Book Reviews

MODERN EUROPE


Policy makers, such as those in the British Treasury (http://www.hm-treasury.gov.uk, *Productivity in the UK: 3 The Regional Dimension*), commonly believe that “industrial clusters” are a key to regional development. The evidence provided by the present study of clusters and industrial districts in England over two and a half centuries is therefore very welcome support for sounder policies, as well as for understanding history.

At the outset the editors define a district as a concentration of firms in an industry, in a single town or in a zone of a city, a cluster as a wider agglomeration of industries connected by common products or technologies or markets or institutions, and a region as consisting of interlinked urban areas. Networks are more contentious and so the editors do not tie themselves down. Their engagement with theoretical developments has created much more than a collection of local industrial histories. The coherence of their project is further increased by Mark Casson’s contribution on regional business networks, interweaving warranted mutual trust, information, metropolitan links, leadership, and institutional sclerosis.

In her study of a warranted mutual trust network, Gillian Cookson’s chapter on Darlington, shows how in 1776 a mere 160 Quakers in a population of 3,500, with suspect entrepreneurial vision, opened up the mineral resources of the Tees valley and transformed the region. No less constructive behavior is identified in the Birmingham jewelry trade of the 1880s. Francesca Carnevali describes how morally confident trade leaders organized to reduce price volatility from dumping of bankrupt stock and overtrading in this cyclically sensitive industry, while maintaining competition. Similarly, spontaneous order breaks out in West Yorkshire between 1800 and 1830, where Steven Caunce’s examination of the finance of woollen textiles reveals effective country bankers providing media of exchange and assessing credit-worthiness. As in Darlington, extra-regional, metropolitan, links mattered for raising very large amounts of capital in Lucy Newton’s analysis of shareholders in new limited liability Sheffield companies between 1855 and 1885.

Mark Casson’s claim that networks create path dependence because they cannot be costlessly reconfigured when conditions change, is supported in other chapters; they identify a persistence in industrial location that is hard to explain other than by path dependence. The glove industry (Richard Coopey) for instance was centered in Worcester and Taunton for a number of centuries, with no privileged access to markets or raw materials. Path-dependent technological experience also contributes to the changing pattern of industry in a given location; Coventry engineers from the nineteenth century to the 1930s shifted out of ribbon-making machinery, into bicycles, machine tools, and cars, with changing demand (Roger Lloyd-Jones and Myrddin Lewis).

A number of authors find that networks constrained, rather than boosted, economic development, all in the north-west; Andrew Popp notes how changed market conditions led entrepreneurs to co-operate in the formation of the huge monopolistic United Alkali Company in 1890. In the British industrial district *par excellence*, Steve Toms and Igor Filatotchev conclude that the interlocking directorates of interwar Lancashire textiles undermined accountability and prevented restructuring. Trust and cooperation disappeared in the vertically specialized cotton industry because the depressed economic environment pitted entrepreneurs against each other, and precluded productivity-enhancing changes in work practice (Sue Bowden and David Higgins). Also in the North West from the 1930s, Till Geiger points out shortcomings of centralized procurement for establishing local networks—the uncertainty of government demand. Yet he also notes English Electric (Preston) in the Manchester industrial district sold the Canberra bomber to the United States.
and produced the Lightning (the only exclusively British Mach 2 jet fighter). John Wilson and John Singleton similarly condemn the clusters and networks of the Manchester industrial district in the twentieth century, despite offering counterexamples to inertia such as the Manchester Ship Canal and Trafford Park Estate; there were “too few firms like MetroVicks, Ferranti and Avro.” But they do not buttress their case with regional performance measures, such as population, income tax payments, or rateable value.

It is at least arguable that the North West was the best performer outside the largest region, the South East (which is unrepresented by any case studies here), and that relative success needs explaining, rather than failure. The claim that “In 1850 the industrial north dominated the British Economy” is contestable because twice the proportion of U.K. population (24 percent) lived in the South East region as in the North West, even in 1861 (p. 77). Income measures convey a similar impression. London was dominant in 1859/60, with 50 percent more adult males than the Lancashire boroughs, and almost three times the proportion of higher rate (D and E) income taxpayers. Bearing in mind the agglomeration economies of the dominant South East, it is striking that the formation rate of firms (relative to employment) was higher in North West during 1930s than in any other region outside London and the South East. Moreover in the 1920s the number of new firm registrations was above that of the South East, excluding London. This type of aggregate data is essential for a correct interpretation of the valuable local studies in John Wilson and Andrew Popp’s volume. For those without a detailed knowledge of English geography, so also is an atlas.

JAMES FOREMAN-PECK, Cardiff University


This well-edited collection of essays will benefit students of the relations between banks and industries as well as all historians mindful of economic imperatives. By looking at the relations between the worlds of finance and industry through three complementary perspectives, and, in each of the corresponding sections, surveying or analyzing a range of meaningful cases, this volume illustrates what a comparative approach can do best—question the stale conceptions of generalists and remove the blinders of specialization.

The history of the financing of modern Europe’s industrial expansion has been built around a series of contrasts and the attendant temptation of generalizations. Behind the clearly distinct experiences of industrialization in Britain, France, and Germany, historians have found and sketched very different financial circuits. Although few explicitly sought to erect these three cases as models, comparisons nevertheless quickly came to dominate reflections on the matter. More problematic even than some of these parallels, however, may be their common denominator—the idea that financial systems made industrialization possible, shaped it, and paced it. These essays challenge this fundamental assumption in a gratifyingly coherent manner.

Four national surveys, four sectoral studies, and four case studies of large and small banks illustrate the reciprocal and unsystematic rather than causal or even permissive nature of the links between banks and industry. The contribution of the first section of the book is perhaps the less remarkable, because national boundaries are increasingly proving poorly suited to the work of economic historians. Still, national surveys need to be updated (if only for teaching purposes), and the British, German, Italian, and French experiences are articu-
lated in new terms here. In the first case, we are invited (not for the first time, but the point bears repeating) to reflect upon the ways in which a long period of leadership shaped the competitiveness of the British industrial complex, rather than upon the role of banking practices in what is too easily seen as its decline. For its part, and contrary to some recent efforts, the essay devoted to the German context reaffirms the importance of the role played by great financial institutions, but also insists on the continuing distinction made by all parties between the functions of financing and producing. In a third paper, sharp periodization and careful attention to the tensions existing between core and periphery as well as other unique aspects of Italian industrialization sketch a scenario that defies easy parallels with precedents north of the Alps. Finally, a more chronologically focused probe suggests that the weaknesses of the support given by French banks to industry may be rooted in fundamental characteristics of the former, but also that a range of circumstances regularly recast the problems facing them. Without negating the distinctions between these four histories, these analyses all draw the attention of readers to the autonomy of the strategies followed by both bankers and industrialists.

The second section is devoted to the French public works and hydroelectric sectors, the Swiss counterpart of the latter, and the diverse but sensitive field of French foreign investment. The conclusions reached by the authors of these essays again point to the distance separating industrial and banking projects. Changing technologies or conjunctures, powerful exogenous factors such as political choices or national preferences, or, conversely, the sometimes surprising persistence of sharp sectorial characteristics, have regularly invited industrial groups to cultivate a variety of strategies to supply their capital needs, while discouraging financial institutions from developing coherent and sustained ambitions to dominate them. A final section drives this theme a little further by looking at the long-term evolution of the choices made by four banks. In the first two cases, we learn that the robustness of two great banking vocations was never fully eclipsed by their substantial involvements into primary or secondary production. And when we turn to two lesser-known banks remarkable for the weight and steadiness of their interest in industrial affairs, we discover a range of factors that paint them as exceptions rather than models.

Sections two and three clearly point toward the need for further sectorial analyses and case studies, while the opening essays remind us that national generalizations are always in need of revisions. Yet, it is clear that we should avoid thinking of banking systems as commanding supports, guides, or obstacles to industrialization. Banks have been and are financial institutions with a conditional interest in industry. The comparative approach that makes this volume one instance of a sum greater than its parts is well chosen to reveal the contingent nature of this relationship.

PIERRE CLAUDE REYNARD, THE UNIVERSITY OF WESTERN ONTARIO


“The desire for socialist change . . . has produced a record which contains far more success than failure, including policies which have vastly improved the lot of those Labour exists to serve. All those associated with this party should feel justly proud” (p. 5). This book, conceived to mark the centenary of the formation of the British Labour Party, has a broadly upbeat message—the party has been a success, a qualified success perhaps but still a success. The evidence in support of this judgement appears here not as a chronological narrative but as a series of essays dealing with different aspects of the party’s history. The essays are on the party’s political thought (by José Harris), its economics (Jim Tomlinson),
Labour and welfare (Pat Thane), Labour and international affairs (Stephen Howe), constitutionalism (Miles Taylor), gender (Martin Francis), trades unions (Alistair Reid), its membership (Duncan Tanner), the electorate (Nick Tiratsoo), a comparative perspective (Stefan Berger), its myths (Jon Lawrence), and finally one on New Labour (Steven Fielding).

In some ways the times might seem unpropitious for the judgement that the Labour Party has been even a qualified success. The state socialism of the communist block is surely discredited, and even the social democratic consensus of Keynesianism and a welfare state might perhaps best be described as having been in retreat since the late 1970s. In addition, most historians, especially those on the left, have been dismissive of the party’s claims, seeing it as theoretically unsophisticated compared to its European counterparts, as lacking socialist vigour, as neglecting issues of gender and race, and as a mere extension of the trade-union interest. This book sidesteps many of these latter criticisms by insisting on judging Labour in its own terms. The party has always acted within certain constraints, among which the two most important here are the electorate and its own internal divisions. Any attempt to bemoan a lack of socialist vigour can thus be met by saying there was no alternative given the constraints. Within these constraints, this book suggests, the Labour Party’s record compares favourably with that of all its rivals.

The book highlights the Labour Party’s achievements in several areas. Surely the most important is its dedication to improving the lot of impoverished sections of society. Anyone who grasps just how awful the condition of the working class in Britain was between the two world wars must allow there has been enormous improvement. Again, even if some of the ideas behind the welfare state came from without Labour, others did not, and anyway it remained to the party to provide the political energy that passed the necessary legislation. In addition, the party has a reasonable, if flawed, history of fighting against prejudice. No other British party, for example, has done more to promote equal opportunity for women. Finally, the Labour Party has taken a moral stand on certain issues, and sought thereby to lead public opinion, even when this has been harmful to its electoral chances.

In highlighting these achievements, several essays in the book standout as of particular interest. The essay by José Harris on Labour’s political thought, particularly when read in conjunction with Jim Tomlinson’s on the economy, does much to show that Labour’s thinkers were more consistent, rigorous, and sophisticated than is normally believed. The studies by Stephen Howe and Stefan Berger suggest the party, including its leadership, was more international, and less insular, than the stereotype of British socialism suggests. And the essays of Alistair Reid and Duncan Tanner give us a sense of the party’s activists as fallible but engaged and passionately committed individuals working to better the human condition for little reward and often in difficult conditions. In these ways, and others, the book manages to rescue the Labour Party—its thinkers, politicians, and activists—from the condescension of posterity.

Generally I applaud this book both for the stance it takes to the Labour Party and for the high quality of the individual essays. However, I would like to express one note of caution. In taking an internal perspective on the history of the party and in judging it on its own terms, this book does not allow sufficiently for the extent to which the party is implicated in the very constraints under which it operates. The fratricidal battles within the party, for example, have done much to increase the suspicion it arouses among the electorate. Likewise, the role in the party of trade unions with a masculine culture surely was one of the most significant constraints on what the party could do on issues of gender. Now, if we open up the possibility of the party transforming the constraints under which it operates, then we must allow that had it acted differently—say, taking a stronger line on gender and race—it might have transformed the constraints—say, creating a more powerful constituency in favor of positive discrimination—in a way that might have enabled it to do considerably more. Debates about what the party might have done thus remain open to a diverse
range of voices. These voices speak, moreover, not only to the past but to what the party now might do and how it might do it. To write about the party’s history is to enter current versions of those passionate, heated, and divisive debates that have been such a prominent part of its past.

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From the wheelbarrows of money needed to purchase a loaf of bread in 1923 to the long queues of the unemployed during the Depression, Weimar Germany is mainly remembered for industrial-strength economic disasters and political instability. Ever since Hitler became Chancellor in 1933, economists, historians, and political scientists have pored over Germany’s first, brief experiment with democracy, producing study after study on economic and social policy, productivity performance, management of the currency, and the reparations issue.

Theo Balderston’s brief text is a heroic attempt to survey and summarize this vast literature, and to highlight key areas of debate. By and large, it succeeds admirably—the book is ideal classroom material for a short course on interwar German economic history. In little more than 100 pages of chronologically organized text, Balderston covers the period from demobilization to the slump’s end in 1933/34. Except for the editorial sloppiness that has become commonplace, the text is highly readable throughout. A glossary of key economic terms and concepts nicely complements a well-chosen bibliography. Balderston, who himself has made important contributions to some of the debates, begins his chapters with an overview of key facts and figures, and then moves on to a summary of controversies in the literature. The discussion is often scrupulously even-handed, sometimes to the point of sitting on the fence as a matter of principle—Balderston often presents all sides of an argument, and then leaves the reader to reach his own conclusions. Graduate students may appreciate such an approach, but most undergraduates would probably prefer more guidance and considered judgment. Students can quickly gain an overview of crucial issues, but Balderston’s approach misses important opportunities to summarize where the scholarly debate now stands—the text often breaks off so abruptly that this reader sometimes wondered if a page was missing from his copy. The section on the inflation, for example, gives good summaries of the various theories that have been advanced—from the quantity theory to the balance-of-payments approach, from structural interpretations to expectational models. Yet after having been treated to a mouth-watering selection of appetizers, this reader felt that the main course was not being served—there is no compelling summary of what, in Balderston’s view, is well explained by existing interpretations and what remains to be done.

In contrast, the discussion of Weimar’s growth record during the late 1920s does not suffer from such defects. During the brief interlude between the currency’s stabilization in 1923 and the outbreak of the crisis in 1930, output, productivity, and employment were all more or less rising in tandem. Nonetheless, many scholars from Knut Borchardt to Harold James and Albrecht Ritschl have detected signs of imminent collapse and underlying economic malaise—principally because profits and investment did not revive as much as one might have expected, given the empire’s splendid record before 1914. Balderston sides with these authors and accepts that profligate spending, over-generous pay deals, and inept economic policy had already undermined Weimar’s political and economic system before 1929.
Balderston also accepts that much of the criticism normally leveled against Heinrich Brüning, the “hunger chancellor” in charge during the Great Depression, has been misguided. The policy of brutal pay- and price-cuts, pushed through with emergency decrees, was largely without alternative because the Reich could not borrow and default was the only alternative. In this section in particular, Balderston makes an impressive effort to incorporate the latest research findings, citing numerous unpublished papers as well as Ritschl’s important revisions of national statistics. Yet some readers may well remain unconvinced that austerity policies were essentially without alternative.

