

## *Book Reviews*

### MEDIEVAL

*Cambridge and its Economic Region, 1450–1560*. Studies in Regional and Local History, volume 3. By John S. Lee. Hatfield: University of Hertfordshire Press, 2005. Pp. xviii, 238. £18.99, paper.

Translating a Ph.D. dissertation into a book is no mean task, but John S. Lee has achieved the feat admirably. He sets out to examine “the relationship between a medieval town and its hinterland” and to explore “the nature and extent of any changes that took place in these links between Cambridge and its region” (p. 1). To this end, the author has carefully probed the records of the borough and the university for the century spanning 1500, paying special attention to information relating to economy and society. There is a healthy balance between primary and secondary source material, with the latter helping to contextualise the former. Even a cursory glance through the bibliography underlines the impressive number and variety of manuscript and primary printed sources consulted. The book is clearly and methodically presented, in an easy-to-read narrative way. It contains over forty figures, most of which supplement the text and help to articulate what a thousand words could not.

In just two hundred pages, Lee covers a wide range of topics, including population, wealth, commerce, land, labor, wheat prices, supply and consumption of food and fuel, and university building projects. He shows how, in many respects, Cambridge was an ordinary mid-ranked English town in the middle ages, but how, in other ways, it was exceptional. For instance, while other urban centres experienced symbiosis between town and castle, in Cambridge the central relationship was between town and university. Indeed, in his chapter on “Cambridge and its society,” the author demonstrates the mutual interdependence and complex alliances that existed between town and gown, and shows that these bodies were not as polarized as many people might have believed.

The greatest strength of this book, however, is its analysis of the town’s commercial hinterland. It provides a particularly adept analysis of the geography of the town’s trade and a comprehensive reconstruction of its marketing network. Although there was a general trend of population decline in Cambridge from 1377 to 1524, numbers remained at between 4,000 and 5,000 for most of the middle ages (pp. 28–30). Lee demonstrates that throughout this time “for many economic purposes, the town was served by its local region of about 10 to 15 miles in radius” (pp. 6, 7). In this regard, Lee’s diagrammatic representations, using five- and ten-mile concentric circles, illustrate just how restricted the economic hinterland of Cambridge was (pp. 87, 99, 156, 157, 158, 159). The point is made, however, that between 1450 and 1560 at least, this local hinterland was just one of “three inter-connecting regions linked to the town”—the others being “a regional hinterland stretching to just over 50 miles away, and more distant ties with other urban centres” (p. 203). Lower value consumables generally came from closer to the town, with more expensive goods being transported over greater distances (p. 172). In time, the colleges at Cambridge appear to have had to look further afield for charcoal and firewood (p. 159)—supplies closer to the town may have been exhausted as forest was felled to make way for food crops.

Having outlined the nature and extent of Cambridge’s interconnecting economic regions, Lee concludes by suggesting that the hinterland of most English towns in the middle ages was more restricted than might be assumed. He concludes the book by saying that “concepts that stress the role of towns as promoters of economic activity

can be useful in identifying areas of growth, and particularly in highlighting the influence of London, but the size of most medieval towns in England restricted their impact on surrounding regions” (p. 204).

MICHAEL POTTERTON, *The Discovery Programme, Dublin, Ireland*

#### EUROPE

*The Political Economy of Stalinism: Evidence from the Soviet Secret Archives.* By Paul R. Gregory. Cambridge: Cambridge University Press, 2004. Pp. xi, 308. \$90, cloth; \$32, paper.

Paul Gregory’s very fine study of the Stalinist political economy represents something of an autopsy report of the Soviet Union as an experiment in socialist economy. Gregory has worked primarily with archive materials, located both in Moscow and in microfilm form at the Hoover Institution at Stanford University. The archival collections include those of the Council of People’s Commissars (Sovnarkom), the industrial ministries, the Central Control Commission of the Central Committee, and the Gulag. One of the many strengths of this book is that Gregory, the leading active American economic historian of the Soviet Union, grappled with the problem of the Soviet command economy for the better part of 25 years before the opening of the archives. He is therefore in an excellent position to know precisely what the archival documents can tell us that we did not know before.

In the preface, Gregory states that “I regard the Soviet Union’s experience with the planned socialist system as the most important social, political, and economic experiment of the twentieth century” (pp. ix–x). Clearly believing that a successful socialist economy was impossible to achieve, he views the Soviet economic project as a colossal failure, with serious consequences that reverberate to the present day for many of the world’s planned systems.

Gregory frames his study by revisiting a question posed by Joseph Berliner: Did the system fail because of a bad jockey (incompetent leaders and irrational policies, with Stalin as “the principal architect”) or an inferior horse (the socialist economic arrangements themselves)? The book addresses this question by reconstructing “how the system really worked, both formally and informally” (p. 5), which is by itself a very impressive accomplishment. Gregory, agreeing with F. A. Hayek, rejects the jockey explanation, arguing that the command economy could only be managed in the framework of a “totalitarian” dictatorship. Another leader might not have shed as much blood as Stalin, but the system nevertheless required an authoritarian leader and the bludgeoning of any semblance of party democracy. Moving away from a Stalin centric approach, Gregory highlights the multiple layers of “little Stalins” at work throughout the planning and economic bureaucracies. He calls this a system of nested dictatorship: petty local autocrats with unlimited coercive authority but with loyalties to their shifting offices that enabled the system to function. The individuals who actually led economic institutions had little importance in this system; bureaucrats’ agendas changed to fit the agency’s interests, but they themselves were largely interchangeable.

One of the book’s major contributions is to illustrate how constrained Stalin and the Politburo actually were when it came to decisions about the economy. Apart from grain collections, foreign exchange, and the investment budget, nearly all decisions about resource allocation were made by subordinates at lower levels. The Politburo

chafed about the extent to which decision making was decentralized; it attempted to reserve more decisions for itself, often through force. This state of affairs contributed to the highly inefficient state of the economy, as Moscow forced local managers to do things they knew were ill planned, irrational, and counter to the information they had at their disposal. Failure to fulfill central orders could mean the loss of one's job and arrest, placing local officials in an utterly impossible situation. In fact, even as the regime established the general economic goals it also tolerated informal forms of exchange and shadow market activity among managers.

Gregory also seeks to highlight the surprising persistence of the administrative-command system after Stalin's death. (Gregory chooses to employ the phrase "administrative-command" rather than the more commonly used "command-administrative" system). The mistrust between Moscow leaders and local managers remained after 1953, even growing more heated. Demands for more leeway in using knowledge of local conditions were countered by Moscow's accusations of local corruption, greed, and disinformation. Gregory concludes that stasis in this system was inevitable and served to perpetuate the system until its collapse. Without real markets or the power to make decisions, managers could not act on their access to superior knowledge. Moreover, very few rules actually governed the system, apart from the general notion that one year's priorities, never explicitly stated in any case, would remain priorities the following year. The initial enthusiasm of the first five-year plans dissipated quickly as the plans in action began to seem irrational, confusing, and contradictory.

Read in conjunction with the work of social and political historians of the Stalinist police-state and industrial economy, such as David Shearer, Oleg Khlevniuk, and Mark Harrison, this excellent history of the Stalinist economy greatly expands our knowledge while opening doors to future research.

JAMES HEINZEN, *Rowan University*

*Nathan Mayer Rothschild and the Creation of a Dynasty: The Critical Years, 1806–1816.* By Herbert H. Kaplan. Stanford, CA: Stanford University Press, 2006. Pp. xvii, 194. \$45.

Any attempt to illuminate the history of Mayer Amschel Rothschild and his descendants is fraught with peril. The stories already planted by historians and others about what may have been the most important family in European finance during the nineteenth century are legion. Moreover, these stories are ground well-overgrown by hardy weeds of myth, themselves well-fertilized by the constraints of available archival sources. With *Nathan Mayer Rothschild and the Creation of a Dynasty*, however, Herbert H. Kaplan, professor emeritus of history at Indiana University, avoids the perils. The volume will be valuable not only for his fellow Rothschild scholars but for anyone with an interest in the financial history of the Napoleonic period or in the development of nineteenth-century financial institutions.

Kaplan dispels various Rothschild myths, such as the long-standing one that had Nathan supplementing his business capital with the dowry of his wife, Hannah Cohen (pp. 9–10). He also offers a method for separating myth from actual history within the context of an extremely complicated network of business relationships. Though he admits the book is neither a biography of Nathan nor a history of the Rothschild family (p. xx), making its title a slight misnomer—*Creation of a Dynasty: Nathan . . . , 1806–1816* might be more accurate—Kaplan's approach may be far more valuable

than either. Painstakingly piecing together sketchy details from personal and business correspondence, Kaplan takes us transaction by transaction through the decade that saw Nathan Rothschild progress from a merchant whose character contemporaries viewed as questionable to a banker at the center of a financial network vital to British political and military success. He teases out one new detail after another from previously unresearched archives on both sides of the Channel, most notably from the Rothschild Archive and the John Charles Herries Papers at the British Library.

By themselves, Kaplan's new sources make the book worthy of an extended look. Even more important, however, is how Kaplan illuminates these sources. Nathan Mayer Rothschild was a man who regularly eschewed careful bookkeeping. Indeed, relatives and associates complained throughout to Nathan about sloppy, contradictory, even ethically suspect, records. Moreover, the decade saw him involved in a dizzying number of partnerships, associations, and "arrangements" with his father, brothers, and associates. Kaplan, however, shows the accounting instincts of a tax auditor as he follows and reconstructs Nathan's transaction trail. And as the trail leads from days where Nathan was mistrusted by almost everyone in Manchester, through years of establishing his reputation by successful specie and bullion smuggling, to the late Napoleonic period where Nathan and his brothers interacted multiple times a day with major players in war finance such as Herries, the British Commissary-in-Chief, one gains new insight into the family's success.

What of negatives? Had I had Professor Kaplan's ear when he was completing the final draft, I would have made two suggestions. First, and more importantly, I would have asked for a book 25–30 pages longer. The book seems too short, not because it contains too little information but because it contains so much. Stories of accounting and finance by their nature require great precision in both quantitative and verbal diction, and Kaplan is a very precise writer. Generally, the book flows quite well—unlike the typical tax auditor, Kaplan is good at making the technical part of the story accessible. Occasionally, however, his precise diction turns dense, assuming more technical understanding than his intended audience may have. The book's introduction and conclusion are outstanding in outlining his argument, but a reader—particularly a nonspecialist, or one whose understanding of how the record-keeping underpinning the income statement and balance sheet actually works might be a bit sketchy—may occasionally get bogged down in other parts of the book. Adding a page or two here, a paragraph or a sentence there, of explanation—connecting the particular details to a larger balance sheet story or a supply and demand/agency costs story, say—would have made the detail more accessible to those of us less adept in "following the money trail."

Second, a list or diagram of *dramatis personae* would have helped. "Keeping the names straight" can daunt nonspecialist readers; so, preceding his text with an annotated list of the players in the story would have been as helpful to them as his six-page list of abbreviations will be for specialists using his voluminous endnotes. Similarly, a summary diagram or three setting out the various Rothschild and Rothschild-connected partnerships would have been invaluable: as Kaplan shows, the network of Rothschild business associations, even between 1806 and 1816, was complex indeed.

Be clear, however, this is an important book. In telling the story of Nathan Mayer Rothschild it is tempting to succumb to one of two hackneyed stories. One can point to the amoral, even unethical, opportunist. Nathan was not a man who mellowed between 1806 and 1816; he remained willing to disregard anything and everything (careful record keeping, the law, the good opinion of father, brothers, or associates) that got in the way of the success of his business. Or one can tell the story of an entrepreneurial

risk-taker, a person who saw opportunities better than everyone else and then focused all his attention on building networks to take advantage of these opportunities. But though each caricature may contain part of the story, Kaplan's work here makes clear that it is only a small part. The history of the Rothschild dynasty remains a story with many complexities unexplored. *Nathan Mayer Rothschild and the Creation of a Dynasty* provides us with a model for further exploration.

WADE SHILTS, *Luther College*

*Exceptionalism and Industrialization: Britain and its European Rivals, 1688–1815.*

Edited by Leandro Prados de la Escosura. Cambridge: Cambridge University Press, 2004. Pp. xv, 335. \$95.

“When traveling should you eat the local specialties?” A widely circulated economic theorem in the United States asserts “No”. If the local specialty was any good, it would be available everywhere. (This perhaps explains why American economists have an international, and not just a national, reputation for dullness). A lesser known corollary amongst academics similarly answers “Should you read *Festschriften*?” with “No, anything good in them is available elsewhere.” Given the incentives of academic life, contributors have an unfortunate motive to deliver what is on the menu elsewhere, or to offer from the larder something that for good reason has been long lying on the bottom shelf untouched.

It is thus a mark of the esteem with which Patrick O'Brien is held by his students and colleagues that this volume, though of variable quality, contains interesting pieces of original research that are unique to this outlet. The 13 contributions each give at least lip service to detailing what allowed Britain to achieve the great lead in industrialization by 1815. But the diversity of answers reveals once again just how mysterious the Industrial Revolution is, and how little hope there seems at present of a convincing answer to the questions “Why Britain? Why 1770?” The papers are loosely organized into six sections labeled respectively: the origins of British primacy, agriculture and industrialization, technological change, institutions and growth, war and hegemony, and conclusions.

This reviewer found the pieces on agriculture and technology the most informative and thought provoking. Nick Crafts and Knick Harley, for example, employ their CGE model of the English Industrial Revolution to ask what explains England's unusually low share of employment in agriculture by 1841. Was it population growth, agricultural productivity gains, or the peculiar institutional structures of English agriculture? Their conclusion vindicates a long-held position of Patrick O'Brien and Caglar Kaydar, that England's labor productivity advantage in the nineteenth century lay more in agriculture than in industry. Crafts and Harley argue that had two-thirds of English land remained farmed by peasant farmers, as they assume was the case in 1770, then the share of labor in agriculture in 1841 would have been 47 percent instead of 22 percent. This assumption has a large effect on employment in agriculture because it implies that two-thirds of all land rents in 1841 were (counterfactually) allocated to subsidizing workers to stay in agriculture.

This conclusion seems, to say the least, a little suspect. Already by 1770 English farmland was mainly owned by large owners and rented out for cash rents at market

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rates to cultivators, who paid the rents to the landlords and not to surplus relatives with low value to their labor time on the farm. And there are strong indications that labor productivity in English agriculture was already high by European standards in 1770 before the Industrial Revolution was under way. But agree or disagree, anyone interested in these issues will want to read this piece.

James Simpson, in another piece original to this volume, also supports the O'Brien/Kaydar hypothesis through a consideration of the details of English agriculture versus that of its continental competitors. This piece again offers a fresh perspective on much-debated issues.

Three essays deal with the role of technological change. The first of these, by Christine MacLeod, though just a survey of technological developments and their institutional structures in Britain and its competitors, is well executed and offers an excellent introduction to the subject. The main conclusion is that, if anything, institutions provided more incentives for innovation in France than in England in the eighteenth century. The other pieces in this section by James Thompson and Rainer Fremdling, while not uninteresting, seem to get lost in the details of the innovations discussed and so to forget the larger question, "Why was Britain different?"

The sections on institutions and war appeared to me less rewarding. Another version of the "restaurant theorem" above is the following: "Can local institutions explain the success of particular economies? No, because if any local institution was particularly valuable it would have been copied everywhere." The authors of these sections seem not to have taken on board this maxim, and attribute wonderful powers to the most mundane institutional differences. Every difference between England and less successful economies is taken as evidence of the true power and wonder of institutions. No matter how trivial these differences might appear, or how little advance theory there is of what Industrial Revolution-producing institutions look like, the writers have faith they matter. Money, banks, government bonds, taxes, naval organization—did any of these make the British Industrial Revolution? To a skeptic it all seems on a par with those fervent believers who see Weeping Madonnas in tree stumps. But with belief every institution becomes wondrous.

GREGORY CLARK, *University of California, Davis*

#### LATIN AMERICA

*Chimneys in the Desert: Industrialization in Argentina during the Export Boom Years, 1870–1930.* By Fernando Rocchi. Stanford, CA: Stanford University Press, 2005. Pp. xviii, 394. \$70.  
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Argentina's inability to sustain the prosperity that, in the late 1920s, made it one of the richest countries in the world has led to much speculation about the causes of that failure. Many have placed a good deal of the blame on Argentina's failure to create a self-sustaining process of industrialization. However, as Fernando Rocchi points out in the book under review, there have been few modern systematic studies of the early stages of industrialization. Many commentators have just accepted the view, principally put forward by the industrialists, that early industrialization occurred in a largely hostile environment: the primary providers of credit, the state-owned banks, proving unwilling to grant loans and the state offering little or no tariff protection.

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Rocchi sets out to shift this vision. His work is wide ranging, well written, and highly perceptive. An impressive array of sources includes, but is not limited to, company and bank records and diplomatic archives. Given the state of the archives and libraries, this is an extraordinary accomplishment. Often showing how individual companies operated, the author gives a micro-level view as well analyzing the larger trends.

As Rocchi points out, Argentine industrialization departed from the classical model, because the country had little in the way of an artisanal tradition. Most of its skilled workers were immigrants. For Rocchi the key hindrance to industrialization lay in the small size of the market, which prevented achievement of the efficiency needed to win markets abroad. Rapid population growth, the creation of a national market, and a culture of consumption could not overcome lack of scale. Argentina also lacked key inputs such as coal and iron and had relatively high labor costs.

Rocchi argues, quite convincingly, that industrialization was not constrained by the inability to obtain capital from the state-controlled banks. Using the archives of the Banco de la Nación and the Banco de la Provincia de Buenos Aires, he demonstrates that numerous large and medium-sized companies did receive loans from state controlled banks. Companies also raised money on the stock market. He does argue, however, that self financing, which was very common, may have been the best strategy given the volatility of consumer demand, dependent on foreign purchases of Argentine exports. However, Rocchi does not really demonstrate that consumer demand was seriously less dependable than in other countries.

How much tariff protection industry enjoyed is difficult to demonstrate, given the complex way rates were calculated, but Rocchi makes clear that industrial production was frequently sheltered by either protective or revenue tariffs. Arguments in Congress show that critical support existed for the idea of protecting industry. Clearly, the conditions for industrialization were much more complex than has usually been thought, and it would be difficult simply to dismiss them as hostile.

Rocchi has larger goals than just showing the beginnings of industrialization. He devotes a well-crafted chapter to the creation of a consumer society in Buenos Aires, one which in many ways resembled that of the contemporary North Atlantic world, with department stores and the like. Harrods, the well known English department store, even had a branch in Buenos Aires. The author also includes a chapter on how Buenos Aires-based firms created national markets for themselves, intensifying the central role of the city in the Argentine economy. Large firms were created, as were trusts. Rocchi also discusses how conflict with the labor force helped forge cooperation between industrialists.

The picture that Rocchi gives us is that of a rather dynamic manufacturing sector that responded relatively well to growing demands but was limited by the size and nature of the economy. This is a generally persuasive argument but not one without flaws. The author is at times somewhat too determined to claim originality for his arguments. He clearly builds on others, and although he has made by far the best case for his ideas, at times he does not give these others all their due. More importantly, although the book purports to cover the period from 1870 to 1930, it distinctly slights the period after the first fair presidential election in 1916. We therefore lack a full discussion of the impacts of World War I and of the 1920s. In the latter period the nature of industrialization shifted due to the intensification of direct investment by foreign firms, particularly from the United States. If one is going to make arguments about the nature of industrialization just prior to the Great Depression, it would be helpful to know more about these changes.

Despite these caveats, Fernando Rocchi has given us an excellent work that reshapes our vision of early industrialization in Argentina. It also helps us to understand

some of the innate problems of the economy and shows why some later prescriptions for the economy did not solve its problems. Like all good books, this should inspire further research. Anyone interested in the problematic history of the Argentine economy will find it a stimulating and convincing work.

JOEL HOROWITZ, *St. Bonaventure University*

#### NORTH AMERICA

*The Economy of Early America: Historical Perspectives and New Directions*. Edited by Cathy Matson. University Park: Pennsylvania State University Press, 2006. Pp. viii, 380. \$55.

Writing about the shortcomings in historical and economic analysis is never easy, particularly when examining 20 years of research. Cathy Matson does well as editor of a volume that collects together papers presented at a conference sponsored by the Program in Early American Economy and Society (PEAES). The conference was titled "The Past and the Future of Early American Economic History: Needs and Opportunities," and it examined research in early American economic history since 1985, which coincides with the first publication of John McCusker and Russell Menard's seminal work *The Economy of British America, 1697–1789* (Chapel Hill: University of North Carolina Press, 1985). The theme "needs and opportunities" repeats the subtitle from McCusker and Menard, making clear the connection in the essayists' intentions. Each essay is thoroughly researched and discusses the good and bad aspects of the current state of research and potentially promising directions for future work. As the title of the book suggests, the essays are more inclined to start debate on an issue than to end it.

Matson starts the volume with a historiographical essay detailing the intellectual development of economic history and the eventual split between econometrically oriented research and a more thematic cultural approach (pp. 5–11). It is a rich story repeated in the essays by David Hancock, Russell Menard, and Lorena Walsh. There is no formal plea for renewed cooperation but the need for reconciliation to advance the field is abundantly clear. Matson's introduction to early American economic history covers 100 years of research prior to 1985. She then examines the field after 1985, doing a wonderful job pointing out the rich opportunities that still exist for research as she provides context for each of the book's chapters.

Hancock, Menard, and Walsh's essays point to the need for renewed discussion and debate as well as the need for a more integrated economic history combining research into cultural institutions and characteristics with rigorous statistical investigation. Hancock reflects on the 20 years since McCusker and Menard's book and how closely the discipline followed the agenda they set out. He finds that many of the questions set forth in 1985 still lack answers, particularly the central questions of growth and development in the colonial times and the early republic. Historians branched out to other topics, not less interesting or worthy of study, but pushing us further away from an integrated economic history of early America. Menard illustrates the need for integration of many fields when he discusses agricultural practices and performance in the colonies. It is not possible to reasonably discuss agricultural productivity growth without also discussing labor markets and practices, capital formation, land policy, migration and other demographic measures, and a host of other factors. Walsh develops a similar idea in her essay on population issues, pointing to the lack of advancement in the estimation of early population figures particularly for the African and African American



segments of the Chesapeake and goes so far as to speculate why some issues remain unresolved: the decline of large scale collaborative projects (p. 125).

Brooke Hunter's essay on the Hessian fly highlights the lack of environmental economic history for this time period. The cyclical response of environment to economy and economy to environment receives insufficient attention in the literature currently and is another important part of an integrated examination of productivity and entrepreneurship in early America. Entrepreneurial activity takes center stage in Donna Rilling's essay on the building trade and the efforts of builders to become self sufficient and wealthy. Rilling's essay highlights the intersections of real estate markets, financial markets and labor markets in Philadelphia at this time and the need for integration of these topics by researchers.

Seth Rockman, David Waldstreicher, and Christopher Tomlins provide excellent discussions of labor market issues. Rockman (p. 336) argues for a revision of the transition to capitalism literature recognizing better the role of "unfree labor" upon economic development after the attainment of political independence. Waldstreicher points to the complex influences working on Benjamin Franklin's thoughts on slavery and how his position changed over time. Tomlins (p. 147) argues for a reassessment of our thinking about the role of "migrant servitude" in the colonial labor force and therefore later institutional developments. Their contributions point to a significant need for altering the involvement of slaves and indentured servants in the political economy of the early republic.

Terry Bouton, Daniel Dupre, and John Majewski focus on monetary and financial matters in their essays. Bouton provides a different view of monetary development in the early republic Pennsylvania arguing that privatization of currency provision exacerbated financial problems and did not improve economic performance as suggested in the recent literature. Dupre examines the rise of sectional and political tensions in the early republic drawing attention to the understudied Panic of 1819. Majewski argues for a reassessment of the "market revolution" in the early republic and an examination of political and social attitudes towards stock ownership, which was more widespread and more in demand than commonly believed.

This book is a valuable first step towards reinvigorating debate and investigation into the economy in early America. It contains challenging restatements and reinterpretations of major issues in the early formation and development of the United States. Although each essay represents an independently rich and complex story, we are better served by recognizing the larger need for a more unified narrative that cuts across regions and markets, and ultimately disciplines, and recognizes the interrelatedness of these ideas. In the end, this book may meet one of the Hancock's (p. 95) aims for further research: scholars may adopt the attitude of McCusker and Menard and become more open to a variety of research from diverse disciplines and start drawing together this wider literature into a unified theme.

DAVID T. FLYNN, *University of North Dakota*

*The First Wall Street: Chestnut Street, Philadelphia, and the Birth of American Finance.*

By Robert E. Wright. Chicago: University of Chicago Press, 2005. Pp. vii, 210. \$25.

*The First Wall Street* examines the role that Chestnut Street, the financial district of Philadelphia, played in promoting economic growth and development in the early United States from the American Revolution until the Civil War. The monograph builds on a large theoretical and empirical literature in economics and finance that

analyzes the relationship between the financial sector and economic growth, the so called finance-growth nexus. This literature suggests that financial institutions and innovations reduce frictions and channel savings to their most highly valued use. A large number of empirical studies find that countries with larger financial sectors and better financial institutions have faster economic growth and greater economic development.

Unlike most studies, Robert Wright employs the historical approach—as opposed to a statistical one—to examine the importance of the financial sector in the early United States. The author examines how financiers and the development of financial institutions and innovations in Philadelphia promoted economic development in early America. The book is divided into 11 chapters that examine various aspects of the financial sector and their importance for stimulating economic activity in the late eighteenth century and antebellum period.

In chapter 1, Wright introduces his hypothesis that a well-developed financial sector is a necessary component of modern economic growth. Chapter 2 notes that Philadelphia was America's most important financial center in the colonial and antebellum periods. Philadelphia had a well-developed securities market, in addition to banks and insurance companies, even in the colonial period. The City of Brotherly Love therefore played an important role in financing early American economic growth. In chapter 3, Wright discusses the financial legacy of the American Revolution and how the Constitution laid the foundation for a “financial revolution” in the United States.

The next several chapters discuss various aspects of the American financial revolution. Chapter 4 reviews the establishment of a stable monetary system with the U.S. dollar as a unit of account. Wright also discusses at some length the construction and establishment of a Federal mint in Philadelphia. In the following chapter, he reviews some of Hamilton's innovative financial plans including the establishment of the Bank of the United States. Chapter 6 details the rise of the insurance business and how families hedged against costly economic events. Chapters 7 and 8 show how early credit markets financed the building of homes in Philadelphia and the construction of roads, canals, and railroads in the first half of the nineteenth century. Chapters 8 through 11 discuss the decline of Chestnut Street and the rise of New York as America's premier financial center. Wright argues that Philadelphia became overshadowed by New York for several reasons, including the demise of the Second Bank of the United States and the death of important businessmen from Philadelphia.

*The First Wall Street* is an interesting and informative account of the role of the financial sector in promoting economic growth in early America. Arguably, the most important contribution of this monograph is the use of the historical approach that provides the reader with a series of stories about financiers and financial innovations in early America. The historical accounts directly show how a financial sector can promote economic development and growth. The historical approach can be more stimulating and convincing than empirical studies of the finance-growth nexus that suffer from two serious identification problems: does finance cause growth or does growth cause the financial sector to expand?; and what is the appropriate metric to measure the size and growth of the financial sector? Economists typically address the chicken and egg problem by using instrumental variables or arguing that the financial sector leads economic growth in a time-series regression. As for the second question, empirical studies of the finance-growth nexus often employ the inverse of monetary velocity or the ratio of liquid assets to the size of the economy as proxies for financial development. Although these statistical remedies and proxies have some validity, it is probably the case that the financial sector causes growth and vice-versa and that there is not a really good way to quantitatively measure the depth and quality of the finan-

cial sector. The novelty of *The First Wall Street* is that Wright uses history to identify the linkages between finance and growth over a long period of time. This is probably a more credible identification strategy.

There are some minor shortcomings in the monograph. The author could have placed greater emphasis on the idea that the historical approach is a very important tool to study the finance-growth nexus given the endogeneity problems in modern studies. The finance-growth nexus is a good example where economic historians and historians can make a significant contribution to a larger audience. Articulating this point in a concise manner could have potentially widened the interest and contribution of the monograph. The book could have benefited from some tables on the growth and size of the Philadelphia stock market. The author could have compared these figures with data from the New York Stock Exchange and maybe some of the other regional markets. The inclusion of a table and discussion that show the rise and decline of Chestnut Street along with the ascendancy of New York as the nation's premier financial center would have strengthened the historical approach.

Overall, *The First Wall Street* is a well-written and researched monograph. Wright provides a detailed overview of the importance of early financial institutions and financiers in Philadelphia that promoted economic development in early America. The author's use of the historical narrative sidesteps the endogeneity issues that plague many of the recent empirical studies on the finance-growth nexus. This book is likely to be of interest to economic and financial historians with an interest in the role that the financial sector played in promoting economic development in early America. Mainstream economists who have an interest in the chicken and egg problem would probably also find the book an interesting read.

MARC D. WEIDENMIER, *Claremont McKenna College*

*Planting a Capitalist South: Masters, Merchants, and Manufacturers in the Southern Interior, 1790–1860.* By Tom Downey. Baton Rouge: Louisiana State University Press, 2006. Pp. xiii, 262. \$49.95.

In 1965 Eugene D. Genovese ignited a debate over the character of slave-based society in the American South with his declaration that “the planters were not mere capitalists; they were precapitalist, quasi-aristocratic landowners . . . [whose] society, in its spirit and fundamental direction, represented the antithesis of capitalism, however many compromises it had to make” (*The Political Economy of Slavery: Studies in the Economy and Society of the Slave South*. New York: Knopf, 1965: 23). Genovese's larger purpose was not simply taxonomic, but to argue that long-term conflict between North and South was indeed irrepressible.

Genovese's characterization sparked a paradoxical dispute. His original challenge ultimately boiled down to a stale battle of definitions: if “capitalism” meant an economy based on the exploitation of waged labor then slavery obviously fell short, but if the term applied to any acquisitive economy based on markets and private property, then slavery qualified, along with most other systems. Few U.S. historians would accept the Marxist categories and analysis that Genovese then employed and most rejected his characterization of the South as “precapitalist” (See James Oakes. *The Ruling Race: A History of American Slaveholders*. New York: Knopf, 1982). But thanks in part to his prodding, specialists also tend to agree that the antebellum North and South were both very similar and very different, so much so that their conflict now seems much more “ir-

repressible” than it did 40 years ago. Whether it was capitalist or not, for example, slavery clearly made an enormous difference in the ways that northern and southern voters interpreted core common values such as liberty and equality, and those differences were strong enough to drive both sides to arms (See for example James M. McPherson. *Battle Cry of Freedom: The Civil War Era*. New York: Oxford University Press, 1988: vii–viii. J. Mills Thornton III. *Politics and Power in a Slave Society: Alabama, 1800–1860*. Baton Rouge: Louisiana State University Press, 1978: 456–61).

The original debate over southern capitalism has thus lost much of its force in recent years, but has not disappeared entirely. Economic historians continue to discuss how slavery may have affected the relative economic strengths of the North and South in the Civil War, and how the legacy of slavery may have shaped the South’s long postwar poverty (see Peter A. Coclanis. *The Shadow of a Dream: Economic Life and Death in the South Carolina Low Country, 1670–1920*. New York: Oxford University Press, 1989: 111–60. Fred Bateman and Thomas Weiss. *A Deplorable Scarcity: The Failure of Industrialization in the Slave Economy*. Chapel Hill: The University of North Carolina Press, 1981. Gavin Wright. *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century*. New York: Norton, 1978). At the same time, other historians have come to believe that a simple “capitalist” label may also violate the complicated reality of the northern economy in this period, and speak of a “transition to capitalism” during the first half of the nineteenth century (for example, see Charles Sellers. *The Market Revolution: Jacksonian America, 1815–1846*. New York: Oxford University Press, 1991).

In effect, our understanding of the northern and southern economies has come to parallel what we know about northern and southern politics and culture: both were similar in important ways and also significantly different. The most recent literature tends to avoid broad-brush generalizations in favor of fine-grained studies of local conditions and nuanced descriptions of the distinctive character of all developments in the antebellum economy (e.g., Christopher Clark. *The Roots of Rural Capitalism: Western Massachusetts, 1780–1860*. Ithaca, NY: Cornell University Press, 1990. Charles B. Dew. *Bond of Iron: Master and Slave at Buffalo Forge*. New York: W.W. Norton, 1994).

Tom Downey’s *Planting a Capitalist South* is a thoughtful and well-researched contribution to this recent literature. He begins by situating his work within the existing debate over southern capitalism, but disclaims any intention to settle that wide-ranging controversy. He desires instead to examine “the interaction of agrarian, commercial, and industrial capitalists” in one corner of the Old South and to see how that interaction “shaped the local political economy and in whose favor” (p. 7). His geographical focus is on the Edgefield and Barnwell districts of South Carolina, two inland plantation counties that were also home to substantial cotton factories that were some of the Old South’s largest industrial facilities.

Downey immediately makes clear that South Carolina capitalism was thoroughly pro-slavery and made no veiled or overt threats to the state’s institutions of human property and racial control. This much is consistent with Genovese’s original declaration (*Political Economy*: 180–220). But neither were the region’s planters inherently hostile to economic development. Prospering from the cotton boom of the early nineteenth century, they welcomed roads, bridges, ferries, and mills and even state-sponsorship of a market town at Hamburg to compete with Georgia’s Augusta. As one thing led to another, the new town spawned a railroad to Charleston, which in turn begat more rural stores and eventually a pair of major textile factories at Vacluse and Graniteville. Like their post-bellum counterparts, Edgefield and Barnwell planters initially agreed that factories could benefit the existing system by giving employment to needy white families and

strengthening the state in its growing rivalry with the North. They were later chagrined to discover that factories, towns, and a vigorous railroad corporation could successfully pursue their own rival interests in state government, sometimes over loud protests from the agrarian community. These clashing interests produced no epic showdowns between rival aspirants to hegemony, but seemed to push South Carolina towards an ordinary competition between rival sectors in a developing pluralistic society.

Tom Downey has carefully traced the emergence of a commercial and industrial order inside the world of South Carolina's slave plantation economy. His findings suggest that commercial and industrial institutions were more compatible with slavery than some have suggested, even though certain clashes were predictable. Although he does not claim to settle grand questions, Downey's work certainly suggests that politics and culture had more immediate relevance to American sectionalism than irreconcilable differences between the northern and southern economies.

