The management of property rights in land has generated paper work ever since the art of writing diffused across Western Europe. Of the documents that defined and authenticated titles to land and the income streams it generated in classical antiquity, no more than an excruciatingly minute sample survives, though we know from contemporary legal texts and notaries’ formularies that the practice of maintaining archives attesting possession persisted through the Carolingian era, when the first inventories arguably describing “feudal” property rights in land and persons appear in fiscal and administrative documents. The present volume reviews the class of documents that recorded such rights in the later medieval and early modern period, which towards the end of the period were sometimes accompanied by maps showing the disposition of holdings. Despite their massive destruction during the French Revolution, the number of these documents may well approach a hundred thousand, constituting a body of source material of the highest importance for understanding how “feudalism” actually worked.

Written by specialists for specialists, the 26 papers in this volume are unlikely to find a wide audience. Nevertheless, they raise issues of utmost importance to the interpretation of European economic and agrarian history. The first question concerns when and how the fiscal obligations inventoried in Carolingian documents were transformed into quasi-private rights recorded in thirteenth-century manorial terriers? Answering this question demands a meticulous examination of the early terriers. The copious documentation from Auvergne suggests that the form of the terrier reflected growing utilization of documentary evidence in legal disputes about property rights after 1200. But since the conveyance of property and fiscal liability in the early Middle Ages also left a paper trail, the later terriers raise the problem of possible continuity hidden from view by the paucity of such documents between the tenth and twelfth centuries.

The second issue concerns the implications of the high the cost of maintaining the terriers. Although the medieval seigniory has been likened to a mafia regime founded by force, the exceptionally high cost of maintaining inventories of feudal rights suggests a legal institution governed by due process. When a terrier was dressed or renewed, tenants and freeholders had to declare their obligations and their holdings to commissioners, who in the absence of prior documentation could not have verified the declarations. Moreover, the inventory had to be approved by the tenants before it could be legally registered, without which approval lords faced heavy court costs often not worth the rights in dispute. Like cadastres and tax rolls of which they are the private analog, terriers were continually becoming outdated by the flux of transfers occasioned by death, marriage, migration, and sale. It is hardly surprising that in much of Europe this cumbersome machinery for extracting part of the agricultural surplus crumbled in the wake of plague and war. The obvious question is how it established itself in the first place. Early medieval fiscal institutions would appear to be the most plausible starting point for investigating that question.

For too long general economic historians have been content to repeat textbook stylizations of seigniorial property based on sociological interpretations dating back a hundred years and more. It is easy to forget that a society as jealous of private landed property as post-Roman Western Europe was likely to possess legal procedures for defining and protecting it. The variety of medieval and early modern claims settled on
landed property—rent in kind and in cash, mortgages and annuities, taxes and other public liabilities diverted into private hands and payable in cash, kind or labor—produced the welter of legal titles that comprised “feudal” property. The study of these regulations was once the province of legal specialists known as “feudists.” If we are to recover the workings of that complex system of overlapping property rights, and thus to appreciate the true economic significance of the eighteenth- and nineteenth-century abolition of feudal property, we will have re-enter their domain and learn the meaning of the texts. This is one of those books that remind us that classification, decipherment, and testing written documents are the bedrock of scientific history.

GEORGE GRANTHAM, McGill University

MODERN EUROPE


This is the second volume of Niall Ferguson’s much acclaimed history of the House of Rothschild, which began with volume 1, “Money’s Prophets, 1798–1848.” Volume 2, “The World’s Banker, 1849–1999,” traces how the Rothschild family tried to maintain the preeminent position they had attained in the previous half-century as industrialization, nationalism, militarism, and imperial rivalries took hold throughout the European continent. The revolutions of 1848 essentially put paid to the Holy Alliance that restored monarchies had tried to maintain after the conclusion of the Napoleonic Wars. As the Holy Alliance fell apart, so did the House of Rothschild, despite the efforts of James Rothschild in France until his death in 1868. The separate branches in Naples, Vienna, Frankfurt, Paris, and London all adapted to the changing political and financial circumstances of their respective nation-states, rather than sustain the family loyalties under the direction of a single patriarch. According to Ferguson, World War I effectively finished off the House of Rothschild as the dominant banker for European government finance. Only after World War II, and especially since the reemergence of today’s global financial markets has the House of Rothschild resumed the international financial functions it carried out in the nineteenth century, albeit now playing a much reduced role in global finance.