Although the text works hard to make economic history accessible to history undergraduates, it highlights the difficulties that the field has had to contend with in recent decades—economic models and terminology are hard to explain in layman’s language and many of the key research issues are difficult to describe verbally. Harold James’s The German Slump and Holtfrerich’s book on the inflation are the model texts in this literature. Balderston’s text does well even compared to such an elevated standard. Yet despite exemplary texts such as this one, it appears that economic history by and for historians is slowly turning into an academic no man’s land. In the intellectual marketplace, this particular brand has not done well. Economists, when they deign to deal with historical evidence, normally insist on the rigor of explicit theoretical models and the systematic analysis of quantitative evidence; historians influenced by the linguistic turn and by history from below have largely turned their backs on macroeconomic concepts such as “growth,” “inflation,” and “unemployment,” using economic “terms” only in “inverted” commas to “make” points. In order to reclaim its niche in history departments, the field needs more undergraduate courses taught on the basis of books like this one.

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Until recently the history of enterprises during the Nazi regime was analyzed primarily in ideological terms. Were members of the managing board also members of the Nazi Party? Did “big business” and the Nazis have common political objectives, or did they at least have the same political enemies? Did big business finance Hitler?

Even today many political historians have difficulties accepting that the Nazi economic policy and the business strategies of individual firms sometimes followed fundamentally different objectives. In contrast, economic and business historians who analyze the Third Reich are now used to thinking primarily in economic rather than political terms. The question is no longer whether a CEO shared the Nazi ideology but to what extent the top management adapted the company strategy to the new institutional setting, and how much it was able to influence this environment. It is not that moral questions are no longer of interest. Rather, they are set into relationship with economic motives. And, the decisive point, they are embedded in a kind of explicit or implicit cost-benefit analysis: What where the opportunity costs of resisting the demands of the Nazis?

Fortunately, this discussion is no longer confined to Germany. In many countries that were occupied or indirectly controlled by Germany during World War II there is new interest in how native firms dealt with the German occupants. This interest has its origin in two interrelated causes. One is the general question of who collaborated with the Nazis, and why. As in Germany, this discussion seems to have started only when the generation of the protagonists faded away. The other cause is legal action. German firms were not alone in being subjected to class action lawsuits in the 1990s. Swiss banks and, subsequently, com-
panies in other countries also faced legal action. It is not an exaggeration to say that litigation that originated in the United States led to a large job creation program for many European (and a few U.S.) historians. In this context the Society for European Business History organized a conference in 1998 on enterprise in the period of fascism in Europe. Thirteen papers discuss the experiences of companies vis-a-vis the Nazis or other fascist dictatorships in Belgium, Denmark, France, Germany, Italy, the Netherlands, Poland, Spain, Switzerland, and the German-occupied part of the Soviet Union. Most are discussed in four shorter comments.

In contrast to what the title may suggest, this is not a collection of company case studies. Most papers focus on a country’s entrepreneurs as a whole or those of a certain industry or sector. Economic historians will like the contributions of Per Hansen on Denmark and Hein Klemann on the Netherlands who support their arguments with ample macro- and microeconomic data. If there is something like a common theme in these quite disparate contributions it is the questions of how firms reacted to the opportunities offered and constraints imposed by the Nazis or other fascist regimes like in Italy and Spain and how far they were able to benefit from that environment. With respect to carrots and sticks, the latter dominated in Poland and the occupied territories of the Soviet Union. All major companies were taken over by the Germans and given either to state holdings such as the notorious Reichswerke Hermann Göring or to private German firms. How far privately run firms were pulled or pushed remains controversial. Peter Hayes and Richard Overy find good arguments to tend to the latter. They corroborate once more Tim Mason’s primacy-of-politics hypothesis, which is hardly challenged nowadays. But a wording like “the larger directions of policy . . . proved to be processes to which firms could either adapt or see themselves superseded” (Hayes, p. 33) underestimates the active role of many firms when it came to participate in plunder, spoliation, and the deployment of forced labor.

In this context, widening the perspective to the European experience is illuminating. In general, firms in Western Europe and Denmark were quite reluctant to cooperate openly with the Nazis. In contrast to German entrepreneurs, their West and North European peers were quite confident that the German occupation would somehow be superseded by at least a status of restricted autonomy under German hegemony or, after Germany’s defeat at the African and Russian war theatres, to full independence. Thus the trade-off between short-term profits due to collaboration with the Nazis and long-term considerations had totally different parameters than for German firms who expected to remain under Nazi rule. This may explain why it was the construction industry—reported for the Danish case by Hansen but very probably observable in the Western European countries as well—that showed a relatively high propensity to cooperate with the Germans, who urgently needed defence fortifications for their “Fortress Europe” and were willing to pay for it.

In conclusion, one might infer from this valuable volume that entrepreneurial behavior—either in states under fascist regimes or in occupied countries—was much less dominated by political preferences than was thought before. It was, rather, sheer economic opportunism that prevailed in entrepreneurial decision making. One might speculate that entrepreneurs are more apt to adapt to changing political regimes because their assets are to a larger fraction fixed and thus “sunk” than those of other professions (e.g., professionals). When fascist regimes established foreign exchange controls (all did), entrepreneurs were no longer able to liquidate their assets and emigrate with their wealth. They were locked in and should not be blamed for trying to save their assets under fascist rule. Many should, however, be blamed for realizing opportunities that could only be offered by dictatorial and predatory regimes even though they had viable alternatives.

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Caribbean and Latin America

Sugar Baron: Manuel Rionda and the Fortunes of Pre-Castro Cuba. By Muriel McAvoy.

Manuel Rionda Polledo was one of the most important Cuban business men of the last decades of the nineteenth century and the first half of the twentieth century. His biography is representative of a trajectory that many Spaniards and Creoles in the Gan Antilla followed, although not all were as successful.

Rionda was born in Spain, in Noreña (Asturias) specifically, as a member of a family with business in the Cuban sugar industry. He emigrated to the United States, where he developed while working for one of the world’s most important sugar runners, Cesas Czarnikow. From that position he eventually emerged to form the independent Czarnikow-Rionda Company.

Although there are still many unanswered questions, the Cuban sugar industry in the period in which Manuel Rionda lived has been the object of many able studies. There is, however, little research with Muriel McAvoy’s biographical-enterprise perspective. This is the major contribution of her book to the topic, and this context provides considerable interest.

McAvoy’s book is not remarkable in its analyses. It mainly describes the framework of relations that composed the sugar business and its evolution between the last decades of the nineteenth century and the 1930s. This overall perspective, however, provides a valuable contribution in the context of a historiography that has analyzed the details of the sector in all its dimensions: productive, technological-organizational, commercial, and financial.

The information that McAvoy provides in Sugar Baron: Manuel Rionda and the Fortunes of Pre-Castro Cuba is a very valuable view from the perspective of a sugar businessman and his personal relations on the evolution of the sector. That view challenges some conclusions of other studies. In particular, the book improves our knowledge of the interest networks that were developed around the sugar industry in the Gan Antilla and the United States at two very significant moments: the last decades of nineteenth century and the period during and immediately after the First World War.

In the last decades of the nineteenth century, several North American investors, especially sugar refiners, began to participate in the transformation of Cuba’s sugar industry. This process continued into the twentieth century, after the independence of the Gan Antilla from Spain. Cane culture and manufacture and the railroads to support it expanded into the eastern half of the island, a region that until then had remained practically unexplored. Rionda participated in that process, especially in association with one of the main American investors, Edward F. Atkins.

By the beginning of the First World War the transformation and expansion of the Cuban sugar industry had consolidated, and the island hacendados took advantage of opportunities arising from the international conflict. The war drastically reduced European sugar production, and Cuban producers took advantage of the opportunity to double their supply. This required investment that could not be supplied by the industry’s usual means. In response, great corporations developed to support the entrance of banks and other financial organizations. His network of personal and enterprise relations and his prestige as a sugar baron allowed Rionda to lead this process. He led the formation of the society that constructed the Central Manatí in 1912 and then led the creation of the greatest sugar company of the world in 1915, the Cuba Cane Sugar Company.

McAvoy’s well-written book allows us to know with considerable precision the framework of business and financial relations involved in both the transformation and modernization of the Cuban sugar industry at the end of the nineteenth century and the opening of the
business to financial capital in the twentieth century. McAvoy also illuminates the problems associated with the process. These problems became particularly acute during the crises of the 1920s and 1930s.

In general, the conclusions of McAvoy’s study confirm the hypotheses defended by other works that have analyzed the problem of the Cuban sugar industry at greater depth and from a wider perspective. It is important, however, to be aware of the underlying evidence that supports its conclusion. The author draws on the Braga Brothers Collection, an archive of the Rionda family, deposited in the University of Florida, in Gainsville. Unfortunately, the author practically uses this as her only source and does not support or challenge it with documentation from other archives, and even seems to ignore parts of the existing historiography. However, despite that defect, the result is a good reference book and an important complementary piece advancing our knowledge about the subjects considered.

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In nearly all his publications, Selwyn Carrington has followed the view put forward in Eric Williams’s Capitalism and Slavery (1944) that the British West Indian economy was in serious decline in the later eighteenth century and that the problems were so grave that the British abolished their slave trade as a result, primarily for economic reasons. Such arguments were first given prominence by Lowell J. Ragatz in The Fall of the Planter Class in the British Caribbean, 1763–1833: A Study in Social and Economic History (London: Oxford University Press, 1928). Williams, however, sharpened Ragatz’s arguments and focused particularly on the economic determinism that led Britain to dismantle its nefarious commerce in enslaved Africans. Carrington has already written The British West Indies during the American Revolution (Royal Institute of Linguistics and Anthropology/Foris Publications 1988), which deals with trading problems in a prolonged conflict that witnessed falling levels of slave imports, reductions in sugar prices, and interruptions to the supply of foodstuffs from the United States to the West Indies. These themes are retreated in The Sugar Industry and the Abolition of the Slave Trade, but the chronological span is greater and the trawl of evidence to support the argument is more thorough. Carrington argues that the British West Indian plantation system experienced declining profits, increased debts, and other economic inefficiencies that made the sugar industry in the Caribbean no longer a viable ongoing proposition in the post-1783 period. British politicians, influenced by these calamitous signs, pulled the life support away from the sugar plantations—so Carrington’s argument runs—by ending the British transatlantic slave trade in 1807.

In making these arguments, Carrington confronts the consensus of modern historians that the British West Indian economy recovered after considerable difficulties experienced during the American revolutionary war; that the British Caribbean slave system was expanding, not contracting, in most years between 1783 and 1807; and that therefore it made little sense to abolish the slave trade on economic grounds. Seymour Drescher’s Econocide: British Slavery in the Era of Abolition (Pittsburgh: University of Pittsburgh Press, 1977) is the main proponent of this more positive line of argument. His views have been broadly accepted in B. W. Higman’s summative assessment in The Cambridge Economic History of the United States, (vol. 1, edited by Stanley L. Engerman and Robert E. Gallman, New York: Cambridge University Press, 1996). Drescher and others, in addition, have under-
scored the British abolitionist achievement in pressuring Parliament to act against the slave trade. Carrington, on the contrary, emphatically believes that abolitionist agitation was a convenient smokescreen for Britain to get rid of the slave trade for economic reasons.

Unfortunately, in making his claims Carrington ignores important published material that contradicts his arguments. He states that “virtually no plantations made any profits” during the 1780s but provides no statistics to support his case (p. 97). No systematic data are produced for the further statement that “the profitability of West Indian estates had all but disappeared at the end of the eighteenth century” (pp.125–26). In making these assertions, Carrington fails to cite the carefully compiled estimates of sugar plantation profitability produced by J. R. Ward (“The Profitability of Sugar Planting in the British West Indies, 1650–1834,” Economic History Review 31, no. 2. (May, 1978): 197–213) and in British West Indian Slavery: The Process of Amelioration, 1750–1834 (Oxford: Oxford University Press, 1988). Ward showed—and no-one has bettered his estimates—that though profits dipped on British Caribbean estates, especially in wartime, the fact remains that on average most British West Indian territories produced plantation profits in the late eighteenth century and that, just as declining profits had previously been recuperated, there was no reason to think the same would not happen again. For Ward, and for most historians, the British West Indian economy was still a viable proposition until at least 1815.

Carrington’s failure to cite Ward on crucial aspects of the British West Indian economy is symptomatic of a broader tendency to omit the work of various historians. Ward is left out from the footnotes to Carrington’s chapters 8 and 9, dealing with new management techniques and planter reforms, though this is a central concern of British West Indian Slavery: The Process of Amelioration. Higman’s summary of the British West Indian economy, mentioned previously, is not addressed or cited. David Ryden’s work, documenting falling sugar prices in the British Caribbean after 1790, is ignored even though its evidence lends some support to Carrington’s case. Drescher’s articles, restating and refining his position in Econocide, are not cited. The statistics Carrington tabulates on the slave trade ignore the data presented in The British Transatlantic Slave Trade: A Database on CD-ROM by David Eltis, Stephen D. Behrendt, David Richardson, and Herbert S. Klein, (New York: Cambridge University Press, 1999). A table of slave prices for 1772–1808 (p. 206) is not compared with statistics on slave prices for the late 1780s that can be found in a volume that Carrington jointly edited entitled Capitalism and Slavery Fifty Years Later: Eric Eustace Williams—A Reassessment of the Man and his Work (New York: Peter Lang Publishing, 2000). Carrington’s discussion of the rum trade between the British Caribbean and North America is rendered redundant by failure to cite John J. McCusker’s comprehensive Rum and the American Revolution: The Rum Trade and the Balance of Payments of the Thirteen Continental Colonies, 1650–1775 (New York and London: Garland, 1989). I could extend this list of omissions, but the point has been made.

The fact that so many authorities are ignored on matters crucial to Carrington’s arguments means that one cannot recommend The Sugar Industry and the Abolition of the Slave Trade. But there are other reasons why it is a seriously flawed book. Though based on extensive research, the plantation records for the Windward Islands, Trinidad, and British Guiana—all expanding areas of the British slave system in the late eighteenth- and early nineteenth centuries—have not been investigated properly. Chapter 6 is based almost entirely on one set of plantation papers for Jamaica. These have been researched skillfully, but Carrington does not tell us why they should have wider application to the entire British Caribbean, or why he has not consulted equivalent papers, which do exist, for the Lesser Antilles. Sugar import figures are given in terms of different containers (e.g., pp. 18–20, citing hundredweights and hogsheads) but a common system of quantities could have been established by using Customs records in the Public Record Office, London rather than the sources cited. Carrington’s main point, following Williams closely, is that “overproduction
[of sugar] coupled with underconsumption, which both had the effect of overstocking the market, provided Britain with the rationale for destroying the slave trade” (pp. 94–95). But to investigate why Britain abolished its slave trade—and this is, after all, the second part of Carrington’s title—one needs to examine the political debates in Parliament on this issue. Carrington fails to do so, but if he had done he would have noticed, as Roger Anstey long ago pointed out, that economic matters relating to slave trade abolition were not discussed prominently at Westminster in 1806 and 1807.

Colin Palmer writes an enthusiastic preface to the book stating that Carrington “clinically” dissects “the arguments and conclusions of less careful and gifted scholars” (p. xv): who these people are is not divulged. Palmer also states that The Sugar Industry and the Abolition of the Slave Trade is “a model of exhaustive research, measured in its analysis and brilliant in its conclusions” that will be “read, reread, and debated” (pp. xvi–xvii). I can only think that this was written in a spirit of West Indian solidarity. Carrington has produced a book that is based on scholarly research but the omissions are of such significance that they cannot be ignored: they really should have been seen a mile away by the University of Florida Press’s readers. Furthermore, as stated previously, though evidence is produced on aspects of economic difficulties in the British Caribbean plantation economy in the 30 years after 1775, the argument that the British eliminated their slave trade for economic reasons is not handled in a convincing manner.

KENNETH MORGAN, Brunel University


Bell’s prize-winning monograph traces the evolution of the rural economy in the grasslands of Brazil’s southernmost state, Rio Grande do Sul. In a country notable for its plantation agriculture and tropical exports, the ranching and farming activities of temperate regions have not, until now, enjoyed the attention they merit. The book follows in a time-honored Brazilianist tradition of local and regional studies of rural economy and society, self-consciously echoing earlier works on agricultural regions by Stanley Stein and Warren Dean.

In establishing the unique character of the regional economy within the Brazilian setting, Bell provides original insights on ranching operations, including a discussion of finance and profitability, as well as the role of slave labor. The book then turns to the changes wrought in the second half of the nineteenth century. Ranchers organized to pursue improvements in livestock herds, and fencing haltingly diffused across the campanha. The traditional processing of jerked beef gradually gave way, with a lag, to the more advanced preparation of refrigerated meat exports in the early twentieth century. Throughout the book, but especially in the chapter on the rise of modern meat packing, Bell provides explicit comparisons with the larger, more advanced, and better-known pastoral economies of the Rio de la Plata, especially Uruguay. Beyond simply charting major changes within southern Brazil, the book provides an array of analytical insights on the main factors explaining the gap between the region and its wealthier neighbors to the south. In a tellingly titled chapter on the “slow path to modernization” Bell exploits his mastery of the primary and secondary sources to consider the broader array of economic and political conditions bearing on the pace of development in the region. Fencing was slowed by an absence of necessary legal reforms, tariff reductions made it difficult to compete with imported beef for decades, and transportation-cost-reducing railroads were late in arriving to the region. These delays nearly reduced the area to an effective appendage of Uruguay’s more successful ranching economy, with its higher levels of economic and legal infrastructure. But, by the end of the
period considered, the *campanha* was catching up. The rise of cereals production helped diversify the state’s economy, and though still behind the Spanish American ranching areas, Rio Grande do Sul was, by 1920, the most technologically advanced of Brazil’s agricultural regions.

Neither foreign investors, nor a more abstract notion of dependency, take on a large role in either the region or in Bell’s analysis. Crafted as a historical geography, this erudite study eschews formal hypothesis testing and econometrics in favor of more traditional treatments of sources and questions. In this regard, the book resembles the richly textured old economic history more than the new. As both varieties remain in short supply for Brazil the book is an especially welcome addition. Brazilianists will find in it a sorely needed study of a neglected regional question, while economic historians, specialists on South America, and students of pastoral economies will find it an invaluable reference for comparative work.

WILLIAM SUMMERHILL, University of California, Los Angeles

AFRICA, ASIA, AND AUSTRALIA


Economic history was at one time a thriving discipline in South Africa. A number of distinguished scholars—including W. M. McMillan, C. W. de Kiewiet, H. M. Robertson, and Francis Wilson—published stimulating studies investigating and explaining the character and causes of the country’s economic growth. But in recent decades there has been a persistent decline, to the point where none of the leading universities now has a chair in economic history, many no longer even offer courses on this subject, and very little original research is being done. The profession was fractured by ill-tempered ideological disputes; scarcely any new recruits are coming in; and the attention of most of those who were at one time engaged in this field has turned to other areas. The editor of this volume, Stuart Jones, is one of the few who have struggled persistently to keep the subject alive, but in a form, and from a perspective, that has not gained many adherents.

The absence of significant new research and analysis is all the more regrettable because so much of interest has occurred in South Africa in the last three decades. From the late nineteenth century, the economy was transformed and driven forward by the development of its vast gold reefs. But the metal is no longer able to play that role; and the output of the South African mines has fallen by over 40 percent since its peak in 1970. It was generally expected that manufacturing would become the new engine of growth, and government policy has assiduously promoted this goal since the 1920s, but the sector proved unable to fulfil this task. By the end of the twentieth century, the rate of growth of real GDP dropped to little more than 1 percent per annum, and real income per head declined to a level 10 percent lower than it had been 25 years earlier. One of the most unfortunate consequences of these trends was a continuous rise in unemployment, and by the late 1990s the number out of work was close to 7,000,000, almost half the potential labor force.

The volume is focused on this period of decline, but its format and method does not really enable it to do justice to the central issues. There are nine substantive chapters, beginning with a useful overall survey of the period 1970–2000, and a concise account of monetary and fiscal policy. There are then five chapters devoted to individual sectors: agriculture, mining, manufacturing, transport, and finance, and two to the external account, covering trade and the balance of payments. The editor is responsible for four of the chapters, and also contributes a brief introduction and a closing comment on the situation as he sees it in 2000. The other chapters are all by economists rather than historians.
Apart from Trevor Bell and Nkosi Madula, the approach adopted by all the other authors is very similar, and might be described as descriptive economics. There are brief accounts of the relevant internal and external developments, and of the introduction and application of major government policies, accompanied by numerous tables and a commentary on the data. The chapter by Bell and Madula stands apart because of its more rigorous methodology, and offers an excellent exploration of the reasons for the poor performance of the manufacturing sector, and for its failure to make the required transition from import-substituting to export-orientated industrialization. The volume thus provides much useful information and some interesting explanations for specific developments, but only Bell and Madula approach their subject by formulating hypotheses and attempting to test them in ways that would be familiar to readers of this Journal.

The other significant weakness from the perspective of someone looking for an explanation of why the South African economy has declined are the omissions in the coverage. In particular, the volume lacks a detailed treatment of the critical developments that occurred during these decades in the labor market. These included dramatic shifts in the relative pay of black and white workers, the steady upward mobility of black workers into skilled and semi-skilled occupations previously reserved exclusively for whites by legislation and custom, changes in capital intensity, and the rise in unemployment. One might also suggest that the adverse effects on economic growth of various Apartheid policies, such as the deliberate neglect of investment in human capital for 80 percent of the population, deserve more attention as contributory causes of poor economic performance. Above all, the volume needs a coherent overall analysis to explore in depth what has changed since earlier phases of relatively successful growth, and why the South African economy declined in this latest period.

CHARLES H. FEINSTEIN, All Souls College, University of Oxford


The rich and high-quality trade statistics published by the China Maritime Customs over a continuous span of 90 years, stand as an outstanding contrast to the confusion and contention that often characterize quantitative studies of the nineteenth- and twentieth-century Chinese economy. Whereas for some, the Maritime Customs, an organization entrusted by the Imperial government but controlled and managed by the Western elite bureaucrats, remains as the embodiment of Western efficiency and transparency, for others, it symbolizes that very cause of China’s downfall in the modern era: Western colonialism. Controversies notwithstanding, scholarly estimates on Chinese national trade statistics from the 1860s to the 1940s were on much firmer ground than any other aggregate production or consumption statistics, thanks largely to the Maritime statistics. The recent contribution by Thomas Lyons is intended to serve as a guide to the voluminous China Maritime Customs statistics, but more importantly, to utilize the rich port-specific and commodity-specific trade statistics to throw light upon questions concerning China regional economic development (p. 2).

The book consists of two parts with a CD ROM of statistics of tea trade for Fujian province. The first part, chapters 1, 2, and 3, provides an introduction to the origin and evolution of the Maritime Customs Organization, the sources, types of its publications, and the format of its statistics. Scholars have long cautioned that changing geographic coverage, shifts in definitions and uses of different prices for evaluating trade as well as changing statistical format could cause serious pitfalls and inconsistencies in using Maritime statistics to derive national time series of trade statistics. Lyons’s presentation on these issues, aided by sum-
mary boxes, maps, and illustrations using actual tea trade statistics in Fujian province, is sufficiently detailed and refreshingly lucid.

The second part, which begins with chapter 4, is a case study of the tea trade of Fujian province based on Maritime port and commodity-specific statistics. With an introduction of the source and format of Fujian tea trade statistics in the Maritime publications, the chapter carefully reconstructs the time series tea exports for three main ports in Fujian province as well as Taiwan, which was ceded to Japan after 1895. The author then proceeds to show the marked inconsistencies in two existing tea export statistics of Fuzhou in comparison with his reconstructed series. Chapter 5 of the second part combines the port-specific trade data to derive tea export time series data for the entire Fujian province for the period between the 1860s and 1940s. Using his trade data as well as other supplementary information, Lyons makes an estimate of the total tea output of Fujian province in the 1910s and 1930s. The chapter ends with a descriptive and graphic illustration of regional flows of tea trade and production in Fujian province.

The accompanying data CD in excel spreadsheets contains the original tea trade data culled from the Maritime publications as well as the step-by-step reconstructions of the time series data illustrated in the text. Each folder of data files also has a PDF file with detailed source and explanatory notes. Undoubtedly, the meticulous care and procedural transparency that Lyons has maintained for constructing the data and statistics is exemplary for future quantitative studies of Chinese economic history.

In view of the historical interest in China’s macro-regions as well as the current burgeoning literature on China’s regional inequality and convergence, the regional emphasis in the book is most welcome and timely. Unfortunately, the book’s relentlessly narrow focus on the tea trade of Fujian province may not appeal to a wider readership. At the end of the book, little was said of the possible linkages, potential biases, or statistical correlations among the tea trade statistics at the port-specific, provincial level and the national level. Neither was there any mention of whether or how the methodology and findings of the case study of Fujian tea trade may be applicable to studies of other commodities or other regions of China. This is clearly a disappointment given the high level of intellectual discipline displayed throughout the book, and given that the title of the book as well as the information provided on its back cover, seem to suggest that the book would be a more general study of Maritime custom statistics and that the data CD that would cover trade statistics beyond the tea exports of Fujian province.

Thus, much work still remains to be done for future scholars to explore the rich statistics on commodities, ports, destinations, volumes, and value-added in the Maritime Customs publication, which, given careful data compilation and statistical analysis, could bring to light some of the fundamental long-term issues and debates on the nineteenth- and twentieth-century Chinese and world economies such as the evolving patterns of intra- and interregional trade, the changing structure of trade and comparative advantages, and much more. Hopefully, this book serves as a stimulus for future research with more ambitious visions and scope.

DEBIN MA, Foundation for Advanced Studies on International Development, Tokyo


Economist, central banker, trade diplomat, university chief, arts administrator, environmentalist, and, finally, outspoken advocate for Australia’s indigenous people: H. C. “Nugget” Coombs was one of the most influential Australians of the twentieth century. This
important new biography of Coombs by Tim Rowse, the first since Coombs’s somewhat elusive autobiography in 1981, is a meticulously researched examination of its subject and, by extension, key episodes in Australian economic and social history.

Coombs’s rise to iconic status in Australia was as meritorious as it was meteoric. Documenting and accounting for it occupies the core of Rowse’s narrative, which begins with Coombs’s birth in 1906 in a small outback town in Western Australia. Rowse explains that the nickname “Nugget” (by which Coombs was universally known) was acquired in his youth and was an allusion to both his small stature and “solid” qualities. From a poor family, Coombs was a scholarship student at the University of Western Australia, and in 1931 he won a fellowship to complete a Ph.D. at the London School of Economics. Coombs arrived at the LSE at the beginning of the ascendancy of Lionel Robbins and Frederick Hayek. Neither of these made an impression upon him, however, and at the LSE he was rather more influenced by R. H. Tawney, Harold Laski, and through them, J. M. Keynes.

Coombs completed his Ph.D. (on central banking) in the rapid time of two years. Arriving home to an Australia still in the grip of the Depression, he found employment at the Commonwealth Bank (then the central bank). In 1939 he was seconded to Treasury and in 1942, at the age of 36, he became Australia’s wartime Director of Rationing. For the next 30 years he was never outside the first rank of administrators and advisers. In late 1942 he was elevated again to head the new Department of Post-War Reconstruction and, in 1948, Coombs was appointed Governor of the Commonwealth Bank. Coombs remained governor of Australia’s central bank until his “official” retirement in 1968.

Rowse uses the story of Coombs’s life as a public official to explore the question he says occupied Coombs throughout his professional life, namely: “how to secure within the political elite an economic rationality that was socially integrative.” For Coombs the answer was Keynesianism. He opened his 1981 autobiography with the declaration that the publication of “Keynes’s General Theory . . . , was for me and many of my generation the most seminal intellectual event of our time.” Of course, as a practical economist, Coombs was only to approximate the application of Keynesian ideals in his work. Nevertheless, a somewhat dogged pursuit of full employment informed most of his actions—both as Governor of the central bank and, more overtly, as Director of Post-War Reconstruction.

Coombs’s retirement from the machinery of economic policymaking scarcely dented his public activities, and it is a strength of Rowse’s biography that these are given their due weight in terms of both Coombs’s life and his impact upon Australian society. From 1968, however, Coombs played the role of public intellectual rather than official. As such he served as Chancellor of the Australian National University, Chairman of the Australian Council for the Arts, and Chair of the Council for Aboriginal Affairs. The last of these marked the pinnacle of Coombs’s role as an advocate for Australia’s indigenous population, but his interest and work on indigenous issues continued until his death in 1997.

Rowse’s biography of Coombs has not been well received by all, but the criticisms boil down to two main issues. Firstly, for some reviewers the biography is not sufficiently revealing of “Coombs the man.” Rowse himself acknowledges that the book is “more impersonal than most readers of biographies would wish”—a result, he says, of commitments made to secure Coombs’s cooperation. In the opinion of this reviewer, the absence of the “tell all” approach does not detract from the contribution the book makes to important questions of Australian economic history. The second set of criticisms concern what some have regarded as an excessive veneration for Coombs. In Australia, as in most countries, economic policymaking is contested domain and Coombs’s political savvy, and success in gaining the ears of generations of politicians, did not endear him to some bureaucratic rivals. One reviewer of the biography, a former Secretary to the Treasury (the great institutional rival to the central bank in Australia) even went so far as to label Coombs a “phony.”
This is a serious book that is a welcome addition to the study of Australian economic and social history, and the role of key officials and advisers in the nation’s political economy.

