Book Reviews

ANCIENT


This book, a translation of the second half of an even more massive French work published in 2003, provides a detailed account of the first 350 years of Roman rule in the Near East (covering modern Syria, Lebanon, Israel, and Jordan). The first five chapters examine the Hellenistic background of the region, the establishment of Roman control, and the subsequent development of provinces and client states up to the early third century AD. The following five chapters deal with civic life and urban development, the countryside, the urban economy, culture change, and the complex religious landscape of the region, while a final chapter looks forward into an episode of temporary instability in the mid-third century AD. Throughout the volume, Maurice Sartre draws on a wealth of literary sources, inscriptions, and archaeological evidence to create a synthesis of the properties of political, military, economic, and ideological power in this region when—for the only time in history—it was firmly embedded within a pan-Mediterranean empire. In this period, government sought to extract surplus without pursuing far-reaching change or homogenization, and cultural hybridization accompanied the spread of Greek language and elite styles.

Readers of this JOURNAL will be most interested in the two chapters on economic conditions. Rural production, the subject of chapter 7, underpinned the regional economy. Parts of Syria have yielded the ruins of hundreds of ancient stone-built villages that afford a unique glimpse of rural living conditions. Textual sources record the existence of imperial estates (most notably encompassing the famous cedar forests of Lebanon), whereas evidence of temple estates is missing. Private landownership appears to have been the dominant mode of land tenure. Stratification in landed wealth is often hard to document and was subject to considerable geographical variation, from strong concentration of property in the hands of urban elites in some areas to lesser degrees of inequality in more self-contained village communities in other parts of the region. Labor regimes are almost completely unknown outside Palestine, where we encounter various kinds of tenants and wage laborers. Given ecological constraints, the water supply was a critical variable, and Sartre collects evidence for irrigation in large valleys and oases. Roman rule brought a measure of change in production: while grains had long been local staples, olives were first introduced on a grand scale in this period, and viticulture also expanded. Sedentary farmers continued their uneasy co-existence with the nomads that inhabited the marginal peripheries.

Chapter 8 shifts the focus to the urban economy, providing long lists of products associated with local artisans: textiles, purple dye, glass, and pottery loom large in the record. Roman rule intensified the process of monetization that had commenced in the preceding Hellenistic period, as imperial denominations and local currencies circulated side by side. Ports and a network of paved roads facilitated the transfer of goods: at the same time, long-term peace and the consequent reduction of transaction costs, as well as the supply demands of the Roman military, may have been even more vital factors that Sartre does not expressly consider. Patterns of trade are tentatively reconstructed from the distribution of archaeological finds. The region
as a whole did not export large quantities of foodstuffs, and tangible evidence of trade is largely limited to durable objects such as ceramics and building stone. Moreover, the region served as a conduit for the international luxury trade, by ship and caravan, from Iran, Arabia, India, and China into the Mediterranean.

Unfortunately, any attempt at economic analysis is missing from these chapters: Sartre’s account is empirical and source-driven to the extent that it borders on antiquarian, eschewing any consideration of broader interpretive frameworks such as Keith Hopkins’s “taxes-and-trade model” which holds that Roman provincial economies were stimulated by reciprocal flows of taxed and traded resources and has been taken up by Nigel Pollard (Soldiers, Cities, and Civilians in Roman Syria, Ann Arbor: University of Michigan Press, 2000: 171–250). Tentative quantification, which features prominently in Ze’ev Safrai’s The Economy of Roman Palestine (London: Routledge, 1994), is likewise conspicuous by its absence. Because of their limitations, these chapters are useful above all as quarries for future interpretation.

Endowed with 272 pages of endnotes and bibliography, this work digests a prodigious amount of primary evidence and modern scholarship, and will long serve as an invaluable resource for academic audiences. However, those looking for an up-to-date survey of the region may also want to consider Kevin Butcher’s splendid Roman Syria and the Near East (Los Angeles: Getty Publications, 2003), which is not only similarly comprehensive and erudite but adds another dimension by offering five times as many illustrations as Sartre’s book.

WALTER SCHEIDEL, Stanford University

ASIA AND PACIFIC


This book offers an explanation for the two contrasting economic paths taken by Taiwan and South Korea after World War II. In particular, it tries to explain why the two countries, facing similar challenges, evolved very different economic organizations. The book is a combined effort by an economist and an economic sociologist. It is divided into two parts: the first primarily theoretical and the second empirical.

Part one begins with a brief review of how business networks have been explained by economists and sociologists. Then chapter two gives a concise overview of South Korean and Taiwanese business groups and how they differ. The authors show that Taiwanese business groups are not only much smaller than Korean groups but also play a more limited role in the economy. Whereas Korean groups are found everywhere, Taiwanese groups are concentrated in domestic upstream and service industries. Previous research has tried to explain the existence of business groups as a response to pre-existing social and political conditions. The authors argue that competition and cooperation among businesses is itself an important factor shaping the economy, and thus business group formation is better explained by a nonlinear general equilibrium model in which business decisions and social and political factors all interact to determine what type business groups develop. The authors’ business group model is that developed in Feenstra, Huang,
and Hamilton ("A Market-Power Based Model of Business Groups." Journal of Economic Behavior and Organization 51, no. 4 [2003]: 459–85) and is described in chapter three. The model assumes that the economy has an upstream and downstream sector and that each sector is characterized by monopolistic competition. Business groups are formed to enforce profit-maximizing, two-part-pricing agreements between upstream and downstream firms. A key feature of this model is that there are multiple equilibria so that similar economies may follow different paths, developing very different business group patterns, which themselves are causative forces shaping the economy. Chapter four quantifies the differences in South Korean and Taiwanese business groups and shows that the model presented in chapter three can be used to simulate many aspects of the two economies.

The second part of the book does not rigorously test the business group model presented in chapter three, but uses the model as an “ideal type” to help make sense of the two contrasting economic stories Taiwan and South Korea present. Chapter five describes the differing political and social conditions in the two economies. This chapter argues that long-term social conditions were key determinants of the different paths taken by the two economies and the contemporary political policies were largely endogenous and played a reinforcing role. Chapter six describes the exogenous rise in international demand to which Taiwan and South Korea responded. This rise in demand is attributed to the retail revolution in the United States. Chapter seven describes in concrete terms how United States retailers and producers in Taiwan and South Korea found each other. Japanese trading firms played a key role in this process, but due to social and economic differences, matching between retailers and producers evolved differently in the two contrasting East Asian economies, affecting their economic organization. Chapter eight shows that Taiwan and South Korea’s export patterns diverged as the economies developed, with Taiwan producing a much greater variety of lower-quality niche goods while South Korean business groups concentrated on fewer types of higher quality goods. This pattern is what the authors’ theoretical model would predict.

This is a stimulating book, which should be on all economic library shelves. The book’s primary strengths are, first, its coherent weaving of insights gained from both economics and economic sociology and, second, its breadth of vision. The authors not only gain insight into various industries by considering each industry’s relationship to the overall national economy, but also advance a strong argument for export-led growth based on their research into the relationship between these two national economies and the world economy. The authors succeed in their limited goal of showing the plausibility of a market-power model, but tests of whether this model is more plausible than competing models, such as new institutionalist models, is largely left to further research.