HARRY L. WATSON, *University of North Carolina at Chapel Hill*

*Cora Wilson Stewart and Kentucky's Moonlight Schools: Fighting for Literacy in America.* By Yvonne Honeycutt Baldwin. Lexington: University Press of Kentucky, 2006. Pp. ix, 248. \$40.

Cora Wilson Stewart (1875–1958) dedicated her life to promoting adult literacy. In her home state of Kentucky, she began the Moonlight School movement of the early twentieth century with the goal of eradicating illiteracy in a single generation. From diaries, correspondences, administrative records, Stewart's own manuscripts, and a wealth of secondary sources, Yvonne Honeycutt Baldwin pieces together Stewart's life and her crusade against illiteracy. Although the book is presented as a chronological account of Stewart's role in the literacy movement, Baldwin intertwines with it broader themes of gender, politics, and education policy in the early twentieth century.

The book begins with a detailed look at the "making of a reformer," focusing on the people and experiences that shaped Stewart's ideals and goals. Early in life, Stewart developed a strong belief that education was the key to economic mobility and participation in the benefits of modern life. In the second chapter, Baldwin recounts the birth of the Moonlight School movement. In the curriculum Stewart developed, after two six-week sessions of evening classes, participants would be able to sign documents, make basic mathematical calculations, write simple letters, and read a few verses of the bible. After several successful Moonlight School sessions, she embarked on a statewide promotional campaign in 1912. Ten counties immediately adopted Moonlight programs. Other counties, some in surrounding states, also began to examine the issue of illiteracy at her urging. Her experiences and the strong friendships and connections she had formed as a teacher and, later, county school superintendent proved indispensable in the burgeoning literacy campaign.

Stewart's struggles to obtain state funding for the Kentucky Illiteracy Commission (KIC), which she and other volunteers had worked to create in 1914, are described in the third chapter. These efforts, which were ultimately successful, resulted in the politicization of the literacy movement. Baldwin discusses Stewart's growing involvement in Democratic Party state politics in great detail.

The next two chapters recount the expansion of the literacy campaign to the national level. By 1919, only a few years into Stewart's Moonlight campaign, the federal government and 18 states had created illiteracy commissions that employed similar tactics (i.e.,

evening adult classes and public awareness campaigns). Patriotic sentiment furthered the literacy efforts in Kentucky and across the country when it was discovered that over one-fourth of the men called into military service in 1917 could neither read nor write.

As Baldwin documents in the remaining chapters, the inability to sustain volunteer efforts and continual fundraising, changing priorities in adult education (e.g., Americanization programs), and growing demands for professional, institutionalized education delivery would ultimately undermine the success of the grassroots literacy campaigns. Despite her “failure” to eliminate illiteracy in a single generation, Stewart’s contributions to promoting literacy and adult education are undeniable. Ahead of her time in many ways, Stewart’s recognition of the importance of parents’ literacy in the educational progress of their children foreshadowed current literacy programs that emphasize literacy training for entire families.

The obstacles that female reformers such as Stewart faced, in a period when opportunities outside of domestic circles were severely limited for women, is one of the book’s major themes. Baldwin emphasizes the challenges to women in finding “socially acceptable ways of interacting in the . . . patriarchal bureaucratic systems” of politics and reform (p. 10). In particular, the efforts of female reformers to expand the limited scope of issues deemed valid concerns for women, the necessity for women to obtain the approval and assistance of powerful male politicians, and their frustration at limited opportunities for holding political office even after the passage of the nineteenth amendment, are all topics Baldwin addresses through Stewart’s experiences.

Another major theme is the opposition that reformers had to overcome in the form of stereotypes and prejudices. For example, although it was commonly believed that learning was nearly impossible for older persons, blacks, and Native Americans, some of the Moonlight Schools’ most eager and successful students were members of these minorities and older whites, who were well into their eighties in some cases. Stewart, like others of her time, was able to use these counterexamples to argue for the extension of education reform to address the needs of individuals of all races and nationalities.

Although the book is filled with a number of other social themes, one final example is the changing nature of progressive activism during this era. Baldwin characterizes the progression of the literacy movement, like other reform efforts, from grassroots beginnings into more institutionalized social programs. Baldwin relates that reformers within both types of work viewed the other as a threat to their success.

Economic historians in search of a highly quantitative description of literacy and education trends in Kentucky (or the South) may be disappointed. Nonetheless, I recommend this thorough account of the development of the Moonlight Schools and the literacy movement to anyone interested in the history of literacy and education activism in the United States. Baldwin successfully traces the literacy movement through the broader context of American history and provides a new perspective on the social and political climate surrounding education reforms during the Progressive Era.

LINDA CARTER, *Vanderbilt University*

*The Democratization of Invention: Patents and Copyrights in American Economic Development, 1790–1920.* By B. Zorina Khan. New York: Cambridge University Press, 2005. Pp. Xvii, 322. \$60.00.

In this NBER monograph, Zorina Khan describes the evolution of thought on the subject of intellectual property in the United States from the early Federal period to 1920 and compares the institutions that emerged with those that had been established

in Britain and France. An underlying theme is that the democratization of intellectual property rights, roughly interpreted as the widespread access of men and women from all walks of life to protection for their ideas, created an environment that promoted invention and ultimately led to higher rates of economic growth. The term “democratization” has recently itself become something of a “catch-all,” often used to represent some intangible social characteristics that may affect economic growth, political processes, credit markets, and a host of other outcomes. But here the author supplies substance to the notion by bringing to bear a wealth of information, both new and culled from her previous work, about the extent of patent applications, grants, and litigation in the United States.

The result is a comprehensive account of how technological progress occurred at the grass-roots level. There are no sweeping “general purpose technology”-type events here, but rather a gradual progression of knowledge facilitated by a seemingly benign U.S. patent authority that allowed returns for innovative thinking to accrue not only to great inventors, but to creators of more mundane accomplishments as well. In contrast, the systems that evolved in Europe are characterized as being concerned with ensuring property rights for the elite. Although the European system may have reduced the number of trivial inventions passing through the patent authorities, it also tended to favor large technical inventions rather than other useful ones that did not require as much expertise. The author argues convincingly that the resulting intellectual property system did not fuel further invention as much as might have been possible.

Khan proceeds to explore trends in patent litigation as well, concluding that courts in the United States were for the most part able to balance effectively between encouraging invention and not simply affirming indefinitely the market power of patent holders. In an interesting twist, she cautions against blind acceptance of the conventional view that U.S. courts made somewhat arbitrary decisions when enforcing patents before the major legislation of 1836 and became more inventor-friendly thereafter. The main point is that restricting attention to actual court decisions can lead to inferences about patent stance that are based only on unrepresentative samples of apparent infringements. By observing instead that earlier court decisions tended to be cited proportionally as much as later ones, the author demonstrates that a clear turning point in judicial attitudes towards patents across the mid-nineteenth century is unlikely. Rather, patent rights included in the original U.S. Constitution and their implications for the democratization of invention were internalized by the courts throughout the first 130 years of the nation’s history.

Two chapters on women inventors and the property rights of married women are among the more novel in the book. The author here points out that women could access the U.S. patent system about as easily as men could, and shows that increasing numbers of women took advantage of it over time. Though based on small samples compared to the wealth of general patenting data analyzed in earlier chapters, Khan nonetheless offers evidence that the growth rate of patents granted to women became higher than that for men as the nineteenth century progressed. Such a result immediately raises questions of why this was the case. Were women simply becoming more creative relative to men? Or, perhaps more likely, did women increasingly overcome the financial barriers to obtaining patents, leading to the release of a backlog of good ideas? Or did women benefit from a legal environment that became more sympathetic to their claims? These questions are not addressed in any depth, with focus instead on how the household had become a locus of invention and innovation. Other researchers, however, will no doubt be motivated by these chapters to attempt to tackle some of these larger questions.

Khan also argues that the stance of the U.S. courts towards copyright infringements differed considerably from the systems existing in Europe. She portrays the United States as a leader in this regard, arguing that by not providing excessive protection to the contents of the printed page, the nation eased frictions that would otherwise have existed in the dissemination of knowledge. The emphasis on not depriving the public domain of important resources seems almost a forerunner of the democratization of knowledge encouraged by the internet today.

I would recommend this book to anyone interested in a quantitative account of the path and patterns of technological change in the nineteenth-century United States. With a nearly exclusive focus on formal measures of innovation such as patents and copyrights, however, the forest never seems to come into full view. Indeed, a macro economist might even step away from the monograph with a view that innovation and technological change are incremental processes that are surprisingly well captured by the “A” parameter of standard growth models. But there are others, including this reviewer, who see technological changes as more lumpy processes that sometimes arrive with a flourish and offer new ways to think about the very act of inventing. Steam, internal combustion, and electrification may well be examples of such technologies. In this alternative world it is the importance of particular innovations and their association with a leading new technology that matter for understanding how innovation affects long-run outcomes, and these dynamics are not well captured by patent counts. More work may be able to integrate the two approaches, and I am confident that more strong contributions by this author and others will in the end achieve it.

PETER L. ROUSSEAU, *Vanderbilt University*

*Working the Navajo Way: Labor and Culture in the Twentieth Century.* By Colleen O’Neill. Lawrence: The University of Kansas Press, 2005. Pp. xvii, 235. \$29.95.

In this interesting book Colleen O’Neill addresses a question that has received surprisingly little attention: how did Indians adjust to economic changes in the twentieth century? Her subject is the Navajo tribe in the southwest. Too often studies ignore “Indian agency”—that is the active choices made by Indians. Indians are too often treated either as members of a traditional society to be studied before it disappeared through death or assimilation or as victims of the modern world. The author sums this up with a quote from a young Navajo woman who, on learning about her project, said “. . . that’s an important story to tell. Most people think we are silversmiths and drunks.”

The author begins by discussing related literature in labor history and ethnic studies. A number of studies look at the formation of class consciousness as part of the evolution of the labor market, with special attention to the efforts to form unions. Indians receive little attention in this literature because they are seen as traditional and not a part the modern labor market. Ethnic historians are often concerned with how native peoples define themselves in relationship to the majority culture, usually without reference to the labor market. O’Neill wants to bridge the gap between the two fields in this book by looking at how the Navajo maintained their cultural identity in the paid labor market. Not surprisingly (but unfortunately) neither literature is informed by research done by labor economists or economic history of the sort familiar to readers of this JOURNAL. Readers interested in applying economics to these events can easily do so for themselves, however.



Prior to the 1840s, Navajos had a well-deserved reputation as fierce warriors who raided other Indians and the Spanish. In 1863 the tribe was defeated by federal troops led by Kit Carson. The tribe was forced to move to a dismal reserve at Fort Sumner in New Mexico. When the Navajo returned to their original territory, members of the tribe adapted a peaceful way of life, and the tribe thrived and became more numerous. Families made a living by tending small herds of sheep and growing some crops. They also sold blankets and other hand made items to local trading posts, and a few men worked for wages with the railroad or as paid laborers in agriculture.

The 1930s brought a major shock to the Navajo economy. The Bureau of Indian Affairs (BIA), working with the newly formed Navajo tribal council, implemented a mandatory program to reduce the number of Navajo sheep. Some families with small herds suffered the most from the proportional reduction in the number of sheep and had a hard time making a living. The program generated serious resentment by many Navajos towards the newly created tribal government and the BIA. O'Neill (and others) is undoubtedly correct in criticizing the way it was carried out. But she does not suggest what might have been done instead. It appears that the tribe was headed to a crisis due to over grazing and ecological destruction as a result of rapid population growth in human and animal population and a "tragedy of the commons."

Navajo families responded in several ways. Some gathered coal that lay near the surface to provide a new source of revenue, much the same way as they herded sheep. BIA officials saw these open pit "mines" as wasteful and inefficient, but it appears to O'Neill that the Navajo worked their mines with a keen eye to market conditions. Some worked in corporate mines, as well, doing lower paid "Indian" work above ground.

Another important household activity was weaving. The hourly earnings were low, but Navajo women could combine this with other household activities and they were comfortable with the work. The sale of the blankets (a skill originally learned from the Spanish) was handled by trading posts, which the author criticizes for low payments, but which offered a familiar setting. Other household activities included making jewelry, growing crops, and maintaining herds of cattle and sheep. Families did not rely on one source of income, mixing farming, weaving, and wage work.

Navajo men contributed by working for wages on and off the reservation. The biggest sources of employment were the Union Pacific Railroad or working on Mormon farms as laborers. In the 1950s the BIA tried to solve the problem of few jobs near reservations by urging Indians to move to cities. The stated goal was a permanent migration and assimilation of Indians into white culture. Navajo workers, of course, made their own choices, and many returned periodically to the reservation to participate in the household economy and more traditional cultural activities. According to O'Neill, ". . . To pull them into the labor market, commercial farmers, mining companies, and other western industrialists had to adjust to Navajos' demands and figure out ways to accommodate culturally defined work practices" (p. 108).

Another topic of interest to O'Neill is the relationship of Navajo workers to formal unions. In the labor history literature, unions are typically seen as a positive force for workers. In the 1930s there were two miners unions, the United Mine Workers of America (UMWA), which represented the traditional (largely white) miners, and the National Mine Workers, which had ties to radical unions in Mexico and represented largely Hispanic miners. To both groups the Navajo were low-wage competition. Usually Indians were relegated to the lower wage, above ground jobs. John Collier and the BIA opposed unions in the 1930s, as did the Navajo Tribal council. O'Neill is "perplexed" by Collier's opposition to the union, given previous statements in favor of unions, although she admits that "Perhaps he recognized that the UMWA was not, at that time, interested in the welfare of Navajo workers" (p. 121).

In the 1950s and later, some white workers in nearby towns saw Indians as even “lower” than blacks or Hispanics, and Indians were at times victims of what today would be called hate crimes. Unions remained problematic for Navajos, at times not operating in the best interests of Indians. More positively, in the 1960s there was a successful Indian led unionization effort among Navajo construction workers.

In the end, Professor O’Neill argues against a simple dichotomy between traditional and modern. She also addresses issues in dependency theory and other debates in labor history and ethno history. Most importantly, the larger question addressed in this book—how Navajo people combined elements of both tradition and modernism in ways that made sense to them—stands on its own, and in this the author makes a useful contribution.

LEONARD CARLSON, *Emory University*

#### GENERAL AND MISCELLANEOUS

*Global Capitalism: Its Fall and Rise in the Twentieth Century.* By Jeffrey A. Frieden. New York: W.W. Norton & Company, 2006. Pp. xvii, 556. \$29.95.

Now that the book has been shut on the twentieth century, it perhaps comes as no surprise that scholars have begun to reflect more deeply upon the legacy of the past one hundred years. In *Global Capitalism*, Jeffrey Frieden makes a pioneering attempt at spelling out the key economic and political events that shaped the global economy during the last century. The task he sets for himself is not an easy one as he must clear away much of the meddlesome brush that stands in the way of illuminating the historical path to the present, yet at the same time, preserve enough of the actors and events that define the path as a unique one. It also involves making critical decisions about the design of the rest of the garden: how much space should be allocated to shocks (such as the Great Depression and the World Wars) versus periods of continuity, and how many continental varietals should be allowed to bloom at any given time.

The particular garden and path he has constructed is a familiar one, delineated by four epochs of the international financial system. The first section of the book (chapters 1–5), entitled “Last Best Years of the Golden Age, 1896–1914,” provides an account of what is often referred to as the first era of globalization—the period between the late 1870s up to World War I when global trade and finance dramatically expanded, the movement of (free) people around the globe became much more commonplace, and the classical gold standard operated as the linchpin for the system of global finance. The next five chapters describe the period from 1914 to 1939—when nations attempted to reconstruct their economies after World War I, resurrected an interwar variant of the gold standard, fell into economic depression, and searched for solutions to their economic woes by turning to fascism, communism, or autarky. As Frieden aptly labels this period, this is when “Things Fall Apart” for the global economy that had been constructed during the first epoch. The third section of the book (also five chapters) focuses on the period immediately following World War II up through the early 1970s. It describes how the global economy was reconstituted under the agreements reached at Bretton Woods. During this period, the United States assumed a central role in economic management of the global economy, providing aid to the war-torn regions of the globe, encouraging nations to open their economies to trade, and anchoring the new gold-dollar standard of fixed exchange rates. The dramatic economic recovery of Europe and Japan, the spread of socialism, and the use of

import-substituting industrialization (ISI) policies in the developing world receive central attention in this section. The last five chapters of the book describe the final three decades of the twentieth century, the massive factor flows that occurred across national borders, and the re-emergence of an integrated global economy. Emphasis is also given to the retreat of communism, the decline of ISI policies in the developing world, the explosive growth of the newly industrializing economies of East Asia, and the dramatic failure of other parts of the world (in particular, sub-Saharan Africa and parts of South Asia) to catch up with standards of living in OECD countries.

Most economic historians will find the arguments of this book, as well as many of the details, quite familiar. Indeed, one of the book's greatest strengths is that it provides a coherent and cohesive synthesis of the research in the subspecialties of international and macroeconomic history, and nicely weaves together the work of Moses Abramovitz, Barry Eichengreen, Peter Temin, Alan Taylor, Kevin O'Rourke, Jeffrey Williamson, and many others. However, what is disappointing from a specialist's perspective is that it offers little in the way of new findings or a boldly original perspective on the events of the twentieth century. Its main purpose appears to be to pull together the new international economic literature of the past 25 years and present it in one easily digestible volume. Other works, such as *Globalizing Capital* (Eichengreen, Princeton, NJ: Princeton University Press, 1996), *Globalization and History* (O'Rourke and Williamson, Cambridge, MA: MIT Press, 1999) and the NBER volume, *Globalization in Historical Perspective* (edited by Michael Bordo, O'Rourke, and Williamson, Chicago, IL: University of Chicago Press, 2003) reach similar conclusions concerning the past 130 years. As Frieden explains in *Global Capitalism*: (1) international trade, investment, and migration have followed a U-shaped pattern over the past 130 years; (2) falling transportation costs promoted factor price convergence in the two eras of globalization; and (3) a small club of countries has experienced phenomenal rates of economic growth over the twentieth century and now shares similar living standards (convergence) while a large number of other countries have been left behind (economic divergence). That he reaches similar conclusions as others before him is not meant to diminish what Frieden has accomplished, as he has managed to tie all three of these themes together into one lively and readable book that nicely balances historical anecdote with economic generalization. There are a few places where Frieden could have provided more discussion (such as the transformation of the Allied economies during World War II), better balance (such as in the discussion of how the classical gold standard affected countries on the periphery), or greater emphasis (too little prominence is given to the role of banking crises in prolonging the Depression); but for the most part, these are minor sins of omission or commission.

Given its structure (*Global Capitalism* has no tables or figures, and economic analysis is kept to a minimum), this is a book that I would strongly recommend to nonspecialists and students of political science and history. For those who teach international economic history, the book may prove somewhat challenging to integrate into syllabi aimed at economics students. When Frieden needs something from the economist's toolkit (for example, Heckscher-Ohlin trade theory), he briefly provides a summary of the argument without allowing the narrative to get bogged down; but such discursions into theory are few and the relative strengths and weaknesses of competing viewpoints (for example, assessing the relative contributions of factor accumulation and technological change compared to factor price equalization) are not addressed. Hence, the original economics or economic history articles, on which Frieden bases his argument, will perhaps be of more interest to economics students who want to relate the history to theory and empirics. That said, Frieden excels at providing the political background to eco-

conomic policymaking of the twentieth century (something that the aforementioned works in the same area do not develop nearly as well), so it is a pity that we have to even think about making such compromises on syllabi. Economics students would surely benefit from his command of politics and history. It also would be a shame if students missed out on Frieden's neatly integrated, two- to three-page biographies of Nathan Mayer Rothschild, John Maynard Keynes, Hjalmar Schacht, Dean Acheson, and a few others that he views as having shaped the global economy of the twentieth century.

KRIS JAMES MITCHENER, *Santa Clara University and NBER*

*Renewing Unilever: Transformation and Tradition.* By Geoffrey Jones. New York: Oxford University Press, 2005. Pp. xvii, 447. \$64.50.

Although Geoffrey Jones's book covers a fairly short period—from 1965 to 1990—it could not have been an easy history to write. Unilever is one of the world's largest multinational conglomerates. Throughout much of its history it behaved more like a portfolio of businesses rather than a unified organization with a strong corporate identity. Even after rationalizing its businesses during the 1980s and 1990s, the scope of Unilever's products remains broad. And the firm has an unusual dual governing structure, with headquarters in both London and Rotterdam.

How does one tell the story of such a complex firm? To manage both the historical and thematic aspects, Jones divides his book into two parts. The first one-third or so chronicles Unilever's history from 1965 to 1990. The rest of the book examines the firm's "Dynamics and Routines," including chapters on marketing and brands, emerging markets, and corporate culture.

If the book has an overarching theme, it is Unilever's protracted attempt to overcome its historic aversion to acting cohesively. Unilever's very origins militated against centralization. It began life as a multinational, the result of a 1929 merger between a group of Dutch companies and the United Kingdom's Lever Brothers. (The dual-nation governing structure was set up primarily for tax purposes.) The firm grew through opportunistic acquisitions rather than organically. In the early twentieth century, the need to secure a steady source of palm oil had led Lever Brothers into Africa, where Unilever eventually built an impressively large and diverse business, then southeast Asia. Later, Unilever's tea interests pushed the firm into India. In stark contrast to rival Procter & Gamble (a comparative latecomer in the multinational arena), Unilever made little attempt to impose a "Unilever way" of doing business on its acquired firms. There was, moreover, an understanding that the Dutch would not meddle with the British parts of the business, and vice-versa. The two parts of the company did not even communicate with one another much, save through the Special Committee. Aware of these shortcomings, the firm in the 1950s implemented a "Coordination" program to better manage its product groups. But the program applied only to Europe and the United Kingdom, and coordinators were given the power only to advise and persuade. Unilever's growing investment in the foods business also worked against centralization because markets for foods tend to be much more localized than those for products such as detergents and personal care.

Unilever was a prime example of a firm that was run through informal networks rather than by formalized, bureaucratic methods that would have allowed for a more analytical approach to yields and returns. Underperforming businesses were tolerated because few in the firm wished to judge performance solely by the criteria of net re-

turn on sales, or return on capital employed. Strong-willed companies within Unilever, such as T.J. Lipton in the United States, succeeded in fending off any interference from London even when intervention was well justified. The United African Company subsidiary became a miniconglomerate in its own right and operated with almost complete autonomy. Unilever's strong respect for local autonomy resulted in an organizational structure based on nation rather than product. The structure was advantageous during crises such as World War II, but it proved much less so when Europe began to integrate, international competition grew keener, consumer tastes began to converge, and retailers and their private-label brands became more powerful. Extreme decentralization kept the firm from achieving economies of scale and scope, hampered the transfer of knowledge among the different parts of the organization, and slowed the internationalization of its strongest brands. One might expect that decentralization would have fostered more innovation and "intrapreneurship," but these did not become a strong part of the corporate culture either.

Where the firm did succeed was in emerging markets. The strengths that Unilever cultivated—networks, informal methods, and national (rather than product) expertise—served it well in volatile places such as Africa and South America. Jones treats this success as an anomaly, and here his business-school orientation and interest in intrafirm transfers of knowledge may have skewed his perspective somewhat. Certainly it would be difficult to craft a good business-school case study about how informal business and government networks, along with that slippery thing called "influence," can play a critical role in a multinational's success. And country-specific knowledge is, by its nature, not amenable to replication. Yet Unilever clearly had impressive skills in these areas, which probably deserve greater attention.

In the mid-1980s, when terms such as synergy and core competency had become management buzzwords, and conglomerates grew increasingly vulnerable to leveraged buyouts, Unilever finally got serious. It shed noncore businesses along with its United Africa Company subsidiary. Simultaneously, the firm bolstered its cleaning, personal care, and foods businesses. By 1990 when this history ends, Unilever was still far from achieving its potential efficiencies. Its stock traded at a 20 percent discount to competitors', a reflection of the market's dissatisfaction with the slow pace of restructuring. (As of early 2006, the jury was still out on whether Unilever would succeed in growing its markets while continuing to improve operating margins.)

A commissioned history is always a delicate balancing act between the needs of the commissioning organization and the professional standards of the scholar it authorizes to tell its story. Jones has balanced these tensions to write an impressively thorough and candid account of Unilever that will stand as the definitive history of the company during this period. Still, the book shows some of the limitations of a commissioned history, including a too-tight focus on the organization at the expense of larger historical developments—in this case the evolution of multinationals, of which Unilever was an early prominent example. And because there are previous volumes covering Unilever's history up to 1965 (Charles Wilson, *The History of Unilever: A Study in Economic Growth and Social Change*. London: Cassell, 1954: Volume 1 and 2. New York: Praeger, 1968: Volume 3), Jones's volume does not contain the whole story of how Unilever's corporate DNA was created and embedded. To gain these broader perspectives, scholars should also read Wilson's earlier volumes as well as Jones's *Multinationals and Global Capitalism: From the Nineteenth to the Twenty-First Century* (New York: Oxford University Press, 2005).

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*The Natural Origins of Economics.* By Margaret Schabas. Chicago: University of Chicago Press, 2005. Pp. xi, 231. \$40.00.

A part of Margaret Schabas's continuing research into the status of economics as a science, this book argues that the economics of David Hume, the physiocrats, and the early classical school was grounded in physical nature, but that a trend toward what she calls the "denaturalization" of economics began with John Stuart Mill and continued unabated to the present day. Although Schabas focuses on the period that goes roughly from 1750 to 1850, she extends her purview to Marshall and the neoclassical economists. The process of replacing natural philosophy and natural theology with psychology and human agency as the conceptual bases of economics coincided with the birth of "the economy" as a new conceptual entity that can be manipulated to achieve socially desired ends.

Schabas argues that economics, from the seventeenth century on, became more secular than other branches of science. Chapter 2 is devoted to developments in the natural sciences from the Greeks to Isaac Newton. The philosophers of the Enlightenment were so impressed with Newton's achievements that they tried to generalize them to the study of society and of the economy. Others based themselves on experimental physics (including "subtle fluids" such as electricity and magnetism) and natural history. In the influential *Oeconomy of Nature* of 1749, the Swedish botanist Carl Linnaeus provided the first picture of an economy combining human activities with plants, animals and natural resources to show how their Creator arranged mutually supportive relations providing for their livelihood based on an equilibrium between them. He also promoted economics as a science, although his cameralist leanings led him to advocate autarky for Sweden, even with respect to tea and cocoa!

French economics in the Enlightenment and thereafter is discussed in chapter 3. Schabas is on solid ground in arguing the overwhelming influence of natural laws on the birth of physiocracy, the first school of political economy whose very name signifies "rule of nature." The fact that its founder, François Quesnay, was a physician throws light on his authorship of the *tableau économique*, where the circular flow of economic activity among landowners, agricultural laborers, and the "sterile class" of manufacturers can be compared to the circulation of blood in the human body. The physiocrats considered agriculture as the only "productive" sector of the economy capable of yielding a surplus over its inputs. They applied the maxim of *laissez faire* to policy measures such as free trade in grains, and combined moral and physical laws under the rubric of "natural law."

Chapters 4 and 5 survey the two major eighteenth-century political economists in Britain, the Scots David Hume and Adam Smith. Although Schabas believes that "the case that he was well versed in and influenced by natural philosophy is a harder one to make with Hume than with Adam Smith" (p. 65), Hume's scientific outlook can be gleaned from the many thought experiments found in his writings, such as the immediate and ultimate impacts of sudden changes in the quantity of money in his essay "Of the Balance of Trade." In elaborating the specie-flow mechanism, Hume drew the analogy of money with water, where both find their own level. He detected cycles of advance and decline in both the physical and moral realms, as illustrated by phases of economic growth and decline in different nations in the essay "Of Money." According to Schabas, "there is enough circumstantial evidence . . . to suggest that Hume regarded economic processes as part and parcel of nature" (p. 78). Of course, if the term "nature" subsumes "human nature," this is true by definition. If it does not, economic processes can be understood and explained only by taking into account historical and other forces that go well beyond nature itself.

Chapter 5 argues that Smith's writings evidence manifold "debts to nature" that Schabas ascribes to his being well-read in natural philosophy, including its latest developments. In his essay on the "History of Astronomy," Smith anticipated parts of T. S. Kuhn's *The Structure of Scientific Revolutions* by showing how Newton's cosmology replaced those of Ptolemy, Copernicus, and Descartes. In both *The Theory of Moral Sentiments* (*TMS*) and *The Wealth of Nations* (*WN*) Smith resorted to metaphors drawn from natural philosophy, such as market prices "continually gravitating" toward "natural prices." Schabas cites scholars who linked the key concept of "sympathy" in *TMS* to the force of gravity, while others linked it to the nervous system and human physiology. The human capacity for benevolence, like gravitational attraction, diminishes with distance. She points to the physicalist nature of "productive labor" in *WN*, which unlike unproductive labor "fixes" itself to vendible commodities. The very concept of equilibrium that characterizes markets in *WN* may derive from that observed by Linnaeus in animal species. Whereas these are apt observations, I find others less persuasive. To buttress her main thesis, Schabas calls the assertion in *WN* that agriculture is the most productive sector of the economy an example of "Smith's Physiocracy" and claims that "[t]he gift of nature could not be more sharply contrasted with the feeble efforts of human manufacturing" (pp. 94–95). She does not mention Smith's devastating attack on several physiocratic doctrines (including their privileging agriculture as the only productive sector of the economy) in chapter 9 of Book Four of *WN*; nor the central importance of the division of labor throughout the *WN*, the fact that it is much more extensive in manufacturing than agriculture, and the widespread benefits of industrialization such as what Smith called "that universal opulence which extends itself to the lowest ranks of the people." In sum, Smith's "debts to nature" are not as extensive as Schabas claims.

As shown in chapter 6, nature clearly played an important role in T. R. Malthus's *Essay on the Principle of Population* of 1798. This is evident in the two main "fixed laws of our nature" cited in the *Essay*: that food is necessary to human existence, and that the "passion between the sexes" will continue and cause population to grow geometrically as long as food is available. Because the supply of food can at best grow arithmetically, nature or humankind must impose checks on population growth. Malthus diverged from both Smith and Ricardo by advocating self-sufficiency for Britain in agriculture. An important difference with Ricardo was Malthus's sympathy with the landlord class and his advocacy of the Corn Laws that protected agriculture and enhanced rental income.

Although Malthus's work influenced that of David Ricardo, Schabas contends that in the latter's writings nature plays a less significant role. Ricardo, however, argued that natural processes characterize manufacturing as much as agriculture, unlike Smith who held that in the former "nature does nothing; man does all." In the diminishing returns to labor in agriculture, the powers of nature are revealed to decline over time, resulting in an increasing share of rental income going to the landlord class. In seeking to support her main thesis, Schabas goes too far when she claims that nature figures prominently in the principle of comparative advantage, where "Ricardo highlighted the importance of natural endowments and local climate in determining what each region of the world would produce most economically" (p. 116). The last sentence more aptly describes the trade theory based on factor endowments that Eli Heckscher and Bertil Ohlin enunciated a century later. International trade textbooks attribute Ricardian comparative advantage to technology rather than factor endowments. The sources of comparative advantage mentioned by Ricardo in his *Principles of Political Economy and Taxation* (Cambridge: Cambridge University Press, 1951: 132) include, in addition to a country's situation and climate, "its other natural *or artificial* advantages" (emphasis added). He often discussed the effects on trade of "improvements" in manufactures or in machinery, some of which

may even cause a reversal of comparative advantage (ibid.: 137–38). Human agency, in the form of discoveries and technical progress, and policy changes such as the liberalization of trade in wage goods, figure prominently in Ricardo's *Principles*. I also cannot agree with Schabas's verdict that "Ricardo, like his predecessors, still privileges agriculture over manufacturing" (p. 117). Most commentators have noted Ricardo's championing of the manufacturing and capitalist classes, his invectives against the parasitic landlord class, and passionate advocacy of repeal of the protective Corn Laws.

Whereas some commentators have labeled John Stuart Mill as an irresolute "half-way house" economist who started but did not complete the transition from classical to neoclassical economics, Schabas identifies him in chapter 7 as the economist who abandoned the reliance on nature's primacy implicit in the writings of his predecessors, and embraced human agency and institutions as key determinants of economic activity. This "enabled him to reinforce the view that political economy is essentially a mental rather than a material science" (p. 127). The last part of chapter 7, and the final chapter 8, examine how the conceptual foundations of economics after Mill focused even further on the mental attributes of human agents at the expense of natural laws and physical processes. An initial fascination with the impact of human psychology on economic behavior on the part of W. S. Jevons, F. Y. Edgeworth, P. H. Wicksteed, and Alfred Marshall explains the importance of the individual agent in neoclassical theorizing. Psychological influences gradually gave way to the more positivist and analytical approaches of Irving Fisher and Paul Samuelson, to whom Schabas could have added Vilfredo Pareto and J. R. Hicks. Samuelson "even purged economic discourse of the concept of utility because it was too subjective" (p. 139).

Economic historians may be especially interested in Schabas's musings in chapter 8 on the views of neoclassical economists on the irrelevance to their subject not only of natural processes, but of history and any type of evolutionary thinking, in contrast to their conspicuous role in classical times until J. S. Mill. As she says, "economists have embraced the view that bygones are always bygones, that the economy is always recreating itself and, hence, that the past does not matter" (p. 151). But is it true that, for the early neoclassical economists, human deliberations became "the proximate causes of all economic phenomena" and "[a]ny given price (for there is no longer a natural price) is essentially the result of hedonic balancing rather than of labor inputs" (p. 152)? Marshall stressed that both supply and demand are needed to determine prices, making the famous analogy between them and the two blades of a scissors, both necessary to cut a piece of paper. The natural price of Smith and Ricardo became Marshall's "normal" or long-run price representing the cost of production, grounded in the current technology and hence in physical processes rather than any "hedonic balancing."

Schabas's book is the product of a deep, lengthy, and excellently documented enquiry into the natural origins of economics, which she convincingly demonstrates. More controversial is her argument that economics became "denaturalized" with Mill and after him. Just as both demand and supply considerations help to determine prices, I would argue that both nature and human agency (though in different proportions over time) have played some role in the systems of economics that have evolved since mercantilist times. This book should encourage further research on an important topic in the intellectual history of the social sciences.

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