SEAN TURNELL, Macquarie University

UNITED STATES AND CANADA


The author, an assistant professor of history at Kent State University, has written a slender but satisfying narrative describing three generations of merchants along the Ohio River. Focusing upon Marietta, Ohio, but extending her case studies from Pittsburgh to Louisville, Kim Gruenwald analyzes how the commerce of the region shaped its identity. The heart of the tale is in the period before 1830, when the Ohio River formed the core of nearby settlers’ sense of place. For the first two generations, their identity was with the west, and they referred to their region as the Western country. But from 1820 forward, the northern and southern areas surrounding the river began to pull away from each other as transportation systems, foreign immigration, and slavery underwrote new identities that were no longer Western but Northern or Southern.

This sense of place is derived from a careful analysis of three generations of merchants; Dudley Woodbridge, his son, and grandson. Their correspondence, account books, ledgers, invoice books, blotters, and letter books form the evidentiary base for this study. From them the reader can see how widely the merchants traded, from whom they got their goods and how they were paid for, to whom they marketed their goods and what they received in return. This close analysis of commercial patterns and practices not only supports the author’s thesis about regional identity, it also offers a first-rate case study of mercantile practice on the frontier.

Dudley Woodbridge arrived in Marietta in 1789, determined to speculate in the lands of the Ohio Company and to engage in entrepot trade between this fledgling town and the Eastern seaboard. His efforts to supply area farmers with goods and markets, while fraught with hazard, proved to be successful, and he became one of the leaders of Marietta. Woodbridge’s ledgers show an amazing variety of goods handled as well as imaginative methods of payment. To facilitate his business, Woodbridge encouraged more settlement by farmers and he recruited artisans, such as smiths, shoemakers, and boatbuilders, to migrate into the Ohio Valley. By the turn of the century, Marietta was a center of trade for southeastern Ohio and northwestern Virginia.

Dudley Woodbridge Junior, was part of the second generation that linked Marietta with the growing regional hubs at Pittsburgh and Louisville. He began to move into wholesaling, buying up lots of goods to be shipped to country stores in the Marietta subregion. Having such access to goods, Woodbridge opened up branch stores as well, relying upon partners to help fund and operate the business. Success here required not only knowledge of markets and the trust of customers, but goods and credit from the regional hubs such as Philadelphia. As the area developed, trade with the burgeoning towns of Pittsburgh, Louisville, and Cincinnati grew. Expanding markets encouraged the development of regional manufactures, such as Pittsburgh’s iron and glass, Cincinnati’s textiles, and Lexington’s gunpowder. Meanwhile, the steamboat made upriver traffic possible and Woodbridge got tobacco and hemp from Louisville, salt from western Virginia and even cotton from Nashville and sugar

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from New Orleans. As he developed wholesaling based upon this greatly extended Ohio River commerce, trade with local farmers shrunk. If the Western country got its identity from the patterns of commercial trade, Gruenwald argues that it was cemented by its shared political interests, as westerners sought both better banking and transport systems.

The new transport systems would erode the importance of the Ohio River system. With two major canal systems crossing Ohio from south to north and connecting with Lake Erie, the Ohio River was no longer a primary route for those heading further west into the interior of the North America continent. The Great Lakes–Erie Canal waterway and good wheat lands led to development of the northern parts of the state. Railroads would confirm this northward shift. Ohioans now identified themselves with the term the Buckeye State.

The author’s focus upon commercial networks and regional identity is well placed. The consequences of the shift in identity from Western to Northern and Southern would have momentous effects in 1861. For economic and business historians, however, the virtues of this book are to be found in the author’s ability to recreate the patterns of mercantile trade during the earliest years of the Ohio territory and statehood. While the Woodbridges assume center stage, the patterns of their commercial exchange are confirmed with account books, ledgers, and blotters of other merchants. The work in these primary documents is impressive and the results will no doubt find themselves in the published work of others.

A classic piece of history, this book does not employ economic models nor does it rely upon advanced statistical technique. For some this may be a limitation, but for others, it means a more readable narrative.

DIANE LINDSTROM, University of Wisconsin


Louis Jordan has put together an impressive volume detailing the historical, political, and economic circumstances surrounding the issue of coinage in Massachusetts. His work offers both a thorough examination of the primary sources and literature on the topic as well as new information and interpretations. Jordan is able to synthesize the vast materials on the topic into a well-written scholarly volume that I suspect will become a standard reference for those interested in monetary matters of the British North American colonies.

Jordan explains every element related to his subject, from account book date recording to the various methods for minting coins. Numerous figures also provide a wonderful complement to his vivid descriptions, allowing you to see the layout of a mint along with pictures of the coins produced. The two appendices provide invaluable tools for researchers: a transcription of the Hull Shop Account from 1671 to 1680 with comments and explanatory notes provided by Jordan as well as a listing of relevant documents and events related to the Massachusetts Mint, in chronological order.

The history of the Hull family and John Hull’s principal partner, Robert Sanderson, is entertaining reading. While attempting to provide well-supported answers to questions such as the location of the mint, Jordan provides a rich backdrop on the development of Boston and the surrounding communities. He also includes relevant political details such as the requirements of the Puritan church and the relations between the colony and England in the turbulent eighteenth century. Jordan’s work offers excellent examples of the process used by England to govern the colonies as we follow petitions and representatives to meetings of the Privy Council and the Lords of Trade and Plantations. Perhaps more impressive than the information provided was Jordan’s ability to keep on point and not get drawn into
discussions of the political tensions between England and Massachusetts that relate to his main point in a purely tangential way.

The book provides technical details about the rules and methods for determining the purity of silver and the operation of a mint. For the period covered by the available account records, Jordan attempts to estimate the productivity of the mint by such measures as turn-around time for orders and the optimal size of a silver melt. Although his analysis requires him to make assumptions, none seem implausible. Jordan arrives at good estimates for the time to produce coins in Massachusetts. The problem of coin weight (pp. 54–63) was well presented though there were some simple steps that would have made it more informative. The lack of a scientific approach to coinage made weight something of a problem, evidenced by the examples Jordan presents (pp. 59–61). He provides an average coin weight in grains of silver but does not give the standard deviation. As a result the reader does not gain a sense of the distribution of coin weight.

The later chapters (Part 5, pp. 148–78) introduce issues of international exchange such as the activities of Spanish mints and the eight reales coin. One again Jordan provides an exhaustive amount of detail as he discusses the different exchange values attached to coins throughout the British Empire at the time, the one based on the silver content and then the legislated exchange value. He does a good job explaining the attempts to legislate the value of a shilling in Massachusetts to keep the coinage in circulation in the colony. This section of Jordan’s work combined with John McCusker’s *Money and Exchange in Europe and America, 1600–1775: A Handbook* (Chapel Hill: University of North Carolina Press, 1978) gives students and scholars alike a detailed set of references to examine for exchange-rate movements and monetary history in colonial America.

DAVID T. FLYNN, University of North Dakota

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It is impossible to write a review of *Gaining Ground* without resorting to superlatives. This is the most meticulously researched and lavishly illustrated (and heaviest) book I have ever reviewed. The subtitle, *A History of Landmaking in Boston*, refers to the leveling of hills and filling of bays that contributed to Boston’s growth. Nancy Seasholes documents this process on a year by year, wharf by wharf, block by block basis. Maps, drawings, and photographs depict the changes.

The book is the result of the author’s involvement in archaeological assessments, part of the environmental review process required of urban construction projects. One of the tasks in Boston is to determine whether the land is original or made land. If the latter, it has to be determined “when it was filled, why, by whom, how, and with what” (p. ix). The book, based in part on Seasholes’s dissertation, covers “the various parts of Boston Proper but also the Fenway and Bay State Road areas, Charlestown, East Boston, South Boston, and Dorchester” (p. ix).

The Shawmut peninsula on which the Puritans settled was 487 acres. Roughly 500 acres have been added, largely by filling tidal flats. Nearby communities have grown in a similar fashion. Landmaking was facilitated by the Colonial Ordinance of 1641 that defined the rights of shoreline property owners as including the flats to the low-tide mark (p. 21). The demand for wharves (later railroads and airports) was one major reason for made land, especially at those times Boston felt it was losing trade to New York City. The demand for a place to live (especially during the Irish immigration of the 1840s) was a factor, as was pollution from wastewater disposal.
The first chapter provides a general overview; the second chapter, a discussion of land-making technology. Although the technology always involved building a perimeter structure and filling behind it until the level was above the high-tide mark, there were changes over time in “the perimeter structures, the foundations on which they were set, the methods used to excavate, transport, and deposit the fill, and even the fill itself” (p. 13). Thereafter, the book is organized geographically, with the chapters each organized (roughly) chronologically. Each chapter begins with a map showing the part of modern Boston under discussion with the 1630 shoreline superimposed.

The simplest way to provide the flavor of the book is to provide an (edited) excerpt from the middle of Chapter 9.

... in January 1833 a group of lawyers, merchants, and businessmen formed the South Cove Corporation for the express purpose of filling the flats between Sea (the Free Bridge) and Front Streets in order to provide land for the terminals of the railroads then being built to Boston from Worcester and Providence. ... In April 1833 the South Cove Corporation paid the railroad a bonus of seventy-five thousand dollars and agreed to sell it land in South Cove for merchandise ... and passenger depots.

In April 1833 this land did not exist, however, and had to be created by filling. The South Cove Corporation had originally planned to purchase and fill the entire seventy-seven acres of wharves and flats bounded by Front, Essex, and Sea Streets and the South Boston Bridge ... , but even before the project began the corporation decided not to undertake so much and to fill only about as far south as Orange (now Herald) Street ... 

Filling of South Cove began in May 1833. To obtain fill the corporation had purchased eighteen acres of marsh about a mile away at the south end of South Bay ... and a three-acre gravel hill in Roxbury, also on South Bay. Scows brought the mud and gravel to South Cove (p. 246).

What this excerpt cannot show is that the text is illustrated by a 1777 map of Boston and its environs, a close up of South Cove on an 1832 map, and an 1835 map showing the filled area.

A short review cannot cover all this book has to offer; there are gems lurking in every chapter. For example, the first railroad in the United States was part of the Mount Vernon project (1805) and was used to carry gravel from the top to the flats (p. 138). And Logan Airport was named after a South Boston judge who was never in an airplane but had a wife interested in aviation (p. 377).

The book closes with an “Afterword” that amplifies three problems identified earlier in the book. First, falling groundwater levels, at least partially due to more efficient wastewater disposal, have led to rotting foundations in some of the filled areas. Second, the fill in some places has led to flooding in others that were not originally filled above the high-tide mark. Finally, even though the last major earthquake was in 1755, it remains true that the extensive (and expensive) capital developed on filled land is at greater risk.

Gaining Ground is packaged as a coffee table book, and it should find its way to the coffee tables of all those interested in Boston history. Others, particularly those interested in urban development, will find it rewarding because the issues Seasholes addresses are common to many cities. Among these are concerns about growing concentrations of bridges and railroads and the need to develop public land to accommodate public buildings (e.g., Faneuil Hall and the Custom House) and public space (e.g., Charlesbank-the Esplanade-Storrow Drive and Frederick Law Olmsted’s Back Bay Fens). Gaining Ground is an encyclopedic work that can be consulted with profit in whole or in part. We are indebted to Nancy Seasholes.

LOUIS P. CAIN, Loyola University Chicago and Northwestern University

In this admirably short book, Rudy Higgens-Evenson encapsulates an important chapter in the history of American governance. Between Reconstruction and the Great Depression state and local governments underwent a fundamental transition, as the scope of their activity widened and as the center of gravity shifted to the states. Higgens-Evanson situates the expanding public agenda within an emerging interest-group politics, which allowed expenditure demands to find expression outside the fiscal restrictions of nineteenth-century partisanship. Government officials and academic experts also played key roles in lobbying for a larger government role, in critiquing the old fiscal regime—one based on a locally administered general property tax, and in shaping the particulars of new tax systems. A chief complaint centered on the general property tax’s inability to effectively tap the wealth of an emerging corporate economy. Land owners felt unduly burdened compared to the holders of financial and corporate property. Government officials and program advocates thought their efforts were being hampered by a constrained fiscal device. And corporations chafed under the complexity and unpredictability of the general property tax, which varied considerably from one jurisdiction to the next, depending on the sympathies of the local assessment machinery.

These concerns merged in the fiscal transformation running through the center of Higgens-Evenson’s narrative: the movement of state governments away from the general property tax and toward an assortment of corporate levies. Political acceptance of the new systems of corporate taxation typically required a compromise among three parties: state governments, which got a new revenue source to satisfy rising expenditure demands; corporations, which got significant exemptions from the local property tax, rationalizing—and sometimes lowering—an important business expense; and local governments, which received revenue from the state to compensate for their diminished property tax base. The growing state dependence on corporate wealth also had important political consequences, as corporations became more directly involved in state policy making, exerting greater influence along administrative, legislative, and intellectual channels.

The precise outcome from this three-way bargaining varied across the states, differing in the types of corporate taxes employed, in the relative winners and losers across economic sectors, in the fiscal arrangements struck between governments, and in subsequent corporate influence over state policy. Also, even though policy expansion and administrative centralization were widespread, the shift to corporate taxation was not universal. Fiscal divergence was underway, creating a division between what Higgens-Evenson calls the “corporate states” and the “Jeffersonian republics.” The former were typically more industrial; they had a larger corporate sector to tax; and they embarked earlier on programs of fiscal expansion. The latter had economies based mainly on agriculture and extractive industries and thus funded their growing, though less ambitious, commitments under the general property tax.

Higgens-Evenson captures this diversity through an approach that aptly combines case study, fiscal data collection, and a synthesis of secondary literature. In addition to tracking the course of tax reform, the book examines prominent expenditure areas, notably education and road building, the latter tied to another significant revenue transformation beginning in the 1920s: heavy reliance on gasoline taxes and related licenses.

My primary reservation about the book falls under the category of missed opportunities. This topic cries out for more explicit quantitative treatment, especially in light of the project’s data collection efforts. The available data may not be perfect and the fiscal sources can be maddeningly idiosyncratic, but the situation is not hopeless. A number of statistical
patterns and relationships could have been explored, quite possibly buttressing the book’s themes, several of which are reinforced by an encouraging, though still underdeveloped, body of recent fiscal research. Missed opportunities aside, Higgens-Evenson has done the field a service by placing a concise narrative on the table, ready for closer scrutiny.

Such scrutiny must grapple with an interpretive puzzle and a historical irony. The puzzle concerns our larger political evaluation of the shift to corporate taxation. Although corporations were targeted with new taxes amid a political environment with some anti-corporate strains, they also were relieved of the hassles and (potentially heavy) burden of the general property tax, which at its core was a tax on wealth. Still unclear in the existing literature is the extent to which changes in tax policy were driven by corporate politicking and ideological predispositions sensitive to corporate interests, rather than by high-minded tax reform or a pragmatic effort to fund a wider public agenda. In rough schematic terms, the present narrative suggests a causal chain running from diverse expenditure demands, through tax reform modulated by expert opinion and interest-group pluralism, and to the (seemingly unintended) consequence rising corporate influence over state governance and politics. An alternative schematic might place a more sustained emphasis on corporate influence all along, in propagating some expenditure demands, in defining the fiscal crisis and acceptable solutions, and in engineering a system conducive to enhanced political influence. Such an alternative might find reinforcement in the historical irony revealed by extending the time frame beyond 1929. Although state governments relied heavily on corporate taxation for a time, the larger trend was very different—away from a taxpaying public defined by property ownership and corporate status, and toward a universal taxpaying public tapped on both its incomes and purchases. The corporate tax may have been the “price of progress” during the early twentieth century, but a rebate was right around the corner.

MONTY HINDMAN, University of California, Riverside, and University of Michigan


This useful book is a publication of the Pension Research Council of the Wharton School. It provides an analytical history of public-sector pensions in the United States, which had important differences in their management, distribution, and investment funding and which offer important lessons for recent proposals to restructure Social Security and to expand defined contribution, employer-provided pension plans. There are 11 chapters, an appendix of historical price and wage indices covering various periods from 1720 to 1939, and detailed references to the history and economics of public- and private-sector pension systems.

The book has three objectives: to provide the first basic reference volume on the history of public-sector pensions in the United States; to identify policy lessons for modern day pension analysts; and to provide a chronology of the theory and practice of pension fund management. As the authors state there is no disentangling the history of public-sector pensions from that of the financial management of the monies in the pension funds and the evolution of finance in the United States. Accordingly, pension history offers insights into the development of American financial markets as well.

Chapter 1 summarizes the evolution of public pensions in the United States that predated private-sector arrangements. Military pensions were the first to appear, and for a while, navy and army pension systems were quite different. Both were aimed at providing replacement income for soldiers and sailors injured in battle, to offer performance incentives, to
arrange for orderly retirements, and to respond to political pressures. Naturally, these objectives were not always mutually consistent. A major difference between the navy and army plans, however, was that the navy plan was financed with monies from the sale of captured prizes. Revenues were very erratic, fluctuating with the fortunes of war and peace. As a result, Congress established the navy pension fund and allowed the trustees of this fund to invest in a wide range of assets, including private equities. The history of the management of the fund reveals the many problems that arise when public pension monies are used to purchase private assets. One problem was investing in politically favored firms that did not offer the highest returns, weakening the fund. There also were other moral-hazard issues. Congress expanded benefits and coverage, and along with low investment returns, these actions caused the fund to become bankrupt. Congress then was required to shift financing to the treasury’s general fund and government debt. Contemporary proposals to allow the Social Security trust fund to be invested in private equities are likely to encounter similar problems. The army pension plan has a much smoother history. It was always financed on a pay-as-you-go basis from general revenues. Other government pensions began with disability and retirement benefits to some city employees, beginning in 1850. The first state retirement system was inaugurated by Massachusetts in 1911, and the Federal Employees Retirement Act of 1920 systematically provided federal employee pensions. The authors point out that private pensions began later and were less generous than were government programs. Private pension systems did not expand dramatically in coverage and benefits until after World War II, but were influenced significantly by public pension practices.

Chapter 2 summarizes pension economics to provide a framework for the subsequent discussion. Chapter 3 reviews the history of military pensions from Roman times through feudal practices in Europe and on to their use by the British government to develop the navy. American colonial practices are described, and the chapter ends with a comparison of the army and navy systems. Chapters 4–7 present the development and management of the American naval pension system. Very useful data series are provided in the chapter tables, including numbers of pensioners, outlays, key dates, benefit payments, and the value and investments of the naval pension fund. Problems with the administration of the fund and its ultimate bankruptcy are described. The authors also suggest how the naval fund relied upon and at the same time stimulated the growth of broader U.S. financial markets. Chapter 8 presents the army’s pension development from the Revolutionary, Civil, and Indian war pensions through more modern conflicts. Once again, the data tables are very complete and informative. Chapter 9 turns to the development of federal employee pensions and describes the growth of public-sector unions in influencing the timing and nature of the plans. State and local worker pensions are discussed in Chapter 10. These began with police and fire department pensions and spread to teachers and other municipal and state employees. Key inaugural dates and state plan provisions are summarized. Funding arrangements also are detailed. Chapter 11 draws some useful lessons from these pension histories to the current policy debate. The authors summarize four points: monitoring costs affect the timing and characteristics of public sector pensions; moral hazard and administrative problems plagued the plans and in some cases were only recently, if ever, overcome; the pensions grew dramatically, pushed, in large part, by politics; and the growth of public pensions and their generosity had effects on labor markets throughout the economy.

All in all, this is valuable economic history that informs current debate. The book serves as an important reference for all scholars who work on U.S. labor market issues, as well as those interested in the provision and financing of certain government services.

GARY D. LIBECAP, University of Arizona

When discussing “markets” today we usually are bandying about abstractions; so it was with great anticipation that I started a book that deals with physical markets: the original sort, places where farmers came to sell food to townsfolk. This short, illustrated book treats the history of what the author calls the “public market system” in the United States from the nineteenth century, when publicly owned facilities “were the principal source of the city’s daily supply of fresh food,” (p. 26) to the early twentieth century, when many markets had closed, replaced by distributed grocery stores and butcher shops. The reason the author calls public markets a “system” is that they embodied, in her view, a “moral economy”: “It was the duty of the state to ensure that the urban populace would have an adequate, wholesome, and affordable supply of necessities,” (p. 3) and the only way to accomplish this was through “regulated public markets.” This preconception runs through the book; against the ideal—a competently managed public market, where farmers and consumers meet directly—all other “laissez-faire” commercial arrangements (peddlers, shops, private market houses) are judged and found wanting. That the ideal frequently was not achieved, as the author recounts, does not weaken her conviction.

Central markets were ancient venues for trade. We learn that in America, laws regulating markets were commonplace. In particular, some laws restricted the selling of food outside public markets except under certain conditions. Markets were often located in the center of town, sometimes in the middle of a street, as was Philadelphia’s High (later Market) Street market. They might be placed on the ground floor of multi-purpose buildings (like Boston’s Faneuil Hall Market) or simply in open sheds. Animals were brought on the hoof through the streets to the butchers’ market stalls. Over time in large cities, with populations spreading out and businesses replacing residences in the old centers, the early markets became inconvenient. Moreover, the rationale for a city operating real estate, rather than regulating food safety and weights directly, was questioned. Thus began agitation to loosen restrictive laws and reconfigure the markets. Food was supplied by peddlers, neighborhood shops, and modern private markets. But the tenants of public market stalls opposed these developments, as did some city fathers. It seems that markets could be important sources of revenue for cities, bringing in more in rents and fees than they cost to manage, and in such cases, cities were reluctant to loosen restrictions. Market buildings—some very large ones—continued to be constructed into the twentieth century by municipalities and private investors.

But demographic and economic trends worked against the central, publicly owned market. Not only was population living farther from the old centers, but farms were located farther from cities. Food arriving from greater distances was handled by wholesalers, and some public markets became wholesale markets. But with growing traffic, wholesale markets needed to be removed from congested areas. Public markets can be found in some cities today, and some cities arrange for farmers’ markets in harvest season. But these play a minor role in a city’s food supply.

What I wish the author had provided, in addition to the focus on markets, is an overall picture of how urban households obtained food. How important were public markets compared with alternative sources? What kinds of vendors actually occupied the markets: farmers, or retailers and wholesalers? Regarding the public markets’ purported advantages, did the markets actually keep food prices down and quality high, and assure food abundance?

The biggest problem with the book for someone interested in economic history is that the author does not analyze the developments in economic terms. She takes for granted that her ideal market arrangement yields the best results for public welfare. Rather than tracing how and why food marketing changed, the author laments the divergence from her ideal. She
plainly loves public markets, but writing as their defender rather than impartial interpreter, she fails to explore the interesting economic questions a study of markets raises.

SARA E. WERMIEL, Massachusetts Institute of Technology


James L. Huston, professor of history at Oklahoma State University, has written books on The Panic of 1857 and the Coming of the Civil War (Baton Rouge: Louisiana State University Press, 1987) and Securing the Fruits of Labor: The American Concept of Wealth Distribution, 1765–1900 (Baton Rouge: Louisiana State University Press, 1998), as well as numerous articles on topics in antebellum history. In this book Huston argues that the cause of the Civil War was economic, generated by the southern defense of property rights in slavery, which would have created an unfair labor market that would have ruined the opportunities for free northern village labor. It was the reaction in the North to this growing slave system, with the possible expansion of slavery into the North, lending to the ruination of that society, that generated the late antebellum political changes that made compromise between the sections impossible. The title of the book, drawn from an 1829 comment by Thomas Cooper, president of South Carolina College and a political economist, on the tariff debates, suggests the southern willingness to make a decision on secession on the basis of a simple benefit-cost calculation.

Calculating the Value of the Union is based on extensive archival research and a thorough reading of the secondary literature on antebellum political and economic history. The 41 page bibliography includes 78 manuscript sources and 126 newspapers and periodicals. In addition there are two detailed appendices on political history, one tracing out “A Theory of Political Realignment” to describe the political changes of the 1850s, and the other providing graphical presentations of the changing pattern of voting by state in the last two antebellum decades. The basic presentation of the volume is in two parts, the first relating to the issue of property rights, particularly those in slaves, to sectional conflict, and the second a rather more traditional narrative on the role of slavery in the political struggles between North and South in the years 1846 to 1860.

There is much of considerable interest said about northern and southern attitudes towards slavery, free labor, and property rights. The author’s major contention that the war was fought over property in slaves, although perhaps not as novel as suggested, does point to a basic aspect of the sectional debate. Whether republican ideology is so easily dismissed as “puny ideals” in contrast with the “brute fact” of slave values, perhaps draws too sharp a distinction. As Adam Smith pointed out, the demand for morality is downward sloping and some trade-offs may exist—between wealth maintenance and belief—although, given the value of southern slaves a compensated emancipation would have been quite expensive for the North (as was also the war). This was undoubtedly the reason that compensation of slaveowners, as done in most previous cases of slave emancipation, was seldom discussed in the United States, but Huston does not say much about the compensation discussions or of the emancipation practices of other countries. He does describe the beliefs in the natural death of slavery held at that time, but little is said in any detail about the period of time over which this would occur, a crucial issue for setting policy.

The timing of the Civil War is linked to what is described as the “North’s paranoia,” due to the fears of competition from slave labor affecting the middle and lower classes in northern agriculture. Southern paranoia, on the other hand, reflected the fear of changes in the
nature of property rights in slaves. Transportation improvements and market integration led to more competition between free and slave labor, and a drive toward territorial expansion in both sections. An interesting counterfactual proposed by Huston (p. 100) is that in the absence of the slave emancipation resulting from the Civil War, “it is not inconceivable to think that around 1890 or 1900 the North would have seceded from the Union to protect its economy from slave labor domination,”—but it is not hypothesized what the southern reaction to northern secession might have been in that circumstance.

Huston’s analysis of the economic and political debates about slavery is first-rate, drawing together a great deal of information. Whether or not one is fully convinced of his interpretations of northern and southern society, much that is useful is presented about the experience with slavery in the United States. Little, however, is said about slavery and abolition in other countries, or why the North ended slavery before the South and in a quite different manner, so that some possible comparative dimensions are not explored. None of this, however, detracts from the contributions made in this most thoughtful and stimulating work.

STANLEY ENGERMAN, University of Rochester


This book represents an attempt to bring historical evidence to bear on issues that maintain contemporary importance in the areas of public health and public policy. Contributors use new historical data sets to explore the factors affecting mortality and health outcomes across populations and labor-supply decisions of the aged.

Most of the papers make use of the Life-Cycle Data on the Aging of Veterans of the Union Army collected under the project title “Early Indicators of Later Work Levels, Disease and Death.” The sample consists of longitudinally linked observations for 39,616 white males in the Union Army. The data set was created by linking information from military records, pension records, surgeons’ reports, and population censuses. It includes a wealth of information that provides a picture of the life history of each veteran. In the first chapter, Larry Wimmer provides an account of the collaborative effort that was required to create this data set.

There appear to be three central themes that connect the articles in this volume. The first is the effect of socioeconomic status on mortality and health. Joseph Ferrie uses a sample of 175,000 individuals from the 1850 and 1860 censuses to examine the relationship between wealth and mortality. Previous studies had found little relationship between wealth and mortality in historical data. Ferrie, on the other hand, finds evidence of disparities in mortality by socioeconomic status in 1860 (p. 31). In an investigation of the effects of socioeconomic status on health outcomes, Clayne Pope and Sven Wilson consider community- and family-level influences that affect adult heights. They find that farmers enjoy a distinct height advantage over other occupations in both urban and rural communities (p. 132). This finding is explained by both better nutrition and reduced exposure to infectious disease for farmers in rural areas.

The second central theme in the book is the effect on mortality of infectious diseases and migration, both important features of nineteenth-century life in America. In a pair of papers, Chulhee Lee and Daniel Scott Smith examine the question “Why did so many Union Army soldiers die of disease during the Civil War?”(p. 110). Whereas Lee focuses on prior exposure to infectious diseases, Smith considers the environment to which soldiers were exposed during the war. Lee finds that prior exposure to infectious diseases reduced the chances of dying from disease (p. 69). This appears to be due to increased resistance. Smith finds that
although prior exposure was important, its effects became less important over time. After
the first year of service, the environment experienced during the war became a more impor-
tant predictor of death from infectious disease (pp. 107–08).

Mario Sanchez examines the characteristics of migration patterns in the nineteenth
century and the effect of migration decisions on later mortality of migrants. He finds return
migration was a common feature of nineteenth-century migration patterns (p. 219). Migration
was also found to reduce life expectancies, a finding that is explained by the increased
probability of dying from infectious diseases (p. 220).

The third theme connecting the papers in this volume concerns the labor-supply and retire-
ment decisions of Union Army veterans. Tayatat Kanjanapipatkul finds that the receipt of
pensions reduced labor-force participation among Union Army Veterans (p. 246). In addition,
the elasticity of retirement with respect to pension income varies across occupations (p. 249).
Chen Song and Louis Nguyen examine the labor-supply implications of poor health for Union
Army veterans. Specifically, they examine the case of hernias. They find that retirement
decisions for Union Army veterans were not influenced by the existence of hernias.

As the papers in this book depend largely on the Life-Cycle Data on the Aging of Veter-
ans of the Union Army, its focus may at first appear somewhat narrow. Yet a particularly
appealing feature of the book is that it carries implications for broader issues in American
economic history. For example, the evidence relating to the costs of migration may help to
explain the existence of large urban wage premiums in the nineteenth century. Likewise,
the evidence on the factors affecting adult heights of Union Army veterans may shed light
on the larger issue of declining heights in mid-nineteenth-century America. Despite these
advantages, it is also true that the Union Army data are confined to a sample of white males
from the North. It is unfortunate that this wonderful data set cannot be used to examine the
experiences of women, children, Southerners, or African Americans.

Overall, the book provides a detailed analysis of the factors affecting health, mortality,
and labor-force participation in nineteenth-century America. As such, it will be of interest
to economic historians, labor economists, and historians. One point that emerges clearly is
the importance of infectious diseases for health outcomes and subsequent mortality in
nineteenth-century America. From this standpoint, there may be implications for developing
economies in which infectious diseases remain prevalent. This book may therefore be of
special interest to those in the field of public health. Finally, the attention devoted to migra-
tion means that this book should also hold the interest of those interested in demography.

MARIANNE WARD, Loyola College in Maryland

The Claims of Kinfolk: African American Property and Community in the Nineteenth-

During the past generation a number of scholars have examined the question of black
property ownership in the South. None of the articles and books, however, has looked at
the question from the perspective of slavery in West Africa and communal or kinship
ownership in the United States. Dylan C. Penningroth uses a case study of the Fante region
on the Gold Coast of Africa (present day Ghana), following the British emancipation in
1874, to show how kinship claims to property were in some ways similar to slave property
ownership in the United States. In both locations, he writes, “ownership often included
multiple, overlapping, and sometimes competing claims” (p. 41). Viewing the history of ex-
slaves’ negotiations “over property and social ties through the prism of African Studies,”
the author contends, brings into better focus black social relationships. It turns attention
away from white–black race relations and shows black kinship and black community life on “their own terms, with all their dynamism and noisy dissent, rather than as mere side effects of their dealings with whites” (p. 185).

The author follows his case study with an analysis of the internal slave economy and the changes that occurred as a result of the Civil War. The major theme that emerges is that the best way to view black property ownership in the mid-nineteenth century is not, as other historians have done, through cultural change (blacks accepting the ethos of acquisitiveness), or as a means of resistance, but rather through the prism of African history where the “negotiations over kinship, labor, and property” (p. 12) were pervasive.

Penningroth uses a wide variety of sources—the WPA slave narratives, black autobiographies, Frederick Law Olmsted’s travel accounts, and others. The author relies particularly heavily on the Southern Claims Commission reports. Created by Congress to reimburse Union loyalists who lost property to Union troops who were foraging for supplies and food during the Civil War, this evidence is skewed toward a few densely black populated sections of the Lower South, especially the Sea Islands and coastal regions of South Carolina and Georgia, and the Natchez and Vicksburg areas of Mississippi. Although the author makes every effort to place the interpretation—that black economic dealings were more social and communal than financial and economic—in a South-wide context, he keeps returning to these locations. Indeed, a few counties, including Liberty County, Georgia, appear to be extremely important to understanding his thesis. To the author’s credit he admits that his evidence is only suggestive and in some respects “inconclusive” (p. 161). The theories advanced, however, are evocative, even if they do not offer a new methodology for conceptualizing African American property ownership.

LOREN SCHWENINGER, University of North Carolina at Greensboro


Robert Bremner’s little classic, American Philanthropy (Chicago: University of Chicago Press), has, since its publication in 1960, been the place to start for anyone interested in a survey of charity and philanthropy in the history of the United States. Bremner’s book—revised a bit and then reprinted in 1988—has of course the historiographical markings of its birth time, the late 1950s. Charity, Philanthropy, and Civility in American History, edited by Lawrence J. Friedman and Mark D. McGarvie, argues that Bremner’s “consensus approach to the American past” (p. 4) is not fit for “our time,” and sets out to fix things. According to Friedman, the “central purpose” of the collection is to “replace Bremner’s American Philanthropy with a book that [reflects] the research and thinking of some of the most sophisticated of the current generation of American History scholars” (p. 21).

The contributors would like the book to be more than simply a restatement of Bremner-without-the-consensus approach. Allegedly unlike Bremner, the contributors believe “[f]irst . . . that charity and philanthropy have always involved intense preoccupations with deeply compelling visions” (p. 10); the authors believe further that “despite the intentions of philanthropists to impose their vision of the ‘good society,’ philanthropy has also involved reciprocity between givers and recipients of ‘good’ qualities” (p. 11); that “philanthropy . . . redefined concepts of gender” (p. 12); that “the lines between ethnic and philanthropic experiences were often deeply permeable” (p. 12); that “philanthropists derived a sense of both empowerment and identification from their activities” (p. 13); that “the American mix of public, private, and voluntary agencies to meet peoples’ needs” fluctuates (p. 13); and
that “American philanthropy can hardly be understood without the benefit of a complex international perspective” (p. 15).

The 19 essays cover a wide range of material, aiming, of course, to articulate features of American charity and philanthropy that Bremner, “preoccupied,” Friedman says, “with elite white male Protestants” (p. 11), had missed. It is not clear whether the book hits its target. For example, Part 1, “Giving and Caring in Early America, 1601–1861,” contains six essays and 155 pages on colonial and antebellum charitable practice but only one essay—“American Indians and the Practice of Christianity,” by Stephen Warren—departs from Bremner’s preoccupation. Parts 2 and 3 certainly reach beyond a concern with elite whites. Here one finds essays sensitive to, if not heavily concentrated upon, the donor-recipient interplay of race, gender, ethnicity, and religion; American philanthropy abroad (chapters 11, 15, and epilogue); and a (perhaps overplayed) distinction between public, private, and nonprofit organizations.

Mary J. Oates offers a compelling story on the politics of Catholic charity in Quincy and Springfield, Illinois, showing from a case study the sometimes-brutal competition internal to the church for the allocation of charitable funds—a competition that was often “resolved,” Oates finds, by the coercive power of bishops (p. 283). But Roy Finkenbine’s essay on “Law, Reconstruction, and African American Education in the Post-Emancipation South” (chapter 7) is probably the chief example of an essay that succeeds in the post-Bremner mode. Finkenbine tells the story of the Slater Fund—“the earliest of the major foundations devoted exclusively to the education of the recently freed slaves” (p. 168). Directors of the Slater Fund, Finkenbine shows, were entwined complexly in the politics and economics of abolition and segregation. Drawing closely upon primary source material, Finkenbine is conservative in his suggestion that the Slater Fund and other foundations “imposed on African Americans—often against their explicit wishes—a curriculum designed [at Tuskegee and elsewhere] to train [African Americans] for political, social, and economic subservience” (p. 178). He points out that W. E. B Du Bois, Carter G. Woodson, Thurgood Marshall, and Martin Luther King Jr. were graduates of Slater Fund and other foundation-sponsored industrial curricula and that after all the foundations were not “the only sources of funding available for the support of African American education in the South” (p. 178). (Perhaps, as Friedman and McGarvie find in Bremner, some “hierarchies” can be found in Finkenbine when he omits Booker T. Washington, a graduate of the Hampton Institute, from his short list of heroes.)

Bringing to philanthropy studies a more explicit rhetoric of race, class, gender, and religion is a valuable and long-awaited revision of the Bremner style of scholarship. Economic historians however will not find fulfillment in some big promises of the book. According to Friedman, the authors “ask whether private nonprofit resources have ever been very significant, proportionate to government resources, in addressing” poverty and other social ills (p. 18). Yet none of the chapters try to answer the question. Philanthropy scholars, Friedman insists, “must demonstrate precisely how much “change” (up and down)—in real dollars, concrete services, and poverty levels—has occurred in America since” the Elizabethan Poor Law of 1601 (p. 20). Yes. Again, however, none of the chapters do so (see, for example, page 400).

*Charity, Philanthropy, and Civility in American History* is not a book for footnote scholars: oddly, despite the editors’ desire to raise the level of scholarship on philanthropy, individual authors were allowed a brief “suggestions for further reading” only, with no room for explicit citations of source material. Still, anyone wanting acquaintance with the current state of scholarship on charity and philanthropy in America shall profit from reading this book.

**Stephen T. Ziliak, Roosevelt University**

The Encyclopedia of Tariffs and Trade in U.S. History is an ambitious project that spans three volumes and over 1,600 pages. Volume 1 is a standard encyclopedia consisting of many entries on trade- and tariff-related subjects. Economic historians will probably find this volume disappointing for two reasons. First, nearly all of the contributors are historians. This is not a problem in itself, but it means that the focus of the entries is limited to political figures, industries and industry associations, a few events, and very few concepts. More importantly, the authors make virtually no mention of the economic-history literature on the various topics under consideration.

Second, most of the entries are very short, just five to seven paragraphs, and do not provide much more than a basic sketch about the subject. One does not find detailed and ambitious essays as in Joel Mokyr’s Oxford Encyclopedia of Economic History, but just a few basic facts and description. The bibliographic citations are meager, serving up a standard reference or two, no more. With this limited detail and basic bibliography, the volume is most likely to be of value to undergraduate students who are starting research on a trade-related topic.

Volume 2 consists of various primary documents related to trade policy debates. These range from excerpts from The Federalist and Alexander Hamilton’s Report on Manufactures, to various Congressional and Presidential statements on the tariff, to the text of the 1947 General Agreement on Tariffs and Trade and a transcript of the Al Gore-Ross Perot debate over NAFTA on CNN’s Larry King Live in 1993. This volume usefully collects various materials that would otherwise be difficult to locate. This volume contains much more historical material than Frank Taussig’s older collection State Papers and Speeches on the Tariff (Cambridge, MA: Harvard University Press, 1893) and has the added advantage of bringing things up to the present. This makes it a valuable resource.

Volume 3 presents the actual texts of the major tariff legislation, from the first tariff in 1789 to the Smoot-Hawley Tariff of 1930. This volume will be valuable for researchers interested in tracing the evolution of a duty on a particular commodity by having all the major tariff acts collected in one handy volume. This is a useful compilation insofar as the main previous reference, Tariff Acts Passed by the Congress of the United States from 1789 to 1909, published by the Government Printing Office in 1909, lacks the Underwood tariff of 1913, the Fordney-McCumber tariff of 1922, and the Smoot-Hawley tariff of 1930, and has generally been banished to off site, remote storage by most libraries. The GPO volume, however, is more comprehensive than the Encyclopedia’s volume 3 as it includes other proclamations and acts related to trade. In addition, the GPO volume has a complete index of commodities so that one can trace the evolution of duties much more easily than one can in the Encyclopedia’s volume.

Overall, these volumes bring together a good deal of useful information and university libraries would do well to acquire them.

DOUGLAS A. IRWIN, Dartmouth College


In 1986, just when many were beginning to question the viability of the American union movement, Robert Zieger published a survey history of the American unions from 1920–
1985. In *American Workers, American Unions*, Zieger sought to provide a “civic history” of American workers, or a history of workers’ collective action in labor unions. As such, the book was a great success. In barely 200 pages, Zieger provided a survey of the organized labor movement suitable for undergraduate history classes that was also of value to graduate students and their professors.

The current third edition is an update of the classic Zieger text with two new chapters and some additional material added to a third. Written by Zieger, the first chapter reviews labor relations before World War I. He shows how competition and the imperative to reduce costs led employers to challenge the position of skilled craftsmen within American manufacturing and to recruit lower-wage immigrant workers from Southern and Eastern Europe. Behind employer anti-unionism, he argues, was a determination “to control the workplace free of restriction” (p. 21). New material added to the next chapter describing labor relations during World War I adds a useful review of that period’s dramatic industrial changes as well as the entry of new workers, especially women and southern African-Americans, into the industrial labor force.

The second entirely new chapter is written by Gilbert Gall whose previous works include a superb biography of Lee Pressman, the lawyer associated with the Congress of Industrial Organizations of the 1930s and 1940s, and studies of the politics of “right to work” legislation. After reviewing the rise of the new “organizing model” for American unions in the 1990s, Gall’s chapter discusses organized labor’s growing political activism through the 1980s and the Clinton administration of the 1990s. Gall nearly reverses the earlier focus on employers and technology to emphasize union strategy and politics. In the new chapter 1, for example, Zieger says nothing about the AFL’s campaign against the judicial injunction and for the Clayton Act (1914); Gall, by contrast, neglects recent technological changes and population shifts to focus on recent union political campaigns such as that against NAFTA.

The enhanced focus on politics and state policy rather than economics and technology in Gall’s concluding chapter continues a trend in Zieger’s own chapters where politics looms more important in the chapters covering the later twentieth century. Zieger and Gall may be suggesting that organized labor’s recent problems may be more in the political than in the economic realm. If so, there may also be a lesson here for labor activists, and for historians.

*American Workers, American Unions* is a good book made better in its latest incarnation. Those looking for a course book for twentieth-century labor history should consider it. Those looking for their own enlightenment will find it a quick and useful read. Each edition has been better; I look forward to the next.

GERALD FRIEDMAN, *University of Massachusetts at Amherst*

**Forging a Common Bond: Labor and Environmental Activism during the BASF Lockout.**


Timothy Minchin continues his solid work on labor in the post–World War II South with *Forging a Common Bond*. Minchin blends labor history with environmental history to produce a narrative of the infamous BASF lockout of union workers at its Geismar, Louisiana, facility in the 1980s. Based on union records, press accounts, and oral histories with participants on both sides of the lockout, Minchin’s latest work tells an important story of an unlikely alliance between unionized industrial workers and environmentalists.

Buoyed by the Reagan revolution and the conservative political tide in Washington, Minchin argues, German chemical giant BASF sought concessions on wages and seniority
from its workers at Geismar in 1984. Union leaders, convinced that the company’s true
intent was to destroy the union, balked at the changes. Workers seemed particularly com-
mitted to resisting alterations to “non-economic” benefits such as seniority. When the
existing contract expired in June 1984 without an agreement, workers refused to strike,
preferring to remain on the job as negotiations continued. BASF locked out its 370 workers
on 15 June. Company officials justified the lockout by arguing that they feared sabotage if
disenchanted union members were allowed into the plant without a contract. The BASF
dispute lasted more than five years before a settlement was finally reached. In the process,
the Geismar local of the Oil, Chemical, and Atomic Workers International Union (OCAW)
forthed alliances with environmental groups such as Greenpeace and the Sierra Club to
prosecute its struggle against the German chemical giant.

OCAW developed an effective corporate campaign against BASF. Like other union
officials in the 1970s and 1980s, OCAW leaders believed that strikes “were becoming
increasingly ineffective” and “looked for other ways to exert pressure against companies
that vehemently opposed organized labor” (pp. 58–59). Corporate campaigns encouraged
consumer boycotts and used the media to spread negative publicity about the target firm.
The campaign against textile giant J. P. Stevens was perhaps the most famous example of
this new union strategy. The textile workers’ victory at Stevens in 1980 inspired other
corporate campaigns, though few duplicated the success of the Stevens campaign.

OCAW’s corporate campaign developed new themes, such as environmental danger, that
had traditionally been ignored by organized industrial workers. Before the lockout, BASF
workers in Geismar viewed the environmental movement “with suspicion, or even hostil-
ity,” exemplified by the comment of one worker: environmentalists “were a bunch of
loonies that . . . were after my job” (p. 17). Although Minchin notes that the hostility of
labor toward environmentalists has been exaggerated, the attitudes of BASF workers were
far from warm and welcoming toward the environmental movement. BASF workers exhib-
itied little environmental awareness or concern until after the lockout began. The union made
no mention of safety or environmental concerns at the outset. Indeed, many workers spoke
of the “trade-off” they had made in going to work for BASF—a slightly shorter lifespan in
exchange for (comparatively) decent wages and benefits. Eventually, however, OCAW
leaders decided to use the environmental issue against BASF.

A disastrous leak from a Union Carbide plant in Bhopal, India, in December 1984, killed
and injured thousands. The event did not go unnoticed by OCAW leaders involved in
coordinating the union’s campaign. The Bhopal disaster, combined with a similar (though
far less deadly) leak at a chemical plant in West Virginia in August 1985, helped push
OCAW leaders toward a greater emphasis on environmental safety. BASF continued to
operate the plant using salaried personnel and temporary workers. The union charged that
this compromised safety. Eventually, the union used the Bhopal accident in its signature
publicity effort. At the beginning of the lockout, OCAW had placed a large billboard along
Interstate 10 in Baton Rouge that read “Stop Foreign Oppression of Louisiana Workers.”
After deciding on the environmental angle, the union changed the billboard to read “Bhopal
on the Bayou? Stop BASF Before They Stop You.” The billboard incensed company
officials but proved an effective public relations tool for the union.

Minchin’s book illustrates the difficulties in uniting workers across national bound-
daries, even when the laborers in each nation are organized. BASF’s unionized German
workers, for example, had difficulty relating to their American counterparts. U.S. unions
were more confrontational than those in Germany. In addition, early in the campaign, the
American union used some questionable tactics that alienated German workers, such as
leaflets that referred to BASF’s participation (through the old I. G. Farben cartel) in the
Holocaust. German workers also were uncomfortable with the American union’s use of
the environmental issue. OCAW leaders, for their part, believed that German workers
were too willing to accept BASF’s safety assurances at face value. In the end, OCAW received more support from German branches of Greenpeace than from its international union brothers.

Eventually, the connections with environmental groups in Louisiana and pro-environment politicians such as Buddy Roemer put enough pressure on BASF to encourage the company to settle. OCAW successfully linked its cause with environmental responsibility and concern for the broader community in Louisiana. The company and the union finally reached an agreement in December 1989 and most locked-out workers returned to their jobs with promised wage increases and seniority guarantees.

The union’s environmental awareness did not fade with victory, Minchin argues, and workers insist that there will be “no more trade-offs” between safety and material well-being (p. 155). *Forging a Common Bond* is an effective and detailed account of a local struggle with broader implications.

**Randall L. Patton, Kennesaw State University**


A survey of nine business college faculty standing in the hallways or whose office doors were open found only three who could correctly answer the question: “Where does the “Chrysler” in Chrysler Corporation come from?” Granted that faculty who keep their doors closed may be more knowledgeable, there is not much doubt that Walter Chrysler is little known today. Such was his fame in the 1920s, though, that when the directors of the Maxwell Motor Corporation recruited him to be president, they renamed the company—and the cars it produced—after him.

The current obscurity of Walter Chrysler is partly due to the very limited access Chrysler has allowed to its company archives. There is, of course, a mountain of biographies of Henry Ford and histories of the Ford Motor Company based on the readily available material in the Ford archives in Dearborn. But Charles K. Hyde has written the first history of Chrysler based on archival material. (Walter Chrysler’s autobiography, *Life of an American Workman*, concentrates on his early life and devotes surprisingly little space to his years at Chrysler Corporation.) Hyde was hired by Chrysler in 1980 to document the Dodge factory in Hamtramck, Michigan, before it was consigned to the wrecking ball. Subsequently, the corporation gave him access to what he calls “the wealth of materials the company had saved.” Hyde has made good use of these materials to write a readable and comprehensive history of the company.

Hyde begins with the early life of Walter Chrysler, sketches the histories of the companies that eventually became the Chrysler Corporation, and carries the story all the way through the 1998 merger with Daimler Benz to form the current DaimlerChrysler Corporation. One theme of the book is that, more than Ford or General Motors, Chrysler’s successes and failures were attributable to the engineering of its cars. For instance, the introduction of the Plymouth in 1928 was an enormous success because the car had features such as hydraulic brakes and aluminum alloy pistons that were not available on competing cars. Largely because of the success of the Plymouth, Chrysler actually sold more cars in 1933, at the trough of the Great Depression, than it had in 1929.

Hyde sheds new light on a number of interesting episodes, including the Chrysler Airflow debacle, an incident not mentioned by Walter Chrysler in his autobiography. In 1934, Chrysler developed a new model that was both more aerodynamically sound and that gave a less bumpy ride by moving the rear passenger bench forward of the rear axle. Chrysler expected this model would allow them to pass General Motors in sales. Instead, the model’s
poor reliability and ugly front end—somewhat similar to Ford’s later Edsel model—made it such a flop that it almost sank the company.

For more recent times there was apparently little material available in the company archives, and Hyde relies primarily on articles from business magazines and the Detroit papers. What little archival material he uses in discussing events of the last four decades consists largely of semi-public documents such as transcripts of press conferences by Chrysler officials.

It is always possible, of course, to nitpick the interpretations of an author whose book covers so much ground. For instance, in the first few decades of the automobile industry only Ford gathered consistent and timely data on final sales. This left other companies vulnerable to accumulating large unsold inventories of finished automobiles. Hyde ignores the role that record-keeping played in the bankruptcy of the United States Motor Corporation—the forerunner of Maxwell, which became Chrysler—and the difficulties that Dodge Brothers encountered in 1927, which led to the sale of the company to Chrysler the following year.

Finally, like most books these days, this one could stand some copyediting. There is an annoying amount of repetition of small facts. At one point the reader is told four times in the space of five pages the exact date—2 November 1978—when Lee Iacocca was appointed president of Chrysler. There are a number of errors—for instance, Walter Chrysler’s age is wrong in the very first sentence of the book—that also might have been caught. But such quibbles aside, Hyde deserves substantial credit for filling a major hole in the business history of the United States.

ANTHONY PATRICK O’BRIEN, Lehigh University


Time and time again, when the U.S. stock market experiences a correction during which investors adjust expectations about future corporate earnings and dividend growth, a number of less-conventional economists take the opportunity to illustrate how, and with the benefit of perfect hindsight, share prices must have become “ruptured” from their fundamental values. Lawrence Evans’s monograph is no exception. After all, the time is ripe for investors in the face of large losses to become receptive to the idea of such a “disconnect.” Some researchers in the field now known as “behavioral” finance, and most notably Robert Shiller, have even argued the point persuasively enough to gain some degree of acceptance, though mostly in nonacademic circles.

Evans aims his pitch more deliberately at academic audiences than does Shiller, and at a glance the view that the 1980s and 1990s were somehow “different” than earlier eras in terms of underlying market conditions seems compelling. It is when Evans uses these differences in chapter 2 to suggest that the efficient markets hypothesis (EMH) is either wrong or no longer applicable to the equity market that things go astray. The EMH in its semi-strong form asserts that stock prices reflect all publicly available information about the future growth of dividends, meaning that the market gets things “right” in an ex-ante sense. But the author seems to interpret the hypothesis as requiring that stock prices be correct in an ex-post sense as well. Under this faulty premise, the sharp decline of stock prices in 2001 suggests that investors must have been acting irrationally.

The treatment in chapter 2 starts off balanced enough, with Evans offering lengthy critiques of the EMH and other theories that use fundamentals to explain stock price behavior in the 1980s and 1990s, including the “new economy” and “declining risk premium”
theories. But it soon becomes clear where the author stands on these alternatives. He reasons that, because the stock market fell, the idea that we are entering a new technological age of higher productivity must have been wrong. Yet informational technology has already had sweeping effects on the way that the world does business. And despite the presence of increasingly sophisticated institutional investors spreading risk across countries and time, Evans maintains that investors could not have lowered their required return enough to justify the valuations of the 1990s. All of this leads to a recommendation that the EMH be discarded in its current form and replaced with a new “supply and demand” theory of stock valuation that the author introduces in chapter 4.

Setting aside for the moment that standard economic theory also takes the notions of supply and demand as central to the formation of prices, including those in equity markets, it is disappointing that the new framework turns out not to be a “theory” at all, at least in the sense of having any analytical foundations. Rather it is a reduced form that includes three new arguments in an implicit function for equity prices: the volume of foreign portfolio inflows; the growth of mutual funds; and the supply of corporate equities. Evans inserts these arguments because evidence presented in chapters 5 and 6 shows that they have some predictive power for stock returns since 1982. The story is simple. Share repurchases in the 1980s and 1990s reduced the supply of equities available to investors, and a bubble formed as mutual funds and foreigners (but for some reason not pension funds) continued to pump funds into the market. When firms could not afford to keep up the repurchases, the bubble burst.

The new supply and demand “theory” is clearly outside of the classic Modigliani-Miller (MM) paradigm, where repurchases are just an alternative to dividends that shift the debt-to-equity ratio with no effect on market values. But the deviations from MM are quite different than the usual taxes or accounting for the possibility of financial distress. Indeed, imperfections in the new theory are driven by herding behavior among investors who either became more risk-loving in the 1990s, or did not discount the potential for financial distress rationally. The empirical evidence that is offered in support of the new framework, however, is unconvincing. Evans presents estimates from several vector autoregressive models and structural (i.e., ad hoc) specifications in chapters 5 and 6 that seem to show mutual fund growth Granger-causing stock returns, but the exercises are at best exploratory, possibly mis-specified, and largely oversold.

Maybe investors did act irrationally in the 1990s. Perhaps the downside risks of equity investments went unappreciated by market participants who discounted lessons of the past too deeply. Evans contends that rational economic agents should have seen the bubble and burst coming, but that real world investors continued to buy shares anyway. It does not necessarily follow, however, that investors did not process the information available to them before calling their brokers. It just means that their analysis turned out to be inaccurate. And a battery of regressions subject to omitted-variable bias, though interesting, will do little to convince the skeptic, or the mainstream economist for that matter, that the author has found the smoking gun that practitioners in behavioral finance seek. After reading Why the Bubble Burst, this reviewer emerged more convinced that conventional economics and its efficient markets hypothesis are alive and well.

PETER L. ROUSSEAU, Vanderbilt University

This book is the “story of the ideological war against communist and totalitarian forms of economic and political order” (pp. 1–2). It argues that not only did the west win a military and economic triumph over socialism; it also won an ideological triumph, and that this ideological war was consciously fought by a group of scholar warriors who, as part of the United States’ cold war strategic plan, sought to “rescue capitalist democracy from the threat of authoritarian socialism” (p. 2). According to Amadae, these scholars “fought with tenacity” and because of their work, the ideological war ended with almost total victory for the side of capitalist democracy. The weapon of choice of these scholar warriors was “rational choice theory” and this book is a “narrative account of the ascendancy of rational choice theory in a variety of fields” (p. 9). The success of the war means that rational choice theory serves “as a philosophic underpinning for American economic and political liberalism” (p. 9).

The book is divided into an introduction, which summarizes the argument; a prologue, which relates rational choice theory to J. Schumpeter, F. Hayek, and K. Popper; a four-part core book, which makes the central arguments; and an epilogue.

In the core book, Part 1, “Rational Policy Analysis and the National Security State,” Amadae describes the beginnings of formal rational choice theory with RAND Corporation at its center. (According to Amadae, “virtually all roads to rational choice lead from RAND” (p. 75).) Part 2, “Rational Choice Theory in American Social Science,” is a story of the various battalions of scholar warriors in the battle; it has separate chapters on K. Arrow’s social choice theory, J. Buchanan and G. Tullock’s public choice theory, and W. Riker’s positive political theory, all of whom Amadae sees as having “rebuilt the theoretical foundations of American capitalist democracy and defeated idealist, collectivist, and authoritarian social theories” (p. 13). Part 3, “Antecedents to Rational Choice Theory,” provides a broader context for the larger battle, and discusses how this ideological war fits with the tradition of liberalism. Part 4, “Rational Choice Liberalism Today,” places the discussion in the present context; it discusses J. Rawls, A. Sen, and younger scholars such as K. Binmore, who is expanding rational choice theory into a broader social, rather than just economic, theory.

As is obvious from the brief summary, this book covers an enormous amount of material and shows impressive erudition. But to me, the book is fundamentally unsatisfying for two reasons. The first is its description of the rational choice research program as a war. Amadae seems to be arguing that rational choice theory was a rationally chosen battle tool, whereas I see it as a research program that scholars followed for the same reasons they follow any research program—because it was interesting and potentially insightful, and because working on it offered them possibilities for articles and books that they needed to advance their careers. Although some of the researchers he discusses may have favored democratic capitalism as an ideology, the ones I know were not especially ideologically committed, but instead were willing to follow the analysis wherever it might lead. Scholars gravitated to RAND because that was where the money and interesting people were, not because organizers of RAND or the scholars had some grand design to construct a research program to protect democratic capitalism.

The second problem I have with the book is its assumed premise that rational choice theory is a central pillar in the support of democratic capitalism. In my view rational choice theory offers little support for democratic capitalism. But that does not matter because an ideology needs no formal theoretical support. Had social choice theory, public
choice theory, and positive political theory never developed, our economic and our political system would still be there, and would have as much, or as little, support as it now has.

Today, in the academic discussions in which I take part, formal rational choice theory of the type described by Amadae is waning, but public support of markets is waxing. Although the new work is based on the premise that rationality in some sense is a reasonable assumption, the type of super rationality that formed the basis of social choice theory is not seen as the most useful version. At best, one may still get some gain from the assumption by considering issues in an evolutionary game theory perspective.

This broader, less restrictive, perspective of rationality has been around for a long time, but Amadae specifically rules it out of his consideration. Thus, Amadae omits any discussion of Milton Friedman, who, among economists, was probably the person most concerned with saving democratic capitalism.

In short, for me, this book provides an unsatisfying narrative that attributes too much global logic both to researchers and to the system in driving the formal rational choice research program. Although it is true that a system will provide greater support for those research programs that support rather than challenge the system, the story of how that takes place is far subtler than the story told in this book.

DAVID COLANDER, Middlebury College


In recent years works by Andre Gunder Frank, R. Bin Wong, and Kenneth Pomeranz, among others, have placed Asian societies in a new historical light. This research contradicted the accepted views of an European exceptionalism. These historians pointed out how the invention of the steam engine enabled European nations to achieve standards of living comparable to those of China or Japan and to gain supremacy in the world economy.

Recent debates about the European role in the world have centered on the comparison of economic developments in Europe and Asia. In contrast, Geoffrey Gunn studies cultural relations in the Eurasian landmass. Instead of comparing economies, he considers the cultural exchanges between Asians and Europeans.

According to Gunn, the European imperialism of the nineteenth century was contingent upon the previous three centuries of cultural exchanges with Asia, “unlocking the knowledge and empowerments necessary for establishing commercial bridgeheads and future colonial empires” (p. 247). Before the nineteenth century, Dutch, British, French, Spanish, and Portuguese influences were confined to outposts of the Asian continent. Japanese, Indians, Chinese, Ottomans, and Persians dictated the terms of trade and the cultural impact of the Europeans was relatively minimal.

The cultural interactions of this 300-year period were made possible by the “Print Capitalism” ushered in by the Gutenberg Press, which made accessible new worlds to European publics. Post-Ptolemaic atlases, cartographic reproductions, and travel accounts were printed and had great impact on European voyages of exploration. For instance, in 1461, Aeneas Sylvius Piccolomini, Pope Pius II, in a geography book rejected the notion of a closed Indian ocean and supported the idea of rounding Africa to reach India. Ludovico Varthema in his Itinerario, printed in several European cities between 1508 and 1550, indicated the places where nutmeg and cloves were grown. Five years later the Portuguese Antonio de Abreu went to the Moluccas.
During the sixteenth century the Jesuits were the main providers of knowledge about Asian lands. In addition to missionary activities, the Jesuits participated in a two-way cultural exchange introducing European science and culture in general to Asian countries and sending back to Europe news about unknown regions. This was not an open exchange, as the missionaries censored secular ideas contrary to Catholic teachings. “Even so, the Jesuits played a key bridging role in interpreting the Orient for European audiences and seeking to change it through demonstrations of ‘superior’ cosmology” (p. 110). It is during the prominence of Iberian powers in Asia that in addition to the coveted species, tea, porcelain, lacquerware, and other merchandise reached markets in Europe and America. In spite of the prominence achieved by the Dutch in Asian waters in the seventeenth century and the arrival of other European powers, “the church canon in Asia remained canonical until breached by the scientific discoveries and knowledge gathered by the great Enlightenment-era discoverers” (p. 110). During the eighteenth century China was admired due to “industry, inventiveness, philosophical strengths, and cultural richness” (p. 146). Voltaire celebrated the Chinese system of government as an “enlightened despotism” that was criticized by European intellectual after the French Revolution as an odious Oriental despotism. Marx considered “Asian regions as not only despotic but also fossilized, incapable of progress and offering obstacles to capitalist development” (p. 167). European imperialism and negative views of Asian societies went hand in hand.

Geoffrey Gunn is right when he observes that without “schools, churches, presses, and missionary activities, the circuits of commerce and trade reaching from Europe to Asia could not have flourished”(p. 284). Nonetheless, the book’s central argument is highly problematic: “Our thesis of Eurasian cultural metamorphoses restores Asia to its rightful civilizational equivalence or even preeminence in an era before the rise of industrialism” (p. 284), in light of his observations about the state of science and technology in Asia. In China, according to Gunn, “intellectual involution appeared to be entrenched across the Confucian world.” The Ottomans lacked technological know-how and “only Tokugawa Japan came to emulate the spirit of Enlightenment inquiry”(p. 279). However, the Japanese, in scientific terms, were far behind Americans and Europeans when Commodore Perry arrived. The author does not address the reasons why the Japanese, the Chinese, and the Ottomans did not have the technological edge that he attributes to “Enlightenment advances in sciences and technology” (p. 279). It appears that in the Eurasian exchange, which is, according to Gunn, the crucial intellectual arena in world history, the Europeans were able to profit the most.

Many of the themes and ideas present in Gunn’s book have been addressed before in detail by historians such as Donald F. Lach, Joseph Needham, Charles Boxer, and Jonathan Spence. Lacking in the present volume, despite the diverse topics, regions, and time periods covered, is a coherent intellectual synthesis of the material. Nonetheless, as an introduction to the cultural exchanges in Eurasia, Gunn’s book is highly useful.

ARTURO GIRALDEZ, University of the Pacific


This is a medley of essays on the political economy of transatlantic relationships in the second half of the twentieth century, offered by a group of distinguished economists and economic historians as a festschrift for the 60 years of their German peer, Carl-Ludwig Holtfrerich. The *Yearbook* encompasses a variety of topics, among which a personal account of the rise of monetarism as a policy rule by Charles P. Kindleberger, a “wise and
A unifying theoretical background is provided by the analysis of twentieth-century monetary regimes through the “trilemma,” the policy trade-off suggesting that a viable international monetary regime has to give up at least one corner of an incompatible triad formed by fixed exchange rates (sacrificed in the early 1920s as well as in the post–Bretton Woods era), free capital mobility (renounced both in the 1930s and under Bretton Woods), and domestic monetary policy discretion (which governments gave up during the Gold Standard). As Harley emphasizes, however, both the Gold Standard and Bretton Woods collapsed under the mounting pressure of the subordinated monetary goal—increasing globalization and capital market integration in the case of the latter (p. 169). This is the focus of Sylla’s contribution, which challenges the standard geopolitical account of the breakdown of Bretton Woods by proposing an alternative story based on a reversed causality (p. 84). It was not, Sylla argues, the Cold War and its impact on U.S. fiscal and monetary policies in the second half of the 1960s that undermined the viability of the system; it was rather the unprecedented transnational thrust of U.S. banks, escaping from an overregulated domestic system—an issue addressed also by Randall S. Krosner in a paper on the economics and politics of branching deregulation in the United States (p. 217)—and following their multinational customers abroad, that sowed the seeds of the transition to the post–Bretton Woods regime.

Quite in the same vein, Richard Tilly surveys the rise of Euro currencies and international capital markets to suggest that deregulation, liberalization, and market-oriented institutional convergence, although triggered by the exogenous shock brought home by the United States, were basically a reaction to the restrictive regulation of financial institutions that prevailed after 1945 (p. 216). Indeed, Heiner Flassbeck suggests that political elites who gave in to such powerful market forces were actually misled by the promise of increased national sovereignty, in terms of insulation from international shocks, embedded in a flexible exchange regime. This, Flassbeck argues, was a fallacious “fiction” that spelt disaster for international trade and financial stability (p. 33). Marcello De Cecco also espouses a critical view of globalization in his learned historical excursus on the secular experience of Europe with capital controls. He warns against the destabilizing potential of massive short-term capital movements and cautions emerging countries against “following incautious advice” to adopt “prematurely” capital account convertibility (p. 65). He is nevertheless skeptical about the effectiveness of unilateral capital controls, and qualifies his open-minded approach to Tobin’s “sand-in-the-wheel” argument by stressing the higher degree of international cohesion that multilateral regulation would require.

The importance of international cooperation is also emphasized by Barry Eichengreen. He raises the question whether in fact the explosive growth of international capital flows of the late twentieth century made national economies alarmingly more vulnerable to international contagion, relative to past periods. After modeling the channels through which crises spread internationally, he empirically analyses the experience of 21 countries over the period 1880–1998, and finds strong similarities between the post-1971 and the pre-1940 period as to frequency, incidence, and determinants of financial crises. At the same time, the pre-1913 globalized system looms up as relatively stable, due to institutional characteristics that reinforced the credibility of national authorities’ commitment to currency stability, thus making globalization politically and socially viable (p. 102). This is a lesson from a distant past that, Eichengreen regrets, political elites of nowadays might have to learn again at high costs for the society.
Harold James expresses the same concerns as to the politics of de-globalization. Can redistributional conflicts or radicalized extremes, he asks, translate again into a backlash against internationalization similar to the interwar one? He suggests that the highly fragmented political spectrum within Western democracies and the absence of any strong national model of antiglobal success has so far prevented the pendulum from swinging back from globality too fast (p.144). Nonetheless, he warns, the emergence of new populisms based on “mild” protectionism against trade as well as capital and labor movements might well drive us into the same mistakes that in the 1920s prepared the ground for the catastrophe of the following decade.

STEFANO BATTILOSSI, Universidad Carlos III Madrid


This slim and eminently readable volume of essays provides an impressive coverage and update on recent research on the causes and course of the Great Depression of the 1930s. It builds on the substantial and authoritative scholarship already deployed by Barry Eichengreen and Peter Temin on the subject. It exhibits depth as well as breadth and offers a panorama of the “chain reactions” set in motion by the initial and concomitant deflationary shock in both core and periphery economies. Despite being obviously aimed at the professional economist or economics-literate historian with a taste for monetary and financial questions, the approach is very reader friendly so as not to discourage the nonspecialists (including this reviewer).

Although the editor has clearly given free rein to the contributors to recount the unfolding of the slump in their area of expertise, what is affectionately labeled by the editor the “ET (or Eichengreen-Temin) thesis” on the primacy of the deflationary transmission of the gold standard provides the fil rouge of the quest for the underlying causes of the slump which carried the West and the rest of the world into an unprecedented cataclysm. In an opening chapter Balderston offers a clear and welcome exposition of the “propagation mechanism” fostered by international monetary arrangements in setting off the worldwide depression. He complements it with a chronological recounting of policy decisions and new estimates on the distribution of gold money stocks before and in the course of the slump. In chapter 2, Pierre Siklos analyses the behavior of the Canadian economy during this period in relation to that of the United States. The immediate geographical proximity makes Canada a prime example for testing the transmission through interest rates and capital flows mechanism of the depression. After investigating the monetary and financial integration of the two economies prior to the shock, he scrutinizes the business cycle(s) and price movements to identify the primum mobile for the Canadian slump. In spite of institutional differences (no central bank until 1935) and the Canadian dollar’s peg on the pound, the two economies exhibit a strikingly similar behavior with regard to the slump’s timetable and subsequent recovery. Begging to differ on the centrality of monetary forces’ thesis, the author concludes that the identification of one single central cause should not detract from the investigation of additional determinants that triggered “such a cataclysmic event.” The next major player in the international gold standard system was of course France whose central bank, along with the Fed, powerfully contributed to deflation by increasing its gold assets at the turn of the 1930s. Pierre Villa (chapter 3) concentrates first on explaining why France entered the depression as late as it did. Using a portfolio model he subsequently shows that the same policy that helped stabilize the franc and foster gold inflows also accounts for France being spared the full blow of the depression until 1931. He reasserts the primacy of
deflationary policies, and hence of policy-makers’ ideology and representations, in triggering the slump. The unintentional combination of restrictive monetary and neutral fiscal policy proved “effective in fighting inflation and boosting growth” but reduced growth prospects in the long run. Villa emphasizes the lack of coordination between central bankers as a key element but exonerates French authorities for the retention of excessive gold reserves during the crucial monetary crunch.

Whereas ET stress that gold accumulation by creditor countries was the obvious sign of a profound credibility crisis, Kitson, reviewing the British case (chapter 4) tends to adopt an “absolutist” view according to which the rigidity of any fixed-exchange-rate regime would have brought about a worldwide recession “sooner or later.” With regard to the swift exit from gold by British authorities in September 1931, Kitson calls into question the strength of the “chain reaction” of monetary deflation to transmit the depression and adds the qualification that the effectiveness of the propagatory mechanism depended on the financial crisis being the “endogenous consequence of prior monetary contraction.” Although the devaluation of the pound contributed mightily to the domestic economic revival of 1932–1937, the British economy was not large enough for its “apostasy” to carry with it the world economy. Ritschl (chapter 5) pushes the case further and contends that the ET explanatory model minimizes the impact of “reparations politics” on capital flows to Germany and that reparations constituted the principal obstacle to the reconstitution of inter-central bank cooperation. In this view, of the “double whammy” taken by the German economy in 1931 (banking crisis in June, Brüning decrees in December), it was clearly the first shock which set the depression in motion: thus, the gold standard turned a foreign borrowing crisis spurred by the Young plan (1929) into a contraction of domestic money (and later of output). But the adoption of a floating rate regime would not have averted the consequent balance of payment crisis.

Both Balachandran’s chapter on India (chapter 6) and Singleton’s on New Zealand (chapter 7) offer fascinating insights in the unfolding of the depression in two periphery primary producing countries. The British government forced monetary cooperation on its empire and dominions; this translated into engineering a gold outflow from India to bolster British reserves which increased four-fold in 1932–1938. As the editor emphasizes, the collapse of the gold standard made the world more dependent on gold as a reserve asset, not less. But as elsewhere, the accompanying deflationary policies for defending the rupee’s peg to the pound resulted in lowering living standards, especially among India’s rural classes. In New Zealand by contrast, the “double-dip” deflation of 1930–1933 allowed a redistribution of income to farmers from their urban creditors and purveyors. Perhaps an additional chapter on one of those South American countries that defected from the gold standard early (Argentina or Brazil) would have illustrated more dramatically the damage endured by primary producing countries at the onset of the slump.

Thus the emergence of an excess global demand for gold seems critical to the effectiveness of the propagation mechanism. The “run for gold” was consequential upon U.S. and French “excess” accumulation soon emulated by a host of core countries but supported by a general distrust in the ultimate sustainability of the system. Even the Soviet Union seems not to have been immune from this fatal bent. Gregory and Sailors offer an in-depth examination of the Soviet economy’s fundamentals in the interwar (chapter 8). Thanks to the former’s unrivaled expertise on Russian and Soviet national accounts, the authors show that GDP, which doubled within a decade, did indeed outpace capitalist countries’ performance. Growth was home-grown, maintained as it was by buoyant investment financed by taxation of the urban sector. Insulated from Western economies, the Soviet economy embraced autarky, a set of policies that amounted in fact to pushing the mercantilist precepts adopted in the West to their limits.

As a conclusion, Eichengreen and Temin present a particularly concise and illuminating “afterword” in which they reformulate their scenario for the international transmission of
the depression and examine three complementary counterfactuals, which strengthen their case. In their view the responsibility for pulling the world into a deep and lasting depression lies firmly with the operation of a gold standard in which the adjustment procedure was deflation rather than devaluation. They do not deny that the initial downturns could have had “independent roots” but the choice of deflation and the inability of governments, made more dependent on their electorate since World War I, to implement it fully, explains the ensuing tightening of credit, profit squeeze, and eventual contraction of business activity as well as the rapid erosion of investors’ confidence. What made deflationary policy such a lethal weapon under the circumstances was the “hegemony” of the gold standard mentalité among central bankers and their political masters. With regard to the German case, they reassert their belief that even without a banking crisis spurred by the dispute over reparations, any improvement would have been “minor.” The implications of fixed-exchange-rate regimes the authors have expounded elsewhere; here they conclude by stressing the far-reaching consequence of poor policy choices and the absence of effective international adjustment mechanisms. After unduly blaming the Fed’s incompetence, it is perhaps time to take on French authorities. After all it was their dogged insistence on fantastic reparations and their (self-destructive) obsession of a gold-backed “franc fort” that hampered the reconstitution of mutually guaranteed foreign exchange reserves.

JEAN-PIERRE DORMOIS, Université Marc-Bloch, Strasbourg
NOTES