Although the authors show a breadth of vision, limited resources have forced them to focus on only two economies, and this weakens support for some of the authors’ conclusions. For example, the Hong Kong economy and its exports bore a close family resemblance to the economy and exports of Taiwan, but the Hong Kong economy took off ten years before the Taiwan economy and in the early years relied heavily on exports to British markets. Thus, although the chapters of this book focusing on the U.S. retail revolution and its effect on East Asia are one of the most interesting parts of this book, one is left wondering whether the importance of the U.S. retail revolution has been exaggerated.

KELLY B. OLDS, National Taiwan University
Korea under Siege, 1876–1945: Capital Formation and Economic Transformation.

This book is a welcome addition to the literature. Based on the works and raw data published in Korean and Japanese, the author studied the heritage of the Korean economy in the traditional and transition periods and concluded that “had Korea remained an independent country, its economy would probably have developed eventually, but at a later date, at a slower rate of growth, and with greater economic sacrifices to Korean people (in order to save more to make up the foregone Japanese investment).”

The book consists of ten chapters. Chapter 1 is the introduction, which lays out the plan of the book. In chapter 2, the author discusses and evaluates economic conditions and different forces that affected capital formation and economic development in traditional Korea in the 1870s. The reasons for Korea’s underdeveloped economy included the low education level of the people; low propensity to work and produce; the lack of savings (due to the high propensity to consume); the lack of capital; and the ineffective and oppressive Korean government, which controlled tightly the commerce and industrial sectors.

In chapter 3, the author discusses how the outside world affected the traditional economy of Korea, Korea’s response to the incursions, and the impact of these changes on Korean investment and economic transformation between 1876 and 1904. This was the period when foreign goods and foreign (mainly Japanese) investments were introduced to Korea. The foreign goods and foreign investments opened the eye of the Koreans and made some fundamental, although small, changes in the Korean economy.

Korea became a Japanese protectorate in 1905. In chapter 4, the author discusses economic reforms and investment under the 40 years of Japanese rule (1905–1945). When Japan took control of Korea, it instituted numerous reforms to transform the backward and traditional economy into one that served the needs of the colonial power. It removed numerous obstacles and established infrastructure to accommodate the workings of a government-controlled but market-oriented economy. The reform measures not only involved changes in the physical environment, they also altered public attitudes and beliefs toward business investment and savings. Western-style education and practical training were provided to Korean workers.

In chapter 5, the author discusses the motivation for investment under Japanese rule. The unique features and patterns of investment differed for segments of the economy, between public and private sectors, and between nationalities. In general, most of the investment came from the Japanese, and the colonial government played a crucial and unique role in investment in Korea.

In chapter 6, the author examines how the colonial government promoted capital formation and transformed the Korean economy from a transitional economy to a modern one. The colonial government used policies such as low taxes, generous subsidies, and financial loans to strategic industries to induce private investors to invest in new sectors that Japan promoted. Although the government polices in general had positive effects, the excessive intervention of the government led to a high incidence of bad loans, and often shifted spending away from socially beneficial investments toward the building of a wasteful military-industry infrastructure, which helped Japan prepare for the war effort. It thereby caused inflation, inhibited economic growth, and ultimately constrained the improvement of living standards of the people. It also tended to aggravate income inequalities and poverty and reduce domestic savings while discriminating against Korean and foreign businesses.
In chapter 7, the author examines the mobilization of savings and resources of both foreign and domestic sources for financing investment in Korea.

In chapter 8, the author shows that there were substantial gains in the production of goods and services during the period when Korea was under Japanese rule. During this period, the Korean economy changed from a predominantly agrarian to a semi-industrial economy oriented toward serving the needs of Japanese empire. Additionally, the industrial sector expanded more rapidly than the primary sector, and the heavy and chemical industries became as important as the light industries.

In chapter 9, the author examines the impact of capital formation, economic development, and structural changes on income distribution, consumption, and saving for investment under Japanese rule. The author shows that the gain from economic growth was not shared equitably. Although farm income increased modestly, most of the gain went to the landlords and the Japanese farmers. Just like the local farmers, the Korean workers also had only modest gains. In the labor market, the Japanese held most of the well-paying, white-collar, managerial, professional, and skilled positions, and the Korean workers typically held most of the low-paying positions. The most important findings of this chapter is that while the standard of living for some upper-strata Koreans improved, the majority of Koreans did not really benefit much.

As a whole the author has done a good job. The literature written on this period by the Japanese tends to conclude that it was the Japanese colonial government that modernized the Korean economy, whereas the literature written by the Koreans focused on the negative effects of the oppressive colonial government. The author is quite successful in providing an objective study of the economic policies and development process during the colonial period. Nonetheless, the study is not without defects. Most of the data (except per capita GDP) were provided in current value. As inflation was a problem especially during the 1930s and 1940s, data without adjustment for inflation were quite difficult to interpret. Also, while most of the data were in yen, some were in won. It makes it difficult to comprehend. On top of these, it would be better if the author could discuss briefly how the political environment under the Japanese rule affected the people, as to a certain extent this would affect the economic decisions of the Koreans.

As a final remark, anyone who wishes to understand the pre–World War II Korean economy should read this book.

MAN-LUI LAU, University of San Francisco


The 11 chapters composing this volume are mainly papers first presented at the meeting of the European Association for Japanese Studies at Warsaw in 2003, along with an introduction by the editors. This volume in Routledge’s Contemporary Japan Series, not intended to be a comprehensive economic history of Japan, adds to our knowledge of that nation’s development by illuminating selective topics. The focus is on Japan after World War II, with six of the ten substantive essays dealing with that time period. Inspired by the work of Douglass C. North, all of the contributors examine how institutions (people in institutions) have affected Japan’s economic development. Some see the roles as being positive, but others view them as more nega-
Business and economic historians interested in manufacturing will find the essays on copper mining, brewing beer, and making cotton textiles to be of most value. Patricia Sipple provides a careful description and analysis of changes taking place in Japan’s copper mining and smelting industry during the Tokugawa and Meiji periods. She finds that “although Western technology transformed mining in the second half of the nineteenth century, knowledge and practices developed before and during the modern era were also important” (p. 13). She emphasizes “the roles of actors at the political and economic center,” especially of government employees, and mining engineers (p. 13). Adopting the approach of Alfred D. Chandler Jr., Harald Fuess explores the development of large, heavily capitalized beer makers from the 1880s into the late 1930s. A mix of consumer tastes, changing technologies, and financial requirements, he shows, led to the rise of big businesses in brewing. Helen Macnaughtan tells how cotton textile companies tried to deal with their women workers in new situations they faced between 1945 and 1975, circumstances shaped in part by American ideas about labor relations (mediated, however, by continuing Japanese practices) and in part by growing labor shortages, especially of young women.

Moving beyond industry, a number of essays focus more broadly on the changing nature of the firm and interfirm relations in recent decades. Andre Moerke chronicles the erosion, but continuance, of keiretsu under the impact of both domestic and global economic changes, concluding “It seems that keiretsu are here to stay—weakened, but existing” (p. 88). Sigrun Caspary examines mergers and acquisitions involving Japanese companies with foreign firms, looking especially at the major increase in their numbers from the mid-1990s onward. After presenting a valuable statistical overview, he investigates in detail the development of Renault’s alliance with Nissan in 1999 (following failed Nissan-DaimlerChrysler negotiations) and DaimlerChrysler’s alliance with Mitsubishi Motors a year later. Caspary then examines the reorganization of the firms and their marketing methods.

Several chapters will be of most value to scholars interested in financial history and Japan’s economic troubles in the 1990s. In a lengthy piece, Takaaki Suzuki argues persuasively that globalization has not eliminated the roles states play in economic matters. Looking specifically at global financial issues, he shows that “the Japanese state actively intervenes in the economy despite, and to some extent because of, the rise of global finance” (p. 92). Mariusz K. Krawczyk offers a cogent explanation of Japan’s banking crisis and financial downturn in the 1990s. While examining the impacts of recent events such as the reevaluation of the yen in 1985/86 and mistakes made by the Ministry of Finance in the 1980s and early 1990s, he also looks, to his credit, at the importance of long-term structural problems as the main cause for the difficulties, concluding that “Japan’s banking crisis was not caused by a single regulatory or macroeconomic factor but has its origins in a set of deficiencies deeply rooted in the country’s post-war institutional and corporate culture” (p. 121).

Institutional and Technological Change is a rich volume, well worth selective reading. Beyond the essays just described, other chapters examine the early history of economic thought, modern environmental controversies, and the changing nature of forecasting in Japan. Historians dealing with a wide range of issues will find parts of this collection valuable.

MANSEL G. BLACKFORD, The Ohio State University

China developed industries with complex technologies and elements of modern business organization more than a millennium ago, but we still have only a relatively small number of studies in depth of most of these industries, and many of these studies are of industries that developed in connection with the opening of China to investment from Japan and the West at the end of the nineteenth and the beginning of the twentieth centuries. Exceptions to this focus on industries developed in response to the Western impact include major works on silk, sugar, and iron and steel that cover periods going back as early as the Tang and Sung Dynasties, but the number of such studies is not sufficient to give us a very complete picture of the nature and sophistication of premodern Chinese industry. This study by Madeleine Zelin of the Zigong salt industry, therefore, is a major contribution to the development of a more complete understanding of China’s premodern industry and Chinese business practices more generally.

Zelin’s study of the Zigong salt industry focuses on the development of the industry in the nineteenth and twentieth centuries when the Western impact of China was well underway, but Zigong was in Sichuan in the western part of China that was little influenced at least in the nineteenth century by modern technology and modern business practices then being established in China’s coastal cities. The technologies and business organization and practices used by this Sichuan branch of the salt industry thus were developed independently of Western influences for the most part. This book, therefore, is a study of how a Chinese industry developed in a traditional environment not unlike earlier periods in Chinese history when Western influences played no role at all.

Another feature of the Zigong salt industry that makes this study so valuable is that the salt industry in Sichuan was based on deep wells (some as deep as 3,000 feet) and the conversion of brine into salt was done with the aid of natural gas that was present near the wells. Given the well drilling technology available at that time and place, drilling a deep well could take years with no guarantee of success and thus considerable amounts of capital were involved. Once the salt was produced, it then had to be shipped long distances to where it was consumed. Acquiring the capital needed particularly in the early stages of the industry necessitated the development of new forms of business organization and enforceable contracts. Over time these business organizations evolved in many cases into large vertically integrated firms with more than 1,000 employees. Zelin describes the evolution of business organization, premodern financial institutions, and other related institutions in rich detail.

Because taxes on salt were also a major source of revenue for the government, this study is also a valuable source of material on government business relations in both the Qing Dynasty and Republican periods. As the book makes clear, the government’s interests was almost exclusively focused on ensuring as large a flow of revenue as possible, but this objective could lead to licensing interventions that on occasion had a major influence on the way the private business of salt manufacture and distribution was organized.

While the greatest value of this study is what it tells us about the way premodern business in China was organized and evolved, the book also has interesting chapters on how the situation facing the Zigong salt merchants changed in the twentieth century. In part the change was brought about by new technology (steam-powered pumps), but politics also played a major role in the decline of the salt business in the
1910s and 1920s. The book describes in some detail how warlord operations in Sichuan in this period forced the salt firms into heavy borrowing to meet repeated warlord demands for funds and how salt merchants attempted to deal with the situation by establishing ties with one military figure or another.

Any book that deals with such a complex subject inevitably does not answer all of the questions about this industry that a reader might have. In my case I would have liked to see more about how contracts were actually enforced. Zelin makes it clear that they were enforced and by some kind of legal process, but it is not entirely clear what the nature of that process was and who the judges were that handed down the rulings. Given the importance of this topic in understanding Chinese economic history, more detail on this topic would be justified. That said, this review has not even mentioned some of the topics that are covered that are also of considerable interest to economic historians. There is, for example, a chapter on labor conditions, on the way labor was organized, and the history and outcome of major strikes.

Zelin’s study, therefore, is a major contribution to our understanding of Chinese business and industrial development both in the premodern period and in the first half of the twentieth century. It is also an important contribution to comparative economic history because Zelin regularly relates the experience of the Zigong salt merchants to early industrial development and business organization in the Western world. As she points out, there were as many similarities between the institutions of premodern China and those of the West at a comparable stage of development as there were differences.

Dwight H. Perkins, Harvard University

UNITED STATES


What’s “new” about The New Suburban History? On the one hand, this volume of collected essays, edited by Kevin M. Kruse and Thomas J. Sugrue, includes histories of “new suburbs,” areas that belie the suburban stereotype of the uniformly white, middle class, and residential. Some of the towns we visit are peopled with striving African-Americans and recent immigrants, while others are host to a large industrial base or a string of high-tech corporate campuses. But, in a more conceptual sense, this is a “new history” of the suburban form itself. The editors lay out a framework in which the unit of analysis is neither the city nor its suburbs in isolation, but instead is the whole metropolitan area treated as a fragmented set of local governments competing with each other for resources.

The best entries in this somewhat uneven volume are those that hew closest to this framework. In “Prelude to the Tax Revolt,” Robert O. Self provides the backstory to California’s Proposition 13, a groundswell against property taxation in the late 1970s which capped tax rates and limited reassessments. Self narrates the breakdown of a decades-long “suburban grand compromise,” under which towns kept residential tax rates low by attracting an industrial base and using corporate taxes to pay for public services. However, as “cities across the state compet[ed] for business investment . . . a constant pressure [built up] on local municipalities to redistribute the tax burden
away from business” (p. 155). With the vise tightening, tax payers revolted, perhaps not realizing that the golden age of the industrial suburb had come to an end.

Competition between suburbs and the central city also, in many cases, doomed the court-ordered desegregation plans of the 1960s and 1970s. County-wide school districts experienced smaller declines in white enrollment than cities whose school districts stop at the city boundary. In his essay “‘Socioeconomic Integration’ in the Suburbs,” Matthew D. Lassiter takes us inside the consolidated school district of Charlotte-Mecklenburg, North Carolina to investigate how this success is built from the ground up. After the initial court decision against the district, rich and working-class whites joined to protest the ruling. The first rift in this cross-class alliance arose from a proposed boycott of the public school system as working-class families complained that private schools were beyond their reach. The alliance frayed further over a bussing plan that exempted the outlying rich areas. Eventually, the coalition collapsed and the size of the school district ensured that well-off whites were not able to escape the consequences.

In these essays and others, political fragmentation is seen as an inherent but unfortunate part of the American landscape. Competition between jurisdictions leads to a race to the bottom, as towns vie to attract industry (Self), or allow the privileged to avoid their responsibilities to the polity (Lassiter). Indeed, the editors focus only on the costs of competition, such as the potential to “replicat[e] inequalities through the creation of suburban municipalities that provide differential services to their citizens” (p. 6). Any benefits of decentralization, including the evolution of a broader menu of local services from which to choose, are ignored. There is no mention of a Tiebout-style process, by which those who value public services most (usually the rich) sort into towns with high tax rates (Charles M. Tiebout, “A Pure Theory of Local Expenditures,” *Journal of Political Economy* 64, no. 5 [October 1956]: 416–24). Also conspicuously absent is Paul E. Peterson’s seminal *City Limits* (Chicago: University of Chicago Press, 1981), which first emphasized the limits on local redistribution imposed by interjurisdictional competition. All told, the editors cite from a closed circle of historians, barely engaging with the more theoretical contributions from economics or political science.

These concerns aside, the essays on local government are more exciting than those on the federal government. Arnold R. Hirsch reprises his important but already well-documented claims that the exclusionary lending practices of Federal Housing Administration (FHA), coupled with urban renewal, which cleared many black neighborhoods and built segregated public housing in their stead, are responsible for the racial residential segregation of today (see Arnold R. Hirsch, *Making the Second Ghetto: Race and Housing in Chicago, 1940–1960*. Cambridge: Cambridge University Press, 1983). He reviews policy statements from a veritable alphabet soup of federal agencies —alongside the FHA, the VA (Veterans Administration), the HHFA (Housing and Home Finance Agency), thePHA (Public Housing Administration), and the DSCUR (Division of Slum Clearance and Urban Redevelopment) each make an appearance—responsible for homebuilding or lending in the postwar period. Rather than formulating a consistent policy on the question of race, these agencies “paid . . . deference to localism,” allowing each city to deny mortgage credit to blacks, if they so desired (p. 55).

This essay adds to our knowledge of what the federal government *did* to foster segregation. But, the historical literature on segregation will not move forward until we have a sense of the plausible counterfactual—how would residential space have been arranged if the federal government had *not* condoned discriminatory lending?
David M. P. Freund’s essay indicates that, while a small share of postwar mortgages were underwritten by the FHA, private banks followed the FHA’s lead in denying mortgages to blacks. Was the private sector simply copying the government, or were its mortgage decisions an independent response to lower incomes and higher probability of default in the black community? And, if not for these mortgage restrictions, would blacks have achieved their dreams of suburban homeownership, or were the majority of black households too poor to afford suburban housing, with or without mortgage assistance?

A more novel contribution on the role of the federal government is Margaret Pugh O’Mara’s essay on high-tech suburbs. O’Mara asks why, if agglomerations are important for innovation, did many high-technology firms choose to locate in the suburbs? She traces this pattern to the Cold War, during which firms with defense contracts were encouraged to locate on the suburban fringe, far from perceived nuclear targets in cities. Firm location was also dictated, in part, by the location of universities, which tended to be in small college towns. Stanford is the quintessential example of a research-industrial alliance, sustained by federal grant money. While Stanford’s “low-rise, heavily landscaped, and distinctively suburban and western look and feel . . . [has been] deliberately imitated [by many] would-be cities of knowledge,” O’Mara argues that the role of the suburbs in this process is more than simply aesthetic (p. 73). Rather, the safety and amenities of suburban living appeal to a highly trained workforce of scientists and engineers. As a result, urban campuses have often failed in their attempts to imitate Stanford’s technology center.

Finally, despite being out of place in this volume, Becky Nicolaides’s punchy tour of “How Hell Moved From the City to the Suburbs” is well worth a read. Nicolaides demonstrates how, over the past century, the “environment that seemed most harmful to authentic community—most likely to kill off any chance for meaningful human interaction . . . moved from the city to the suburbs” (p. 80). The soulless, anonymous city of Fritz Lang’s Metropolis, with its hulking skyscrapers, was replaced by the conformist, and equally soulless, suburbs of the Stepford Wives and later of American Beauty and The Truman Show.

LEAH PLATT BOUSTAN, University of California, Los Angeles


Sanford Jacoby, a professor of management, policy studies, and history at UCLA, has produced a fine comparative study of corporate management in Japan and the United States. It can be read as a work in business administration or in business sociology. It is also explicitly historical, and as a student of Japanese economic history, I will focus here on its Japanese and historical sides.

Jacoby deals with personnel systems, a subject that builds on his earlier books on U.S. employment practices (Employing Bureaucracy: Managers, Unions, and the Transformation of Work in the 20th Century [revised edition, Erlbaum, 2004] and Modern Manors: Welfare Capitalism since the New Deal [Princeton University Press, 1997]). In U.S. corporations, as Jacoby indicates and as I can testify from my own pre-academic work, personnel/HR departments generally have a peripheral status. The hiring of professionals and managers is conducted by the units concerned, as are promo-
tion and reassignment decisions. Most U.S. employees deal with HR staff only on the
day or two after they are hired, the day that they leave, and when they have questions
about their insurance or other benefits. The gendered division of labor and the low
status and “service” character of HR is indicated by the fact that this is one of few U.S.
corporate functions in which women routinely become executive managers. There
have been significant ups and downs in the position of U.S. personnel departments, as
Jacoby’s history indicates in summary but satisfying detail. Wartime labor shortages,
labor disputes, and government regulations especially enhanced the importance of the
personnel department, with World War I bringing a first “personnel boom” (p. 81).

When it comes to Japan, Jacoby’s subject has, if anything, a more pivotal impor-
tance than he himself claims for it. As the scholar of Japanese law John Owen Haley
once memorably described it, the only American institutions in which the central per-
sonnel administration has an authority comparable to that of a Japanese corporate per-
sonnel department are the branches of the U.S. military and the Catholic Church. A
key similarity here is permanent membership—or the lack of a good “exit” option, as
Jacoby puts it—as regular male “members” (seishain) of a Japanese corporation be-
long to the organization for their entire careers. The authority of the personnel depart-
ment in the classic Japanese big corporation extends throughout a company member’s
career, from recruitment and hiring to training, performance evaluation, transfers, and
promotions. These decisions largely determine who will come to sit on corporate
boards. The personnel function is both authoritarian and welfare oriented, as its duties
include labor relations (with company unions, whose offices are often physically
housed within the personnel division), setting salary and allowance policies, and man-
aging recreation, housing, and other welfare functions. The personnel department’s
power over tightly held information is backed by its function of maintaining dossiers
on all company members. Thus, like the U.S. military or Catholic Church, the classic
Japanese corporation might aptly be compared as an employing organization to a min-
istry or firm in a classic socialist state. All of this makes the personnel department a
powerful and prestigious part of the Japanese corporation, and managers on the elite
management track routinely cycle through an assignment there on their path to the top.
However, as Jacoby also details, this authority has tended to break down to an extent
amid the recession and restructuring of the 1990s.

By means of survey data and careful field studies of Japanese and U.S. companies
in diverse industries, Jacoby also indicates that this is not a case of undifferentiated na-
tional contrasts. He examines comparable Japanese and U.S. companies in the electri-
cal, electronics, auto-parts manufacturing, package delivery, construction, and finan-
cial securities industries, revealing various industry-specific dynamics that create their
own consistent cross-national similarities. This book thus becomes relevant to the
study of the corporate “cultures” specific to individual industries and firms as well as
to the dynamics of national-level business cultures.

Among the larger subjects that Jacoby brings into play are the complexities of intra-
corporate power politics—usefully analyzed here as distributional struggles—and their
systematic relationship to larger macro-social shifts (e.g., in the mix of “market” and
“bureaucratic” logics within corporations). This is the social “embeddedness” to which
he refers in his title. Among these macro-social dynamics are globalization and finan-
cialization; here, against the prevailing impression, Jacoby concludes that Japanese
and U.S. corporate practices and governance are now diverging more than they are
converging.

Altogether, this is a important comparative study of the modern corporation as such,
rich in detail and insight, philosophically and methodologically attuned, and much big-
ger in its scope than its subject matter might suggest at first glance. It is also nuanced, balanced, and well written. It could function well as a classroom text and is first-rate as a contemporary business book. The footnotes are extensive; unfortunately there is no bibliography, which would have been very useful. As a work of both business and labor history, it complements histories of Japanese employment practices such as those of Andrew Gordon and belongs next to the classic institutional studies of R. P. Dore. Combined with recent work on the transformation of the Japanese employment system by scholars such as Charles Weathers and the many valuable contributions on the subject in the Social Science Japan Journal, it brings a new deepening of the ongoing English-language dialogue on the Japanese employment system.

MARK METZLER, University of Texas at Austin


Gavin Wright’s Slavery and American Economic Development derives from his 1997 Fleming Lectures at Louisiana State University, and deals with a theme to which he has perceptively contributed for a long time. With judicious use of quantitative analysis, including 39 tables, figures, and maps, and detailed regression analysis, Wright carefully presents his view of the antebellum southern economy. From the start, I must confess to not being a disinterested party, as we have long debated these issues, although that has not detracted from our warm and mutually respectful relationship over the years.

Wright describes his book as representing “an effort at reframing several debates relating to the economics of slavery, emphasizing the economic concept of property rights, and the historical context of the onset of modern economic development in Europe and North America between the eighteenth and nineteenth centuries” (p. ix). The former issue concerns Wright’s wish to distinguish the analytic usefulness of describing the importance of property rights over slaves from those definitions that focus on slavery as primarily a form of labor relations and production methods. It was the property rights that permitted slaves to be purchased and sold, relocated by owners, and assigned to any task that the owner wished, among other aspects of control. Wright contends that the “apparent productivity performance of slave labor in the antebellum era may be attributed more aptly and consistently to property rights than to work organization and physical efficiency” (p. 122). While it might be argued that without property rights slavery would not be possible, presumably in the absence of the appropriate conditions for production and labor, the demand for these property rights might not exist. The second contention described in the preface represents a defense of both aspects of the Eric William thesis, with the early growth of the slave economy leading to economic growth in Europe and the Americas, but then a reduced economic importance of slave labor, making slavery an expendable institution, to be replaced by free labor. Wright concludes that, in general, Williams may have a correct conclusion regarding the relation between abolition and industrialization, but not precisely for a correct reason, because the affinity “may have been more curiously circuitous than he imagined” (p. 40). Wright contends that “one would have difficulty making a case that slavery had retarded economic progress in the South in the colonial era” (p. 57). But, he argues, measures of population growth and wealth in 1850 and 1860 indicate that in these antebellum years slavery had not only led to a relative (but ap-
parently not absolute) worsening of the economic conditions of the South in this period, but also “that the roots of postbellum regional backwardness are plainly visible in the antebellum data” (p. 61). This theme of southern antebellum backwardness remains a central argument of Wright’s analysis of the southern economy.

Wright’s arguments on the southern economy are, however, not free of controversy. There are two general points to note about many scholarly debates. First, what appear to be disagreements about answers to questions often reflect rather the asking of different questions, which then can explain the differences in interpretation. Second, in long-standing debates, such as that over American slavery, there is often some convergence of belief on some issues. In the case of southern slavery, many of the currently agreed-upon set of interpretations represent a significant shift from the views that were predominant in the past. Thus Wright’s depiction of the southern planters’ search for profits, the variety of work undertaken by slaves, and many aspects of southern markets portray a view of the southern economy quite different from the earlier widely held views of Phillips and his students.

There are occasional puzzling aspects of Wright’s descriptions of the late antebellum period. He correctly points out that because cotton is only a fraction of southern output, to focus on cotton alone in describing the southern economy is misleading. Yet he points to the “far above normal” level of cotton production and exports in 1860 (p. 92). There is no disagreement upon the high level of cotton output that year, but some debate as to whether this reflected input shifts, random movements, or other factors that might have reduced total southern production. Wright’s principal new source concerning the high level of cotton output indicates that, for whatever reason, there was a relative shortfall in corn output that year. And in placing the South in world perspective Wright compares measures of regional wealth (not income) for the South with measures of relative incomes (not wealth) for other countries, a procedure that can pose some problems in ranking, particularly give the relatively narrow differences in a number of cases.

As always, Wright is very stimulating and provocative in presenting his views of antebellum southern economic change and in clearly providing specific hypotheses for the period. As with most important books, there is much to agree with and also various questions where disagreements remain. This reviewer looks forward with interest to further works of this most stimulating scholar.

STANLEY L. ENGERMAN, University of Rochester

GENERAL AND MISCELLANEOUS


“The Reluctant Economist” is a set of essays that is cross-sectional in more ways than one. First, it cuts across several decades of Richard A. Easterlin’s own research. Most of the included works have been published elsewhere—about half since 1995 but some earlier pieces are also included. Second, the book draws on a wide range of disciplines, including economics, sociology, public health, psychology, and history. Third, the research problems tackled represent a cross-section of some of the most intriguing and important questions that the social sciences strive to answer: what causes
growth and what stagnation, why people are healthier (yet not happier) than in the past, how people’s family life interacts with their professional life, and so on. And although each article offers a well-argued answer to its specific research problem, the tenor of the articles (and of the autobiographical introductory essay from which the book takes its title) indicates that the ulterior motive of this publication is to make several important methodological points. After all, it is the reservations about the methodology in economics that makes Easterlin a reluctant economist.

This reluctance takes the form of reasoned objections against some of the basic assumptions that permeate a large fraction of economic research. These include the strong preference for free markets (and its usual corollary of distrust of government), the assumed fixity of preferences, the undisputed positive relationship between consumption/income and personal satisfaction, and others.

Ideologues of all stripes will surely dislike this book. One clear impression emerging from its pages is that, in most cases, reality does not neatly fit into a one-size-fits-all, simple framework. Easterlin argues, for example, that the secular improvements in material well-being and the secular improvements in health come from different sources (economic growth versus public health policy), are based on a different kind of technology (physical versus medical knowledge) and developed thanks to different institutional incentives (free market versus public sector activity). This non-ideological manner shows in the overall approach, too. In contrast to the oft-adopted research strategy whereby a sophisticated model is designed to explain a few stylized facts, Easterlin prefers to apply broadly formulated, “stylized,” explanations to carefully scrutinized empirical observations. The book seeks to be “more accessible to the non-specialist” (p. xvii), so high-grade math is mostly absent, but the author is unforgiving in requiring readers to investigate the numerous tables and graphs that anchor the analysis.

Easterlin shows that by relaxing certain standard assumptions that are commonplace in economics he can shed light on some puzzling issues. But at the same time, there seems to be a danger of taking the idea too far. Consider the use of subjective testimony. Chapters 2 and 3 are devoted to questions concerning personal happiness, lifetime goals and expectations, material aspirations, and career choice. All are areas where questionnaire surveys can be of help. Does this helpfulness go so far as to make subjective testimony one of the methodological cornerstones in economics? “It seems to me,” Easterlin writes, “that an economics model is fundamentally flawed if its presumed cause-effect relations are belied by the subjective testimony of the agents” (p. xvi). I am not so sure, as subjective polls and surveys can be treacherous. Numerous studies show, for example, that people often perceive themselves as smarter, prettier, wiser, and more charitable than they actually are and relative to the rest of the population. Talk is cheap; and seeing humankind as the lying and self-flattering lot that we are, economists prefer to “follow the money” rather than to heed *vox populi*. If an economics model is “belied by the subjective testimony,” it could be a bad model but it also could be that agents do not put their money where their mouths are, in which case it does make sense to treat opinion surveys with suspicion.

I also happen to think that Easterlin’s enthusiasm for government policy in bringing about the mortality revolution could be more nuanced. Stating the problem in terms of a dichotomy of free market versus government policy may be an oversimplification. It seems to ignore that large part of a community’s life that is neither “for-profit” nor government policy and is usually referred to as civil society. Reading between the lines of Easterlin’s account of the mortality revolution (chapters 6 and 7), I sense that a substantial portion of it was due to the citizens’ mutual education, campaigning, and
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gradually changing socialization in matters of food handling, personal hygiene, and so on. After all, governments can legislate compulsory inoculation, but they can scarcely enforce regular brushing of teeth and washing of hands.

This is a thoroughly enjoyable book. It addresses a wide range of topics in a readable, lively style; it offers many interesting insights and presents some remarkable facts. Equally well expressed is the author’s polemic with the prevailing understanding of how the economic science should be done. Ultimately, I perceive Easterlin’s book as an open-ended offer—if the economic profession would become more broad-minded, he would become a less reluctant economist.

TOMAS CVRCEK, Vanderbilt University


Much conventional wisdom is that, in the countries of the postindustrial core of the world economy, the role of the state in the economy reached its apogee in the 1970s, and that the coming of Reagan in the United States, Thatcher to Great Britain, and neoliberalism everywhere began a process of rollback: a return to more “market” and less “state” in the postindustrial “mixed” economies that dominate the world and are the most successful political systems of economic regulation that the world has ever seen.

To some degree this conventional wisdom is true. Governments in the 1970s tried to shape the industrial structure of their economies—engaged in “indicative planning” and other such exercises—to a degree rarely seen today. Governments both liberal and conservative in the 1970s put their thumbs on the scales in support of union movements in a way that they do not any more. The idea that activist fiscal policy could and should be used to guarantee full employment has proved to be yet another utopian delusion.

But in two very important areas the state has not retreated but has advanced. The first is that of central banking—an extraordinary island of central planning in our modern economies, paradoxically the domain of technocrats springing from a monetarist tradition that fears and hates the idea of central planning. The second is the domain of Peter Lindert’s Growing Public: social spending. Public spending on health, education, welfare, and other forms of social insurance has not peaked and has not retreated. It has, rather, continued to grow as a share of national income, and promises to grow still further in the coming decades as aging populations, improving medical technologies, and rising educational expectations call forth additional demands.

The instinctive reaction of neoclassically trained economists—even those who put a very high weight on equality in their implicit and explicit social welfare functions—is that these high levels of social spending are at worst a disaster and at best a pity. It is not the case that most social spending is redistributive. Middle-class people go to subsidized museums where admission fees have been reduced because of their taxes. Taxes collected from middle-class people pay for doctors and nurses to treat middle-class people “for free.” The middle-class young pay taxes that are used for pay-as-you-go pensions for the middle-class old. Everywhere there are tax wedges pushing private rewards to work and industry away from social values, and for what?
So that resources can be collected from some middle-class people and used to benefit other middle-class people.

Neoclassically trained economists will, when pushed, admit to a valid insurance motive as well as a valid redistributive motive. But, they will say, the logic of the social insurance and welfare states is not economic, it is political. Better if some way could be found to float every middle-class tub on its own bottom, and reserve taxing and spending for true redistribution and insurance.

Peter Lindert’s *Growing Public* challenges this picture. Lindert believes and argues that the economic efficiency costs of growing public social spending are small. Lindert wants to argue that a large public spending total is, in today’s developed world, more likely to be a sign of a nation that is successful as a political-economic enterprise than the reverse.

To this task Lindert brings, as Bob Margo wrote in his review of *Growing Public* for eh.net, a “command of a vast array of historical and contemporary evidence” that is “incredible.” He uses this command to make a complicated historical, comparative, and panel-data statistical argument that attacks two puzzles, which Lindert calls the “Robin Hood” and the “Free Lunch” puzzles. The “Robin Hood” puzzle is that among advanced countries social spending tends to be highest where people do not need it that much: it is highest where poverty and inequality are lowest. The “Free Lunch” puzzle is that Sweden and company are not punished for their high levels of social spending with substantial efficiency losses that leave them relatively impoverished. These puzzles are broad and exist at a more than cross-national level: even within the United States, social spending on the state level has been on a rise and has been positively associated with state-level wealth. Connecticut, New Jersey, and California are not punished for their generosity relative to Wyoming, Mississippi, and South Carolina.

Lindert lists what he sees as his nine major conclusions starting on page 20 of the first volume of *Growing Public*. I see fewer major conclusions: six. They are: (1) that the rise of social spending tracks the rise of political voice; (2) that decentralized school funding gives effective political voice to local interests that want high education spending; (3) that social spending is a superior good, so the spending share demanded rises with income; (4) that Roman Catholicism was a negative influence on taxes and transfers up until but not after the destruction of the antidemocratic right in World War II; (5) that social insurance states are significantly larger where the working class and the poor look like each other and trade places; and (6) that larger social insurance states with more redistribution are not associated with heavy drag on economic growth for two reasons—first, nations with larger social insurance states have less inefficient tax systems, and second, universal benefits avoid the surprisingly heavy incentive and administrative costs of strict means testing of benefits.

The thumbnail sketch takeaway is that large-scale government spending on social welfare and social insurance is here to stay: it has, Lindert argues, few if any economic efficiency costs, offers powerful redistributive and insurance benefits to society as a whole, and offers powerful political benefits as well, for the modern social insurance and social welfare statements are very popular among the voters. In Lindert’s view, we should not expect the social insurance state to be eroded over time. It is not under pressure to shrink as a result of its inefficient handing of too-great tasks to the thumb-fingered government. Rather, the social insurance state is here to stay as the most successful set of human governance institutions of all time.

To my mind the historical and comparative analyses are the strongest. I, at least, find the regression analyses offered in volume 2 of *Growing Public* as less convincing than the other components of Lindert’s tripartite historical-comparative-statistical
story that make up the bulk of volume 1. Serial correlation is rampant, identification disputable, the number of truly independent cases low. Hypothesis testing is, as a result, unconvincing—or would be unconvincing if Lindert rested his case on panel-data regressions alone. These problems are inevitable in large-scale historical-comparative work, and will be until we can run natural or unnatural experiments on whole planets set up with similar initial conditions. The regressions work best, I think, as ways of summarizing the data: ways to convince readers that the stories of historical causation and the comparative arguments made in Lindert’s case studies are in fact representative of the universe of advanced postindustrial nations, and not atypical. At this task Lindert’s regressions are completely convincing, especially because of the large amount of heavy lifting that has gone into the quantitative-comparative foundation of Growing Public. To get a sense of how much heavy lifting was involved, the curious and researchers can find Lindert’s data for Growing Public at <http://www.econ.ucdavis.edu/faculty/fzlinder/Lindert%20data%20CUP%20book/CUP%20data%20page.html>.

One reason for Lindert’s success, I believe, is that his argument has its roots not so much in James Buchanan-Gordon Tullock-style public choice as in a more sensitive and catholic tradition that I think of in terms of Albert Hirschman’s Exit, Voice, and Loyalty. Individuals are loyal to others in that they have a utility function that can depend not only on their own consumption but on that of other groups, and so pressure the government for redistributive programs and transfers. How much individuals can pressure the government depends on their voice—which in Lindert’s analysis is taken as a given historical trend rather than as a dynamic process in its own right.

Lindert finds confirmation of his general voice-and-loyalty model in the pre-twentieth-century history of poor relief. The early history of poor relief is simple: there was not very much of it. In England starting about 1750, however, and only in England, poor relief becomes much more generous. Lindert follows George Boyer in interpreting English poor relief as a program for redistributing wealth upwards rather than downwards. According to Boyer and Lindert, increasing commercialization of agriculture created a high, seasonal demand for labor on the part of the truly rich landed elite who controlled the levers of political power in eighteenth-century England. By taxing the general rate-payers of the parishes to provide seasonal poor relief, the truly rich elite could reduce the wage costs they would otherwise have to pay if their workers had to subsist for 12 months on what they were paid for working eight. The rural poor benefited, but the rural really rich benefited as well. With the spread of the franchise at the coming of Reform, the political “voice” that supported this program lost out in the more-general clamor. Poor relief expenditures fell, not to rise again until the political incorporation of the working and other poor into the electorate in the twentieth century.

The other major pre-twentieth-century focus of Lindert’s book is education. Why is it that the leaders in education were America, Germany, and—it turns out, somewhat surprisingly—France? Lindert attributes their lead in education to a combination of local control and widespread voting rights. Where education policy and spending is decided on at the national level, things are too dispersed to support a political pressure coalition to create high education spending. Your national-level taxes will be used to pay for the schooling of their children, and you do not know them. By contrast, when school funding is decided on the local level the degree of social cohesion and loyalty is strong enough to support a large and well-funded public school system. Both widespread voting rights and local control are needed if a country was going to be a leader in public education before the twentieth century.
In the twentieth century, Peter Lindert takes on the puzzle of why high education spending and high education duration in the United States are associated not with higher but with lower scores on standardized tests in America than in other countries that spend less and have earlier ages at which students leave school. The right-wing view is that American education has been captured by the teachers’ lobby, and that as a result America gets relatively little bang for the bucks it spends on education, at least on primary and secondary education. Lindert does not really want to accept this interpretation, but if I read the book correctly he cannot find an alternative interpretation that fits the facts. He prefers to leave this situation as an unsolved puzzle.

For the twentieth century as a whole, Lindert provides an explanation of the “Robin Hood” puzzle that I find satisfactory. Cross-class loyalty between the middle class that dominates the electorate and the poor who need redistributive transfers, increasing wealth, aging populations, and the extent of the franchise together provide a sufficient explanation for a rising demand for social insurance, health, and education spending that political parties seeking to win elections have competed to supply. Countries where inequality is rampant are, by and large, also countries where there is little cross-class loyalty between middle and poor (and also are countries with younger populations and, often, restricted franchises). Before World War II, these also tend to be places where the religious Catholic hierarchy has aligned itself with the antidemocratic right. Thus the countries where it is easiest to assemble the political coalition for redistributive social spending are also the countries where the objective need for it is least.

I find Lindert’s explanation of the “Free Lunch” puzzle to be less satisfactory. In a case study of Sweden that examines “universalism” in benefit programs and analyzes the care with which governments design their tax policies, Lindert argues against the “elegant fiction” of the Laffer curve in its believable form: the claim that revenues rise less than one-for-one with tax rates because a developed country with high tax rates will suffer from considerable tax deadweight-loss-caused drag on its economic growth. Lindert’s case hinges on a negative: he cannot find any pattern in which larger social spending is associated with lower levels or growth rates of GDP.

I was unconvinced by the case study of Sweden. Sweden is just too weird a country for what goes on there to be taken as representative or illustrative of processes that go on in other advanced postindustrial economies. The peculiar nature of Sweden’s Labor Organization, the role of the Wallenberg family, the extremely high export orientation of the Swedish economy, its extraordinary homogeneity, the fact that it had a House of Peasants in the Ständsriksdagen since the 1436 meeting at Uppsala in the aftermath of Engelbrekt Engelbrektsson’s rebellion, and so on make me extremely leery of drawing conclusions from and attempting to apply the Swedish or more broadly the Scandinavian model elsewhere.

The rest of Lindert’s explanation of the “Free Lunch” puzzle is his belief that the relatively high marginal tax rates and notches created by means-testing social welfare programs do have significant efficiency costs through their incentive effect, and his belief that the fiscal stakes matter in that a country such as Germany that seeks to raise a huge amount of money—nearly 50 percent of GDP, say—will pay great attention to making sure that its taxes are as close to nondistortionary as possible, whereas a country such as the United States that seeks to raise less money—30 percent of GDP, say—will pay much less attention to the details of tax code and wind up with as large efficiency losses from its tax system as the higher-tax country.

It is certainly possible. Adam Smith did write in the *Wealth of Nations* that the French found their tax system three times as burdensome as the British found theirs, even though the French system raised only one-third the per capita revenue of the Brit-
Most public finance economists teach that the deadweight losses from the U.S. system of capital taxation are very high. But I cannot convince myself that these are laws of nature rather than accidents of history. In my mind’s eye I do see the possibility of a United States with equally redistributionist taxation as a whole but also with much lower capital taxation and stronger economic growth. In my mind’s eye I do see continental European countries that might have relied less on sales and more on wealth taxes to fund the expansion of social spending that the left wanted for its own sake and that the center-right felt it had to support in order to cement the national consensus for the Cold War. That it is hard to argue convincingly that continental Europe suffers in economic growth from the high average tax rates needed to maintain its social spending does not mean that the standard blackboard explanations of deadweight loss are wrong. But here I sound like some Marxist arguing that the failure of the rate of profit to fall is still consistent with the claim that the rate of profit tends to fall. Lindert has history on his side. All I have is economic theory.

In the middle of volume 1 Peter Lindert leaves economic history behind and gazes into his crystal ball. Like many others, he sees current levels of social insurance benefits as unsustainable. Governments cannot keep their pension and health promises, he believes and argues in chapter 8—especially as faster economic growth raises expectations about the scale of publicly provided geriatric health care. The tax base cannot, Lindert believes, be willing to support the further expansion of tax rates that would be required. Lindert’s argument here is not inconsistent with his explanation of the “Free Lunch” puzzle, but the fit is awkward. It is, after all, the parents and relatives of the current taxpayers who are receiving publicly provided health and pensions. As long as they remain middle-class societies, they may view cutbacks in the pension that Uncle Fred in the back room is receiving with as much alarm as they would view increases in the tax rates on their own income.

In chapter 9 Peter Lindert asks a different question: does the history of the most developed countries present to the less developed an image of their own future? Lindert says, “yes.” The political and economic processes that led to the rise of social spending in Europe and America are at work in the rest of the world as well. In time, developing countries will face the same opportunities, problems, and challenges that have led to growing public spending in richer parts of the world, and for some of these countries—China, for instance—the future will soon be here.

Brad DeLong, University of California, Berkeley

The Business of Empire: The East India Company and Imperial Britain, 1756–1833.


Huw Bowen’s The Business of Empire is an insider’s history in two respects. Bowen has spent most of his career writing about the East India Company, and he steeps his story in the abundance of primary and secondary sources that have accumulated since the Company commenced business in 1600. He is also, in this book, writing specifically about the directors, clerks, and warehousemen who worked inside the Company’s miniature empire in the City of London as it navigated the transition from trade to imperial governance between 1756 and 1833. With some justification, he argues that this side of the story has received short shrift compared to the sustained focus on the Company’s activities in Asia. In the process, he also takes sides in the debate over
whether British imperial policy during this period was mainly carried out in London or in its colonies.

Bowen divides his book into three sections, covering relationships between the Company and the British state; a social history of the Company’s shareholders, directors, and clerks; and the procedures with which its directors attempted to govern its newfound empire and manage its declining trade with India. The first two sections draw heavily on existing historical accounts, although Bowen infuses these with numerous discoveries of his own; the final section brandishes a wealth of new material from the Company’s archives. His analysis of Company-state relations assesses the benefits that the Company claimed to provide to the Crown via tax revenue and military assistance during Britain’s recurrent wars with France. In a separate chapter, he traces the evolving relationship between the Company and the Board of Control, the state-appointed body that oversaw its affairs (but rarely dictated its policies) after 1784.

Bowen’s analysis of Company shareholders reveals a major decline in Dutch investment, from 34 percent of Company stock in 1774 to 1.2 percent in 1830; and reveals much more as well about the changing social composition of investors. Although this information will be very useful for readers interested in the history of the London stock market, its relevance to Bowen’s argument is undercut by his conclusion that shareholders wielded very little influence over the Company’s governance from the 1790s on. Moving from shareholders to staff, a central theme that emerges is that most directors and clerks lacked firsthand experience of India throughout the period under review. Even after 1800, when more former Indian servants managed to win spots on the Board, the fact that seniority determined leadership positions meant they had to wait many years to make a difference in policy. Bowen’s descriptions of the Company’s staff errs on the side of equivocation: directors were narrow-minded but competent; many among the hundreds of head-office clerks were “useless time-serving drones,” but “many managers and clerks worked very hard indeed” (p. 144).

Bowen’s chapters on the Company’s methods of imperial governance and trade confront most directly the question of the relative influence of the metropole versus the periphery on Indian affairs. His conclusion, in brief, is that the Company’s directors worked hard to impose their will on servants in India, but were stymied more often than not by intractable agency problems—not the least of which was the yearlong delay between sending instructions and receiving replies. On most issues, Bowen goes at least partway toward conceding the “peripheral” perspective taken by historian such as C. J. Bayly, which leaves him with the rather weak refrain that “those at the East India House were doing their best to tackle the near-impossible task of supervising far-distant territories and employees” (p. 217). In places, he directly criticizes some aspects of Company policy—for instance, its decision to divert Bengali cash to the China trade in 1766 on the mistaken assumption that the funds could be recovered from land taxes. Mainly, though, he gives the directors an A for effort, concluding that their ceaseless collection of information and delivery of dispatches achieved “incremental improvements” over time (p. 183).

Bowen’s defense of the relevance of the East India Company’s London office is on firmer ground in the book’s final chapter, which tallies its various economic impacts in Britain. This chapter reinforces a truth that most British historians acknowledge but rarely spend enough time unraveling: namely, the hugely important role played by London (as opposed to the industrial north) on Britain’s economy in the eighteenth and nineteenth centuries. Bowen provides richly detailed cases studies of people who supplied cloth, metal, and ships to the Company, convincingly arguing that the vast
sums which the Company annually spent on goods and services left a deep mark on the local economy. Besides directly profiting from their trade with the Company, its many suppliers and shippers used their connections to rise to prominence both in the City and the provinces.

Although Bowen’s approach sometimes leads to a frustratingly narrow, if not apologetic, perspective on the London side of the East India Company, the wealth of detail he has amassed will be invaluable for all who seek to reexamine the question of colonial-metropolitan influence. Likewise, his conclusions will require historians to revisit Britain’s economy in the early nineteenth century with a view to including in their story one of that country’s largest, and longest-lasting, commercial survivals from the early modern era.

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