Ferguson was privileged to examine the correspondence of the London branch with the various other branches of the family over the period from 1798 through 1918, with the hitherto undecipherable script in Judendeutsch read aloud to him by Rothschild assistants. From this mass of confidential material, Ferguson unveils much of the mystery and misapprehensions that have plagued the Rothschilds since their emergence from the Jewish ghetto in Frankfurt over two centuries ago. The Rothschild correspondence and personal diaries dispel many of the myths about their unrivaled power and influence over the commanding heights of European finance and governments—myths that fueled virulent outbreaks of anti-Semitism throughout Europe culminating with the Holocaust during World War II. For the period after 1918, however, Ferguson was forced to rely mainly on coverage in the European financial press, so the coverage of the twentieth century is much briefer and less original, if still quite useful as a guide to the increasing complexities of international banking. Summary statistics of the accounts of the Rothschild firm for the late nineteenth century are included as appendixes.
Three themes recur as Ferguson takes us from 1848 to 1999. First, the Rothschilds were, and are, a large family with unusually tight family ties. But, being a family also meant that they were subject to internecine disputes and rivalries, and sometimes differing concepts of what were family responsibilities and goals. Second, the Rothschilds were immensely wealthy, encumbered with the same cares about social status and responsibility as other wealthy families. Third, they were Jewish, so concerns of family and wealth had to be reconciled with maintaining their Jewish identity and faith. These three characteristics motivated how the House of Rothschild dealt with the changing demands and opportunities for government finance over the period 1849–1918, and then how they coped with the breakup of Europe after 1918.

The emphasis on family determines the structure of the book: “Uncles and Nephews” from 1849 to 1870; “Cousins” from 1870 to 1914 and “Descendants” from 1915 to 1999. The successive generations of the family tree illustrate the increasing diversity of the extended family, as well as the weakening of the blood ties over time. Each part begins with the varied experiences of members of the family, emphasizing their increasingly visible role as essentially the royal family of European Jewry while gradually obtaining formal political influence within Britain, France, Austria, and Germany (the Naples branch withdrew from the firm in 1863). The separate efforts of each branch of the family to obtain political influence within their respective countries tended naturally to weaken their business ties over time as well as the family ties. But the Prussian-Austrian war in 1866 and the Franco-Prussian war in 1870 made the national fractionation of the family definitive, while strengthening the connections of the English and French branches.

In the interval between the Paris Commune and the defeat of Napoleon III in 1870, the Rothschilds were dominated by James Rothschild in Paris, who, as the youngest of the original five Rothschild brothers, increasingly assumed the role of family patriarch after the death of Nathan, the London brother, in 1836. James also took the initiative in financing of railroads, first in France and then in much of southern and eastern Europe. Here Ferguson takes issue with the standard interpretations by Rondo Cameron and Alexander Gerschenkron that emphasize the leading role of the Pereire brothers and their Crédit Mobilier. Style, rather than substance, differentiated the joint-stock investment bank of the Pereires from the private merchant bank of the Rothschilds. Both emphasized investment in railroads; both benefited initially from Napoleon III’s liberal granting of rail concessions; and both tried to finance railroads in the rest of Europe through affiliated banks. But James was risking his own capital and pursuing profit only whereas the Pereires were risking their shareholders capital and pursuing a Saint-Simonian ideology. Ultimately, the Crédit Mobilier was challenging the conservative monetary policy of the Banque de France, while James was relying on its conservatism to protect his wealth. In the next generation, James’s son, Alphonse, became a dominant regent of the Banque de France, bringing the French branch of the Rothschilds into a dominant role in the French financial sector while the other branches were losing their relative standing, even in Britain.

It was the profitability of French railroads that made the Paris Rothschilds immeasurably wealthier during the Second Empire. But the Rothschild archives reveal constant distrust of Napoleon’s imperial ambitions and his willingness to take on military adventures in Savoy, Mexico, and ultimately Spain. Revolution and war create uncertainty; markets in government bonds fall when uncertainty increases, and the bulk of Rothschild holdings were in government bonds. Obviously, they cared little for either revolution or war, and all Rothschilds feared the risk-taking irrationality of a charismatic politician, regardless of nationality. Nevertheless, they did very well in this pe-
period, as all the warring states needed to float bonds, not having the tax base necessary to finance increasingly expensive modern wars.

But the key to Rothschild success in this period, Ferguson argues, was the strategic decision taken by the London and Paris branches of the firm to invest in mines. In Spain they had already obtained control of the famed mercury mines of Almaden, a much more reliable source of income than the bonds issued by successive regimes of the Spanish state over the nineteenth century. From 1880 on, under the umbrella of the Exploration Company, the fortunes of the Rothschilds were sustained by moving into gold mining on a huge scale in cooperation with other leading merchant bankers in London and Paris. Eventually, this made them the managers of the world’s gold market, enabling the London and Paris branches to help equilibrate the money market pressures that seemed perennially to threaten the stability of the gold standard, especially after 1905. The fascination with gold and the spread of the gold standard also led to profitable investments in other minerals—copper under the Rio Tinto Company, diamonds with the DeBeers, and petroleum in Russia. Throughout these foreign ventures, Ferguson insists, the Rothschilds were always wary of revolution and war, but each branch backed its respective government resolutely whenever it did face these financial shocks.

World War I was devastating to the fortunes of the family despite the huge increases in war finance that massive modern warfare required. It did not help that the initial German advance threatened Paris itself and forced the Paris Bourse, the Banque de France, and the House of Rothschild to depart to Bordeaux. More important was the lack of Rothschild contact with the New York financial center, which proved to be the ultimate source of war finance for the Allied powers. Finally, Ferguson notes with regret, the banking abilities of “Natty,” the grandson of Nathan Rothschild and patriarch of the London branch were not up to the level of his forebears, or, for that matter, of his competitors in the London money market. It turns out that double-entry bookkeeping was resisted by the head bookkeeper in the London bank even after Natty’s death in 1915, because it would take too much effort!

After 1918 only the London and French houses remained viable, and the fortunes of the fabled Rothschilds were in disarray without any strong family patriarch to focus its energies and talents. Efforts to resume their role in government finance led to ill-fated investments in reconstruction loans that defaulted in the collapse of the gold exchange standard after 1931. Only when international capital markets began to reemerge in the 1970s did the Rothschild fortunes improve, especially in Britain under the leadership of Evelyn de Rothschild after 1980. Today, NM Rothschild group is alive and well, with branches in 30 countries, a resuscitated operation in Paris, and a foothold in New York. Its rebirth, according to Ferguson, rests on reasserting the international family ties and by focusing leadership once again on one dominant individual—Sir Evelyn de Rothschild of the London branch at present. But, even with reported profits in 2001 of $134 million, the Rothschild bank is a minnow in today’s financial markets compared with a whale such as Citigroup (before tax profits of over $21 billion in 2001).

In the process of revealing the Rothschild family disputes, concerns, and efforts to maintain their Jewish identity, their wealth, and their political influence in England, France, Germany, Austria, and Italy over the past century and half, Ferguson has done the Rothschilds a great service by dispelling the most egregious myths of their power in the past. But he has also provided the history profession with a classic work. Both volumes of his history of the House of Rothschild are essential reading for economic historians and will remain so for many years to come.
In 2001 the bank-sponsored Institute for Bank Historical Research, Frankfurt, held a conference on the topic of “The Private Banker: Niche Strategies Past and Present.” The papers are published in this volume, organized around three main questions: How and why did private banks lose business, and were forced to give up or merge? How did the survivors adapt and secure their economic survival? and What lessons are to be learned? There are five papers in all, one by a social historian (Morten Reitmayer), two by economic historians (Dieter Ziegler, Youssef Cassis), and two by economists (Christoph Kaserer/Marlise Berner and Thomas Hartmann-Wendels/Christoph Börner). The historians cover the late nineteenth century to the present, with an emphasis on the late nineteenth and early twentieth centuries. The economists focus on post-1945 developments and the 1990s respectively.

In the first paper, “Structural change and its consequences for private banking in Germany until 1914,” Reitmayer draws on his thesis about bankers in the German Empire. He distinguishes five processes that shaped the German banking community. First, private banks lost control over their own foundations, the universal banks, as evident in their declining representation on the supervisory boards of these big joint-stock banks. Second, they lost business with big industry to the universal banks. Third, they became less important in their erstwhile other domain: the issue of foreign securities. Fourth, legislation, above all the Börsengesetz of 1896, favored big banks over smaller banks, especially through the prohibition of futures trading. Fifth, at the turn of the century the universal banks began to branch out and started to compete with small local banks, ruinously for them. Reitmayer puts the story into perspective. He notes that the private banks’ decline was relative, not absolute. Until 1914 the whole banking sector expanded. But growth in terms of business volume was slower for private banks than for universal banks.

Ziegler portrays the resulting specialization of the private banks in the interwar period as a “(doomed?) attempt at survival.” He discovers that private banks did not only dominate “their” niche, asset management, a relatively small niche, but were still very active in investment banking. The twenties saw a short heyday of consortiums of a couple of dozen private banks engaged in investment banking proper. This only changed with the 1931 bank legislation in favor of big universal banks, and with “Aryanization.” Ziegler is most detailed, reflecting his latest research interests, in the part about how “Aryanization,” but also denazification, affected private bankers’ fate, and has some interesting new insights to offer here.

Cassis’s paper on “Private Banking in Twentieth Century Europe” Anachronism or Competitive Edge?” (English in the original) is broad, though shorter. He focuses on the haute banque in Britain, France and Germany, which he follows through three stages: persistence (with a relative decline) up to the 1930s, decline until the 1970s and revival since the 1980s. His paper is more of an overview of his extensive in-depth research, above all on British banking. It offers the necessary comparative perspective to German developments. Kaserer and Berner present an argument well founded in public-choice theory and law. They advance two theses: the first is that there was an absolute decline since the 1970s, but statistics might hide the fact that private banks withdrew from highly regulated areas like credit and deposit banking and went to off-balance-sheet areas like investment banking and private banking. The second is that banking regulation keeps favoring big joint-stock banks over smaller,
private banks. Hartmann-Wendels and Börner see all banking groups in Germany at the end of the twentieth century as affected by the globalization of capital markets, a rapid change in information and communication technology, and a changed, more critical customer, shopping around.

This conference volume is, all in all, commendable reading, with good summaries of current lines of thought in financial history and occasional glimpses of interesting new research. Here Reitmayer and Ziegler offer perhaps the best value for the historian’s money.

MONIKA POHLE FRASER, University of Halle


French historians of France’s agricultural past have commonly deployed their erudition and talent to compose geographical tableaux using evidence drawn from the copious legal and administrative documentation of the Old Regime and its nineteenth-century extensions, and only rarely attempt the examination of a restricted class of phenomena across extended periods of time. The aim of the tableaux is reproducing a picture of the total life of a particular place and time. The resulting general history assembled from them, however, tends to be a sequence somewhat of disconnected pictures loosely linked by demographic or socio-political dynamics, giving an overall impression of stasis. The present study by an eminent historian of France’s early modern agricultural history tries to correct that impression by looking for signs of movement over six centuries of French rural history. Encyclopedic in scope, the work is a serviceable introduction to the recent literature on that history. Its organizing theme is the tension between the evident stability of agricultural techniques and factor productivity between the twelfth and nineteenth century and the movement impelled by the evolution of markets.

In contrast to the last generation of French rural historians, who explained that tension eclectically in Malthusian and Marxist terms, Jean-Marc Moriceau emphasizes changing commercial opportunities as the dynamic element in the exploitation of France’s land endowment. This is a cogent motif, but instead of using it to organize the historical material into a narrative of interaction between markets and agrarian structure, he retreats to the conventional dichotomy of capitalist and traditional culture that inspired social historians in the early decades of the last century. As a result, the reader never learns why, or even how, things happened. Nevertheless, the book contains much new and valuable material. The first part reviews the dating and consequences of deforestation, soil erosion, and climatic variation, recording the punctuated evolution in land use, and the slow transformations of farm layout. The second section reviews the evolution of crop rotations, tools, and the introduction of new crops between the fifteenth and nineteenth century. Much of this material is drawn from the author’s monumental study of capitalist farmers in the Île de France between 1400 and 1800 (Les Fermiers de L’Île de France. Paris: Fayard, 1994). Those unfamiliar with that study will be surprised to learn that in size and capitalization the French farms had nothing to envy their English counterparts. The final section studies tenant farmers as a social class through two cases: the exceptionally wealthy Navarre, who first appear as farmers in the Île de France in the fifteenth century and by the eighteenth century were among the wealthiest non-noble families in France, and a family of small-scale
winegrowers situated just west of Paris, whose history is tracked from the early seventeenth century to the early twentieth. The family histories are developed from the notarial documentation that makes it possible to track the generation and transmission of wealth across multiple generations. A final chapter reviews the impact of the Revolutionary land redistribution.

Despite the author’s superb command of detail and bibliography, the book is nevertheless ultimately disappointing. Bits and pieces of narrative surface in commercial histories of specific farm products, the secular itinerary of representative farm families, and the wrenching displacements caused by the Revolution, but they are not welded into a coherent story linking the whole agricultural past from the twelfth to the mid-nineteenth century. That history could have been built on three themes the book addresses: the evolution of farming technique, the evolution of the market for farm produce, and the evolution of farming organization. Instead, the book asserts what all specialists have long known—that there was movement and that there was diversity in that movement. Much of the book seems to be addressed to graduate students in search of topics for a seminar paper; it lists topics for research but refuses to venture hypotheses or advance arguments. Indeed, the author affects a supercilious attitude towards analytical and especially quantitative history, on the grounds that generalization is impossible without more data geographically arranged and mapped to illustrate the theme of diversity. This is an old-fashioned book stuffed with new facts. Its model is the kind of history advocated by the geographer Vidal de la Blache. One longs for explanation; what one gets is aesthetically arranged description.

GEORGE GRANTHAM, McGill University


When the first edition of The Conquest of Smallpox was published in 1977, Peter Razzell intended it as a contribution to the heated debates over the causes of population growth in Britain between circa 1750 and circa 1840. A quarter century later, in a world stricken by the AIDS epidemic, Razzell’s research on the history of smallpox has acquired new relevance as an historical study of the first major infectious disease to come under human control—smallpox—barring its revival at the hands of terrorists. Thus, the new edition can be reviewed from two perspectives. The first concerns how its arguments have fared in the quarter century since it first appeared; and the second involves what it tells us about the importance of economic constraints on limiting the diffusion of an effective medical innovation.

At the time the first edition appeared it had long been assumed that British population growth in the late eighteenth and early nineteenth centuries was mortality led, and that medical advances contributed to declining mortality. One such medical advance was inoculation (later vaccination) for smallpox. Razzell boldly argued that this one innovation alone accounted for most of the decline in mortality. Unfortunately for Razzell, at just that time Thomas McKeown began receiving attention by denying that medical progress had anything to do with the early decline in British mortality. Instead, mortality-driven population growth was a product of economic growth, rising per capita incomes and better nutrition.

To complicate matters further, Anthony Wrigley and Roger Schofield presented evidence that British population growth in the eighteenth century was fertility led, not
mortality led. As a result, Razzell’s book on smallpox was marginalized from the start by two rival interpretive perspectives.

But time passes and things change. As Razzell notes in his “Introduction to the New Edition,” McKeown’s arguments have been heavily criticized, and Wrigley and Schofield’s fertility estimates remain just that—estimates in search of solid evidence. Meanwhile solid evidence for an early mortality decline continues to accumulate.

Obviously it is time to reevaluate the demographic importance of inoculation for smallpox in the last half of the eighteenth century, as well as vaccination in the early nineteenth century. As the author reminds us, with the apparent retreat of bubonic plague by 1700, smallpox became Britain’s most deadly epidemic disease and a leading cause of death. Very possibly it took one out of ten lives in eighteenth-century Britain. Therefore any innovation that could prevent recurrent outbreaks, or reduce their intensity, was bound to have a major impact on mortality.

But with commendable honesty, Razzell reviews the research carried out after 1977 and shows that it has not verified the dominant importance of smallpox inoculation and vaccination. The decline in mortality between circa 1770 and 1840 affected both infants and adults, but smallpox was primarily a childhood disease, especially in Northern England. Because almost no adults were struck by smallpox in the North, other factors must have been at work as well. Thus far, the age-specific mortality data for children between ages five and 15 years of age, when most inoculation and vaccination took place, are too sparse and inconclusive to settle the matter. Not surprisingly Razzell concludes that more research is needed.

The author now sees mortality-led population growth as part of a more generalized process of medical innovation, which included better personal hygiene and the rebuilding of houses. Why either form of change, hygiene or housing, should be considered a medical innovation is not explained. Clearly, more thought is needed, as well more research.

Nevertheless Razzell’s book remain a valuable contribution to the history of epidemic disease and its control in Western Europe, if only because of the attention he pays to the critical role of economic factors in disease control. His research demonstrates that even when the technology is available, and officials are interested in applying it, costs can check the diffusion of a life-saving innovation.

Inoculation was introduced into Britain by the leading physicians and surgeons who treated the royal family. To minimize the dangers associated with inoculation (it required insertion of smallpox matter from an infected, but recovering person, into the bloodstream of someone not yet exposed) the royal physicians made inoculation into an elaborate and expensive procedure. Between 1720 and 1750 inoculating a single person cost about 26 pounds, roughly twice the annual salary of a single agricultural laborer. Such high costs meant that parish poor law officers could not even think of paying a physician to inoculate the entire parish even if they wanted to. (The whole parish had to be inoculated in order to prevent a few newly inoculated persons from spreading the disease to the rest). That meant that each time an epidemic broke out the dead poor had to be buried, and the sick poor provided with nursing or medical care. Because smallpox disabled a certain number of survivors, the parish had also to provide them with long-term support.

In the 1750s, roughly 30 years after inoculation was introduced, an innovative surgeon named Sutton bucked the medical establishment and simplified the procedure. Costs fell dramatically, and mass inoculation began. Between 1750 and 1800, even before vaccination replaced inoculation (after 1800) smallpox ceased to be a leading cause of death in Britain, even if it did not disappear altogether.
Smallpox and AIDS are both infectious diseases transmitted from person to person, the former by air and the latter by sexual contact (as well as blood transfusions). Then as now both diseases terrified vulnerable populations, inspired intensive medical research and led to public health campaigns. In both cases the first effective means for controlling the disease was developed for the rich or, later, for rich countries, so that in the case of AIDS the cocktail of retroviral drugs now available costs many times more than the annual income of an ordinary person in a poor country.

Although Razzell does not draw parallels between his history of smallpox and the current history of AIDS, any imaginative reader can do so. For that reason alone a book published almost three decades ago is still well worth reading, although the author himself no longer agrees with his own earlier arguments.

SHEILA RYAN JOHANSSON, Cambridge Group for the History of Population and Social Structure

LATIN AMERICA


This is a highly commendable book for economic historians curious about Mexico. It is essential for historians of modern Mexico interested in the economy during the Porfirián and Revolutionary periods (without a grip on which they will not understand anything else about Mexico, then or since then).

The editors introduce the collection with an essay on “the new institutional economics [NIE],” or “positive political economy [PPE],” and its uses for modern Latin American economic history. Eight substantive papers follow in three parts: financial reforms during the Porfiriato, tariff and customs reforms in the same period, and changes in industrial relations, 1900–1930. All deserve notice.

On financial reforms, Noel Maurer and Stephen Haber show legally “facilitated” heavy concentration in Porfirián banking, and that the big banks, serving as “investment clubs,” hugely favored insider firms “no more productive” than others. NIE-wise, they argue that “more liberal bank incorporation laws” would have created “a larger and less concentrated textile industry,” maybe “more efficient” too. Maurer separately tests NIE theory on Mexican banking, to measure concentration’s effects on its efficiency. The Banco Nacional de México (1884– ) enjoyed tremendous privileges, but hogged the benefits, reducing competition and efficiency, he argues—“not good for [maximum] economic growth.” Carlos Marichal explains the BNM’s privileges as necessary for Mexico’s recovery of foreign capitalist trust, which yielded foreign investments (railroads), foreign loans, and political stability; anti-NIE, the bank gave the state “credibility.” Paolo Riguzzi explains why a new Civil Code, facilitating mortgages, did not prevent the Banco Hipotecario’s long monopoly in the mortgage market, and “financial repression” in agriculture and ranching. Here is a sharp sentence: “Mexican businessmen aspired to act like brokers of privilege: to obtain a favorable concession from the government for the undertaking of a particular enterprise and then to transfer this privilege to other groups interested in operating the business” (p. 138).

On Porfirián reforms of the rules for foreign trade, Sandra Kuntz Ficker shows a new “developmentalist commercial policy” from the 1890s onward. Both “liberaliza-
tion” and “protection” (lower average tariffs, but effective rates very low for inputs and high for outputs of selected domestic industries) plus simplification of customs procedures, she argues, increased Mexico’s “integration with the international economy” and its industrial development. Edward Beatty discounts other reasons for Mexican industrialization (international markets, ad hoc politics). Focusing on tariff schedules, he explains a “cascading” protection. “... tariff reforms,” he argues, “were increasingly designed to favor developmental rather than fiscal objectives,” and “a strong correlation” exists “between tariff levels and the ... concurrent process of import-substituting industrialization.” The structure of protection was “the product of policy-makers’ attempts to create incentives to invest in large-scale manufacturing enterprises, whether or not financial interests in them already existed” (pp. 206, 231), which succeeded.

On industrial relations, Jeffrey Bortz sketches some “revolutionary” social and legal constraints on business, emphasizing Veracruz’s textile industry, 1912–1927. Aurora Gómez-Galvarriato demonstrates “a substantial transformation in the relative power of workers and employers” in the great Orizaba Valley cotton mills. Workers in strong, militant unions there doubled their average weekly real wage from 1910 to 1929, whereas workers in regions of weak labor movements gained much less. Profits from the Orizaba mills never recovered their Porfirian levels. “Investment collapsed during the revolution, ... and remained low in the 1920s” (p. 303). But on the same, ever-older machines, labor productivity did not decline. The institutional changes mattered. Workers won “a greater share of the surplus” (p. 314) and new security. But they also blocked technological improvement and condemned the industry to decline.

Haber concludes according to PPE that nineteenth-century Mexico had a “commitment problem” with only “a second-best solution.” The Porfirian government finally promised a “subset of asset holders” to “enforce [only] their property rights,” in return for their investments. Hence, “crony capitalism,” “official or quasi-official monopoly,” economic growth, but “politically generated rents,” “misallocation,” higher prices, and “goods of lower quality,” instead of “more perfect competition” (pp. 324–27)—and therefore an elite divided between favored and disaffected. “The upshot was the Mexican Revolution,” which “changed the calculus of the division of rents, but ... did not do away with the basic rent-seeking mechanisms ...” (pp. 331, 333).

Altogether these essays contribute remarkably to modern Mexican history (with interesting implications for modern Latin America at large). They reveal much more clearly than before the strategic businesses in Mexico between 1870 and 1930 (banking and railroads). They prove the deliberate concentration of capital for closed corporate industrialization. They explain most credibly the logic of the domestic conflicts that exploded in a revolution in 1910; if the conclusion is not new, the argument is much more robust. Most significant may be Maurer’s and Haber’s explanation of grupos, and Rigozzi’s of the mortgage market: Mexican capitalism featured not just private, but secret enterprise. Most original and convincing is Gómez-Galvarriato’s case that the workers who secured most for themselves during the revolution were in the long run “victims of their own success.” This collection would benefit even cultural historians of Mexico, if they learned its discourse. If institutions are rules, then they are culture.

But questions remain. Why ignore the businessmen who made the deal that saved the first Porfirian government, the Mexican Central Railway, which made them fortunes, and the Mexican republic bankable? Why on finance does no one use the abundant French primary and secondary sources? What about Mexico’s biggest corporation after 1905, in its most strategic industry, Ferrocarriles Nacionales? Or the country’s
most strategic and militant union, the Unión Mexicana de Mecánicos? Or the electrical
industry, or the Sindicato Mexicano de Electricistas? Or oil? Or imperialism? Is the
argument about more efficient Mexican development positive, or normative (retro-
spectively prescriptive), or objective, just to suggest what history cost Mexico then?
Without partitioning Mexico, was any other development feasible?
Questions remain too about the theory here, its conceptual “priors.” Is the NIE (or
the PPE) Douglass North’s neo-classicalism, or Oliver Williamson’s social econom-
ics? Why assume “the priority of political rules”? And whatever happened to Schum-
peter’s entrepreneurs, not opportunistic, adaptive predators, but innovative, creative
organizers? Prevalence of the former does not mean absence of the latter. It means an-
other question of economic history: Why do the former win, the latter lose?

JOHN WOMACK JR., Harvard University

UNITED STATES AND CANADA

Sacred Debts: State Civil War Claims and American Federalism, 1861–1880. By Kyle

The sacred debts of Kyle S. Sinisi’s title refer to expenditures and losses by loyalist
states and private claimants related to the U.S. Civil War. His fascinating history ex-
amines not so much the cost of the Civil War as the processes by which states, the
federal government, and private claimants jockeyed to determine just who would pay,
how they would pay, and when they would pay. Sinisi’s first chapter summarizes cen-
tral government reimbursement of colony or state war-related expenditures in the co-
lonial and antebellum periods, including the 1790 assumption of state Revolutionary
War debts by the federal government. Despite this precedent, the Civil War claims re-
quired the evolution of a much extended federalism and Sinisi persuasively justifies
his study as providing a window into “. . . the working system of intergovernmental
operations.” (p. xi)

Congress initiated the intergovernmental operations that Sinisi examines when it
passed, on 27 July 1861, a broad act to indemnify the (loyal) states for war related ex-
penses. In the turbulent summer of 1861 scrutiny of the act was minimal and its pas-
sage hasty, leaving much room for bureaucratic interpretation. That interpretation fell
to the Treasury Department and its ill-humored Secretary, Salmon Chase. Federal ex-
penditures rose from $172,000 per day at Lincoln’s inauguration to over $1,000,000
per day by the summer. Secretary Chase, described by Sinisi as the perfect bureaucrat,
was concerned more with the potential size of federal liability in the event of the law
being interpreted broadly than he was with his political popularity. He therefore estab-
lished stringent rules for settling state claims. Chase’s indemnity rules required that
claimants present original vouchers or receipts, use standard categories, and adhere to
set military pay schedules, transportation rates, and supply costs. The paper trail
flowed into Treasury’s Office of the Third Auditor for recording, traveled to the War
Department for validation, then returned to the Third Auditor’s office for painful veri-
fication of each expenditure. The time and effort involved, especially before the era of
office machinery and cheap telecommunications, or even standard forms, guaranteed
that Chase’s rules would embroil the claimants and the federal government well into
the twentieth century, even as they apparently succeeded in limiting fraud and corrup-
tion.
Sinisi’s description of the claims process alone is worth the price of the book. He continues with more detailed examination of claims by three states: Missouri, Kentucky, and Kansas. These states, he contends, convey the range of claims settlement processes, but also differ significantly from each other both in initial circumstances and in their pursuit of claims. Both state and private claims emanated from these three state’s systems. Some states paid for their war expenditures by raising taxes; others sold bonds. Those states that financed war expenditures through debt pushed for indemnity payments to include interest on their war-related debts. Missouri and Kentucky entered the war as loyalist slaveholding states; Kentucky in particular had a strong Confederate following that led Washington to doubt its loyalty and to act accordingly. In 1861, Kentucky was in good fiscal health, but Missouri faced a large state debt and perilous financial circumstances. Missouri’s fragile finances led it to seek compensation through congressional action rather than the Third Auditor’s Office. Missouri’s success with Congress in 1862 set the precedent for other claimants to by-pass the Treasury Department thereby complicating the claims process. Because Kansas became a state only in 1861, it entered the war with virtually no state government apparatus. Kansas and Missouri both faced incursions from rebel raiding parties that had to be defeated with attendant damages. Sinisi describes in detail the varied use in these cases of state employees as agents to press war claims, contrasting this approach with the use of private agents who received a percentage of their collections. He compares actions under the 1861 Indemnification Act with those that pressed Congress for special legislation. He considers how states arrived at claims figures and also the roles of state and federal claims courts. His description of the speculative sale and resale of private claims at discount is fascinating.

Sinisi’s book does not contain cliometric tests of hypotheses about the extent of federalist cooperation or corruption in the late nineteenth century. Its analysis is verbal rather than quantitative, though it does provide amazing detail based on primary sources including the Congressional Record, local newspapers, state legislative papers, and private letters, speeches, and journals. The complex details never become mind numbing, however, as his narrative guides the reader through the complexities of nineteenth-century politics and bureaucracies. Sinisi’s book on this interesting era of intergovernmental activities is both informative and engaging.

ANN HARPER FENDER, Gettysburg College


There is an extensive literature (to which I have contributed), affirming and denying the idea that the Wizard of Oz by L. Frank Baum reflects the major political and economic controversies at the turn of the twentieth century. In this book, Ranjit S. Dighe has done a superb job of summarizing and extending this literature. Apparently, Dighe left no stone unturned as he searched Baum’s writings, the secondary literature on Baum, and the history of the period for relevant material. The resulting book has a Talmudic structure. At its heart is the original text of the Wizard of Oz. Then in detailed footnotes Dighe presents the allegorical interpretations previously suggested, his shrewd comments on these interpretations, and a number of new interpretations. Dighe also provides brief essays on the monetary history of the period, on Populism, and on
the quantity theory of money—everything in fact that a teacher will need for a successful lecture.

For readers who use the *Wizard* in their teaching, the most important question will be whether Dighe has uncovered some new stories to add to their repertoire. Here the book does not disappoint. One episode, to which Dighe draws attention in an appendix, appears in the *Marvelous Land of Oz*, the sequel to the *Wizard of Oz*. The Scarecrow faces an untimely end because he has lost his straw. He is saved when his friends stuff him with paper money—a clever illustration of the idea that money has the power to revive the ailing farmer. Another example is Dighe’s extended footnote on Coxeys Army. Others have suggested this famous 1894 “jobs march” as a possible inspiration for the *Wizard of Oz*. Dighe, however, goes on to point to one of the strangest characters involved as a possible source for the Wizard. This gentleman referred to himself while on the march as the “Great Unknown.” He was later identified as a patent medicine salesman from Chicago known as “A. P. B Bozarro” and as the “WIZARDO Supreme.” Dighe also refers the reader to secondary sources that are likely to be unfamiliar to economic historians. Michael Riley’s persuasive comparison of the Emerald City with the “White City,” the site of the Chicago World’s Fair, (“The Great City of Oz: L. Frank Baum at the 1893 World’s Fair,” *The Baum Bugle*, winter 1998, 32–38) is a good example.

One of the interesting findings of recent research on Baum is that he was more conservative than had been believed. It had been assumed, based on his son’s biography, that Baum was a Democrat who had supported William Jennings Bryan. New evidence shows that Baum was probably closer to being a Progressive Republican. He was certainly highly skeptical of politicians including Populist politicians. Some critics have fastened on Baum’s Republican leanings to conclude that an historical reading of the *Wizard of Oz* is mistaken. In my view, and I believe that Dighe’s book supports this, the historical reading works better when it is freed from the assumption that Baum was a staunch Bryan supporter. Criticism of Populist politicians, of course, did not exclude sympathy for the people who became Populists or, at times, sympathy for their causes. In any case, Baum has proved difficult to pin down. For example, as an editor of a newspaper in South Dakota Baum wrote some extremely harsh words about Native Americans, and he has been rightly castigated for this. However, the wonderful fictional character that Baum created for his newspaper, “Our Landlady,” joins the Populists and expresses a more sympathetic attitude toward Native Americans.

How does Dighe treat the debate over Baum’s intentions? Dighe stresses the lack of direct evidence for an allegorical intent, and the need to warn students about this. Dighe then maintains that the story “works” as an allegory and goes on to develop the analogies in detail. This is sufficient to justify using the historical reading as a teaching device. However, if we are going to use the literature from a period to explore economic history, we will need to consider the likelihood that writers of fiction can be influenced by the political, social, and economic context of the times in which they write, even when they are not trying to convey a carefully worked out interpretation of their period.

Ranjit S. Dighe has written a fine book that will serve both the practical purpose of helping teachers use the *Wizard of Oz* in the classroom and the scholarly purpose of helping economic historians use the historical reading of the *Wizard of Oz*.

**Hugh Rockoff, Rutgers University**
This book emerged from Glenn C. Loury’s W. E. B. Du Bois lectures delivered at Harvard University in April of 2000—in his words, a “meditation on the problem of racial inequality in the United States focusing specifically on the case of African-Americans.” It offers no new evidence, but presents an encompassing conceptual framework “to clarify how the phenomenon called ‘race’ operates so as to perpetuate the intergroup status disparities that are so readily observed in Americans social life” (p. 3). This brief review cannot convey the depth and breadth of this small book. I highly recommend it to anyone who thinks seriously about racial inequality in the United States. It is an enlightening piece of interdisciplinary theorizing that reveals the honed craft of a seasoned and impassioned intellectual.

Chapter 1 is a brief introduction that defines the book’s subject matter as the “social” determinants of racial inequality in the United States. Assume there are no innate differences across “racial” groupings, but that society interprets the bodily marking of “blacks” as a signal of innate group difference. What structures and processes can reproduce this “racial bias” in perception and outcome?

Chapter 2, says it is self-fulfilling prophecies, vicious circles, and discrimination in contract and contact. Loury argues that information-hungry agents use bodily markings to classify information in an uncertain world. Stereotypes reflecting what “blacks” signals about the unobservable characteristics of blacks in general (statistical discrimination) set in motion processes whereby the presumption of guilt produces a response from the accused that produces guilty behavior. This comes out of a long tradition in the social sciences. The structure is very flexible and appealing. In the past, Loury has used it to argue against affirmative action. This time he uses it ultimately to support affirmative action.

Chapter 3, “Racial Stigma,” adds to self-fulfilling prophecies, vicious circles, and discrimination in contract and contact. Loury argues that information-hungry agents use bodily markings to classify information in an uncertain world. Stereotypes reflecting what “blacks” signals about the unobservable characteristics of blacks in general (statistical discrimination) set in motion processes whereby the presumption of guilt produces a response from the accused that produces guilty behavior. This comes out of a long tradition in the social sciences. The structure is very flexible and appealing. In the past, Loury has used it to argue against affirmative action. This time he uses it ultimately to support affirmative action.

Chapter 4 is entitled “Racial Justice.” Here Loury argues that sensitivity to a history of racial injustice can be reconciled with libertarian notions of color-blindness so as to justify calls for racial justice. The libertarian Axiom of Anonymity is often imposed on the relationship between individual values and collective public choice to ensure
that liberty is not a sham: the choice of one social outcome cannot be preferred over another simply because the identity of the placeholders has changed. