Book Reviews

ANCIENT AND MEDIEVAL


Unlike most modern multi-authored historical surveys, this work is not mere synthesis, but in many of its 51 chapters (by 39 contributors, many highly distinguished) breaks new ground or suggests new interpretations of known data. It is, moreover, the first attempt to discuss in any substantial form the various economic facets of the Byzantine Empire. Angeliki Laiou, a sometime Director of Dumbarton Oaks, is to be congratulated on master-minding a project whose importance can hardly be exaggerated.

Although the Roman Empire was ruled from Constantinople as early as A.D. 324, the chronological span of these volumes begins in the seventh century both because it was only then that most of the distinctive features of the mediaeval empire were in place and also because the “late antique period” has been covered in great detail by A. H. M. Jones’s magisterial *The Late Roman Empire, 284–602: A Social, Economic and Administrative Survey* (2 vols., Oxford, 1964). Sensibly, however, two chapters deal with the sixth century, one discussing evidence unknown to Jones and more recent interpretations, the other being a case-study (a useful and repeated aspect of the whole work) of an individual city, Anemourion.

After two introductory chapters the work is divided into six parts. The first concerns “The Natural Environment, Resources, Communications, and Production Techniques” (nine chapters), and includes a brave attempt by Laiou at once more estimating the population of the empire at different periods, while the techniques discussed are those of agriculture, mining, metallurgy, stone-working, and textiles. The second is that on the sixth century. The third, “Structures, Organization, and Development of Production” (21 chapters), includes, among more obvious subjects, further information on techniques in artistic media and the production of books, glass, and pottery (unfortunately appearing too late for consideration are the important works by Sharon E. J. Gerstel and Julie A. Lauffenburger, eds., *A Lost Art Rediscovered: The Architectural Ceramics of Byzantium* [Baltimore and University Park, PA: Walters Art Gallery and Pennsylvania State University Press, 2001] and Ken Dark, *Byzantine Pottery* [Stroud: Tempus, 2001]). This part includes also valuable case-studies of Sardis, Pergamon, Thebes, Athens, Corinth, Kherson, Preslav, and Turnovo. The fourth part, “Exchange, Trade, and Markets” (seven chapters), includes a valuable study with many tables by Cécile Morrisson and Jean-Claude Cheynet on prices and wages and the special topics of Monemvasia and Shipwrecks. The fifth, “Economic Institutions and the State,” consists of chapters on the production and circulation of money, monetary mechanisms, an especially important study by the late Nicolas Oikonomides on the role of the state in the economy, and five (three by Eleutheria Papagianni) on legal aspects of the economy. Part 6 consists solely of two chapters by Laiou, on “Economic Thought and Ideology” and an all too brief overview. The work is lavishly supplemented by 129 black-and-white illustrations and 32 in color (generally quite excellent), including 37 of maps, a few of which are rather bizarrely colored and for which there is no index, as there is not for the numerous tables or for modern scholars mentioned. The two indexes provided are “Thematic” (very useful indeed) and “General” (selective and mainly of personal and topographic names and Greek terms). There is no general bibliography, but many chapters conclude with their own, while for others readers are left to hunt through the notes.

The length of this review precludes any detailed criticism, but this reviewer thinks that a few points should be made. He would have liked a more detailed examination of the economic and political effects on Byzantium of the Italian mercantile states’ increasing hold
on Mediterranean trade in later centuries. He was surprised also that the economic effects of slavery are largely ignored. Even though after the sixth century Byzantium was not as dependent upon slavery as in late antiquity (see the role of slavery in A. H. M. Jones’s work), yet there were clearly still large numbers in the middle Byzantine period, which cannot but have had economic implications. However, apart from a detailed table on the purchase prices of slaves from the tenth to the fifteenth century they are vouchsafed only six usually cursory mentions. He believes that an essay trying to estimate levels of literacy and numeracy, admittedly a notoriously difficult task, would have made an important contribution to certain economic considerations. He thinks also that insufficient notice was taken of Eastern European scholarship such as that of Alexander Kazhdan before his move to the West. M. F. Hendy’s *Studies in the Byzantine Monetary Economy c. 300–1450* (Cambridge, 1985), signal mention by Laiou and the nearest previous approach to a synthesis of the Byzantine economy, also retains its importance and is indeed an essential adjunct to the present work.

The *Economic History of Byzantium* will remain for many years an indispensable source of reference for all interested in Byzantium and, owing to its frequent comparisons with western Europe and references to the Islamic world, in mediaeval economies in general. It should, moreover, have an even wider audience, as Byzantium is an early and fascinating case of the government of a large state attempting to direct and control many aspects of its own economy. But the work is more than merely a source of reference: its new interpretations will provoke further study, and its admissions of ignorance will give young scholars a plethora of virgin subjects to pursue (notable in this respect is Anthony Bryer’s “The Means of Agricultural Production: Muscle and Tools,” the first part of which is largely a list of questions with suggestions of where to look for the answers).

A. R. LITTLEWOOD, University of Western Ontario

**MODERN EUROPE**


The *Crédit Lyonnais* (C.L.) suffered serious difficulties in the 1990s, so that *The Economist* renamed it Discredit Lyonnais. However, c.1900, it had been the biggest bank in the world, and in 1981 it came second, based on deposits, behind the Bank of America. This 1,020-page volume is thus not out of proportion with the C.L. role in the banking world. Moreover, the C.L. has preserved rich records and has been very liberal in opening them to historians; eventually, in 1991, it established a department of historical archives. As a consequence, starting with the late Jean Bouvier and his pioneer work (*Le Credit Lyonnais de 1863 à 1882*. Paris: SEVPEN, 1961), many scholars have recently worked on the history of the C.L., which has indeed been the most studied among French banks.

The editors of this book stress that it is neither a history of the bank (two adequate works are available), nor the product of a collective project about one single theme; they have wanted to gather studies by persons—both well-known historians and young scholars—who have recently worked in the C.L. archives and in other primary sources, and who have something new to say about the bank history. Such a collection might have been a hodgepodge of disjointed papers, but the 41 essays are arranged in an orderly and rational manner, around five major centers of interest, and each of the five parts starts with a synthetic survey by one of the editors. One must add that some of the papers are interviews with six former
top executives of the bank and two union leaders. The editors admit that those testimonies are somewhat biased; they are nonetheless interesting, for instance, the long interview (over 60 pages) with Jean Deflassieux, who was president from 1982 to 1986. Moreover, this book covers a longer period than most histories of French banks; still, the editors frankly state that they have thought it impossible to deal with the disasters which the C.L. met with in recent years, inasmuch as this crisis is still sub judice.

Part 1 of the book deals with the bank as a business firm, i.e., with its organization, management, and personnel (the first female permanent employee was recruited in 1884). It includes a good biography of Henri Germain, the bank’s founder and first president; and an outstanding paper on the C.L. department of financial studies (established as early as 1871), in which Marc Flandreau challenges current views on the pre-1914 gold standard ethos. Part 2 is devoted to the role of the C.L. in the financing of the French economy; the problem is disaggregated, by considering it from the point of view of different industrial branches (railroads, automobile, aircraft, and electricity). It emerges that the bank’s strategy, for most of its history, was close to the English model, but that, both in its early and in its recent years, it rather followed the German model of mixed banking. Anyhow, criticism of the C.L. as Althusian which was popular in an earlier generation of Marxism-influenced writers, is dismissed by Michel Lescure (pp. 364–66).

Part 3 is of special interest: the C.L. as a major international bank. Very early, it opened branches abroad (London, 1870) and it became involved in financing international trade and investment. The activities of the C.L. in London, in Geneva, in Spain and Portugal, in the Ottoman Empire and Egypt, and in Norway, are analyzed by experts such as Youssef Cassis and Samir Saul. With the exception of London, the heart of the world financial system, where the C.L. had to be present (despite the hostility of the City establishment), the bank did not operate in the most advanced countries (a branch was opened in New York in 1879 and closed in 1881). Those foreign activities were, of course, dependent upon the international environment, including political disturbances: the Egyptian branches, which had long been important, had to close in 1956; the bank then turned towards Brazil and other Latin American countries. Eric Bussière has written on C.L.’s European strategy from the 1950s to the 1970s.

The C.L. in its environment is the theme of Part 4; it deals with relations and competition between the C.L. and other major French and European banks. Youssef Cassis concludes (p. 723) that there was no victor in the war of the systems, i.e., between mixed and deposit banking. Relations with the state and public opinion on the C.L. (some newspapers had built up a “black legend” about it) are also considered.

Finally, Part 5 deals with short-term problems, i.e., the difficulties the C.L. encountered because of economic crises and world wars. It includes a long, fascinating, and remarkably objective paper by Alain Plessis and Philippe Verheyde about the C.L. under German occupation. The bank management showed neither sympathy for the Nazis nor interest in collaboration; but it operated under heavy constraints and its overwhelming purpose was the bank’s survival; so it enforced, some times to the letter—though with no zeal—German and Vichy orders, especially as regards its Jewish customers. One consequence of World War II for the C.L. was its nationalization; one can regret that this upheaval and its effects—which Plessis describes as rather negative (pp. 848–49)—have not been given more space.

In their foreword, the editors recognized that the depth of analysis was not uniform throughout the 41 chapters (p. 8). However, a large majority of them are of high quality, and the mass of new data and ideas which this book contains is remarkable. Moreover, it displays an interesting change in French banking history, from a macro to a micro (and deideologized) approach. This has enabled Plessis to define the “cultural identity” of the C.L., thanks to which it overcame crises and wars for 140 years. It had been inclined towards entente rather than competition, it was eager to collect information, its strategy was
rather cautious. When it deviated from this culture, which Plessis sees as representative of the French banking system and as rather efficient, it courted disaster.

FRANÇOIS CROUZET, Sorbonne, Paris, Emeritus


Paul V. Dutton’s narrative of the evolution of French social policy in the interwar years is a pioneering contribution. Subsequent studies will doubtless supplement and even substantially revise his account but they will all be in his debt. However much one might disagree with his ordering or omission of causal factors, this book must be read by anyone interested in the development of twentieth-century French social policy.

Dutton’s analysis of the welfare-promoting forces in early post–World War I France is succinct; first came the desire to reward the troops and their families: second, the fear of workers’ revolution; and third, the need to integrate the German-inspired social insurance system in a reacquired Alsace Lorraine with that of the rest of France. Dutton also rightly focuses on the existence of a pronatalist political consensus as an important force in French social policy. In a notably fractious society, pronatalism won support from almost every important political actor, including Communists and feminists.

In a short book, Dutton cannot be expected to detail every factor in the formation of French social policy. His list of explanatory factors is heavily weighted in favor of class relations—certainly a plausible orientation in a heavily class-divided society. He stresses industrialists’ attraction to a low-cost “salaire vital” providing benefits to relatively rare large families and avoiding a more-expensive “family wage” based on the male household head. Although the book claims to deal with the period 1914–1947, its real focus is the interwar period. The better-analyzed post–World War II years are treated summarily and interested readers should go elsewhere.

Although making important points, the book sometimes lacks balance. While helping to explain the familial orientation of French social policy, Dutton largely ignores its other peculiarities, such as its slow adoption of unemployment insurance. He focuses on some key actors in the construction of national social policy, for example, the Comité Central des Allocations Familiales (CCAF), the employers’ association that developed its own network of private firms offering pronatalist incentives, and the Fédération nationale de la Mutualité Française (FNMF), the federation of mutual aid societies that exerted enormous political influence. Still, he ignores other powerful political actors, such as regional welfare institutions. Almost no attention is paid to regional private and public institutions, such as the great Hospices Civils de Lyons, which provided aid to large numbers of people. Such institutions were critical to educating the public about the possibilities of social policy. Purely political factors are also given short shrift. The extraordinary role of the center-right in shaping the French welfare state is little analyzed.

Dutton does important work in providing an overall summary and analysis of interwar legislation. He explores the growth of the French welfare state in the interwar years from Edouard Grinda’s landmark parliamentary report of January 1924, with its support for compulsory contribution and the coverage of major health risks, to the passage of social insurance legislation in 1928 and 1930, and the Family Code of 1939. He shows how the Popular Front transformed the character of family allowances by putting them on the bargaining table in 1936 and making them subject to arbitration in 1937. Government subsidy of agricultural participation in social insurance broadened the plan considerably but offered peasants more favorable access to insurance. The significant role of right-wing rural demagogues in expanding the system to the countryside is cogently explained.
The least successful elements of Dutton’s study are his efforts to provide a comparative context for his study or to demonstrate its contemporary relevance. The theme of the French welfare state as success story is inadequately explored. By the 1980s, he tells us, the once laggard welfare state had become one of “remarkable generosity and durability,” offering benefits of “almost Scandinavian proportions.” Unfortunately, Dutton’s study concludes decades before even the most charitable could consider France as a paragon of social policy. Were the successes of the 1980s implicit in the advances of the 1930s? Were there unique characteristics of French policies established in the interwar years that enabled it to endure the stormy 1980s? Does pronatalism retain its hold on French public opinion? Dutton briefly concludes that the French welfare state survived because it “demonstrated close continuities with the nation’s past, its culture, and popular perceptions of national crisis.” Without considerably more elaboration, this is perilously close to tautology with more than a nod to presentism.

Dutton’s effort to place the French welfare state in comparative context is thoroughly admirable. Instead of trying to address issues of contemporary relevance, however, it might have been much more useful to have placed it in interwar comparative perspective. Why was the French center right so much more amenable to welfare-state reform than the British and American? What were mutual aid societies so much better able to defend their position in France than in the United States or the United Kingdom? Why did France fail to develop unemployment policies during an era in which unemployment was the leading social problem. Although Dutton does not address these issues, his book will certainly be of great value for those who will address them.

MICHAEL HANAGAN, Vassar College


As late as 1948 the leader of the Lancashire cotton spinners’ union, echoing the sentiments of many of the leading actors in the industry, remained unconvinced that organizational and technical change was inevitable. “He knew of one firm with mule spinning machinery dated 1878 and he challenged anybody to deny that it was running quality goods in quantity better than any machinery used today” (p. 191). Although successive generations of economic historians have tackled directly the veracity of this claim, Alan Fowler brings—refreshingly—a social historian’s perspective to the issue. Fowler argues that Lancashire’s success in postponing or pushing back the inevitable was not only the result of a specific technology (indeed Fowler observes that the spinning mule reached its limits of adaptability about 1900), but also the product of social and political institutions and a culture of leisure built around the mill floor. Thus, while economic historians have focused at times narrowly on the substitutability of raw cotton and the range of counts spun, Fowler finds flexibility in the wider collective bargaining system, in the relation between the state and the industry, as well as in the scheduling of family and work responsibilities. It was a holistic system with many margins of adjustment that buttressed workers’ ability to withstand cyclical pressure of wage cuts, as well as layoffs and industry downsizing. Such claims are not entirely novel, but Fowler succeeds because of his thoroughness in marshaling a wide variety of sources and anecdotes from secondary and primary sources, including trade union archives, the Cotton Factory Times—Lancashire’s family newspaper—and life histories.

The book is organized on a thematic basis with 1900 as much the start as the midpoint of the story. Fowler begins with an overview of the rise of the cotton textile industry and
its regional and organizational specialization between spinning and weaving. He then gives a brief history of the three main unions in the industry, representing spinners, weavers, and carders, before describing the intricacies of the piece-rate lists that were the backbone of the collective bargaining system. Workers’ experiences across sectors and regions were not uniform and the industry had a high degree of occupational segregation by sex. Although eminently Victorian, Lancashire was unique in the high proportion of women at work. Life outside work complemented that within factory walls. Communities developed a range of social practices and leisure attractions, from the music hall to cooperative libraries, giving comfort and pride to Lancashire workers and families. They also provided an escape from crowded homes. Safeguarding these institutions and arrangements, pressure groups campaigned for economic and political reforms, although Fowler asserts that Lancashire would have had more clout in London if it had been as actively engaged as the coal mining districts. Many elements of the industrial relations system and community networks were well in place by 1900, if not 1870, thus providing the foundation stone of Lancashire’s long nineteenth century that lasted until 1958, when Britain’s imports of cotton cloth exceeded exports for the first time since the early years of the industrial revolution.

As often is the case among labor historians, the method of analysis presumes a concept of class formation as set out by E. P. Thompson. Workers were made by their own experience, and in the “process of that struggle that a solidarity and a collective identity was forged” (p. 208). The role of employers in shaping this identity is minimized—the debate on paternalism is bypassed—and the state’s identity is passive, meeting competing demands on a first-come-first-served basis. Moreover, Fowler does not bring an international or comparative perspective to many of the issues he treats. Does Fowler miss other readings of his evidence? One is struck by the role of individual families as the basic decision-making unit. In this regard, the evidence fits well with Trevor Griffith’s challenge to labor historians’ common use of class in his recent book, *The Lancashire Working Classes: c. 1880–1930* (Oxford: Oxford University Press, 2001).

That said, Fowler’s study contains many more rewards than drawbacks. The compelling evocation of family life and the heartfelt commentary on how cotton operatives dealt with health and safety issues are just some of the highlights. All told, economic historians will need to consult this book because it provides a coherent alternative explanation to conventional views of Lancashire’s durability.

MICHAEL HUBERMAN, Université de Montréal


*The Rise of Commercial Empires* is a big book. The focus lies in detailing how England’s “increasingly coherent mercantilist objectives succeeded in undermining Dutch commercial hegemony, in ways which contributed to the restructuring of the North Sea staplemarket system” (p. xiii). In order to document this assertion, the book is divided into three separate parts. It opens with a discussion of the nature of markets. In the main, the focus here lies not with classical or neoclassical exchange but rather with a consideration of a Wallerstein world systems view contrasted to a more regionally oriented Braudelian approach. The difference in construct between an exchange dominated in the main by rules of mutual gain and that of an exchange dominated by inequality in power creates an underlying tension that emerges throughout the book.

The central body of the book examines particular English–Dutch trades. These are the woolen, linen, grain, coal and energy trade, and the re-export trade. Each of these trades is
discussed in great detail wherein David Ormond looks at the nature of the trade in England and in the Netherlands. He examines the role played by the various pieces of legislation passed, the role of war and the role of peace. In each case, he also examines the nature of the commercial organization of the particular trade. Undoubtedly, it is very difficult to provide both an extensive survey and a synthesis of a huge body of work in the space of a single chapter. But each of these chapters provides a good starting point for anyone interested in any one of these trades. At the same time, each of these chapters raises many questions that could be the basis of further research. One question that seems to emerge in each of the commodity-based trades has to do with the nature of technology transfer; when, where and how does it occur. Dr. Ormond documents clearly that these are integrated regional trades but also describes situations where technology differs.

The final section tries to come to grips with the forces generating English expansion and Dutch decline. The main foci here are the roles of war and of protection. Undoubtedly some number of the changes that occur both at the sectoral level and at the national level are the result of the wars that occurred during the time period. These wars had an effect both by closing down regions to trade and also by increasing the costs of trade as a result of the higher levels of protection needed. The point being made in the various chapters is that war affected England and The Netherlands differentially and Ormond discusses how each of the trades responded to these shocks.

There is also an extensive discussion of the various Navigation Acts as they applied to each of the linens, grain, coal, and woolen trades. Here Ormond tends to take the various pieces of legislation as given, in the sense that there is little by way of a political-economy discussion. He then examines how the trades in both England and the Dutch economy were affected. In some ways, this is perhaps too big a topic for a mere chapter, but it does provide anyone interested in the subject with a very thorough overview. More problematic is the issue of the Navigation Acts themselves and their impact on overall economic growth Ormand talks about these acts as a national economic development strategy, where “[M]ercantilist practice was clear in distinguishing beneficial from damaging trades, and in discerning manufacturing interests worthy of protection from those less deserving” (p. 321). Any government’s ability to do this must surely be questioned. Such a discussion of the political-economy dimension is missing.

There is also an underlying tension in the book between an argument that would suggest a loss of leadership by the Dutch or a situation of economic decline. Are we looking at a relative or an absolute change? It is never clear which position is being taken by Ormond. The impact of this is to leave the reader somewhat uncertain and perhaps even at times a little confused as to the exact story being told. This matters because the underlying question is whether there was a loss of leadership on the part of the Dutch economy or whether there was an overall loss of competitiveness. Of course, these two issues are related. But as we have seen over the longer sweep of history with British dominance in the nineteenth century and American economic dominance in the twentieth, relative position can change without an absolute decline in the economy formerly in first place.

ANN M. CARLOS, University of Colorado


In this revised version of a Cambridge Ph.D. dissertation, Richard Toye traces the vicissitudes of economic planning in the Labour party between the collapse of the second minority Government in August 1931 and the end of the first majority Government in 1951. Throughout, the emphasis is on the relationship between ideas and the Labour party, but
after the formation of the Coalition Government in 1940 there is inevitably a concern with the formulation and execution of government policies where the webs of influence become even more complex. As one might expect, the book is firmly grounded in the secondary literature as well as a wealth of archival research in the usual sources. However, in the post-1945 discussion, there is the introduction and extensive use of a new source, the diaries of R. W. B. (“Otto”) Clarke, a brilliant and opinionated Treasury Under-Secretary.

The main theme of the book is that, despite all the talk and writing about planning in the 1930s and early 1940s, Labour came to power in 1945 without having fully thought through, much less agreed on, the implications of what it meant to plan. As a result, despite its 1945 public commitments and its claims to have undertaken some planning during 1945–1951, the reality of practice fell rather short of its aspirations.

The main weakness of the book lies in its narrowness. Economic planning was only one of the departures of Labour from past practice. Other departures, both related to planning, included widespread nationalization and macroeconomic management. These were similar to economic planning in that although much had been written about them before 1945 relatively little concrete thought had been given to what they necessarily meant on the ground. In the case of macroeconomic management, given the state of the empirical side of the subject, the learning process was prolonged, but learning there was. The case of nationalization had more parallels with the planning of which nationalized industries were to be an important part. There the would-be nationalizers had not fully thought through what nationalization was supposed to achieve. As a result, as Sir Norman Chester’s volume *The Nationalisation of British Industry, 1945–51* (London: Her Majesty’s Stationary Office, 1975) made clear, Labour did not provide the newly nationalized industries with a governing structure that might reflect its aspirations. (There are, of course similar, parallel problems in the regulatory environment that accompanied the Tories’ program of denationalization after 1979.) In other words, Labour did not follow Maynard Keynes’s 1944 injunction:

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\text{I should have supposed that the whole purpose of nationalisation is to allow considerations of general and social advantage to take their proper place in price policy. . . . [A]n industry should not be nationalised if it ought to be run largely on commercial considerations. Ergo if it is nationalised, it should not be enjoined to pretend that a social purpose is out of bounds. . . . (Collected Writings of John Maynard Keynes (ed. D. E. Moggridge), 22, London: Macmillan 1978, 465–67)}
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There is also a certain patchiness in the coverage of particular subjects. For example, the discussion of the “socialist calculation debate” is truncated with the impression created that it ended in 1938 rather than with Friedrich Hayek’s “Socialist Economic Planning: The Competitive Solution,” *Economica*, May 1940, but this may have been the case among members of the Labour Party. Again in the discussion of post-1945 external economic policy there are small, irritating muddles in the details at almost every stage of the treatment of the evolution of the Article 7 discussions, the loan negotiations, and the development of the Bretton Woods institutions through to 1951.

Although these quibbles reflect this reader’s disappointment that what was obviously a very very good dissertation was not more thoroughly revised as it became a book, readers of this review should recognize that this is an important contribution to the literature on the Labour Government of 1945–1951.

D. E. MOGGRIDGE, University of Toronto
Book Reviews

ASIA


There is no question that the rise of a modern indigenous Chinese banking sector has been a neglected area in the literature on China’s economic development over the first half of the twentieth century. There have been studies of the traditional Chinese banks, work has been carried out on foreign banks (notably HKSBC) and the political-economy aspects of the relationship between the Nationalist Government and the financial sector has been analyzed, but the unavailability of primary source material has precluded the detailed analysis of the development of the modern Chinese banks. Access to the records of the nine principle banks (including the Bank of China) has now enabled Linsun Cheng to produce a wide-ranging and meticulously researched account which not only provides important information about the banks but challenges a number of established views about their operations, role, and significance.

In essence the book makes three points. The first is that the new banks quickly broke the late-nineteenth-century dominance of the “Three Kingdoms”—foreign banks, piaohao (Shanxi banks), and qianzhuang (native banks)—each of which occupied about a third of the market in terms of capital power in 1894. By 1925 the piaohao had been squeezed out and by 1936 the Chinese modern banks dominated the financial scene, accounting for over 75 percent of capital power. Secondly, this rise did not derive from their close relationship with the government and in particular it did not result from handling, or speculating in, government bonds. Prior to 1927 holdings of government bonds did little more than keep pace with the growth of business. They never exceeded 10 percent of the level of loans or deposits and remained below 5 percent of total assets. During the subsequent Nanjing Decade holdings increased more rapidly, but still never exceeded 13.2 percent of the value of loans and always remained below 8 percent of total assets. Involvement in bond holdings simply came too late to account for the rise of the banks and never came to dominate their business—or their profits. Instead, and this is the book’s third theme, their development can be accounted for by the ability to capture, generate, and profit from normal commercial business. This, in turn, stemmed not just from the maintenance of a prudent, essentially conservative, financial strategy but from a series of important financial innovations and the introduction of a number of managerial developments, some borrowed from the West others drawing on traditional Chinese practices.

Cheng demonstrates in detail how the banks evolved new approaches to generating deposits, improved customer services, put loans on a firm collateral (rather than personal) basis, developed a credit investigation service, overcame obstacles to the acceptance and circulation of Chinese banknotes, developed a sound reserves strategy and initiated an effective staff recruitment, training, and welfare policy. In the subsequent examination of the careers of the chief executive officers the portrayal is one of quintessentially innovative Schumpeterian entrepreneurs dedicated to their task of creating a viable modern banking sector and eschewing the acquisition of personal fortunes. Their longevity of tenure and close regional links gave an underlying stability, and they forced the modernization and professionalization of the sector through a combination of Western techniques and Chinese traditions.

The contrast here with Thomas’s recent study of the Shanghai stock exchange is instructive. Whilst together they present a picture of Chinese entrepreneurial dynamism, innovation, and enterprise in the face of highly unstable markets, Cheng’s portrayal of the banks reveals an ability (at least on the part of the principle banks) to rise above rather than cater for, or suc-
cumb to, the speculative element in the market. Together they undoubtedly played integral parts in fostering Shanghai’s emerging commercial culture—the so called Nanjing Road phenomenon—though on the arguments presented here the banks were consciously seeking to play a national role rather than being driven by local stimuli. Cheng’s archive-based research has considerably forwarded our understanding not just of the scope and dynamics of the development of the Chinese banks but of their relationship with both the government and the economy as a whole. The analysis complements, qualifies, and extends our current thinking and will provide the indispensable reference point for anyone seeking material on Chinese financial markets in the precommunist period. The book will also act as a stimulus basis for further research into such topics as the extension of modern banking practices into the provinces or a deeper analysis of the bank’s client base.

PHILIP RICHARDSON, University of Bristol


This book by Parks Coble, one of the West’s most thoughtful and productive historians of modern China, builds on insights from his two previous books, The Shanghai Capitalists and the Nationalist Government of China, 1927–1937 (Cambridge, MA: Harvard East Asian Monographs, 1986) and Facing Japan: Chinese Politics and Japanese Imperialism, 1931–1937 (Cambridge, MA: Harvard East Asian Monographs, 1991), and examines the fate of China’s capitalists under Japanese occupation during the war. It is in two parts. First, three chapters deal chronologically with Japanese policy towards Chinese industry and capital. Then a series of illuminating case studies analyze the experience of China’s most important capitalist family, the Rongs, of other textile and consumer-goods capitalists, and of those involved in the chemical and rubber industries.

Throughout the book, Coble develops two major themes. First he asks how far China’s capitalists during the war conformed to the model of “patriotic capitalists,” which is now the dominant narrative in China, or whether they rather should be seen as collaborators. He finds little evidence that nationalism was the dominant motivation for their actions. Where they did act patriotically, he interprets this as showing a lack of alternatives: those who best fitted the “patriotic capitalists” model were those in the chemical industry, which was at greater risk of Japanese control and expropriation. On the other hand, he questions (pp. 208–09) the trope of collaboration, arguing that its negative connotations originate largely in the active and ideological complicity of the Vichy regime in the holocaust. Although the dichotomy between collaboration and resistance is as strong in much Chinese nationalist writing as it is in Europe, he argues that the driving force of the capitalists was not ideology—whether of nationalism or of fascist collaboration—but rather the imperative to preserve their businesses. Where collaboration aided that end (as it tended to do towards the end of the war) then that was the path chosen. The reason that capitalists on the whole were reluctant to collaborate was the poor offer made to them by the Japanese, especially earlier in the war. On the other hand, cooperation with the Nationalists could also threaten their businesses, as the “bureaucratic capitalists,” particularly the Song family, increasingly encroached on independent capitalist enterprises.

Coble’s second concern (which will probably be of particular interest to readers of this JOURNAL) is to ask how the business culture of Chinese capitalism influenced the fate of the capitalists during the war and how the war in turn influenced that business culture. He identifies (p. 107) two key features of Chinese business culture: the dominance of the family firm and the widespread use of personal and informal networks rather than formal struc-
tures. The book focuses mainly on the family firm and shows convincingly how this influenced the response of Chinese capitalists to the Japanese invasion. For example, the family organization of most firms allowed a flexible strategic response to a complex and changing situation. Economically it facilitated the transfer of funds between different parts of an enterprise group, though this sometimes allowed over expansion of the group with insufficient account taken of the real cost of capital. In the sphere of politics, it allowed families to hedge their bets: Liu Hongsheng, China’s “match king” himself eventually joined the Nationalists in Chongqing, but left a brother behind to work with the Japanese in Shanghai, while a son took care of the family enterprises in Hong Kong. However, the family firm in China, as elsewhere, was vulnerable to drift and internal division after the death of the founder or dominant figure: the clearest example was the weakening of the Rong group after Rong Zongjing’s death in 1938.

The use of personal connections resulted from the need for protection against a predatory or ineffective state, such as was the case in different ways under the warlords, the Nationalists, the Japanese, the Communists, and even the British in Hong Kong. During the war, China’s leading chemical industrialist had to play on his political connections in an attempt to revive his enterprises in Western China.

The book is less successful in arguing that the war strengthened these features of Chinese capitalism and influenced the way Chinese capitalists operated after the war, both in Hong Kong and overseas. The author’s hypotheses in this regard must inevitably remain largely speculation. Confirming or refuting them would require a much wider study of Chinese capitalism outside China since the Second World War, and that is not the focus of this book.

In all, this book is an important study of major issues in modern Chinese history. It will probably be of more interest to political than to economic historians, but it will be required reading for anyone working on mid-twentieth-century China.

TIM WRIGHT, University of Sheffield

UNITED STATES AND CANADA


A History of Small Business in America grew out Mansel Blackford’s concern that historians had underestimated the importance of small firms in American economic and social life. The first edition (New York: Twayne Publishers, 1991) came out at a time when big business had faltered and some scholars had pinned America’s economic revitalization on small firms. While small firms have not proven to be the engines of growth and innovation their champions had predicted—Blackford admits as much in the second edition—they remain vital components of a rich and diverse national economy and, therefore, deserve scrutiny. Blackford skillfully documents the changing fortunes of small business in the United States over the past two centuries. He explores the strategies small firms employed to remain competitive in an economy increasingly dominated by large firms, evaluates the impact of government policies on small business, and considers the role and meaning of small business in American culture.

While retaining the chronological structure of the first edition, the second edition incorporates short case studies on retail booksellers, specialty steel producers, a vertically integrated seafood company, and Maui’s unsuccessful attempt to launch its own version of Silicon Valley. Blackford divides his history of small business into five periods. Prior to the 1880s, the U.S. economy consisted almost exclusively of small farmers, merchants, artisans, and small manufacturers. A distinction between small and large businesses emerged in the
1880s, as large concerns came to dominate the economic scene. Small business began to cluster in pockets of the economy uninhabited by larger concerns. They remained of central importance in the retail, service, and agricultural sectors. In manufacturing, small firms remained viable by adopting flexible production techniques to produce specialty items for niche markets and by banding together in industrial districts. Between 1921 and the end of World War II, big business made gains in agriculture, sales, and manufacturing. Although some small businesses joined forces to promote protective legislation, those that continued to prosper did so by pursuing opportunities in niche markets and avoiding head-to-head competition with large firms. The story of the postwar period for small business was similarly one of declension, with protective legislation ultimately failing to halt the advance of big business into all corners of the national economy. Only in the last three decades of the twentieth century, as big business faltered in the face of international competition, did small business witness a modest resurgence. Yet, as Blackford notes, small business became more dependent on large firms as subcontracting arrangements proliferated and big businesses adapted to the global upheaval by becoming more nimble and flexible.

Blackford draws upon a handful of groundbreaking monographs published in the 1990s to expand his discussion of small business ownership among women, African-Americans, and immigrants. Although ownership rates have been lower among minorities than whites historically, small business has served as a means of independence and economic advancement for groups excluded from or on the fringes of the corporate workforce. Female ownership of small businesses has increased over the years, even as barriers to corporate employment have lowered. Blackford’s discussion of government policies is sharper than in the first edition, largely as a result of new research completed in the past decade on the Small Business Administration and on the impact of federal laws on small and large firms. All three branches of government enter the story as complex and powerful institutions that have shaped the business environment in ways that have both helped and hindered small firms at different points in time. Short sections dispersed throughout the book on the history of small business in Europe and Asia, furthermore, provide ample opportunity for drawing comparisons and placing American developments in a broader international context.

The two mantras of the book are that small businesses have survived by exploiting market niches through flexible production and marketing strategies and that small business has been important in America for more than just economic reasons. Small business owners, according to Blackford, have occupied a special place in the American imagination. Long admired for their self-reliance and independence, small business owners were depicted during the Cold War as bastions “of political as well as economic democracy” (p. 4). Worthy of further scrutiny is Blackford’s claim that independence has been as important as economic gain as a motivation for striking out on one’s own. His assertion that most small business owners have sought “a reasonable income,” rather than riches and rapid growth, appears to be a fairly safe claim to make, but it tends to obscure differences among businesses and denies change over time (p. 203). I was also disappointed that Blackford did not at least mention vice industries, such as gambling and the sex trade, which faced the same challenges, including encroachment from large firms, and developed the same strategies, including niche marketing and clustering, as other small businesses.

Readers of the first edition will notice certain improvements in format. The type in the second edition is easier on the eyes, the chapter “additional reading” lists are an improvement over the selected bibliography format, and the removal of certain charts and graphs improves the flow of the text. Yet, the detailed and copious endnotes of the first edition have vanished. If not for the “additional reading” lists and occasional references to specific books and authors in the text, nonspecialists and novice readers would have no way of
determining the sources of certain quotations and ideas. The lack of endnotes seriously detracts from this otherwise excellent survey.

GLEN R. ASNER, Carnegie Mellon University


Allan Bogue, Professor Emeritus at the University of Wisconsin, describes Ontario agriculture and the Ontario farm that he grew up on in the 1930s in this book. The farm was just outside of London, Ontario on the North Talbot Road. Actually, there was more than one farm where the Bogues lived, though they were all very close to each other. For most of the period, his father George and brother Len farmed the various properties. Allan was much younger than his brother and helped, but he was mostly a student during the 1930s. For example, he never had morning barn duties when he was a student. After Len married in the mid-1930s and left the farm to work with his father-in-law, Allan played a more important role, but only briefly. George Bogue and family moved into London in 1939 when it became clear that Allan was not going to become a partner with his father.

The Bogues ran a mixed farm. Though they concentrated on dairy in the early years described, they moved more into market gardening in later years. Throughout the period, Allan’s mother, Eleta, sold chickens and eggs, as well as geese. She also sold maple syrup and sometimes helped during haying, especially after Len moved from home.

Their dairy cattle were never the best in Middlesex County, but during World War I George Bogue began to improve the dairy herd with the purchase of Holstein-Friesian purebreds. His brother named many of the calves after young women in the neighborhood and Allan was particularly warned at one stage not to mention this when a certain Irene B. appeared for dinner and was eventually to become Len’s wife. The young males were named after the starting lineup of the New York Yankees. Milking was still done by hand and when George thought that Silverwood Dairy was inaccurately measuring butterfat, he bought a separator and sold cream instead of milk. Later, Bogue learned that they did not really have a large enough dairy operation for it to have been profitable as a dairy farm.

Len decided that market gardening and especially tomatoes were more profitable than dairy farming. They built a greenhouse to plant the tomato seeds early, for early tomatoes could bring five to six times as much as tomatoes at the height of the season. They also (surprising to me) found that pink tomatoes sold at a higher price than the red ones. They staggered the transplanting so that they would have tomatoes throughout the season. They sold at the London market. The operation was large enough so that they hired many laborers during harvest season and Bogue particularly enjoyed the social aspects of these hires. Although tomatoes were the largest market crop, they harvested almost every imaginable crop during this period. Overall, Bogue enjoyed this experience (except for the hoeing) because there were always new crops to understand.

Horses still powered farm operations and the Bogues never owned a tractor, but many of the neighbors were using one by the end of the period. Allan, himself, used a tractor in the 1940s working on his brother’s farm. George took good care of the horses and it was clear that “they were more than mere animals,” but Allan never developed the rapport with the horses that he developed with his calves. The strength of the book is the description of what actually happened on the farm—in this case, the personalities of the horses, the way they were used, their illnesses and deaths, etc. This is characteristic of every part of the book. I could recount many stories and explanations as to what they did, but it would just be easier and more worthwhile to read the entire book.

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Bogue explains that he “tried to describe how we did thing on an Ontario farm during the 1930s. I have not tried to write an autobiography or detail the growing pains that I experienced while I moved through my early and middle teens. Nor did I wish to write a family history. When completed, my account contained elements of all of these things.” The trained eye of the later agricultural historian provides incredible detail that should be of enormous benefit to the agricultural historians of this period. As a nonfarm boy who has written about the Ontario dairy industry, I can say that I learned a lot about the day-to-day operations that Bogue experienced first-hand. He mentions John Ise’s Sod and Stubble as an example of what he was trying to do. “My experience with Sod and Stubble reinforced my belief that accounts of individual families and their farms in other times and places could have broader meaning and usefulness” (p. xii). I think he succeeds in this. Nevertheless, one could have wished for more.

Bogue points out that times were bad during the Depression, but, perhaps, not as bad as in other places in Canada and the United States. The Bogues did not go bankrupt and he does give some information about revenues from various operations, but he never really puts it together to give us some idea about how the overall operation was going.

More importantly, I would like to know more about how he came to agricultural history. Bogue quotes his father’s veterinarian, “George,” he said, “I hear that you are going to be selling out. You shouldn’t. You have as promising a bunch of young stuff as there is around. You would stay with it.” “Oh,” said George. “It’s time; the boy does not want to farm.” Allan continues, “That was hardly true. The boy did not know what he wanted at the time” (p. 196). When and how did he finally know?

ROBERT ANKLI, Gulf University of Science and Technology, Kuwait


Martin Campbell-Kelly’s book fills a long-neglected gap in the story of how computer technology has transformed the world in which we live. Histories of the people and companies that developed and marketed computers abound. Several books have explored the lives and achievements of the most exceptional software developers. And a number of books tell the stories of successful software entrepreneurs, though most of them are about Bill Gates.

Campbell-Kelly has provided us with the first comprehensive survey of the software industry from its origin in the firms that provided software under contract to the government in the 1950s to the home and recreation software industry of the 1990s. In the process, he shows clearly that, despite the preponderance of books devoted to Gates, Microsoft is not “the center of the software universe around which all else revolves.”

Campbell-Kelly segments the software industry into three sectors—software contractors, corporate software products, and mass-market software products—and shows how these three sectors developed, one after the other, as a result of the evolution of computer technology. Though each sector reached a certain level of maturity before the next emerged, the result was not the replacement of one sector by another. Rather, each new sector expanded primarily into new markets, bringing applications of computer technology within the reach of more and more customers, so that today the three sectors continue to thrive and grow side-by-side.

The earliest firms to emerge as “independent” software firms, i.e., companies selling software services that were not hardware manufacturers, were the software contractors formed in the mid-1950s. These were founded by entrepreneurially-minded individuals who had received their technical training as employees of the computer manufacturers or large computer users. The increasing use of computers by government and industry created a
growing demand for these skills throughout the 1960s, and many thousands of software-contracting firms were formed. Most of these started small and stayed small but a few, such as Computer Sciences Corporation, Informatics, and ADR, became substantial publicly held firms.

The demand for the services these firms provided was so great, and their resulting growth so dramatic, that the late 1960s saw a major stock market boom in which the value of the shares of these companies multiplied many times, virtually overnight. As Campbell-Kelly relates: “By the summer of 1968...both investors and entrepreneurs had lost their prudence and had been caught up in a frenzy for computer stocks that seemed to know no bounds” (p. 79). The inevitable crash in the early 1970s put such a damper on the market for software company stocks that there were no further software company IPOs until Cullinane Corporation went public in 1978.

The Corporate Software Product sector began to emerge in the mid-60s when firms such as Informatics and ADR began to offer off-the-shelf software. A major factor in the development of the software product market was IBM’s release of its 360 series of computers. The compatible operating systems across the various models of the series, and IBM’s incredible success in marketing the 360s to thousands of customers, meant that there was a large enough market for off-the-shelf products that it became possible to recoup the large development costs required to create software that was flexible enough and robust enough to be used by multiple customers with minimal customization.

When IBM announced in 1969 that the cost of software would no longer be “bundled” into the cost of the hardware, a major barrier to the growth of the software product industry began to crumble. IBM’s action changed the perception of software as a free good, opening the door for competitors to pitch the cost benefits of acquiring software from other sources.

Just as the 1960s was the decade of the software contractors and the 1970s was the decade in which the corporate software products industry grew into a significant market force, the 1980s saw the emergence of the mass-market software industry, triggered by the evolution of computers small enough to be “personal.” Campbell-Kelly covers a broad spectrum of the companies that were created to serve this vast new market and shows how Microsoft eventually came to its dominance of the personal computer software market.

One of the most important contributions of this book, however, is that it places that dominance of one sector of the market into the context of the much broader and deeper market for software used in many different environments and supplied by thousands of different vendors. For the first time, the true scope and influence of what has been largely an invisible industry has been made clear in a readable, informative narrative.

Luanne Johnson, Charles Babbage Foundation and Software History Center


Carville Earle’s The American Way is a book of many virtues. It ambitiously covers the interaction of American geography and political economy for more than three centuries. It contains a wealth of statistical and geographical information, usefully presented high-quality charts, tables, and maps (I have flagged several of these for inclusion into my undergraduate lectures). It not only analyzes the intersection of economics, geography, and history, but also incorporates political ideology and party politics into its basic framework. It has an extensive bibliography that reflects Earle’s wide-ranging and interdisciplinary interests. This book, in essence, has all of the makings of a major historical synthesis.

The American Way, for all of its promise, ultimately disappoints as a work of synthesis. Burdened with an unpersuasive conceptual framework, the book manages to be both frus-
tratingly complex and overly simplistic. Its wealth of data is shoved and pushed into categories that become unwieldy and sometimes contradictory. Perhaps that is the inevitable fate of a book that attempts to find “The American Way” of dealing with more than 350 years of economic, geographic, and political change. In trying to develop a model that explains almost everything, Earle ends up explaining very little.

Earle divides American history into cycles of policy regimes with their own geographic and economic imperatives. The colonial period is divided into three regimes—“Republican” then “Liberal” then back to “Republican” again. It is a bit confusing, to say the least, but the picture gets even more complicated after the American Revolution. Earle labels the early national period (1788–1830) as the “First Republic” that entailed a producer revolution, protectionism, geographic expansionism, and demographic concentration. After a period of crisis, the next regime was the “First Democracy” (1830–1877) that included a consumer revolution, free trade, geographic consolidation, and demographic dispersion. Alternating cycles of “Republics” and “Democracies” characterize the rest of American history. (We are now, according to Earle, in the era of the “Third Republic”). Earle sees long-run economic crises accounting for the transition to different policy regimes, each of which had its ideological roots in seventeenth-century political theory. Classifying more than 300 years of history in terms of nine policy regimes is bound to involve some reductionism, which Earle embraces wholeheartedly. Consider the first sentence of chapter one: “The roots of American geography—everything from NAFTA to flexible specialization, from edge cities to globalization—run deep into the subsoil of early modern England, to the seventeenth-century origins of liberalism, republicanism, and the half-century crises by then endemic in the capitalist societies of maritime Europe” (p. 15).

To use J. H. Hexter’s famous distinction between the “lumper” and the “splitter,” Earle is the ultimate “lumper.” I am a “splitter” by nature, but even the most devoted lumper would find it difficult to make use of Earle’s complicated model. At times, The American Way stretches the meaning of key concepts until they are almost meaningless. Take Earle’s treatment of “liberalism” as an example. Most historians identify liberalism as a political doctrine associated with the extension of individual rights. Earle, however, associates liberalism with a rather vaguely defined economic elitism, so exactly who is a liberal and who is not is always in question. He surprisingly lumps Thomas Jefferson together with Alexander Hamilton as part of a liberal, nationalist elite that opposed the extension of democracy and equal opportunity (p. 345). This seems self-evidently flawed, especially as Earle himself notes a few pages later that Jefferson “came as close as anyone to recreating the classical republican ideology of nationalism and equal opportunity” (p. 348). Even more puzzling, Earle suggests that Franklin D. Roosevelt’s domestic policies were not really “liberal,” but rather harkened back to classical republican ideology (p. 46).

On more straightforward economic matters, Earle’s framework is also problematic. He insists, for example, on drawing tight distinctions between “producer revolutions” (associated with republican policy regimes) and “consumer revolutions” (associated with democratic policy regimes). One would suppose the periods of greater production would be correlated with periods of higher consumption, but in Earle’s framework they are “inseparable alterations” (p. 110). He thus labels the period from 1880–1932 as a “producer revolution.” Dramatic changes in production did indeed occur, but the era also saw the emergence of a national market, the rise of the department store, the growing popularity of branded goods, and the introduction of new consumer durables such as the automobile. His failure to seriously analyze these developments suggests that Earle does not in fact have convincing criteria for distinguishing a “consumer revolution” from a “producer revolution.”

Earle’s framework might have been more persuasive if he had developed a clearer narrative. Unfortunately, he tends to shift topics with little attention to chronology. Because he often uses terminology in rather unconventional ways, the book’s organization adds to its
frustrations. The result is beautifully produced book brimming with big ideas, but one that ultimately fails in its quest to achieve a grand synthesis.

JOHN MAJEWSKI, University of California, Santa Barbara


Fitzgerald thoughtfully discusses the transformation of U.S. agriculture from family farms to its current status as an industrialized sector. She considers three factors that influenced the pattern and speed of the shift to industrialization. First, variation in soil quality, climate, and terrain created diversity in crop production across regions in the United States. Industrialization came first to California, which was initially dominated by wheat production, and last to the Atlantic Coast, which produced fruits, vegetables, dairy, and poultry. Second, although demand for U.S. products was high during World War I, at war’s end foreign demand for these products declined and farm prices fell rapidly, precipitating a farm crisis. Third, bankers, the USDA, insurance agents, and newly minted agricultural engineers and agricultural economists all pointed to the efficiency of the factory system as a model for agriculture.

The second chapter describes the role economists played in agriculture’s modernization. The USDA felt it was important to collect information about the agricultural sector, largely to make it possible to control farm activity and agricultural research from afar. For example, the Bureau of Agricultural Economics (BAE) suggested collecting cost-of-production data for grains, which all states quickly took up, using nearly identical methods to collect cost of production information. It was at this point (1922), according to Fitzgerald, that “it became national policy to ensure that farmers operate in as standardized and routine a manner as possible” (p 36). Land grant universities, created with federal money, struggled because the faculty often had little knowledge of regional farming practices, and so began collecting information on farming practices whilst encouraging farmers to account for their revenues and expenses. A rift emerged among those promoting the quantification of agriculture: some were interested in creating economic models to influence policy while another group was more hands-on, seeking to improve farming techniques. Two professional associations emerged from these distinct groups of professionals—the American Economic Association and the American Farm Management Association.

The third chapter focuses on how agricultural engineers, through their problem-solving approach, had a large impact on farm modernization. A small group of engineers from academia, industry, and government—interested in issues such as farm implements and barn design—formed the American Society of Agricultural Engineers (ASAE). Engineers were interested in promoting on-farm efficiency through farm layout and farm standardization, and thought human labor should be replaced with machines. The early unreliability of tractors led engineers to question whether horses should be supplanted by tractors, but despite the debate, farmers began buying tractors in the early 1910s. The next major innovation, the combine, was able to cover a large amount of ground in a short time, encouraged farmers to increase farm size.

In the fourth chapter, Fitzgerald discusses the emergence of large-scale farming. According to the BAE, a large-scale farm was five to eight times as large as the average farm in the same area producing the same crops, and could be a single farm or a group of farms under the same management. By 1930 Texas, California, and Alabama had the greatest number of large-scale farms, which produced a variety of crops: cotton, beef cattle, sheep, fruit, dairy, and grain. One type of industrial farm was the chain or group farm, comprised of farms owned by banks or farm management companies, directed by farm managers, and
operated by tenants. The owner dictated most farm practices, including types of seeds to sow and machines to use. Large cooperatives—such as the California Fruit Grower’s Exchange—were another form of industrial farm. A large debate (one that continues today) ensued as to whether the industrial farms were “better” than family farms.

The fifth chapter and sixth chapters are case studies of the Campbell Farming Corporation and the Soviet collective farming experience. By virtue of four reasons, the Campbell Farming Corporation, in Fitzgerald’s opinion, was an industrial farm. The farm depended on machinery, economic, and engineering expertise, and relied on outside capital. Wheat prices were low, necessitating that Campbell work precisely and efficiently to turn a profit. In contrast to the success of the Campbell farm, Fitzgerald’s second case study details the complete failure in the Soviet Union of the techniques used by American industrial farmers.

In closing, the industrialization of agriculture has long been debated by those interested in American agriculture. In this thoughtful and well-executed book, Fitzgerald gives a large share of the credit (or blame, depending on your perspective) for the modern agricultural system to economists and engineers as well as to the financial industry. Although questions about the relative virtues or vices of industrialized agriculture are likely to linger, Fitzgerald provides much-needed insight into the factors contributing to agriculture’s industrialization.

CAROLYN DIMITRI, Economic Research Service, USDA


Most scholars credit the New Deal for the rise of the American welfare state. And, indeed, many of the major policies intimately associated with the welfare state originated in the 1930s. More recently, these origins have been pushed back into the last quarter of the nineteenth century. Citing the burgeoning system of Union military pensions in that period, Theda Skocpol has concluded that the United States created a somewhat precocious social welfare system (Protecting Soldiers and Mothers: The Political Origins of Social Policy in the United States. Cambridge, MA: Harvard University Press, 1992). However, this system was both highly discriminatory and rife with administrative abuse and fraud. The latter, Skocpol contends, explains the reluctance of state agents to experiment with more modern welfare policies even as the steady demise of Union veterans contracted the size of the pension system. Thus, the late-nineteenth-century pension system was a false start in the development of the modern welfare state.

The book reviewed here pushes the origins of the welfare state even further back in time, to the founding of the republic. The key element in this interpretation is the notion of an entitlement policy: a statute conferring “public benefits to groups of ‘like’ individuals programmatically, on the basis of the statutory eligibility criteria of deliberately enacted legislation” (p. 8). For example, the Continental and Confederation Congresses both passed legislation entitling disabled veterans of the Revolutionary War to a pension. These programs were reenacted and thus continued by the First Congress of the United States. As Laura Jensen nicely describes, such policies were open-ended in their obligations (e.g., no one knew when a pensioner would die and thus cease to draw upon the Treasury). In addition, because the number of people who might fall within the categorical boundaries of eligibility was often poorly known, the size of the fiscal commitment was unpredictable. Even so, such entitlement policies frequently consumed large portions of the Federal budget, even in the ante bellum period. For that reason, they were often intertwined with revenue policies; as Jensen notes, the protective tariff was the revenue twin to entitlement expenditures throughout the nineteenth century. Although, as she narrates the story, pen-
sions tended to drive tariff policy, the tale could be told the other way around (with trade protection dictating the steps in the dance). Either way, the almost simultaneous adoption of expanded pension benefits and higher tariff schedules in 1828 (p. 113) and 1832 (p. 116) demonstrates their mutually intimate link before the Civil War.

Even more importantly, entitlement policies necessarily construct boundaries between those who are made eligible and those who are not. These boundaries, from Jensen’s perspective, are essential to understanding the salient identities of the early republic’s members. To be made eligible for a Revolutionary War pension, for example, was to be both identified and honored as a “patriot.” The importance of such boundaries and identities was reinforced by their contestation; those who were excluded from benefits struggled for a redefinition of categories which, at the same time, was a claim on identity. One of the very nice things about Jensen’s book is the way she illustrates, time and again, the apparent inevitability of such contestation and the ever broadening definitions of eligibility that ensued. However, she does not sufficiently acknowledge the self-liquidating character of pensions based on service in a particular war. Over time, for example, the number of veterans of the Revolutionary War dwindled. Their decline in numbers meant that an expansion of eligibility did not necessarily mean an expansion in the size of the state. For example, when Congress increased Revolutionary War pensions to $500 per year in 1868, the increase in benefits went to only two men (p. 216).

This aspect of military pensions (and land distribution, which is also extensively discussed) is one reason Skocpol and others have downplayed the precedent-setting role of antebellum programs; they were both comparatively small even when most veterans were still alive and largely defunct by the time the post–Civil War system was constructed. Jensen is thus correct when she comments on the parallel politics of antebellum and contemporary entitlement policies and exaggerating when she assigns a path-breaking role to the former. This reviewer, for example, is confident that Civil War pensions would have been invented from whole cloth even if no previous system of benefits had been enacted. Jensen also takes to task scholars (this reviewer included) for under-estimating the state-building potential realized through antebellum pension and land distribution programs. From my perspective, her criticism appears to confuse state expansion (which was certainly abetted) with the development of strong state institutions. When the Civil War began, for example, former federal officials throughout the new Confederate States hauled down the Stars and Stripes and ran up the Stars and Bars with no apparent hesitation. Pensioners who had stridently paraded their patriotism in making demands for federal benefits were nowhere to be seen. In fact, as Jensen notes, the Union Congress assumed southern pensioners were all disloyal and thus stopped payment on their pensions. It is in that sense that “the early American state was a limited, underdeveloped, premodern shell at the center” (p. 227). In fact, central state institutions were probably strongest just around the time the Pension Act of 1818 was adopted, only to decline more or less steadily over the decades leading up to the Civil War.

But these are minor quibbles, far outweighed by the book’s contributions. We now have a careful and convincing interpretation of entitlement policy during the early Republic, an interpretation set within the political discourse of social construction and the law as a communal language of identity and social standing. As Jensen amply demonstrates, the antebellum politics of pensions and homesteads—of patriots and settlers—was as modern as contemporary efforts to create a national health insurance system. For that reason alone, this book will find an enduring place in scholarship on the history of the American welfare state.

RICHARD BENSEL, Cornell University
Economic historians and historians of technology can learn much from David E. Nye. His latest work, *America as Second Creation*, traces the use of the “technological foundation stories” through which many Americans viewed their place in the postrevolutionary era. For Nye, “the second-creation story asserted that the original perfection of the world could be enhanced with new technologies” (p. 283).

His discussion of the narratives by which Americans embraced the axe, mill, canal, railroad, and irrigation systems as bearers of prosperity and civilization offers keen insight into how these Americans came to understand their place in the world. These narratives viewed technology to be as much a part of America’s Manifest Destiny as was westward expansion.

The basic narrative unfolds as follows: technologies such as those mentioned above were instruments of a new creation. These technologies provided the opportunity for economic self-improvement while fostering the development of new communities, and bringing civilization to seemingly undeveloped land. Such benevolent effects were often viewed as divinely inspired, perhaps even ordained. Nye grounds these narratives in four key shifts in perceptions that took root after the revolution. The shifts—both psychological and political—include the division of land into “abstract grids,” the rise of a “free-market ideology,” and beliefs in “natural abundance” and in a world of “. . . clear, quantifiable causes and effects” (p. 21).

The book is much more an addition to the history of American thought than to the literature on technological change. Nye underscores the extent to which nineteenth-century Americans such as Whitman, Emerson, Webster, and Lincoln were optimists in rejecting the focus of Ricardian and Malthusian political economy on scarcity. These Americans, and many others like them, embraced a view that abundance was the norm to be achieved through technological change.

Nye uses literature, news accounts, periodicals, and art to demonstrate that many Americans readily welcomed a succession of technologies, and the institutional prerequisites of capitalism. These same media are used to document Americans’ pervasive and persistent debate about the consequences of technological change. Although the dominant narrative viewed invention as a crucial force in transforming the natural world for human advancement, a darker vision of technology’s effects grew up alongside, or in reaction to, this foundation narrative.

Alternating chapters juxtapose the mainstream technological narrative with a counter-narrative that emphasizes the impact of technology on the land, indigenous tribes, the homesteader and the ecological system. These counter-narratives contradict the optimistic assumptions of freely available land, harmonious development, and a bountiful but untapped natural environment enhanced by white settlers. Nye embraces these counter-narratives while recognizing that such views were specific, localized, and personal.

The counter-narratives include critiques of the second creation story from populist, labor, environmental, and Native American perspectives, but display few uniting principles. The accuracy of these alternative views does not diminish the ideological or practical significance of the axe, mill, canal, railroad, or dam in America’s development. Indeed, despite increasingly complex social relations and the growth of state power, each successive technological narrative retained its key assumptions and a basic form that endured until at least the twentieth century.

Nye does not seek to add to our understanding of the forces that lead to the invention, improvement and application of these technologies. He shows convincingly, however, that these technologies both practically and ideologically underpinned the settlement of the
west, and suggests that various accounts of America’s economic development would scarcely be possible in the absence of ever more complex technologies.

The final two chapters update Nye’s account with a clear demonstration that scientific advances did not diminish the fundamental tensions between second creation stories and the counter-narratives. His conclusion identifies a new “recovery narrative”—a synthesis that informs contemporary environmental thought and legislation. It accepts much of the critique offered by later counter-narratives even as it depends on science and technology as antidotes to the environmental degradation that earlier technologies made possible.

Nye wants us to understand that these narratives were more about Americans trying to understand their world than about technology per se. One might argue that the key political and psychological shifts identified by the author were at least as important as these technologies. Some versions of these narratives suggest as much. In some instances, the narratives are as concerned with explaining and justifying the unfolding society of newly settled areas as with the application of technology.

More frustrating is that Nye’s focus on a critical dissection of these narratives occurs at the expense of any consistent effort to discern their real impact on decisions, public and private. Economic historians, however, should acknowledge that these narratives reflect an institutional and intellectual environment that fed the appetite of some Americans for technological solutions, and reinforced other Americans’ distaste for new technologies.

JOHN S. NADER, SUNY at Delhi


This is an oddly disturbing and somewhat distressing book. In its construction, Facing East from Indian Country has the potential to be original and exciting. As Daniel Richter clearly points out, most work on the interaction between European and Indian looks inland from the Atlantic and thus the view is looking west from the Atlantic. Instead, he is asking that we look at these same events as seen looking eastward from Indian country. This is a very important observation. Indeed, asking the reader to take a different perspective is becoming more widespread as is evident in David Northrup’s Africa’s Discovery of Europe 1450–1850 (Cambridge: Cambridge University Press, 2002). The problem for Richter occurs in what lies between asking the reader to take this alternative perspective and the writing of a native history of early America. Unlike other situations, Richter has scant evidence on which to base his history.

My concerns are threefold. The first is that during the early years of contact between Indians and Europeans, there is no Indian documentary evidence about their perspective. This is a reality with which all in the field must work. Oral history for the period has been passed down through generations and thus must be treated with care. The documentary evidence that does exist was written down by Europeans. Even when Europeans intended to provide exact translations, we know now that the understanding of the words used could be quite different between Indian and European. Thus in his very first chapters, Richter has a problem. He solves the problem by providing a number of case studies of what he imagines particular Indians would have felt. In the book, these imaginings are not right-hand justified as is the remainder of the text. But they still pose a serious problem. These imaginings are not history. To be sure, the careful reader will recognize this. Yet on a methodological level we must be concerned about the bounds of historical speculation. Where does speculation based on other case studies or other time periods end and pure imaginings begin. Should such a line be drawn? Is there a responsibility to draw that line?
My second concern is that while trying to present a work that views the contact between Europe and America from an Indian perspective, the narrative is incredibly Eurocentric. Indeed, we end up with what I consider to be a very old fashioned view of Indian life after contact. Richter sets up for us a world of dependency. For instance, “[t]he inability to make a decent pot or airworthy flint arrowhead (even if one had wanted to make such things) meant that their replacements now had to be purchased...commercial hunting was likely to crowd out almost all other economic pursuits, and to make communities almost entirely dependent on European trading partners for nearly all their supplies” (p. 51, italics in original). This is a story of glass baubles (p. 48) and dependency. Yet while this might have been the historical reality for some very few communities, it was not the reality for the majority of native communities in North America for the first hundred or two hundred years of contact. Richter claims to be writing a Native History of Early America but this is a book about only those communities that actually had contact. It is not a history of all Native communities many of whom would still be making decent pots.

If we take only those communities which had contact as the relevant set, my third concern lies in the way that the early centuries of history are compressed. The arrival of new people, new goods and new diseases did change lives. But how were lives changed? The emphasis in the narrative is that all change is the result of colonial interaction. Colonial interaction becomes the focal point. Yet for much of the first two centuries, it is surely better to think of the arrival of the colonists as an exogenous shock to the system and Indian communities responded to this shock as to any other exogenous shock. It caused a change in the power balance which played out in particularly Indians ways. Indian communities could respond to changes in the relative power between groups with war. This is to argue that Indians reacted to the power change brought about by the arrival of Europeans as they would have to an epizootic. But by focusing on the fact that the shock is the arrival of Europeans, Richter is saying that the resulting changes were European induced rather than shock induced. Again this is a Eurocentric view. By the time we get to the new republic, relationships between Americans and Indian have changed. There are now specific Indian–American tensions and war. But are we justified in calling these wars ethnic cleansing? More than anything else, what is gained by labeling all interaction in this way? In summary, I found this an odd book: Interesting in conception but flawed in delivery.

ANN M. CARLOS, University of Colorado


This book is a good idea—a recent economic history of America, without footnotes but with in-text references and a good bibliography. The length of 312 pages is manageable for thoughtful, but nonprofessional readers. And the format, with chronological sections of chapters by topic, makes sense. However, the book lacks any quantitative data in the form of either charts or tables—an intolerable inadequacy for a book on economic history. And the choice of subjects is imbalanced. There are several chapters on labor relations, the author’s own specialty, but none on economic regulation or deregulation or other microeconomic phenomenon of interest, such as defense, education, health care, social entitlements (e.g., Medicare, Medicaid, and Social Security), the structure of fiscal deficits and debt, or the structure of America’s balance of payments.

Organized in 12 chapters, the book deals primarily with macroeconomic history and labor relations. After an introductory chapter on mobilization, Samuel Rosenberg organizes his narrative in three parts: the making of an institutional framework (1945–1960), development strains (1960–1971), and the unmaking and remaking of a new institutional frame-
work (1971–2000). Within each section, the author deals with macroeconomic policy and growth, with labor relations, and with exchange-rate conditions and international competitiveness. A departure, chapter 7, deals with race, gender, and income inequality.

The macroeconomics is adequate and clear, but hardly masterly. Keynesian economics is explained briefly, but never developed in depth. There is no discussion of income multiplier, of the gap between real and potential output, of total factor productivity, of real interest rates or real exchange rates. Likewise, the coverage of supply shocks—especially the two oil shocks in 1973–1974 and 1978–1980, has too little substance or descriptive color to give the reader an adequate sense of the shocks’ effects on America. The same applies to the effects of Reaganomics on the dollar, the budget, and the government’s debt.

The book’s strongest chapters cover labor relations— chapters 3, 6, and parts of 1, 10, and 11. This is where the author bases his text on a strong background of research. So one learns of the empowerment of labor under Taft Hartley, and its subsequent growth. Then one reads of a standoff, between labor and business during the 1960s, the slow decline during the 1970s and its relations to wage-price controls, and the sharp fall in institutional power and size during the 1980s.

The author intensely dislikes the Reagan era and is extremely critical of the Reagan administration’s effects on the budget, taxes and inequality, labor and international competitiveness. After characterizing the Reagan Revolution as a market-based, conservative strategy, the author concludes that it “did not lead to a more well-functioning economy. The rate of economic growth did not accelerate and an investment boom did not materialize. What supply-side economics lead to was growing economic inequality and growing economic deprivation” (p. 235). This is not a glowing endorsement. He concludes that “Clinton did what Reagan espoused” (p. 304).

An egregious failure of the book is its failure to develop the phenomenon of economic deregulation after the mid-1970s. After all, huge segments of American economic life—banking and securities, airlines, railroads and trucking, petroleum, natural gas and electricity, and telecommunications were deregulation during this period. There were a series of causes why the old regulated systems began to fail, and a whole analysis of the consequences of deregulation, which contributed substantially to America’s macroeconomic performance. Of this, however, there is little mention.

The book would have been vastly improved had the author utilized some simple quantitative data, in tabular or clear graphic form. GDP growth rates, inflation, unemployment, balance of payments, productivity and budgetary data could all be presented clearly and simply, vastly enhancing the readability and understandability of the text.

Professor Rosenberg concludes with a nice section on the budgetary reforms of the Clinton administration, and on the powerful “new economy” at the end of the twentieth century. In a parting shot, though, he warns against smugness, noting that Japan, which looked unstoppable in 1989, appeared pathetic just a decade later. The same, he suggests, could happen to America. Indeed, one wonders what the author would say three years later (July 2003), at the time of this review?

RICHARD H. K. VIETOR, Harvard Business School

GENERAL AND MISCELLANEOUS


Peter Bernholz surveys the global-historical experience of inflation, and argues that there are important commonalities in the political and economic sources of moderate (less than 50
percent per month) inflations and of hyperinflations, and in the recipes for successful termination of inflation. What are they? Firstly, all governments have an inflationary bias, and hyperinflations in particular arise from monetizing deficits. Secondly, in a moderate inflation, prices may not rise as fast as the money stock, so real money balances may rise, whereas in a hyperinflation the opposite occurs and real money balances may plummet. Thirdly, under a commodity money system, a hyperinflation was virtually impossible—the only hyperinflation earlier than the twentieth century, being the assignat period in revolutionary France. Finally, moderate inflations can be concluded by adoption of a monetary constitution while hyperinflations require the kind of (economic) “regime change” described by Thomas Sargent (“The End of Four Big Inflations” in Robert Hall, ed., Inflation: Causes and Effects [Chicago: University of Chicago Press, 1983]). This summary will suggest to the reader that the overall picture presented here is not very new, and to an extent I think that is right, but a point form summary does not do justice to the breadth of the evidence that Bernholz brings to his discussion, and the book will be a useful reference for anyone who wants a broad overview of inflation and hyperinflation. In addition, it provides a wealth of source material; the appendix, for example, contains detailed sources for the study of all 29 episodes of hyperinflation.

The book presents a series of event studies linked by a theoretical model. After two brief introductory chapters, there are three chapters describing in turn experiences of moderate inflations under metallic standards and under paper money standards and then hyperinflations. The next chapter presents a theoretical model of the inflation process and the final two chapters discuss the ending of moderate inflation and then hyperinflation.

The model in chapter 6 captures the core of the inflationary process that is uncovered in the earlier chapters. Begin with a small open economy that uses only coins and where the government is monetizing a constant real deficit in a stationary economy: At first, the paper money issued to finance the debt simply replaces coin in the public’s hands with a more convenient medium and is a good thing; then the public has “enough” paper so that the paper money starts to substitute for reserves; when the reserves are depleted, nominal money stocks rise, prices rise (not quite as much) and the exchange rate peg is broken; finally, as inflation rises more rapidly the public stops using the paper money, switching to either barter or the good money (possibly ignoring government threats). At this stage, the government has an incentive to stabilize the monetary system.

Bernholz emphasizes the link between the economic and political aspects of inflation, and especially of any attempt to end inflation. Thus he emphasizes that moderate inflation is typically accompanied by undervaluation, generating a short-run boom that is politically popular, so that a stabilization may be unpopular, though such feelings can be mitigated if the reform entrenches an undervaluation, for example by fixing the exchange rate at an undervalued par, or is accompanied by adoption of a monetary constitution that in some way ties the hands of the monetary authority, for example by establishing an independent central bank, or a currency board, or by adopting a fixed exchange rate. In contrast, high inflation, by leading to a breakdown of the monetary system, creates an environment where there is broad support for reform, and a new government that stabilizes the currency will receive popular support. Bernholz suggests a very straightforward recipe for a successful stabilization which focuses on preventing the monetization of deficits and creating confidence amongst the population that this is the case.

A benefit of any study on such a broad canvas—from Roman times to the 1990s, across the globe—is that it is very likely to suggest unanswered questions, and this book suggests intriguing questions for monetary economists and historians. For example, an event study may tell us that all inflations originate in fiscal deficits, but it cannot tell us how often, and under what economic or political circumstances, fiscal deficits lead to inflation. Similarly, the observation that all hyperinflations lead to stabilization because the economy collapses cannot explain the diversity of maximal inflation rates in hyperinflations—the range is...
from 53 percent per month in Belarus in 1994 to 309 million percent per month in Serbia in 1994.

Today the media and indeed central banks seem to fear deflation rather than inflation, and it might seem that this book is out of step with the times. But as this chronicle makes clear, history is on the author’s side: there have been many more disruptive inflations than deflations, and as the author’s analysis of the nature of economic and political systems shows, this bias is likely to remain.

ANGELA REDISH, University of British Columbia


Between independence in 1821 and the final settlement of 1887, Mexico defaulted on its debts with depressing regularity. Michael Costeloe’s book, Bonds and Bondholders, is a narrative history of the British holders of that debt. It asks three questions. How were Mexican taxes translated into British interest payments? How did British bondholders defend their interests? Who were the bondholders, and why did they purchase Mexican securities?

The first chapter is about the actual bonds themselves. Much of the chapter is of interest only to collectors and scripophilists, but Costeloe’s exhaustive research has allowed him to determine the details of every bond issue, rescheduling, and default. Costeloe’s research shows that Mexico made interest payments—albeit sporadically—more than many historians have realized.

The second chapter concerns the mechanisms by which “pesos minted in northern Mexico ended up as pounds, shillings, and pence in the pockets of . . . bondholders throughout the British Isles” (p. xvii). In theory, Mexico’s debts were secured by specific dedicated taxes: customs revenues or silver export duties. In order to insure that these taxes were actually collected, British investors tried posting agents in Mexican ports. Investors also appealed to the Foreign Office to protect their rights and halt the widespread smuggling of Mexican silver into Britain.

Unfortunately, the bondholders failed to create mechanisms that would insure steady repayments. Mexican governments refused to allow the bondholders’ agents to take their posts. Widespread smuggling reduced the revenues from the export taxes. The British government refused to embargo Mexican exports. Costeloe provides evidence that, even more astonishingly, the Royal Navy was deeply involved in silver smuggling.

British creditors formed committees in order to negotiate with Mexican governments as a unit. The first of these committees was formed in 1830, three years after Mexico’s first debt default. Chapter 4 details the history of the committees. The hope was that by negotiating as a unit, the bondholders could induce Mexican governments to begin payments on their outstanding debts. The committees met limited success. They negotiated several debt reschedulings with various Mexican governments, and at times succeeded in collecting interest, but these agreements invariably collapsed in the face of Mexico’s endemic political instability.

Chapter 5 asks who were bondholders, and why did they hold Mexican securities? Although Costeloe lacks comprehensive statistical information, he provides anecdotal evidence to support the hypothesis that not all the investors in Mexican bonds were independently wealthy. The contemporary stereotype of widows and churchmen bankrupted by Mexican bonds appears to have had at least a grain of truth. Costeloe also provides two reasons why investors bought Mexican offerings. First, they believed that Mexico was a fundamentally rich country brought low by political instability. At some point responsible government would arise, or it would be imposed from the outside, and creditors would receive their payments. Second, after the initial defaults of the 1820s, Mexican bonds were
cheap. Investors speculated on the possibility that interest payments might be met in the future.

Finally, chapter 6 explores the relationship between the bondholders and the British government. The British government only moved to support its citizens' claim against Mexico twice, in 1838 and 1862, and both actions were initiated by France. In 1838, the British sent a squadron to Veracruz and mediated an end to a war between France and Mexico over Mexican debts owed French citizens. In 1862, two years after Mexican soldiers raided the British legation in Mexico City and stole £120,000, the British supported a French invasion of Mexico; an action also undertaken in support of claims owed French citizens. In neither case did British action secure the rights of British creditors in the long term. The political economy of British passivity is an interesting topic for future research, given how many parliamentarians owned Mexican bonds.

In short, Bonds and Bondholders is an exhaustively researched and engagingly written narrative history of the British holders of Mexican debt. The book provides an immense wealth of institutional and biographical detail, and is an invaluable source for anyone interested in the history of British financial relations with Latin America.

NOEL MAURER, Centro de Investigación Económica, Instituto Tecnologico Autonomo de Mexico (ITAM)


In its general lines, the subject of this book is well known. The twentieth century was par excellence the era of fiat money, one of the great achievements of human ingenuity, arguably on a par with the steam engine and electricity. To work properly, however, it had to be used with caution because “swept along by animal spirits, markets can behave emotionally and perversely,” as the vice-chairman of the Fed reminds us in his postscript here (p. 242). Therefore, this had to be also the century of the central bank, an institution that emerged during the turbulent times after World War I and, through many metamorphoses, endures still today. Could a soothsayer (or even an economist) of the inter-war period have foreseen the shape of the brave new financial world of the early twenty-first century? It is unlikely. That deregulation and competitiveness could ever come to be considered as important as macroeconomic stability, that flexible exchange rates be regarded as acceptable, and that the lines between banking, insurance, and securities might become so blurred, would simply have been too extravagant to contemplate. The merit of this volume is that it shows, with a wealth of detail, how this unpredictable journey was made by eight west European countries and how diverse were the paths they followed.

The book consists of an introductory background section, followed by a chapter for each country, and ends with a section on Prospects for International Banking and Financial Systems, by four distinguished central bankers. The eight “national” chapters vary somewhat as to their focus but all of them do a more than satisfactory job. The hero throughout is the central bank, the plot its unremitting struggle for stability, a goal which in itself was far from stable, as society’s concerns switched from systemic aims in the inter-war period, to a more macro-economic and exchange-rate-oriented approach, between the 1940s and the 1970s, to price and systemic stability again from the 1970s until now. Along the way, regulatory functions changed many times too. In the 1930s central bankers learned to be lenders of last resort and to impose structural controls on banking. In the 1950s and 1960s, the discount rate came to the fore, credit directives to banks were issued liberally, and most central banks became subordinate to political authority. During the last 30 years an earthquake hit financial systems. Banks came to be regarded as any other firms, slow growth was no longer blamed on them
because now they were enjoined to be “efficient,” no longer simply “stable.” Liberalization, internationalization, and innovation all led to intense competition, which posed anew the problem of systemic stability and the need for supervision to be thought of in a completely differently way, in a world where financial institutions now were no longer just banks and did a bit of everything, everywhere. *Ex post* remedies were replaced by *ex ante* solutions, and capital and liquidity requirements had to be adjusted for risk.

The multiplicity of outcomes depicted in these pages is hardly surprising when one considers how much monetary and financial policies were shaped by (changing) theories, path dependence, economic conditions, and politics, and how these varied over space and time. Logically, the Bundesbank in the 1950s and 1960s was immensely different from the Belgian National Bank or the Banco de Portugal. But as Pablo Martín Aceña writes, there was also a learning curve to being a central banker and it was not the same for all. Factors such as the country’s external exposure, the degree of internal competition and the extent of subjection to the Treasury, all conditioned perceptions and led to some tools being favored over other ones. For all central banks, however, the question that has to be posed and would have merited a fuller and more systematic answer here is what future do national central banks have in Europe? Are they on a circular course, in some sense back to their nineteenth-century identity, as suggested by Aceña, are they simply on their way out, as Marco Onado seems to think, or will they have to reformulate their role in order to survive, as Olivier Feiertag claims? The issue hinges on the future of bank supervision, central banks’ last remaining important function. Undoubtedly, its importance is enormous and bound to grow, as liberalization fans the animal spirits and allows the markets to behave emotionally and perversely. Do only central banks have the local know-how to perform it and must it be carried out internally when the time is one of rising supranationalism and cross-border financial activity?

The weakness of this book resides in the dispersion of its contents, a frequent problem in this genre of conference volumes, perhaps even an intrinsic one. The reader gets lost in the welter of detail, the profusion of approaches and the low adherence of contributors to a common agenda. The remedy would be a strong introduction that provided a clear framework of analysis, that wove the disparate parts together and generated stimulating comparisons. This is lacking but it is compensated for by some very interesting general comments in the chapters by Onado on Italy and Prast on the Netherlands, as well as by remarks in the final Prospects section. The net result is a book that contributes to an important and much visited field.

**JAIME REIS, Instituto de Ciências Sociais, Lisbon**


There are some themes in the study of history that must never be forgotten. The Atlantic slave trade is one of those that require consistent efforts from the part of historians and history teachers to keep the discussion alive. It is thus a stimulating event to have another recently published book that manages to shed light on new angles of a theme that has rightfully received a large amount of attention. Joseph Dorsey’s *Slave Traffic in the Age of Abolition* is also particularly interesting because it addresses the subject during one of its least documented periods, the nineteenth century, and in one of the less studied Caribbean regions, Puerto Rico. The “Age of Abolition” in Puerto Rico, states Dorsey in his thoughtful conclusions, was made out of less enlightened contents than its name suggests.
Based mainly on diplomatic evidence, Dorsey undertook the task of presenting a detailed account of slave traffic after it was internationally abolished and became a clandestine activity. He introduces a much more nuanced version than the diplomatic saga in Arturo Morales Carrión’s classic work (*Auge y decadencia de la trata negrera en Puerto Rico (1820–1860)*, Río Piedras: Centro de Estudios Avanzados de Puerto Rico y el Caribe–Instituto de Cultura Puertorriqueña, 1978). The story is presented by decades, from the 1820s through the 40s and 50s, when Puerto Rico’s slave imports finally ceased. Dorsey describes several of its more intense moments, mostly as they appear on the British Foreign Office accounts. A contemporary estimate of the dimensions of the illegal trade states that for every ship captured, 20 managed to escape; it can thus be surmised the implications of the 29 aborted attempts to land Africans in Puerto Rico between 1834 and 1859. (p. 122)

The British abolition campaign was fraught with ambiguities and likely to bend easily in the face of higher diplomatic concerns, especially with Lord Aberdeen as foreign minister in the 1840s. The fact that the nation states that had Caribbean possessions agreed to halt the trade would not be an obstacle for private French and Dutch parties on the West African coast to continue smuggling Africans out of the continent.

In its more attractive and innovative parts, the author presents the island’s role in the extension of the “odious commerce” in close connection to its West African side. Despite British diplomatic efforts to halt the extraction of Africans from still active distribution centers of the Western regions, roughly from Guinea to Nigeria, traders managed to evade the law. Internal supply was maintained and coexisted with other local economic activities of the chiefdoms and states, such as export agriculture. The British crusade was inspired not only by the humanitarian quests of the abolitionists, but by specific policies of African penetration.

Puerto Rico relied heavily on slave supplies brought by French, Dutch, and Danish traffickers, many times using their Caribbean islands as entrepôts. Dorsey reads attentively the nuances of the 1817 treaty between Great Britain and Spain for the prohibition of the trade. The Spanish colonial government found in it the loophole created by its failing to mention specifically that inter-Caribbean trade was also abolished, thus pretending that slave ships arriving in Puerto Rico’s ports brought Caribbean or Creole slaves, when it was really Africans coming in via the foreign island’s ports. Most of the innovative and thus attractive arguments of this book rest on fragmentary, albeit convincingly woven evidence. I do not consider this reliance on circumstantial evidence a defect. Rather I find this one of its most enjoyable traits. Previously hard to imagine internal interests in Africa come to life in Dorsey’s minute and imaginative construction of a story in which chiefdom politics intertwines with peculiar landscapes of marshes and multiple rivers that served as hiding places for smugglers. The effects of Islamic conversion in certain African areas are contradictory and multiple.

The contrasts with Cuba are tantalizing; Cuba was able to keep a stable and direct supply of slaves, mainly Yoruba, strongly tied to their African traditions. Puerto Rico depended on the lesser suppliers, more surreptitiously bringing out other African ethnic groups. The author seems to want to point to the consequences of this fact for the islands, either in the forms of slave experiences or in the political cultures. This argument remains rather obscure and undetermined. Understandably, the subject is complex and out of reach for the scope of the book. But the author would have benefited by framing these ideas in the experience of Puerto Rican slavery and the workings of slave owners in the plantations of the Puerto Rican seaboard. Slaves coming in through the port of Mayagüez, which Dorsey so well contextualizes and explains, were seemingly scattered individually or in small groups (perhaps as a result of cash shortages) throughout distant plantations of the island coastal fringe, to judge from the examples seen in the north.

Astrid Cubano-Iguina, University of Puerto Rico
Book Reviews


This book, written by two IMF economists and an academic for an audience of peers and policymakers, has two major strong points. First, it offers an effective guide to the modern literature on exchange-rate regimes and their potential—although not unambiguous and highly conditional—consequences on macroeconomic performance. Second, by adopting a “unabashedly empirical” approach, it tests theory against the experience of 147 IMF member countries in 1970–1999. A comprehensive bibliography and a CD ROM including the whole data base used by the authors are just additional valuable assets of the volume.

Atish Ghosh, Anne-Marie Gulde, and Holger Wolf start with a concise historical prologue. This chapter however serves only to emphasize that the collapse of Bretton Woods marked a watershed. A century-long era during which countries’ choices were “circumscribed by prevailing norms of the international monetary system” (p. 23) was then brought to an end, giving way to a “brave new world” in which governments became virtually unconstrained in their policy choices. However—and this is the book’s only serious flaw—no analysis whatsoever of the determinants of such choices is offered. In fact, the sizeable literature on optimal regime choice is barely mentioned in an endnote, and recent studies on the political economy of exchange rate policy (for a review, see J. Frieden, E. Stein, “The Political Economy of Exchange Rate Policy in Latin America: An Analytical Overview.” Inter-American Development Bank, Research Network Working Paper no. R-420, 2000) are simply ignored.

On empirical grounds, identification problems—that is, how to decide whether a country is actually running a pegged, an intermediate (such as a target zone or a managed float) or a full floating regime—are addressed by using a “de jure” classification that focuses on the central banks’ announced commitment. Moreover, the authors check the robustness of this forward-looking approach against a mixed “de jure-and-de facto” classification that takes into account the observed time path of the nominal exchange rate. This method allows them to exclude ambiguous cases such as “soft” pegs and “hard” floats, thus considerably improving the reliability of their results (pp. 46–51).

The authors provide empirical generalizations that compellingly demonstrate a strong link between pegged regimes and better inflation performance in the long run, confirming the existence of both a “monetary discipline effect” and a “credibility bonus.” As expected, the link was found to be more relevant for developing and emerging countries where alternative disciplinary devices, such as central bank independence or higher capital mobility, were weaker. Robustness tests controlling for country-specific fixed effects and for a possible bias stemming from endogenous regime choice (i.e., due to the fact that low-inflation countries are more likely to adopt pegs) confirm these findings (pp. 81–89). On the contrary, the evidence of a long-run nexus between exchange rate and growth performance is much more ambiguous. In fact, the authors find a growth bonus statistically associated with pegs, but, after controlling for a number of possible determinants, conclude that country-specific fixed effects and an endogeneity bias account for most of it (pp. 93–99).

Switching to a short-run analysis in the final part, Ghosh, Gulde, and Wolf suggest that the so-called Syndrome of Exchange Rate-Based Stabilizations, according to which peg-based stabilizations are more prone to fail, to lead to banking and currency crises, or to entail extremely high costs in terms of negative economic growth, should not be generalized. Durable disinflations, they argue, depend in the end on the extent of fiscal adjustment and monetary discipline achieved (p. 119), and the experience of Argentina in the 1990s demonstrates that a peg is no “magic bullet” without robust fiscal and structural reforms (pp. 135–47).
Although the volume has no explicit normative purposes, the authors convey a clear message in favor of pegged regimes combined with some degree of flexibility. To support this view, they show that, in the long run, floats do not necessarily entail greater volatility of the real exchange rate—indeed, they may help developing countries to reduce it by offsetting inflation differentials (pp. 54–60). They also insist that governments should learn carefully to design transition strategies in order to voluntarily exit unsustainable pegs, thus avoiding the “fear of exit” syndrome that so often in the past decade delayed exit until speculative attacks forced spectacular collapses. Elaborating on the drawbacks of Argentina’s currency board experiment in the 1990s, they actually suggest that a “floating currency board” might be used to get the right mix of credibility and flexibility (pp. 162–63). This policy suggestion adds further interest to a book that makes a valuable contribution to the current debate on the future international exchange rate system.

Stefano BattiLOSSI, Universidad Carlos III Madrid


A broad synthesis on the Great Depression by such a productive and original scholar as Harold James could only have much appeal. The result is surprising, not very homogeneous, but with many very interesting insights and much solid scholarship. The book has six chapters. The introduction presents the nineteenth-century globalization and its limits in a broad, interdisciplinary, perspective. The three following chapters are much more in the tradition of modern economic history. Their relative sizes reflect results both from the relevant literatures and from the previous works of the author, maybe exaggerating their actual relative importance: money and banking (chapter 2) as well as trade and tariffs (chapter 3) benefit from a much more detailed analysis (70 pages each) than the “reaction against international migration” (chapter 4) or chapter 5 on “the age of nationalism versus the age of capital,” with only 20 pages each. The book ends with a substantial “can it happen again?” conclusion.

The really good point of the book is its unification of three traditions of scholarships, up to now frequently separated—money, trade, and migrations—within a unique framework. The common framework is given in the introduction, which proposes a broad methodological perspective for the study of globalization, based on the hypothesis “that globalization fails because humans and the institutions they create cannot adequately handle the psychological and institutional consequences of the interconnected world. Institutions “... become the major channels through which the resentments against globalization work their destruction” (p. 5). This justifies the focus of the book on those institutions created by the nation states (themselves the highest level institutions protecting against global problems) to handle globalization: tariffs, central banks, and immigration legislation. In addition to that methodological proposal, which is developed throughout book, James also discusses globalization in a broad intellectual tradition from the Reformation to the “nineteenth century and its sins,” with the examples of Ortega y Gasset, Marx, Wagner and Lord Salisbury (among others) intellectual, artistic, and philosophic reactions against globalization.

Chapter 2 on monetary and financial problems is a perfect and less surprising synthesis of a now well-established literature. It places banking crises at the center of both the international and national crises, and somewhat less emphasis on the responsibility of the gold standard as a transmission mechanism than has been fashionable recently. It details the day-to-day events of the crisis, but also gives a more original account of its structural origins by insisting on the tensions between national (budgetary) and supranational (gold-standard) objectives as well as those between nations.
Chapter 3 on trade and tariffs gives a high level of responsibility for the depression to U.S. tariff politics and policies. It also insists on the links between the gold-standard crisis, the fall in capital movements, and the spread of tariffs. Chapter 4 on migrations is more brisk. It mostly convinces on the economic and political importance of the links between migrations on one side, welfare and trade policies on the other, not only during the depression but in a long-term history of globalization. Chapter 5 analyzes briefly the relationships between economic events and the rise of nationalism in the special case of the tensions between monetary policy and international politics in France and Germany in the years preceding the Second World War.

One excellent feature of the book is that James tells stories with historical actors well incarnated and present through archival documents and precise chronologies, even as he also integrates the most recent advances in the economists’ understanding of history. In the introduction, he tries to propose a global historical view of globalization and its problems, and I hoped he would answer K. Polanyi’s antiglobalization attacks. But most of the following chapters do not address the fears he had mentioned. In the conclusion, which I liked less, James turns to his vision of the contemporary world, a vision with surprisingly little nuance: the opening-up of any economy is for him always and undeniably a good thing; the 1980s and 1990’s path of liberalization both in international relationships and within national economies (privatization, labor market deregulation, etc.) resulted of “sustained reflection about appropriate policy” (p. 208), not of power (imperial) politics or of intellectual mode. As a consistent liberal, James not only thinks that today “there is no coherent intellectual package that links the resentment” (224) against globalization, but also that—structuralism being dead—only ethical problems remain, mostly with leaders of third world countries. Because he has little confidence in international institutions, it looks like the only way is to follow George W. Bush’s unilateralist policies. Not everybody will agree. But they should read this book.

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Technological determinism is back on the agenda—with a vengeance. Fundamental to modern economic growth, argues Joel Mokyr in this important and provocative new book, is the upsurge in the stock of “useful knowledge” that characterizes the last two and a half centuries. Not only has humanity acquired much more knowledge but the costs of accessing it have plummeted over the same period, thanks largely to new communications and transport technologies.

At the heart of Mokyr’s thesis is a carefully construed identification of two types of useful knowledge and the relationship between them. Propositional knowledge (episteme or Ε) he defines as a society’s collective understanding of the natural world, acquired through both the observation and classification of phenomena and the discovery of regularities or “natural laws” that describe their operation. Prescriptive knowledge (techne or Α), comprises all the techniques available for the manipulation of nature: at any time, only a small sub-set of these instructions or recipes is in use, and an addition to the set is “an invention.” If Ε is knowledge “what,” Α is knowledge “how,” and a technique cannot exist without an epistemic base, however narrow, in Ε. This is the crux: “the wider and deeper the epistemic base on which a technique rests, the more likely it is that a technique can be extended and find new applications, product and service quality improved, the production process streamlined, economized, and adapted to changing external circumstances, and the techniques combined with others to form new ones” (p. 14).
Mokyr insists that propositional knowledge is broader than “science”: it encompasses everything from folk wisdom about health and the weather to “engineering knowledge” (empirically derived formulae and procedures, based on classical mechanics and simple chemical reactions). Nevertheless, it is “science” that he privileges in his account of the widening and deepening of the epistemic base, which is at the root of modern economic growth. In particular, it is the emergence of a virtuous circle between episteme and techne in the mid-nineteenth century that he identifies as the key to the sustainability of that growth. What made the Industrial Revolution revolutionary was that, unlike all previous cases of economic growth, it did not fizzle out: the accelerating symbiosis of knowledge production tipped “the balance of the feedback mechanism from negative to positive” (p. 33).

In effect, Mokyr relegates the Industrial Revolution (1780–1830) to a preliminary (proto-scientific) phase, overshadowed by the “Industrial Enlightenment” of the eighteenth century. This cultural phenomenon provides the historical bridge between the seventeenth-century Scientific Revolution and the Second Industrial Revolution, which stemmed from the new intellectual resources offered by organic chemistry, metallurgy, electrical science, and thermodynamics. In the footsteps of Peter Mathias (“Who Unbound Prometheus: Science and Technical Change, 1600–1800,” in Science and Society, Cambridge: Cambridge University Press, 1972), Mokyr contends that the bridge was constructed less from new discoveries, more from new experimental methods, a novel faith in the intelligibility and tractability of nature, and a utilitarian (Baconian) ideology that recruited natural philosophy to the service of manufacturing and commerce. Thirty years on, Mokyr is able to embody this more subtle link in the networks of progressive manufacturers and natural philosophers that criss-crossed western Europe (most fruitfully in Britain), thanks to the research of historians such as Margaret Jacob (Scientific Culture and the Making of the Industrial West, New York: Oxford University Press, 1997) and Larry Stewart (The Rise of Public Science, Cambridge: Cambridge University Press, 1992). What merited fuller examination here were the means by which this ferment of largely amateur scientific activity struggled during the nineteenth century (especially in Britain) to achieve recognition and financial support from the state, the universities, and industry. Exploration of the social context in which science and technology developed after 1800 diminishes, however, in inverse correlation with the author’s emphasis on its increasing economic importance. There is always a danger too that this stress on “applied science” may lead to an underestimation of the difficulty of invention and of the elements of design and technical skill that it involves.

In a book that cries out for a closer engagement with the work both of other economic historians of the industrial revolution (especially J. R. Harris, Kenneth Pomeranz, and E. A. Wrigley) and of social historians of science and technology, it is a pity that Mokyr proceeds to pursue his ideas about knowledge at the expense of his thesis about the role of science and technology in industrialization. In chapter 4 he offers an ingenious, but ultimately unconvincing, contribution to the debate on the origins of the factory, which locates it in a concentration of in-house expertise. Chapter 5, while ignoring most of the relevant demographic and labor history, links the falling employment participation rate of married women with the early-twentieth-century decline in infant mortality, via the housewife’s wholesale adoption of the germ theory of disease: newly enlightened mothers simplistically swept and scrubbed their way to healthier babies (and exhaustion).

Mokyr returns to the plot in the final two chapters, with a theoretically insightful (if empirically sometimes outdated) exploration of the role of institutions in either promoting or resisting the adoption of new technologies. Ultimately, he acknowledges that the acquisition of useful knowledge involves not only “a game against nature,” but is also “part of the social game of people against one another” (pp. 284–85). An appreciation that such power games might influence the configuration of new knowledge at the point of its generation,
not merely filter it at the stage of innovation and diffusion, could allow a fertile rapproche-
ment between economic historians and students of science and technology.

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The End of Diversity? Prospects for German and Japanese Capitalism. Edited by Kozo
401. $49.95, £31.50 cloth; $24.95, £15.95, paper.

This volume of ten essays with an introduction is the second product of a collaborative
project examining the origins and development of the two major national variants of non-
liberal capitalism in the twentieth century. It focuses on the consequences of increasing
economic internationalization and recent technological transformations for the major institu-
tions governing economic activities in Germany and Japan, and considers whether these
developments are likely to lead to institutional convergence with liberal market economies.
Five main subject areas are analyzed in the ten substantive chapters: International relations
(Erica Gould and Stephen Krasner, Peter Katzenstein), innovation and technological change
(Kozo Yamamura, Robert Boyer), labor relations and production regimes (Kathleen Thelen
and Ikuo Kume, Ulrich Jurgens), financial and corporate governance regimes (Sigurt Vitols,
Gregory Jackson), and political and party organization (Steven Vogel, Herbert Kitschelt).

Adopting a broadly institutionalist approach to economic governance and organization,
the contributors seem agreed that, whilst some features of the political economies of Ger-
many and Japan have changed significantly in the 1990s, particularly in the areas of corpo-
rate law, accounting standards and relationship banking, others have altered much less,
notably those governing labor relations and levels of employer-employee commitment. On
both empirical and theoretical grounds, most authors consider the dominant pattern of
change in these kinds of cooperative or organized market economies to be one of hybridiza-
tion and differentiation between sectors and firm type rather than radical transformation of
their national institutional frameworks towards Anglo-Saxon patterns. This is partly because
the distinctive, path-dependent institutional regimes governing capital and labor markets
in these countries, as well as different political structures and cultures, lead key collective
actors such as investors, managers, and employees to understand their interests and appro-
priate ways of dealing with internal and external pressures differently to those prevalent in
liberal market economies. As a result, how they seek to reduce costs, innovate, and find new
markets continues to diverge from the low commitment societies of the United Kingdom
and the United States.

Such conclusions are not perhaps very surprising or controversial, except for market
fundamentalists, but the value of this volume, as with its companion one published in 2001,
lies in its detailed working through of the processes by which particular institutional frame-
works led various collective actors in Germany and Japan to understand and manage eco-
nomic and technical changes in specific ways. By showing how the national constellations
of historically developed institutions are resulting in distinctive macro and micro adjustment
processes, the authors make a significant contribution to our understanding of how differ-
ently organized political economies continue to generate contrasting forms of economic
organization and economic outcomes. Combining detailed analyses of changes in two
national governance regimes with their international integration and patterns of technical
innovation highlights the connections between these phenomena and suggests a number of
avenues for further work.

However, the book also draws attention to three major concerns about such broad com-
parative political economy approaches to the study of economic organization and develop-
ment. First, the appropriateness of the assumptions implicit in the framing of the central
question may be doubted. This is expressed by the editors as: “whether Germany and Japan, confronted with the political and economic exigencies of technological revolution and economic internationalisation, must thrust aside their distinctive institutions and the competitive advantages they have yielded in the past, or whether they will be able to adapt and retain such institutions and preserve the social cohesion and economic competitiveness of their societies” (p. 4). In particular, whether internationalization of competition and coordination inevitably leads to institutional convergence.

Accepting for the moment that growing international integration and the development of new information and communication technologies are the key factors impinging upon national variations in institutional frameworks and economic outcomes, which is certainly contestable, it is not at all obvious that these automatically generate strong pressures towards institutional convergence. As a number of the contributors to this volume have suggested elsewhere, they might equally encourage specialization and variation in prevailing governance systems, as differently organized and directed firms seek to develop distinctive capabilities and competitive competences that differ from those characteristic of firms in liberal market economies.

Furthermore, it remains unclear exactly which aspects of internationalization and technical change can be expected to affect which features of dominant institutions and firm behavior, and how, and it is a pity that none of these papers systematically considers these processes. By accepting this phrasing of the core issue to be considered, the editors and contributors seem to have tacitly accepted the view that cross national competition, imitation and digitalization are likely to encourage convergence to the norms of the dominant liberal market economy.

Second, in common with most work in the political economy tradition, it is assumed that the nation state is the inevitable and appropriate unit of analysis for considering economic development and change. There are, of course, some good reasons for focusing on national democracies as key political and economic entities in the late twentieth and early twenty-first centuries, but it would have been useful to have some discussion of why this is so, and under what circumstances it might change. At the very least, an indication of when we would expect national institutions to be so complementary and mutually reinforcing in their implications for economic organization and behavior that they would result in distinctive and homogenous outcomes would have been helpful. Presumably, not all states have such complementary arrangements, and indeed a number of contributors indicate that earlier complementarities of institutions governing financial and labor markets are breaking down, so a synthesizing account of the circumstances in which various kinds of complementarities should, and should not, occur at different territorial and political levels, with their likely consequences, would have added greatly to this volume.

Similarly, a systematic analysis of how much change in one arena, such as capital markets, would be required for the basic nature of the institutional framework of a country to be judged to have qualitatively shifted would have been welcome, as remarked by the editors. The contributors might additionally have considered how and why some continental European countries, such as Denmark and the Netherlands, seem to have managed to develop more flexible external labor markets without fundamentally altering their largely coordinated nature, as mentioned in passing by Kitschelt. Such examples further emphasize the contingent and variable nature of national institutional cohesion and complementarities.

Third, the dominant focus on national institutions and actors, characteristic of much work in the political economy tradition, downplays, if not completely ignores, the role of firms in constructing economic outcomes. Although a number of contributors do draw attention to important differences between large and small companies, and between firms in different sectors with varying exposure to international markets, in explaining both macro and micro adjustment patterns, relatively little attention is paid to specifying how different groups and
firms deal with international competition and technical change. If these pressures are regarded as the key factors inducing institutional change in Germany and Japan, then presumably certain kinds of firms and groups dominating them are the major mediating actors that are likely to encourage, or inhibit, modification of current arrangements. A more systematic account of how particular kinds of firms, investors, managers and employee groups are functioning in this role would, then, have been especially useful.

These points apply to much recent work in the comparative capitalisms literature, and are by no means only relevant to this book. However, the interesting analyses presented here do highlight these major issues for the institutional analysis of economic development and change and emphasize the need for them to be carefully considered in future work.

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