or the ability to spell such names when they did exist, former slaves had to convince a not unsympathetic, but often perplexed pension bureaucracy of the legitimacy of their claims. The stories of the pursuit of pensions tend toward both the poignant and the ironic. We learn how the government resolved issues where the deceased soldier had two families with potential claims, a circumstance often brought about because of family separation during slavery. Regosin also reveals cases where former slaves were dependent on former slave owners to verify the informal slave marriage between a Union veteran and a widow claiming the veteran's pension.

As with any study there are issues that Regosin has left unexplored. She might, for example, have used the pension records to discuss geographic variation in slave family structure. But doing that would have probably required a larger sample and perhaps a sacrifice of Regosin's narrative approach to the records. *Freedom's Promise*'s greatest strength lies in its ability to link legal development with cultural change. The need to present an understandable record to a federal bureaucracy, a need that also existed with respect to state agencies, helped change Afro-American culture. It hastened the adoption of family names and the development of formal family norms that had previously existed in free society. Through her examination of the Civil War pension records, Elizabeth Regosin has made a solid contribution to the study of African American family history

ROBERT J. COTTROL, *George Washington University*

The Soul's Economy: Market Society and Selfhood in American Thought, 1820–1920. By Jeffrey Sklansky. Chapel Hill: University of North Carolina Press, 2002. Pp. xiii, 313. $45.00, cloth; $19.95, paper.

Histories of social science in America are being offered nowadays by historians who were raised up in a literary or cultural vein and who find frustration with the ahistorical and rational-choice approaches to models and descriptions of "the self." To them the subject of Jeffrey Sklansky's *The Soul's Economy: Market Society and Selfhood in American Thought, 1820–1920* is in fact a growth industry. Alice O'Connor's *Poverty Knowledge* (Princeton, NJ: Princeton University Press, 2001) and *The New Economic Criticism* (London: Routledge, 1999), edited by Martha Woodmansee and Mark Osteen, are other examples in the Sklansky vein: each tries to recover for today's scientists some aspect of an embedded self, a "social self," which, as Sklansky puts it, "modern social science once made conceivable" (p. 12).

To mainstream economists, critical questions concerning the basic motivating forces of "the representative agent"—the self—are considered to be either settled or insipid. Social economists, feminist economists, and many historians do not of course believe that the model of human agency is so settled; in fact, they do not believe that one single model can meaningfully explain or describe the rich diversity of human life in a world of class and race and gender differences.

In *The Soul's Economy*, Sklansky "charts a change" in the "generally accepted terrain and borders" of the self and "social relations" (p. 4). The book "focuses," he says, "in particular on shifting conceptions of human nature and the nature of society, conceptions rooted in the Enlightenment quest for the universal laws that govern human affairs" (p. 5). Sklansky's promise sounds more ambitious than is the end product. The book is in truth a collection of essays on Emerson, Horace Bushnell, Margaret Fuller, and some dozen social scientists whom Sklansky believes largely to have defined the rhetorics of selfhood in American "political economy." *The Soul's Economy* is arranged by chapters that can stand alone: first the essay on the transcendentalists, and then essays on the antebellum origins of American sociology (Henry C. Carey, George Fitzhugh, and Henry Hughes); the postbellum crisis of political economy (Henry George and William Graham Sumner); the "new psychology" of the Gilded Age (William James, John Dewey, and G. Stanley Hall); the sociological turn in progressive social science (Simon N. Patten, Thorstein Veblen, Lester F. Ward, and Edward A. Ross); and corporate capitalism and the social self (Thomas M. Cooley and Charles H. Cooley).

If Sklansky had kept his focus on close textual readings of transcendentalist and social-scientific conceptions of the self and its relation to both exchange theory and economic outcomes, he could have delivered (as Alice O'Connor has, in her work on poverty think-tanks) an important and timely addition to heterodox economic critiques of post-Samuelsonean utility theory and methodological individualism. For example, if in the history of ideas the Emersonian notion of *philosophical* "self-reliance" emerges triumphant—or anyway *emerges* in social-scientific discourse, as Sklansky evidently would like it to—then the reader would expect a close textual and empirical reading of Emerson and of the economies historical and present-day in which philosophical self-reliance is defined and seen to be apt. The best we get from Sklansky, however, is the assertion that "class analysis" (p. 232) and the analysis of "the distribution not only of wealth but of the resources required to create it" (pp. 3–4) are necessary in any "reopening" of the "vital debates about selfhood and society" (p. 3). He wanders from his target, in other words, and with enough frequency and abandon to weaken any force of the arguments within. To take just one example (from the author's introductory remarks concerning the purpose the book) Sklansky greets his reader with distracting ambiguity: "A central premise of this book," he says, "is that nineteenth-century concerns about property and sovereignty amounted to something more than fear of progress, while twentieth-century notions of interdependence and social selfhood represented something less than an unambiguous advance upon outdated ideals" (p. 4). *More than fear of progress*. *Less than an unambiguous advance*. That's how the book goes, more or less, for the remaining 228 pages.

The strength of *The Soul's Economy* is in its novel juxtaposition of the rise of social psychology with social economics—a history that is apparently little known even by today's economic psychologists. Economic historians of consumer culture, and economic historians with an institutionalist bent, will appreciate the sections on Lester Ward, Edward Ross, Thomas Cooley, and Charles Cooley. But Sklansky's interpretations of the other figures—including Emerson and Sumner and Henry George and Veblen and William James—are hardly original. It is in Joseph Dorfman's corpus, and in Robert Heilbroner's *The Worldly Philosophers*, and in scores of abstracts; to the implied audience. Sklansky's interpretations of the transcendentalists, the pragmatists, and the turn-of-the-century social economists add little to Cornel West's *The American Evasion of Philosophy* (Madison: University of Wisconsin Press, 1989) or Christopher Lasch's *The True and Only Heaven* (New York: Norton, 1991).

Economic historians will find little use for Sklansky's central premise. But the subtext—that mainstream economists have abandoned concerns with class and its relation with the "social self"—invites reflection.

STEPHEN T. ZILIAK, *Roosevelt University*

Engines of Enterprise: An Economic History of New England. Edited by Peter Temin. Cambridge, MA: Harvard University Press, 2000. Pp. viii, 328, $18.95, paper.

Regional economies histories remain scarce in spite of growing scholarly interest in the role of clustering and agglomeration in growth. The new economic geographers have relied almost exclusively on mathematical modeling rather than historical research to understand these phenomena. The essays that comprise this economic history of New England demonstrate the insight that historians can provide into the dynamics of economic growth. Merritt Roe Smith's contribution to this volume, for example, reminds us of the role of federal war-time spending in seeding both the nineteenth-century machine tool industry that clustered around the Springfield armory in Massachusetts and the postwar information-technology clusters around universities such as MIT in Cambridge and the Route 128 area.

Engines of Enterprise is a unique and carefully constructed collection of essays that survey the four centuries of New England's economic history, starting with the earliest settlers and entrepreneurs and ending with the turn of the twenty-first century. Each of the five essays that provide the central narrative for the book is an original contribution that could stand on its own; however, the combination provides a rich overview of the sweep of the region's economic history. We learn in detail about the rise of a capitalist market system out of a colonial agricultural and slavery-dependent region enmeshed in the highly profitable Atlantic trade, about the accelerated industrialization of the nineteenth century (a transformation that Peter Temin describes as "comparable in scope and intensity to Asian 'miracles' of Korea and Taiwan in the half century since World War II" and "as fraught with implications for other regions as the more recent growth"), about the region's loss of its distinctive role as the rest of the nation "caught up" through industrialization and urbanization in the late nineteenth and early twentieth centuries, and about the costs of deindustrialization alongside the postwar reinvention of New England as a knowledge economy based on advanced business services and information technology due to unprecedented levels of peacetime federal defense spending as well as ongoing and heavy investments in higher education.

This is an engaging volume; the essays work extremely well together, in spite of the multiple authors (all of whom are economists or historians that specialize in the periods they write about.) The volume is so well written that it is easy to get caught up in the evolving drama of the region's economic origins, growth, maturation, and adaptation. It is a tribute to the authors and editors that the volume has the qualities of a good novel—without oversimplifying the historical narrative or interpretive debates. This makes it a book that should appeal to an unusually wide audience. I would strongly recommend it for both undergraduate and graduate courses in economic history, regional economic development, and related topics. But it is not a book for students and scholars alone. The introduction by Peter Temin clearly explains the economic concepts that are used throughout the book, from comparative advantage and human capital to agglomeration economies, making it easily accessible to the general reader as well as the specialist.

The final chapter features short reflections on the themes raised by the prior narratives on the New England economy. This is the one piece of the book that reveals its origins in a conference because the sections read more like commentaries than like the well-researched essays that precede them. However the deep knowledge that these authors bring to their subjects make the reflections particularly stimulating: Bernard Bailyn argues that slavery and population growth were critical to explaining growth in the colonial economy; Merritt Roe Smith details the role of the federal government in the development of industrial skills in the nineteenth century as well as in the development of engineering and management skill at the U.S. military academy at West Point; and Paul Krugman speculates on the future of New England.

This book is designed to serve as the intellectual foundation for a New England economic history museum being built by the Federal Reserve Bank of Boston. The historical photographs and illustrations that are scattered throughout the book bring alive the different periods addressed in the individual essays. One can only hope that the museum demonstrates the care and quality of work that is evident in this volume; the combination will be a true contribution to our understanding of the history of the country's oldest regional economy.

ANNALEE SAXENIAN, *University of California at Berkeley*

Water and American Government: The Reclamation Bureau, National Water Policy, and the West, 1902–1935. By Donald J. Pisani. Berkeley: University of California Press, 2002. Pp. xviii, 394. $49.95.

No person alive knows more about the history of water in the American West than Donald Pisani. His many contributions to this subject have been seminal and wide ranging, and he has deftly exploded many myths and misconceptions that abound in American water histories. His two main books on water history, *From Family Farm to Agribusiness* and *To Reclaim a Divided West*, are testaments to the traits that make him an outstanding scholar: his vast knowledge of previous studies, his keen synthetic sense of where our understanding is and where it needs to go, and the way he tirelessly marshals and appropriately uses voluminous evidence, much of it from primary sources, to support his arguments. For all these reasons, the publication by Pisani of a new book on water history, this one examining the early history of federal reclamation policy (mostly) in the western United States, is an event richly deserving of scholarly attention.

In *Water and American Government*, Pisani does not disappoint. This book is a masterful study of a crucial period in American resource history, in which government policies aimed at promoting more efficient use of natural resources came to the fore. The onset of federal reclamation at the turn of the century occurred at the intersection of two key western trends: increasing recognition that water supplies were becoming scarcer and steadily increasing liberality in the terms offered for land settlement by the federal government. The book carefully chronicles the early years of the Bureau of Reclamation (originally the Reclamation Service), the government agency chiefly responsible for developing irrigation water supplies for farmers in the American West. It paints a complex picture of an agency laboring under flawed enabling legislation, muddling along under a succession of more- and less-effective administrators, possessing unclear legal rights to the water it was supposed to develop, lacking true powers to enforce payment for water supplied, and subject to continual turf wars and political and interagency infighting. It is, indeed, a fascinating case study of government management and mismanagement that contains some object lessons concerning how programs can grow dramatically in scale and scope over time, and how constituencies served by those agencies can effectively engage in rent seeking to obtain more rents than anyone could possibly have expected. In making his case, Pisani does exactly what he is well known for: bringing massive amounts of evidence to bear to support each and every crucial juncture in the argument. The overall result is persuasive and compelling.

Pisani's objective is to take us beyond existing scholarship on early reclamation policy. One problem with existing studies is their tendency to view the early reclamation world in black-and-white terms where the bad guys are readily identifiable by the black hats they wear. Though there is plenty to criticize in early reclamation policy, one of Pisani's important contributions in this book is his more even-handed approach in which institutional defects, legal constraints, and political rent-seeking dynamics play key roles alongside incompetence, personal vendettas, and sheer bloody-mindedness. Another of his quarrels with existing studies is their overemphasis on policymaking at the top (e.g., Congress, the bureau), at the expense of attention to cause-and-effect at the bottom (e.g., farmers, rural communities). Observant readers will recognize this as a theme he started to develop in *Divided West*, which ended with the passage of the Reclamation Act. To counterbalance this top-down emphasis, Pisani embeds, within the main big-picture narrative of reclamation wheeling and dealing, two fascinating chapters that contain detailed micro-studies of local rural communities affected by the policies of the Bureau. These two chapters are nice additions to the overall narrative and serve well their function of reminding us of the real effects that reclamation policies exerted on the lives of farmers and rural communities.

It is great, of course, that historians and economists sometimes speak to each other. Economists benefit tremendously from the unearthed evidence and practical insights of good histories. Historians benefit from application of the insights and analytical tools in the economist's arsenal. Pisani's book provides all manner of seductive evidence that should entice economists to investigate further. For example, one of the most intriguing phenomena of this period was the gradual expansion of the subsidy to farmers served by reclamation projects. This is interesting because it seems to coincide with other aspects of agricultural policy during this period that increasingly favored farmers. Pisani weighs in obliquely on this issue, painting a picture of rent seeking and political capture which were particularly effective during times of crisis (e.g., agricultural depression, drought). But the explanation is incomplete and the question remains of how farmers were able to capture such sizable rents through reclamation policy. The dynamics of that political story, in which enforcement costs and transactions costs will figure prominently when finally told, deserve far more treatment than Pisani provides.

Another question of interest to economists is why, throughout the history of federal reclamation, there have been so many bad (read: non-cost-beneficial) reclamation projects. Here, a plethora of hypotheses abound; for example, unanticipated cost overruns, the spread-the-wealth clause in the Reclamation Act, empire-building within the bureau, and log-rolling among congressmen. Some of these possibilities Pisani addresses, although not truly in a systematic fashion. It would be useful to test these hypotheses because the answers would help to evaluate the long-term performance of the bureau. I, for one, would view the bureau in a much more positive light if it turned out that bad projects were chiefly the result of institutional constraints as opposed to, say, bureaucrats looking to make work for themselves.

The book may spur economists to pursue some of these issues themselves. However, I will not fault Pisani for not being more of an economist rather than what he is: a mighty fine historian. In this book, Pisani has written a terrific contribution to American resource history and continues to cement his well-deserved reputation as our greatest living water historian.

MARK KANAZAWA, *Carleton College*

The Challenge of Crime: Rethinking Our Response. By Henry Ruth and Kevin R. Reitz. Cambridge, MA: Harvard University Press, 2003. Pp. x, 374. $35.00.

My state recently became the thirty-fourth to pass a concealed-carry law, despite opposition by the majority of residents. The ensuing discussions have been full of hand-wringing and finger-pointing. Notoriously absent are cool heads and hard data. I would like to make *The Challenge of Crime* required reading for all of us.

This is a superb and thoughtful piece of work, studded with statistics and stocked with solid policy suggestions. Of particular interest to economic historians, however, is the copious use of historical data to establish the authors' major claim: A sharp break in crime response occurred around 1970. Before that time, the United States was characterized by a fairly liberal, reasonably humane approach. After Lyndon Johnson's National Crime Commission (which included Henry Ruth) issued its landmark report in 1967, the approach to crime in the United States became increasingly punitive, partly as a response to a perceived increase in criminal activity during the 1960s. Yet this harshness continues despite a substantial drop in violent crime over the last decade.

Why is this? One suggestion is that our current public system is an anachronism. We are stuck with late-nineteenth- and early-twentieth-century institutions—police, judges, prosecutors, prisons—as we struggle with twenty-first-century problems. The natural tendency of bureaucracies to perpetuate themselves hampers our flexibility. Adding to the problem, contemporary American crime policy focuses on the patchwork and the short-term. Finally, the authors point to the ideological influence of James Q. Wilson, despite what they say are major shortcomings in his analysis.

Although much of the book is oriented toward current policy, economic historians will appreciate the authors' call for the long view in crafting appropriate responses to crime. They request a careful look at accurate, historical data and a nonpartisan appraisal of past programs to see what works and what does not.

They follow their own advice: I especially admire the authors' abundant use of ancillary research and pertinent data, both to set the stage and to support their recommendations. They are well acquainted with the literature (although I think they give short shrift to Gary Becker, Isaac Ehrlich, and George Stigler) and they present their statistics well. For example, they calculate the average American's lifetime chance of ever serving probation (p. 23) and the cost of additional incarceration for each murder avoided (p. 100).

Offerings with a more historical bent include a brief history of crime and punishment in the United States, a look at the professionalization of the criminal justice system in the twentieth century, a nice use of homicide data to establish the recent trend downward in violent crime, and a historical account of prisons and capital punishment. We can find fascinating statistics about racial disparities among criminals and victims throughout the last century. A series of tables and charts give a succinct view of illegal drug markets and theft statistics over the last 30 years as well.

Separate chapters devote attention to assessment, prisons, private and public security measures, guns, alcohol and illegal drugs, and juvenile crime. I was intrigued to learn that we might have had crime statistics collected by state bureaus rather than police, if only

Roscoe Pound could have overwhelmed J. Edgar Hoover (p. 40). Equally fascinating is the information at just how hamstrung the ATF is in carrying out its responsibilities (p. 183ff).

Although the authors disavow any particular theory or ideology, economics looms large in their analysis. They point out, for instance, that people most likely to be in jail are poor and poorly educated with few career alternatives (pp. 29, 222). They stress the opportunity cost of public funds used for corrections as opposed to educational and social-welfare expenditures (p. 92). They refer to incentives for bureaucrats to protect turf and for gun dealers to move business to rural areas if gun laws are more lax there (p. 204).

I have only a few minor criticisms. The research of others is sometimes presented too uncritically. Statistics occasionally grow overwhelming and might better have appeared in footnotes. I was not persuaded that better use of funds is all we need to implement their suggestions, rather than more funds (p. 65). At the very least, transition costs could be enormous. I would have liked to see a little more comparison to other countries.

This is not a book to read in one sitting. Although the writing is lucid and compelling, there is so much information to absorb that one needs time to ponder it. Yet it is a book to re-read often for its wealth of statistics and reference material as well as its dispassionate and succinct approach.

The authors are optimistic that Americans can be more rational in their response to crime. But they also say we will have to overcome short-time-horizon politics (p. 282). Unfortunately, therein lies the rub.

JENNY B. WAHL, *Carleton College*

Fighting for the Farm: Rural America Transformed. Edited by Jane Adams. Philadelphia: University of Pennsylvania Press, 2003. Pp. 338. $55.00, cloth; $22.50, paper.

The essays in *Fighting for the Farm* will be well referenced by the economic historian with the fortitude to cut through the tough hide of jargon that is formed by the introductory remarks to the volume and by the introductory sections of most of the essays. The 13 essays address issues of rural political economy in the twentieth century. The contributors are primarily sociologists; there are a small number of political scientists; two contributors have economics backgrounds. No effort is made to make the jargon of the various disciplines accessible to the others. For example, the reader not trained in sociology will often be stopped by phrases such as "an attempt to shift paradigmatic assumptions through alternative tropology (p. 5)." Readers should, however, try to complete the book because the essays have a lot of meat. And, taken together, the essays seem to come from a single animal, which is a welcome contrast to the many collections of essays that seem to be randomly assembled from parts of disparate animals.

In her introduction, Jane Adams writes that most scholars of twentieth-century political economy have studied the state (for example, the law or administrative policy), rather than the people (p. 3). *Fighting for the Farm* provides contrast to the state-centered studies by providing case histories of the political participation by agricultural interest groups. Space considerations prevent discussion of each of the 13 essays here; instead I present an example from each of the four main sections of the volume.

The first section considers the relationship between farm owners and workers in North America and world agricultural markets. In the final essay of this section, Miriam Wells considers the 1960–1990 period when improvements in shipping technology allowed central California growers to become a force in world fruit markets. Wells shows how the political alliances of strawberry sharecroppers changed over the period. She argues that ethnic identification of sharecroppers with wage workers led to political realignment. The reader is left wondering, however, whether there might also have been changes in the strawberry sharecroppers' contracts over the period that might have created incentives for their political realignment.

The second section contains three essays devoted to discussion of Progressive and New Deal agricultural policies and one essay on the financial crisis of the 1980s. The essay on the financial crisis, contributed by Barry Barnett, does not mesh well with the other essays, but provides a primer on twentieth-century international economic history. The other essays in the section have more in common. Mary Summers contributes an argument for re-examination of the internal politics of the agricultural New Deal that will be of particular interest to economic historians; the questions she raises about the way New Deal policies evolved are hypotheses begging to be tested by an enterprising cliometrician.

The third section, comprised of just two essays, considers moral and ethical dilemmas in modern agricultural production. Laura DeLind contributes an essay on community supported agriculture projects that is sure to provoke further research by economists interesting in micro-finance as well as those interested in estimating the value of the "organic" label. Community-supported agriculture is a risk-sharing arrangement with a moral twist: A farmer or group of farmers contract with a group of consumers to deliver a share of the harvest, plus membership in an "agricultural community" in exchange for payment up front. DeLind presents evidence, based on surveys of members of community supported agriculture projects, that consumers put little value on community, although they may place high value on the fresh and organic attributes of the produce.

The fourth and final section contains three essays on environment, politics, and farming. Ann Reisner's contribution looks at the controversy over genetic modification of food and fiber crops. She compares the arguments of leaders of groups who object to genetic modification to the arguments of proponents of genetic modification in agriculture, and shows that the arguments of the objectors have influenced public policy. Her method, formal discourse analysis, may be unfamiliar to many economists, but the texts she examines are informative for the scholar interested in the politics of interest groups.

Again, *Fighting for the Farm* is a volume that should be read. I fear, however, that many will not read it. The stated aim of the volume is to present a diverse collection of "disciplinary and theoretical perspectives" (p. 3) on the development of modern agricultural politics. In this, the volume succeeds. But it leaves, still, the task of bridging disciplines so that scholars can communicate more effectively across their various theoretical perspectives.

MARY ESCHELBACH HANSEN, *American University*

Making and Selling Cars: Innovation and Change in the U.S. Automotive Industry. By James M. Rubinstein. Baltimore and London: The Johns Hopkins University Press, 2001. Pp. ix, 401. $45.00.

Disaster in Dearborn: The Story of the Edsel. By Thomas E. Bonsall. Stanford, CA: Stanford University Press, 2002. Pp. v, 230. $35.95.

Books on the Civil War are the backbone of American history publishing. A few years ago, the top five sellers for the History Book Club were all Civil War books. Car books play a similar role in business history publishing. Each year brings a new crop, devoted to examining issues large and small. The two books under review here are useful additions to this very large literature.

James M. Rubinstein's history of the U.S. automobile industry focuses on the evolution of car company practices in three areas: production, labor relations, and marketing. He devotes a chapter to early industry practice in each area and a second chapter to current practice. Rubinstein knows the primary sources well and he does a good job of presenting detailed material on the nuts and bolts of the industry. For instance, he does a superb job in setting out the evolution of the basic automobile production system from the early days of Henry Ford down to the present. I never expected to read a better account of Ford's introduction of the moving assembly line than David Hounshell's (*From the American System to Mass Production 1800–1932: The Development of Manufacturing Technology in the United States*. Baltimore and London: The Johns Hopkins University Press, 1984), but Rubinstein has managed it. Although Hounshell is better on how the moving assembly line came to be developed, Rubinstein is better on the details of how it actually worked.

Here we come to the two main shortcomings of Rubinstein's book. First, it is terrific at telling us how the automobile companies did what they did, but weak on telling us *why* they did it this way. There is little in the book on corporate strategy. It is noteworthy that he makes no reference to any of the work of Alfred Chandler. Because Rubinstein's book lacks any unifying themes, it is hard for the reader to gauge the significance of the mass of detail presented. For instance, he spends 15 pages discussing the evolution of the system of supplying each car sub-assembly. The reader is told a great deal about the companies involved, what they did, and how they did it. Much of it is interesting, but is there a larger point?

Second, Rubinstein's constant shifting back and forth between historic practice and current practice leads to repetition and, occasionally, contradictions. Most accounts of the early history of the U.S. automobile industry, following Chandler, put the struggle between Ford and General Motors during the 1920s front and center. It is hard to know what Rubinstein thinks of this key episode. He takes three passes at explaining it. Here is his first explanation of how GM overtook Ford for leadership of the industry:

> Enough Americans were fed up with Henry Ford's ignorant, bigoted pronouncements and brutal treatment of workers that they refused to buy Ford cars. Ford's market share fell from 51 percent in 1924 to 20 percent in 1942 . . . . (p. 29)

Here is his second:

> Ford's Model T was offered only in black because other colors of paint took much longer to dry. GM passed Ford in sales during the 1920s in large measure by applying fast-drying paint in rich deep colors. (p. 81)

And here is his third:

> Ford built one car for the masses, and that was good enough in the 1910s. By the end of the 1920s GM was selling cars to people in every social class, from the humblest factory worker to the loftiest aristocrat. (p. 193)

With a more linear narrative, Rubinstein would have had to make up his mind about what his conclusions were on this and other issues.

So, Rubinstein has written a book that provides a wealth of information on the evolution of the automobile industry. In particular, it is the best discussion of the evolution of the process of manufacturing cars that I know of. But anyone interested in an explanation of what it all means needs to look elsewhere.

The Ford Edsel is the Titanic of business history: a famous disaster that still fascinates in the *n*th retelling. In this revisionist account, Thomas E. Bonsall argues that the Edsel was not such a bad car after all. In fact, the "Edsel was actually a modest success that deserved continued support" from Ford management (p. 204). This is kind of like arguing that the captain of the Titanic could have made it to port if he had just steamed on and ignored that hole in the side of the ship. Yet Bonsall—who has also written on the Titanic—makes a plausible case.

By 1954, Henry Ford II was having some success in reviving the company he had taken over from his grandfather in 1945. Sales of the Ford model were essentially equal to sales of GM's Chevrolet, and Ford's Lincoln had respectable sales in the luxury car market. It was in the market for mid-priced cars that the company was lagging. Ford had only the Mercury to offer against GM's Pontiac, Oldsmobile, and Buick and Chrysler's Dodge, De Soto, and Chrysler. Mercury's share of this market segment appeared to have stalled at about 12 percent.

In 1955, Ford executives decided to launch a new mid-priced model: the Edsel, named after Henry Ford's son and Henry Ford II's father. To manufacture and market the car, a new division within the company was created, complete with a new network of 1,500 dealers. The Edsel was launched with great fanfare in September 1957, but within weeks Ford executives realized they would be unable to meet their annual sales target of 200,000 cars. In fact, first-year sales amounted to only 63,100 cars, and second-year sales were an even more dismal 44,891. In January 1958, the Edsel division was merged with the Lincoln-Mercury division and thousands of white-collar workers were fired. Finally, in November 1959 the Edsel was discontinued.

Why was the Edsel a failure? The conventional view is that its unusual front end, including a vertical grille—the infamous "horse collar"—made it one of the ugliest cars ever produced. It also had severe reliability problems. Bonsall quotes an early review by a test driver for *Motor Trend* (p. 145):

> [The Edsel Corsair being tested] burnt out one transmission motor . . . and wore sheet metal that appeared to have been fitted in the dark . . . . Unhappily, the Corsair windshield leaked madly during a rainstorm, as did a yet unfound hole under the dash which filled my left shoe with water.

The decision to set up an entirely new dealer network, rather than sell the car through existing Ford or Lincoln-Mercury dealers, was probably also a mistake.

But Bonsall argues that the failure of the Edsel was really a failure of Ford management. The Edsel was launched one month after the start of the 1957/58 recession. The recession hit the automobile market hard, particularly the mid-priced segment. Edsel sales during 1958 were disappointing, but so were those of its major competitors. According to Bonsall, Buick sales were 64 percent below their 1954 levels, Pontiac sales were down 57 percent, and Oldsmobile sales were down 48 percent. The Edsel captured about 5 percent of this now smaller mid-priced market, about in line with original projections, albeit with a much smaller unit volume than projected. That Ford executives failed to see the Edsel as a relative success was partially due to the appointment of Robert McNamara to the position of executive vice-president with responsibility for the company's car and truck divisions in May 1957. He replaced Lewis Crusoe, who had been a strong supporter of the Edsel. McNamara, on the other hand, was convinced that the company needed to purge its slow-selling models. Skeptical of the Edsel from the start, McNamara appears to have decided as early as the fall of 1957, shortly after the first disappointing sales numbers were in, that the Edsel should eventually be eliminated. Bonsall believes that "the Edsel would have made it if the company had just hung in there. Instead, Ford Motor Company turned a potential silk purse into a certain sow's ear . . ." (p. 204).

Bonsall's book is a quick read at 230 pages, at least half of which have photographs. Although published by a university press and based on archival research and original interviews, the book reads like something written for car enthusiasts. There is no credit line for any of the photographs (something that, as I recall, the Ford Archives requires when reproducing any of their photographs). There are a scattering of endnotes, but at key points —including his accounts of some of the meetings of Ford executives at which the most important decisions about the Edsel were made—Bonsall does not indicate his sources. Many of his endnotes read simply: "Ford Motor Company papers, author's collection." He provides no elaboration of what these papers may be—memoranda, minutes of committee meetings, or what have you—or where they came from. Are they photocopies of original documents in the Ford Archives and other collections—in which case citations to the original sources would have been helpful—or are they original documents collected by Bonsall? Despite these shortcomings in his scholarly apparatus, Bonsall has made a useful contribution to the history of managerial decision making in the postwar automobile industry.

ANTHONY PATRICK O'BRIEN, *Lehigh University*

Airlines and Air Mail: The Post Office and the Birth of the Commercial Aviation Industry. By F. Robert van der Linden. Lexington: University Press of Kentucky, 2002. Pp. xvi, 349. $35.00.

Historians have long known, in general terms, that the federal government supported emerging transport industries—stagecoaches, steamships, railroads, and airlines—by awarding contracts to carry the mail. F. Robert van der Linden adds flesh to this skeletal knowledge. In the first book-length study of the Post Office's involvement in one such industry, van der Linden finds that mail contracts determined both the basic structure and minute operational details of American aviation through its formative years and beyond.

In van der Linden's account, Progressive Republicans of the Coolidge and especially Hoover administrations wielded lucrative mail contracts to support the fledgling airline industry and produce public benefits beyond simply transporting letters. Most notably, the Post Office Department deployed its resources to foster air transport service that would attract passengers. In the long run, visionary postal officials believed, an airline industry earning most of its revenue from passengers would be in better position to carry mail than one largely dependent on government contracts.

The early barnstorming operations—small, poorly capitalized airlines flying unsafe craft over short routes on irregular schedules—did not suit the needs of the Post Office or most passengers. Dangling mail contracts as incentives, the department encouraged private firms to provide regular service using larger, safer equipment (the Post Office paid a premium to those whose craft could carry passengers) over long distances. The Post Office looked askance at new, small firms founded solely to land mail contracts. Although the upstarts could underbid larger competitors, the department exercised its contracting discretion to minimize such "destructive competition" among firms (p. 155).

The cozy relationship between the Post Office and vertically integrated aviation holding companies provoked a backlash when Democrats took control of Congress during Franklin Roosevelt's administration. Small airlines that coveted mail contracts aligned with Populist Democrats who resented the aviation oligopoly protected by the Post Office. With Senator (later Supreme Court Justice) Hugo Black spearheading the investigations, Democrats claimed to find evidence of fraudulent collusion between postal officials and aviation holding companies. Van der Linden argues that Democrats misunderstood or intentionally mischaracterized this relationship. Nevertheless, Senator Black prevailed on Roosevelt to abrogate most of the airmail contracts. Army Air Corps pilots stepped in to carry the mail until a number of fatal accidents prompted an embarrassed administration to put the contracts up for bid again. Although this round of political maneuvering had been partly inspired by small airlines angling for mail contracts, in the end the larger established carriers reemerged to demonstrate that they best managed the complex operations involved in flying the mail.

Van der Linden thoroughly details how the decisions of postal officials influenced airline investors, mergers, research and development, equipment, routes, speed, cost accounting, lobbying, public relations, and more. Van der Linden's analysis follows, as he acknowledges, in the tradition of Ellis Hawley's work on the associative state. The Post Office became the de facto airline regulator until Congress limited officials' discretion in 1934. The same legislation also barred units of aviation trusts from bidding on mail contacts, prompting American, Eastern, and Trans World Airlines to divest their manufacturing interests (the United Airline-Boeing combine also restructured). Despite this congressional tinkering, van der Linden argues, the industry structure fostered by the postal establishment in the 1920s and early 1930s prevailed through the era of oversight by the Civil Aeronautics Board—indeed, until deregulation in 1976.

Although van der Linden's study has much to commend, the work suffers from a few flaws. A handful of tables—there are none—could have nicely summarized postal expenditures, contract amounts, volume of mail transported, and other basic data. Imprecise word choices mar the writing in places. And the author ties together the book's themes with a few paragraphs at the end when a full concluding chapter is warranted. The index, however, is quite serviceable.

This book helps historians appreciate the manifold ways that the airline industry responded to levers in the hands of the Post Office. More generally, it stands as yet another reminder of how private enterprise depended on government enterprise. Although not a perfect model, *Airlines and Air Mail* could inspire studies of how the Post Office influenced other transportation industries, most importantly railroads.

RICHARD B. KIELBOWICZ, *University of Washington*

Defining Global Justice: The History of U.S. International Labor Standards Policy. By Edward C. Lorenz. University of Notre Dame Press, 2001. Pp vii, 318. \$43.95, cloth; 23.95, paper.

This is the sort of book many neo-classical economists, myself included, will want to sit down and have an argument with. Not only because the author has some unkind criticisms of neoclassical economists but also because economists in general view labor standards as a means of limiting international trade. Once one gets over the urge to argue, however, one finds this history of U.S. international labor standards policy, and the International Labor Organization (ILO), an intriguing story that offers insights into the evolution of international organizations and their ability to define norms of behavior.

Few economic historians would disagree with the author, Edward Lorenz, that the biggest and most successful international labor standards movement was the abolition of slavery. The author approvingly cites Robert Fogel (*Without Consent or Contract*, W. W. Norton, New York and London, 1989: 410) who, in turn used Seymour Drescher's term "econocide," to describe the abolition of slavery: "incited by men ablaze with moral fervor." In the United States, and internationally, there were direct links between the abolitionist struggle and international labor standards. This leads directly into the forces, and "moral" principles, that could sustain both a movement and the ILO over such a long period of time.

The ILO was created by the Treaty of Versailles and was part of The League of Nations. It has survived in basically the same form from 1919 to the present and is currently a part of the United Nations. It functioned, and continues to function, with member states appointing delegates representing business, labor, and government. The ILO passes conventions that are adopted by member states and have the force of treaties. It was the only organization the of League the United States ever joined (in the 1930s).

The basic principles that were adopted in 1919 were that "labor should not be regarded as a commodity"; freedom of association should be guaranteed, as should wages high enough to maintain a reasonable standard of life, equal treatment of all workers legally resident in a country, abolition of child labor, one day a week free from labor, and for women, equal pay for equal work.

A later convention against forced labor created great controversy and perhaps illustrates why labor standards remain relevant and controversial. Forced labor was a common practice in Stalinist countries. Oddly enough U.S. governmental and business delegates opposed the convention. Business delegates because they saw this as an opening for other conventions they opposed. The State Department, ironically, because it said they could not authorize government action on any convention dealing with labor standards because they were a state prerogative under the Constitution. This left many Americans either confused or angry because this was precisely what they had thought the Civil War had settled.

The U.S. adopted this convention, passed by the ILO in 1957, during the early nineties under the Bush administration. Under the Clinton administration the Senate also unanimously adopted the child labor convention.

This is a valuable book in that it carefully and fully lays out the philosophical and legal arguments for labor standards and documents when they have been applied over a long period of history. My own conclusion, and one the author misses, is that any reasonable market system must have institutional safeguards that respect the right of individuals to form contracts. As such there are powerful neoclassical arguments against both forced labor and child labor. In neither case can we say contracts are freely entered into, and, as a result, one would conclude that labor markets are "inefficient" in a "positive" sense and "immoral" in a "normative" sense. The value of this book is it challenges us to think through our assumptions and look once again at the way we view globalization in both the past and present.

LAWRENCE W. BOYD, *University of Hawaii*

The Cotton Dust Papers: Science, Politics, and Power in the "Discovery" of Byssinosis in the U.S. By Charles Levenstein and Gregory F. De Laurier with Mary Lee Dunn. Amityville, New York: Baywood Publishing Company, Inc., 2002. Pp. xvi, 160. $32.95.

Byssinosis is an occupational lung disease found among textile workers who labor in factories filled with "cotton dust," a mixture of lint, plant particles, pesticides, soil, fungi, and bacteria. Most sufferers are found in the dustiest parts of the mills where cotton bales are opened. Scientists now identify the specific cause of the disease as the toxin "bract" found in the leaves around the boll. When inhaled, the toxin releases histamines leading to coughing, chest tightness, and shortness of breath. It can be fatal when it leads to a heart attack or pneumonia. During the 1970s, public health officials and reformers drew considerable attention to byssinosis when Ralph Nader dubbed the disease "brown lung," linking it to the "black lung" of coal miners. Elderly sufferers of "brown lung" testified before Congress from their wheelchairs in presentations interrupted by wheezing. Then in 1976, the Occupational Safety and Health Administration issued standards requiring mills to monitor air quality, install ventilation, and test workers' health.

The tone of *The Cotton Dust Papers* is passionate with authors determined to overturn what they characterize as the "hegemonic ideology" surrounding occupational health and safety, "the compensation-safety apparatus." The authors define this "apparatus" as a professional ethos that requires public health officials to accept laissez-faire capitalism without question, to avoid government intervention in business, and to seek cooperation over confrontation.

The core of the book is a valuable examination of the efforts of industrial hygienists and medical researchers to confirm the existence of byssinosis. Beginning in the 1930s, public health officials in North Carolina and Georgia knew of lung diseases among textile workers but could not gain access to the mills to document their suspicions. One chapter opens new primary evidence from a lawsuit filed by victims of byssinosis against the leading insurer in the textile industry, Liberty Mutual, a company that restrained its researchers from publishing findings about the lung disease.

The other important element of this study is a comparison of the discovery of byssinosis with that of shuttle kissing in early twentieth-century Massachusetts. Shuttle kissing was the practice of weavers who threaded a shuttle by placing their lips over its eye and inhaling the yarn. The authors argue that shuttle kissing became illegal in 1911 because of the public's fear of tuberculosis and because of the support of the Draper family, the manufacturers of the automatic loom that made yarn inhalation obsolete.

The authors find a similar set of conditions surrounding the discovery of byssinosis—a climate disposed toward reform and an "enabling" textile company. During the 1970s it was Burlington Industries that separated itself from the rest of the industry and allowed health researchers to test for byssinosis in its cotton mills. The key executive implementing Burlington's health program was a vice president named Charles McClendon. The authors believe that both Draper and Burlington could afford to recognize diseases because they were large, profitable companies easily able to install the latest technology and wanting to create a favorable public image.

Despite its reliance on newly opened primary sources and its compelling comparisons, this book must be read carefully. In the first chapter, the authors confuse yarn with thread, two entirely different textile products. There are also a number of spelling, grammatical, and proofing errors that lower confidence in the study. In addition, the authors write without a firm mastery of southern labor and political history. For example, it is crucial to know that the Burlington executive, Charles McClendon, who allowed testing of workers, was the grandson of the Progressive North Carolina governor, Charles B. Aycock, and the son of Major McClendon, a lawyer who specialized in union busting. It is also frustrating that the authors ignore the classic study of southern textile workers, *Like a Family* (Jacquelyn Dowd Hall, James Leloudis, Robert Korstad, Mary Murphy, Lu Ann Jones, and Christopher B. Daly. Chapel Hill: University of North Carolina Press, 1987). In *The Cotton Dust Papers*, southern workers are almost invisible, docile and oppressed, a portrait that is in strong contrast to that painted in *Like a Family*.

Despite these discomforting failings, these authors do contribute to the field of southern business and labor history. From now on, notice will be taken of the industrial hygienists, of the industry's leading insurer, Liberty Mutual, and of the few companies that defied the industry's mind-set. On the broadest level, this study complicates the history of the textile industry introducing new actors in the struggle for control of the work place. Ironically, the authors themselves mask their accomplishments with their ideological rhetoric about the "compensation-safety apparatus." Although their narrative reveals how complex southern textile history can be, they continue to support simplifications.

ANNETTE WRIGHT, *University of North Carolina at Chapel Hill*

Problems of Plenty: The American Farmer in the Twentieth Century. By R. Douglas Hurt. Chicago: Ivan R. Dee, 2002. Pp. xiii, 192. $24.95.

Problems of Plenty is a concise but rich portrayal of American agriculture during the twentieth century. Published without footnotes and aimed at an undergraduate audience, it nonetheless has much to offer to scholars in agricultural history as well as the related fields of economic and political history. R. Douglas Hurt writes clearly and covers a wide range of material, and even though he adopts a narrative style and organizes his book chronologically, he does advance a thesis. Throughout the twentieth century, American farmers have grown increasingly dependent on the federal government in spite of the older agrarian tenets of self-sufficiency and independence. And, even though the thrust of government policy has been to raise farm prices by limiting production, advances in science and technology have led to fewer and fewer farmers raising more crops and commodities and becoming ever more productive. These "problems of plenty" have been compounded by the diversity of interests among the nation's agriculturalists, which has prevented them from speaking with one voice in the political arena, as well as by the sheer and precipitous decline in the absolute numbers of farmers after World War II.

The book consists of six substantive chapters, each dealing with a discrete period in U.S. agricultural history in terms of the development of agricultural policy, key changes in agricultural technology and farm life, and the roles of different agricultural organizations. The Progressive Era witnessed a marked expansion of the regulatory state in the countryside through the Country Life Movement, the rise of the modern cooperative movement, and the further development of agricultural education and extension services as well as a raft of legislative measures directed at railroad regulation, tariffs, good roads, and the rural banking system. The agricultural depression of the 1920s posed new challenges and responses, however, and Hurt focuses his second chapter on the emergence of the Farm Bloc and the intricacies of the McNary-Haugenism and the Capper-Volstead Cooperative Marketing Act, as well as the dramatic effects of the tractor and power take-off and hybrid seeds on agricultural production. Chapter 3 provides a balanced view of the alphabet soup of the different New Deal agencies directed at the problems of rural America, and the discussion of World War II and the postwar period chronicles the return of prosperity coupled with the declining numbers of people employed in agriculture and the growing importance of off-farm employment. Hurt ends his survey with two chapters on more recent trends during the 1960s, 70s, and 80s, which provide a useful context for understanding such notable developments as the Farm Crisis, the American Agricultural Movement, and the tractorcades of the 1970s, as well as more recent Republican efforts to reduce the government's role in the agrarian economy. The last chapter, "Century's End," introduces such present-day concerns as the impact of genetically modified crops, the environmental politics of large-scale agribusiness, and the rising importance of consumers as a new force in the political economy of our nation's food production.

Given his predominant concerns with agricultural production and technology and issues of farm policy, Hurt employs a very traditional conception of agricultural history to frame his study. Even though his analysis would be strengthened by more attention to recent work on rural social history, his synthesis is extremely valuable because so few scholars write about these important issues. Even as a "traditional" work of agricultural history, however, *Problems of Plenty* would also benefit from more incorporation of the rich theoretical literature by historical sociologists and political scientists on state formation and more attention to the increasing importance and impact of globalization and international issues —not only in terms of farmers' markets but also as avenues for U.S. agricultural extension and expertise. These caveats aside, I cannot think of a better or more accessible discussion of the changing political economy of U.S. agriculture during the twentieth century.

HAL S. BARRON, *Harvey Mudd College* and *Claremont Graduate University*

GENERAL AND MISCELLANEOUS

The Backbone of History: Health and Nutrition in the Western Hemisphere. Edited by Richard H. Steckel and Jerome C. Rose. Cambridge: Cambridge University Press, 2002. Pp. xx, 633. $75.00.

Few books force readers to rethink fundamental paradigms. Even thoroughly researched and beautifully written studies often succeed at refining only minor issues. Collections of essays, which always contain inconsistencies in style and normally contain parts that do not necessarily work together to form a satisfying whole, are even less likely to achieve such success. Yet *The Backbone of History*, a book crammed with highly technical papers from a variety of disciplines, offers the kinds of detailed evidence and thoughtful analyses that force its readers to consider anew basic aspects of the past.

This is not a pleasurable read; it lacks a clear narrative and sparkling prose. The subject is also bleak. A map opposite the title page shows the "localities of burial sites" from which skeletal remains have been exhumed and subjected to intensive analysis, and the contents consist of the kinds of technical reports commonly found in scholarly journals. Only on occasion do authors offer substantive comments on others papers. Further, those not trained in bioarchaeology, paleodemography, or physical anthropology might find much of the technical discussion difficult.

Still, even a reader put off by the book's style will be impressed by each paper's analysis of parts of what the editors call the "Western Hemisphere database," a set of data that includes "observations on 12,520 individuals who lived over the past several millennia at 65 composite sites in the Western Hemisphere" (p. 583). Close reading of the 19 substantive essays here (in addition to four shorter contributions) offers astounding rewards. The first group of chapters provides a methodology for understanding the database. It is here that the reader first confronts the technical language and methods that are common in the later chapters, which focus on particular regions. Over the course of the book, the reader becomes sensitive to the things that skeletal remains can and cannot reveal. Bones and teeth, which are what the database is all about, provide insight into a variety of physical conditions, notably linear enamel hypoplasias (LEH) caused by deficient diet, degenerative joint disease (DJD) caused by certain kinds of labor, and stature, which provides clues to diet and nutrition. But bones and teeth cannot tell much about certain infectious diseases, including (as Philip D. Curtin points out in his brief but suggestive review of the findings), smallpox, influenza, or yellow fever, "the most devastating of the newly introduced diseases" that arrived after 1492 (p. 606). In other words, the data to be extracted from the remains are enormous, but, as authors here acknowledge, far from perfect.

Still, despite the limitations of the evidence, the studies here make certain points clear. First, a number of the articles confirm that the development of agriculture tended to have negative consequences for human health. That finding might be surprising to some, but various authors here note that the decline in health reflects the tendency of agricultural peoples to limit their range of foods (compared to the foods consumed by those who hunted and gathered), engage in more sedentary behaviors, and cluster together in larger numbers. In other words, some of the defining features of what Europeans considered "civilization" came at a high cost. This confirmation of the unfortunate consequences of the shift toward agriculture has a second meaning that is crucial for understanding the history of the Americas: the native peoples of the Western Hemisphere before 1492 inhabited societies in which their health was already in decline, at least compared to those who lived earlier. That recognition does not diminish the significance of the so-called Columbian Exchange and the lethal spread of contagious diseases that reduced indigenous American populations by perhaps 90 percent from 1492 to 1800. But it does put to rest the idea that Native peoples in the Americas lived in a pristine paradise free of health problems. Even the cultivation of maize, the crop most responsible for population growth, created problems when communities came to rely on it too extensively and hence suffered from nutritional deficiencies. Third, no single pattern characterized the indigenous peoples of the Americas; the health of particular peoples reflected their diet, economy, and availability of natural resources. Some groups were among the healthiest in the Americas, others the least healthy. Climate, surprisingly, played only a minor role in determining the health of any population. As Ann L. W. Stodder and her coauthors demonstrate, in the American Southwest the indigenous population has withstood extraordinary stress (including devastating droughts) over the past 1,500 years yet its modern population might equal the number there at the time of European contact.

It is a testament to the editors skill that each of the essays in the volume deserves mention in a review. Among the most engaging are those that took advantage of records beyond bones and teeth. Rosanne L. Higgins and her colleagues produced an extraordinary account of the Monroe County Almshouse in mid-nineteenth century Rochester; and C. S. Larsen, Alfred W. Crosby and their collaborators crafted a masterful survey of the health of the residents of the Georgia Bight. Richard Steckel and Jerome Rose, who guide the reader through this labyrinth of detail, ably summarize the findings and relate the skeletal analyses to better known historical forces. "The populations most easily conquered were also the least healthy," they conclude, "who lived in upland urban areas and relied heavily on domesticated plants" (p. 578). Contrary to expectations, the residents of hunter-gatherer societies posed a more severe challenge to the invaders. That insight, combined with the many others in this extraordinary volume, now must be confronted by every scholar of the Native American experience. The fact that it came from scientific analysis of human remains and not the more traditional methods of historians suggests the benefits of genuine interdisciplinary inquiry.

PETER C. MANCALL, *University of Southern California*

Merchants and Migrations: Germans and Americans in Connection, 1776–1835. By Sam A. Mustafa. Aldershot, UK, and Burlington, VT: Ashgate Press, 2001. Pp. xvii, 284. $79.95.

In the last two decades scholars of the Americas, Europe, and Africa have addressed the transformation of the Atlantic World and have focused on voluntary and forced migration, colonization, and commerce, emphasizing the central role of Britain. In *Merchants and*

Migration, Sam Mustafa provides an alternative view of Atlantic connections by examining the developing relationship between German and American merchants following the American Revolution to the demise of the Napoleonic Empire. Mustafa's Germans are primarily Hanseatic merchants from Hamburg and Bremen, and to a lesser extent government representatives of Prussia and merchants from Saxony. Likewise, he features the role of American merchants and the "first generation" of American political leadership. Mustafa identifies three factors that defined U.S.–German relations during this period: the prominence of the Hanseatic cities of Hamburg and Bremen, the lack of state intervention in the establishment of commercial relations, and the slow evolution of diplomatic relations based on commercial connections.

Mustafa's focus on the crucial relationship between Hanseatic and American merchants is key to this study. He explores the common "Merchant Culture" of Hamburg, Bremen, Baltimore, and Philadelphia and highlights independent merchants with a commitment to free trade, prosperity, Protestant ethics, political republicanism (although republicanism on each side of the Atlantic is not clearly defined), a disdain for the nobility, and a cosmopolitan world view. Mustafa argues that, despite official treaties with Prussia, American merchants were drawn to "the free-trading capitalist republics of the Hanse," which reflected their own values and society (p. 179). His view of a shared merchant culture is intriguing, but the issue seems more complex. His argument would have benefited from tangible evidence of common commercial culture, through a comparative analysis of the cities' merchant presses or associations, for example. Nevertheless, his thesis, based on individual merchant records, diaries, and correspondence, that the Hanseatic cities provided a crucial "conduit" for U.S.–German commercial relations is a clear contribution to our understanding of Atlantic history.

Mustafa examines the migration of individual merchants and their families, rather than large groups of immigrants. He recounts lively stories of commercial successes and failures to illustrate the range of opportunities for prosperity and numerable pitfalls of trans-Atlantic trade. These stories also depict functioning merchant networks based on kinship, friendship, and personal initiative. And in the case of Charles Buck, John Parish, Arnold Delius, and Fredrich Jacob Wichelshausen, merchants also became consuls, representing the commercial interests of the young American republic. It is at this point that Mustafa sees a correlation between the world of commerce and diplomacy.

Mustafa also presents German–American diplomacy in the decades following the American Revolution as "confused and ineffectual." The exception, he points out, were the consuls: "the lowest official levels, where diplomacy intersected with trade." The consuls, according to Mustafa, acted as "agents of commerce, transmitters of information, and conduits of diplomacy (p. 99)." Yet, he emphasizes commerce between the Hanseatic cities proceeded without any treaty or state intervention, whereas the U.S.–Prussian treaties failed to initiate a substantial commercial relationship. Viewed from a purely economic perspective this may seem odd, but Mustafa locates his study in the midst of the tumultuous Revolutionary and Napoleonic Wars. During these years when international security was preeminent, especially for second rate powers such as Prussia and small neutral states such as the Hanseatic cities, the contradiction between commercial and diplomatic relationships is not surprising. The United States could not have provided Hamburg and Bremen with the necessary security to defend their autonomy and commerce amidst international warfare, but it could provide colonial goods and markets. Mustafa depicts clearly how Napoleon's Continental System destroyed both German–American trade and the Hanseatic economies. After the Napoleonic Wars, both the United States and the Hanseatic cities (now acting as one entity) established a reciprocal trade treaty in 1827. Mustafa concludes his study with the assertion that the new American agreements with German states (including Prussia) in the 1820s and 1830s established a "meaningful diplomatic and commercial relationship that witnessed the recovery of German-American trade (p. 242)." Thus, he suggests that those late-eighteenth-century merchants and consuls laid the groundwork for the developing German–American relations of the 1800s.

Economic historians will benefit from the microeconomic details of trade between the U.S. Atlantic port cities and the Hanseatic cities before and after the Napoleonic Wars. Mustafa charts the importance of American tobacco to Bremen and that of Central American goods to Hamburg and illustrates the difficulty of German products, except linens and metalwork, in the U.S. marketplace. His research into American archives is impressive. On the other side of the ocean, it is clear Mustafa is most comfortable with Bremen's sources. He relies too heavily on old secondary sources for Hamburg and Prussia, and would have benefited from Burghart Schmidt's *Hamburg im Zeitalter der Französischen Revolution und Napoleons* (Hamburg: Verein für Hamburgische Geschichte, 1998) analysis of Hamburg's economy. Finally, a tighter organization would have limited repetition. The strengths of this study, however, far outnumber its weaknesses, as it presents an important comparative approach to explain developing commercial relations between Atlantic port cities.

KATHERINE B. AASLESTAD, *West Virginia University*

Technological Change and the Environment. Edited by Arnulf Grübler, Nebojsa Nakicenovic, and William D. Nordhaus. Co-publication of Resources for the Future, Washington, D.C., and Institute for Applied Systems Analysis, Laxenburg, Austria, 2002. Pp. v, 409. $49.00.

This collection focusing on the roles of policy and price signals in shaping technological changes affecting greenhouse (CO_2) gas emissions is based on a series of high-quality workshops at the International Institute for Applied Systems Analysis held between 1997 and 1999. The 12 papers include contributions from leading scholars and roughly one-half of the papers (or earlier variants) have already been published in journals. The organizing theme is to explore the applicability of the induced technological change (ITC) approach in environmental economics models.

Economic historians will likely be familiar with debates over induced innovation and may well have some interest in global environmental policies as well. But many will feel a little distant from the analytical issues motivating much of this volume, namely, how best to incorporate ITC into large-scale computer models capturing the effects of carbon taxes on CO_2 emissions. The underlying policy issue is that such taxes may, in addition to reducing pollution in the short run through substitution channels, induce "environmentally-friendly" technological changes over the long run. This increased efficacy has important but controversial implications for the optimal level and timing of carbon taxes. Some assert ITC effects increase incentives to employ such taxes. Others (for example, Lawrence Goulder and Koshy Mathai in chapter 9) argue ITC effects imply the solution to rising CO_2 levels may be easier (justifying lower tax rates) and less pressing (full-scale abatement efforts can wait until the more efficient technologies are developed).

Three chapters in the volume hold special interest for economic historians. Chapter 2 by Vernon Ruttan skillfully lays out three main approaches to studying technological change: the induced innovation model, which he has so persuasively advocated; the evolutionary approach as formulized by Richard Nelson and Sidney Winter; and the path-dependence approach developed by Brian Arthur and Paul David. Ruttan understandably favors the first approach, arguing it has enjoyed more widespread and successful empirical application than the other two, which he views as chiefly heuristic models. But he concludes all three approaches are individually exhibiting decreasing returns and must be integrated into a more general theory.

A counterpoint to Ruttan's ecumenicalism is offered in Joel Mokyr's contribution, chapter 3. Mokyr, here as elsewhere, is a strong proponent of "technology-push" as opposed to "demand-pull" explanations. He uses advances in medicine in his critique of ITC thinking, tellingly arguing that although demand for health-improving techniques has been "more or less constant over history," little progress occurred until 1800 (p. 42). Drawing on numerous examples from the history of medicine, Mokyr develops an intriguing evolutionary model involving blind variation and selective retention. Reading these two chapters provides a quick, but stimulating way to catch up with the state-of-the-debate about ITC models.

The first half of Nebojsa Nakicenovic's chapter 7 is also valuable for its original insights into the history of U.S. energy consumption. He documents a 200-year trend of "decarbonization"—a falling ratio of CO_2 emissions to real GDP—in the American economy. The ratio has fallen at the impressive rate of 1.3 percent per year. Total emissions have risen because income growth is about 3 percent per annum. As a result, the United States to date has emitted about 100 gigatons (billion tons) of carbon, GtC, out of the estimated world's total of 250 GtC. Decarbonization appears a pervasive feature of economic development and is associated closely with transitions between energy sources. A striking observation for those nostalgic for the Arcadian past is that the "carbon intensity of fuelwood and animal feed is substantially higher than that of coal" (p. 166).

Readers wishing to survey the modeling research reported in the volume can start with chapter 12, by Leon Clarke and John Weyant, which provides something of a book review within the book. My take is that to count on the ITC to solve environmental problems requires that inventors be able to make fairly accurate projections about future price trends and technology prospects. The volume's modeling exercises do not lend much confidence. Based on computer simulations representing 130,000 scenarios (generated from "520 alternative technology dynamics," each with "250 alternative deterministic scenarios"), Andrii Gritsevskyi and Nebojsa Nakicenovis report in chapter 10, predictions of CO_2 emissions in 2100 ranging from 7 to 31 GtC. The two papers—chapter 5 by Richard Newell, Adam Jaffe, and Robert Stavins and chapter 8 by William Nordhaus—that attempt (through vastly different methods) to gauge the magnitude of ITC effects, both find they are present but relatively small. I am led to conclude with Nordhaus: "we should not look to regulatory stringency or high emission taxes as a way of forcing inventors to solve our global environmental problems" (p. 205).

PAUL RHODE, *University of North Carolina, Chapel Hill*

La piastre et le fusil: Le coût de la guerre d'Indochine, 1945–1954 [The Piaster and the Gun: The Cost of the Indochina War, 1945–1954]. By Hugues Tertrais, Paris: Comité pour l'Histoire économique et financière de la France, 2002, Pp. ix, 634. €35.00, paper.

This is an important book. If scores of studies have been written on the military, political and diplomatic aspects of the Franco-Vietnamese war (1945–1954), little serious research has gone into the economics of this war. Hugues Tertrais, associate professor at the University of Paris I (Sorbonne), has provided the first comprehensive and analytical study of the cost of the Indochina war for the French side. To do this, he went directly to the sources, above all to the archives of the Ministry of Economics and Finance and the Ministry of Defence. This research served him well.

Tertrais seeks to understand how the war cost the French in a variety of complex ways between 1945 and 1954. His major argument is that the *piastre* (the French currency for Indochina at the time) was as important as the gun. This is a very important subject for those interested in the Franco-Vietnamese war. If Jacques Marseille's *Empire colonial et capitalisme français,* (Paris, Albin Michel, 1984) provided a new understanding of French colonialism (and France) by looking at the cost of the French Empire, Tertrais provides us with equally important insights into the costs of France's first war of decolonization.

Tertrais identifies three time periods for analyzing the changing cost of the war. The first occurs between 1946 and 1948, when the war was marked by a relatively minor cost. The second covers the years between 1949 and 1951, when the Chinese communist victory and the arrival of the Cold War transformed the war into a much more costly and violent affair. Lastly, between 1952 and 1954, Tertrais shows how the financial burden of the war became such that the Americans began to underwrite it and, in so doing, increased their political leverage in Indochina to the detriment of the French. With these three periods in mind, Tertrais then divides his analysis of the question into three parts that form the structure of the book, each covering the duration of the war. The first section provides a general historical examination of how these three chronological situations developed over time. Tertrais pays close attention to the different actors and the changing nature of the financing of the war. The second part gives us a detailed economic evaluation of the cost of the war during this period: spending outlays, sources of income and financing, and the financial management (and debates) on running the war economically. The final section provides an analytical overview and pays particular attention to the financial impact of the war on the French economy, its relationship with Europe, the gradual decolonization of the colonial economy in Indochina in favor of new nation-states, and the concomitant entry of the Americans into the financial (and political) picture. Tertrais provides a large number of excellent and very helpful graphs and charts to make his point.

It is impossible to mention here all of the major contributions of such a richly researched and well argued book. Tertrais is certainly right to underline the importance of the overvaluation of the *piastre* after World War II, its belated though crucial devaluation in 1953, and how this impacted upon the financing of the war and the economic situation in both Indochina and France itself (as well as the commercial exchanges between the two). He also shows why the French suppression of the 500 *piastre* note in 1945/46 was designed to halt inflation, re-establish French sovereignty economically, and hit the enemy, the Viet Minh, where it hurt most: in their own reliance on stocks of the Indochinese *piastres* to finance their side of the war. Tertrais sheds new light on fascinating debates among a wide variety of actors about the financial politics of war, moving from the ranking ministers on high, to the military leaders on the ground in Indochina, down to the colonial lobby and illicit economic actors (his treatment of the trafficking in the *piastre* is particularly welcome). Tertrais looks at the economics of destruction, such as the impact of the war on rubber plantations and mines in Vietnam. He presents a convincing picture of how economics manifested itself nationally, with the creation in 1949/50 of the Associated States of Cambodia, Laos and especially Vietnam. And at the international level, the author successfully demonstrates how the study of the financing of war can shed new light on the changing nature of France's relationship with the United States, with Europe and, perhaps, with the remaining parts of her colonial Empire. In short, the economics of war can shed new light on its diplomatic, strategic, and military dimensions.

If Tertrais provides a large body of new and welcome French archival sources, one is struck by the absence of secondary French and English-language studies of interrelated economic and political aspects of the war and American financial aid. There are no primary sources from the U.S. archives. Nor is there any mention of the archives of the "Associated

States," held in the Ministry of Foreign Affairs in Paris and open to all since the late 1980s. Similarly, his discussion of the Viet Minh economy is superficial, overlooking recent studies of the question in French, English, and Vietnamese, which go far beyond the handful of boxes he consulted on the Viet Minh in southern Vietnam in the French military archives. The recently published memoirs of the Vietnamese Minister of Finances during the Indochina War, Le Van Hien, are not mentioned, nor are new publications in English and French examining Chinese aid to the Viet Minh and the use of opium during the war.

For the French side, however, Tertrais has made a major contribution to the economic history of the Franco-Vietnamese war in particular and to the history of France's post-1945 wars of decolonization. And the one in Indochina was not the last one the French would have to finance.

CHRISTOPHER E. GOSCHA, *Université de Lyon II*

Coping with Crisis: International Financial Institutions in the Interwar Period. Edited by Makoto Kasuya. Oxford: Oxford University Press, 2003. Pp. 235. $65.00.

This book contains papers from the Twenty-Sixth International Conference on Business History, September 2000, sponsored by the Business History Society of Japan. The title is misleading. The essays are not about international financial institutions, such as the Bank for International Settlements. They are instead about large banks, insurance companies, and securities houses, some with no international business, in Japan, the United States, Britain, France, and Germany. The essays on Japan are particularly valuable, given financial history's traditional concentration on Europe and the United States. Most of the authors are business historians, and most of the essays are narratives informed by theory and statistics, rather than presentations of sharply defined hypotheses. The book benefits from excellent copy editing (by Edmund R. Skrzypczak) and organization. Kasuya's introductory essay not only summarizes the papers and his views of general themes: it also outlines financial institutions and history for each country treated by the authors—a great idea! A question about any collective volume must be: is it more than the sum of the parts? Should the papers be read together? Here the answer is yes. There are obvious parallels and contrasts between developments in different countries, as recounted by the different authors. Less obvious ones might have come up in conference discussions but these go unrecorded in the book.

Youssef Cassis' essay, the only one making explicit international comparisons, describes personal backgrounds (class, education) of bankers in Britain, France, and Germany, and bank management and business strategies. Michael Collins and Mae Baker write about British commercial banks' longstanding avoidance of illiquid long-term loans, with a deft summary of the literature on whether this represented a market failure, and show that banks stuck to the policy through the 1930s despite political pressure to change it. They conclude that the policy "minimized both monitoring costs and potential loss from client default. For the wider economy, it also ensured bank stability . . . and the advantages that flowed from that stability" (p. 57). Eugene White describes how U.S. banks *did* take up long-term lending over the 1930s, not so much because of political pressure—though it existed—but because deposit insurance reduced the danger of deposit runoffs, and companies that would have been able to issue bonds in earlier decades, but "found it hard to issue new securities in the depressed markets" (p. 75) of the 1930s, wanted to borrow. White observes that the resulting "mismatch of assets and liabilities created no significant problem" through early 1960s, but "Banks and savings and loans were to a considerable degree unprepared for the increased economic volatility of the 1970s, and the abrupt rise in interest rates in 1979 was devastating" (p. 78).

So British banks were right after all, the reader says to himself. Long-term lending appears among other themes in Shinji Ogura's essay on Japan's Mitsui bank, but in a fundamentally different context. Mitsui had started as an "insider" bank designed to scoop up capital for a zaibatsu's business enterprises. I was struck by forebodings of Japan's current banking fiasco: Mitsui was frequently forced to make bad loans, first by the zaibatsu, then (in the late 1930s) by the government (pp. 90–96). Banks tried to escape the consequences by "cooperating" with each other to bail out and "reorganize" the borrowers (p. 103).

Eric Bussiere's essay on French banks, focusing on Paribas, suggests that inflation and capital controls crippled the recovery of Paris as a financial center: "between 1919 and 1928 only a few operations were permitted by the financial authorities when political or major national economic interests were involved. Any return to international activities between 1928 and the beginning of the 1930s was for a short period and in limited areas, thanks to the monetary restrictions of the 1930s" (p. 119). Harald Wixforth's essay depicts German universal banks sailing stormy seas of hyperinflation, depreciation, capital loss to themselves and potential borrowers, and heightened competition from other lenders, ending with the crisis of 1931. What about "bankers and Nazis"?

Mariko Tatsuki's informative essay on Japanese life insurance companies covers a lot of ground, relying, perhaps necessarily, on puzzling assertions such as "In the life insurance business, customers of the big companies were mainly upper-class people, because the insurance premiums were too expensive for everyone else" (p. 157). (Why did the companies fail to offer less coverage for less money?) Edwin J. Perkins' masterful essay on Charles Merrill, of Merrill Lynch, sets Merrill's accomplishments on a background of an excellent short history of stock markets. Against the conventional wisdom of the time, Merrill insisted on going after small customers whose fees did not cover account management costs: "he anticipated that many would, in time, initiate more frequent trades and thereby advance into the ranks of genuinely profitable accounts" (p. 195). Brokerages are reversing this policy now—what changed? Was Merrill wrong? Makoto Kasuya gives a general history of Japanese stock and bond markets—oddly free of direct zaibatsu involvement—through the early 1930s, with a slight focus on the Yamaichi Securities firm.

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The Worlds of the East India Company. Edited by H. V. Bowen, Margarette Lincoln, and Nigel Rigby. Woodbridge, Suffolk: Boydell Press, 2002. Pp. xvii, 246. $75.00.

For students of European chartered companies, *The Worlds of the East India Company* is must reading. The product of a conference of the same name held in July 2000 at Britains's National Maritime Museum and in association with the University of Leicester, the volume provides an interdisciplinary update of current research on the English East India Company. The conference commemorated the four-hundredth anniversary of the founding of the company. During the summer of 2002 the anniversaries of both the English and Dutch East India companies were further observed through thoughtful exhibits at the British Library and at the Dutch maritime museums in Amsterdam and Rotterdam. On balance the museum exhibits portrayed the optimistic note on which the conference volume concluded: "Until at least the end of the eighteenth century, . . . [the Company] was the channel through which both sides [Britain and Asia] could adapt from one another in relatively equal exchange" (p. 237).

The phrase "relatively equal exchange" points to two enduring themes in company historiography: commercial and political engagement. As the introduction explains, "A primary aim of this volume is . . . to explore the origins, operation and influence of the East India Company as a trading organization, one which remained firmly engaged in maritime commercial activity even as it began to act as the powerful agent of British territorial expansion on the Indian subcontinent." The primary aim thus illustrates the creative tension found in much of the literature: the East India Company's history is as much about the pursuit of profit as it is the progress of power.

With the exception of the lead essays and the conclusion, the volume shows that these two master narratives are not easily and, in some contexts, not necessarily entwined. The 11 central essays fall beneath the umbrella of company history. Yet each can stand alone, illustrating the depth and maturity of company historiography. One specialized line of inquiry can proceed rather independently from others. For example, Shompa Lahiri's essay portrays the debates surrounding the company's treatment of lascars, the Asian men who labored aboard company vessels. She suggests that the relationship provides, "valuable evidence of early Indo-British encounters in the period before direct rule" (p. 169). Her work bears little connection to Andrew Cook's authoritative account of the routes actually taken by those ships (pp. 119–36). Similarly Sanjay Subrahmanyam's account of English diplomacy at the Mughal court and Geoff Quilley's essay on commemorative, maritime art have little in common save a nominal relationship with the East India Company.

Is this enough to make the chapters cohere? The editors divide the company into four admittedly overlapping "worlds": the company in Britain, the company in Asia, the maritime commerce of the company, and the cultural influences and legacies of the company. For an organization of global significance, a strict delineation of topics by geography can be hazardous. The lack of international perspective on company operations can cause important, intrafirm, comparisons to be overlooked. James H. Thomas's essay, for example, explores the company's agency work within the British Isles, a topic on which little has been written. Yet he leaves for others the making of explicit comparisons with the company's overseas agency relationships, about which much has been written.

Anticipating such a critique, the editors also provide for a "world" of maritime commerce and one of cultural influences and legacies. These worlds provide organizational space for Femme Gaastra's excellent overview of the company's rivalry with the Dutch East India Company, 1600–1800. The strategies and legacies of the two firms have more similarities than differences and further comparative work along the lines set by Gaastra would be most welcome. Yet the editors' framework also invites comparisons that seem rather weakly connected to the company. David Hancock's careful investigation of Madeira wine merchants shows how their production and marketing techniques changed in response to growing demand from the tiny European-immigrant community based in India. From this he draws the astounding conclusion that "Atlantic traders imported to India" the decentralized, personal nature of Atlantic commerce and "In doing so, they brought India into their self-organizing Atlantic system" (p. 167). The linkages between company commerce and the wider spheres of indigenous Indian Ocean, Mediterranean, and Atlantic maritime commerce are undeniable. An understanding of their magnitude and importance, however, requires a greater sense of proportion than statements such as "Madeira wine merchants brought India into the Atlantic" would suggest (p.155).

For all its compromises, the editors' "worlds" framework may be the most viable way to arrange the diverse, active research lines connected to the English East India Company. Their search for a coherent approach to Janus-headed organizations such as the English East India Company is indeed praiseworthy.

SANTHI HEJEEBU, *University of Iowa*

Capitalist Development in the Twentieth Century: An Evolutionary-Keynesian Analysis. By John Cornwall and Wendy Cornwall. Cambridge and New York: Cambridge University Press, 2001. Pp. 269. $59.95.

The focus of this important book is the macroeconomic development in the twentieth century of the foremost OECD economies, and particularly the United States of America. It is not a conventional economic history but a critique of the explanatory limitations of mainstream macroeconomics and an attempt to develop a richer and more persuasive alternative theoretical approach. Like John Maynard Keynes, the authors emphasize aggregate demand in economic development, and uphold that there is no self-righting process in a market economy that can automatically bring it out of any recession. However, John and Wendy Cornwall go much further, to also emphasize the importance of macroeconomic structure and the evolution of institutions in explaining economic development. Building on their extensive former work in this area, they synthesize important aspects of institutionalist, evolutionary, and regulation-school thinking with Keynesian economics.

The text is clear and well structured. The first part, consisting of seven chapters, discusses the theoretical framework. As well as a useful critique of standard macroeconomics, they develop an extended Keynesian model with institutional and evolutionary features. To the Keynesian emphasis on aggregate demand, they add considerations of economic structure and institutions that have been missing in much preceding macroeconomic theory. The second part, consisting of four chapters, addresses major phases of capitalist development in the twentieth century, principally the Great Depression, the 1945–1970 "golden age," and the return to higher unemployment in its last quarter. The authors use their "evolutionary-Keynesian" analysis in an attempt to provide a superior explanation of these key phases of capitalist development. The final part, consisting of two chapters, addresses the political conditions of capitalist success and failure, included the existence or otherwise of an accord between trade unions, employers and government.

Overall, this work is largely successful, both as a guide to several important contemporary debates in macroeconomic theory, and as an attempt to enrich Keynesianism by giving more emphasis to institutional, structural, and evolutionary issues. This is possibly the best book that the Cornwalls have produced so far. Its blend of theoretical and empirical common sense is highly appealing. The attempt to synthesize Keynesian with evolutionary and institutional approaches is an important milestone. In sum, it is strongly recommended.

At the same time, the book reveals some areas of weakness that should be addressed in further attempts to develop such a promising theoretical synthesis. In some respects it reads as a patchwork quilt of theoretical explanations, each part of which is brought in to explain particular sets of empirical phenomena. Although this is a work of great historical sensitivity, which recognizes the uniqueness of different national capitalisms and different phases of capitalist development, its over-arching theoretical framework is only partially developed. This points to a widely neglected problem of great theoretical depth and seriousness; how can economics escape from an over-general theoretical framework and exhibit greater historical sensitivity?

Some evolutionary principles could arguably serve as part of a general theoretical framework for historically sensitive theory. In the tradition of Karl Marx, Joseph Schumpeter, Ulrich Witt, and others, Cornwall and Cornwall see evolution as involving "an emphasis on endogenously generated change" (p. 1). However, I believe that this emphasis downplays the importance of exogenous shocks in economic development. Several social scientists—including Mancur Olson and myself—have argued and produced evidence that much of modern economic development can be explained in terms of major institutional changes prompted by revolution, war, or foreign invasion. To a large extent these events have been brought on by exogenous factors. Furthermore, their emphasis on endogenous change would seem to exclude any Darwinian other version of evolutionary principles that emphasizes the openness of economic and other systems. However, although nominally and initially endorsing Schumpeter's misleading emphasis on endogenous change, Cornwall and Cornwall are eventually obliged to give exogenous factors equal and warranted emphasis (pp. 104 ff.).

I have other quibbles with aspects of their argument, such as their idea that the Great Depression can be largely explained by technological and structural changes in the U.S. economy. But to raise these detailed issues here would be beyond the point and scope of this review. The most relevant issue is that this courageous and inspiring attempt to enhance Keynesianism with evolutionary and institutional theory raises deep questions concerning methodology, meta-theory and the very meaning of explanation of economic phenomena. Some of these issues are only beginning to be addressed by a significant number of scholars. It is to the credit of the authors of the volume under review that they have achieved so much and also pointed to an important agenda for future research.

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The Problems and Methods of Economic History. By Wittold Kula. Ashgate: Aldershot, 2001. Pp. li, 479. $104.95.

It is of course relevant to ask to what extent the Polish economic historian Wittold Kula who died in 1988 at the age of 72 can be of any interest to a contemporary public interested in the development of economic history as an independent field of study. Most certainly, during the 1960s and 70s Kula was an influential scholar in Central Europe and perhaps particularly for the French Annales-school. When his most famous little work, *An Economic Theory of the Feudal System: Towards a Model of the Polish Economy 1500–1800,* was published by New Left Review Books in 1976 he also gained some influence among Anglo-Saxon scholars, especially of Marxist and neo-Marxist inclinations. His critique of the use of modern economic analysis when dealing with precapitalist modes of production was refreshing and could be seen as a counter-weight to the hegemonic ambitions of the cliometricians at the time. Should we then today read Kula mainly for historical reasons—as an interesting individual example of a now rather defunct specie: the economic historian?

Simon Szeter who is the editor of this translation of Kula's *magnus opus* first published in Polish in1963 and republished in 1983 with additional chapters definitively thinks we should not. He believes that Kula's methodology and definition of the subject matter of economic history is of interest also for modern economic historians. I have difficulty in making my mind up whether I find this position attainable or not. Hence most of what Kula says on the history of economic history, the subject matter of economic history, on periodization issues, the methodology of micro investigation, historical statistics, and macro analysis (studies on national income, etc) is well taken although to a great extent part and parcel of the current orthodoxy. Some other chapters, especially those dealing with historical demography and social structures now seem a bit dated, to be honest. We must of course remember that when this book first was published it was in the close aftermath of the Stalinist nightmare in Poland. From this point of view the problem is not that Kula was badly informed concerning the current status of the *bourgeois* literature on economic history—perhaps to a surprising degree given that as a leading academic person he was able to read western literature and travel even during a period when the borders were almost closed for most ordinary citizens. His peculiar geographical and political location is rather spotted in his cautiousness about passing the obvious borders that he must have faced as a Polish academic living through this grim period. When Szreter says that Kula provides us with a "living voice from the East" this is certainly true. However, this has both negative and positive sides for current readers of his by all means impressive book on the methodology of economic history.

If this still is a living text today it is perhaps for these three reasons. First, it is refreshing to note that Kula treats economic history as an independent field of inquiry and not as some specialized sub-discipline of economics or social history. According to my view this is too seldom practiced today. However, with the revival of historical institutionalism there is certainly also a place for the renaissance of economic histories which certainly have dealt with the issue of institutional change and stasis since way back. Secondly, Kula pleads for the use of theory in economic history and the necessity of developing theoretical tools for understanding economic and societal change. In this context his discussion on the problems of using some of the standard concepts in economics when dealing with past economies (especially feudal society) is still most accurate. Thirdly, Kula is still worth listening to when he pleads for a comparative method in economic history. It is only through such comparisons that we can make sense of the generalizations that are an indispensable aspect of economic history as an academic field of study. At a time when it is once again *comme-il-faut* to write national histories and biography is back in full force, it is refreshing to note Kula's internationalist approach and his insistence upon the universality of man's historical experience. If Kula can inspire further developments in this direction—i.e., to return to what was worthwhile in the "old" program of economic history—the reading of this book should be warmly recommended.

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Famine Demography: Perspectives from the Past and Present. Edited by Tim Dyson and Cormac Ó Gráda. Oxford: Oxford University Press, 2002. Pp. xiv, 264. £45.00.

This book examines the demography of modern famines, taking up an issue that has been a key part in studies of European famines in the period 1400–1800 but less evident in studies of the modern era. The essayists, who are among the most talented and well-informed historical demographers now at work, discuss not just the scale of mortality associated with a famine, but also foregone births, migration, causes and patterns of death, and other issues. Modern famines, the editors contend, ". . . tend to be more class-specific and they seem even more likely to target males than females" (p. 1). The text consists of ten case studies from the period 1845 to 1987 dealing with Europe, Asia, and Africa, plus a concluding essay by Kate Macintyre exploring why females survive famines, especially modern famines, more often than males.

Contributors to this volume make a number of important points about famine demography, of which three can be singled out. First, they draw a distinction between famines where public health and medical resources were well enough developed and deployed to reduce the risk of deaths from communicable diseases, such as in Greece in the early 1940s (which Violetta Hionidou examines in an essay on the island of Syros in this volume) and the Dutch hunger winter of 1944/45, versus cases where such knowledge did not yet exist, such as in Ireland during the potato famine, or where it was not deployed, such as Ethiopia in the 1970s and 1980s. Second, Arup Maharatna's close study of the scale of undernutrition in Indian famines in the period 1870–1910 allows him to classify them as examples of mass starvation rather than localized dearth. Third, considering food entitlements over time, this volume suggests that a decline in food availability has played a smaller role in famines in the second half of the twentieth century than it did in earlier famines, whereas war and human failings have played a bigger role.

Reworking cause-of-death data, Joel Mokyr and Cormac Ó Gráda show that few people starved to death in the Irish famine of the late 1840s, but died from "opportunistic diseases that took advantage of the fall in nutritional status and the general environmental deterioration" (p. 21). Considering the same famine Timothy W. Guinnane and Ó Gráda examine deaths in the workhouses distinguishing some facilities that, through good management, saved lives and others that, through bad management, cost many extra lives. Mokyr and Ó Gráda conclude that the knowledge necessary to reduce the famine's toll from disease and hunger was not yet available in the 1840s, but Guinnane and Ó Gráda imply that many useful things were already known and embodied in good workhouse management.

Kari J. Pitkänen, focusing on excess famine mortality, finds little evidence of a sex bias in nineteenth-century Finnish famines and questions the view that men have been more vulnerable in subsistence crises. Tim Dyson examines the toll of two famines in Berar, in central India, in the 1890s and points up cases where one period of dearth was soon followed by another; the inhabitants of Berar were poor, but among them the poorest suffered most. Christian Thibon reviews the history of famine in Burundi since the 1880s, using mission records and summarizing parts of his 2000 book. Serguei Adamets asks whether the Russian famine problem intensified during the nineteenth century and the Soviet era, and estimates a corrected series of deaths for the period 1927–1938. Michel Garenne, Dominique Waltisperger, Pierre Cantrelle, and Osée Ralijaona study the 1985–1987 famine in Antananarivo, Madagascar as a case of the commonplace and milder dearth mismanaged so badly that many people died. And Osamu Saito, evoking Thomas Malthus's interest in the issue but inverting Malthus's reasoning, considers the frequency of famine in Japan since the seventh century and finds that in certain periods famines had a compelling impact on population growth because the rate of growth was already so low.

These are stimulating essays that significantly advance the study of famine. They will be interesting to scholars, and many of the conclusions will also interest people in a position to formulate policies intended to anticipate or relieve famines.

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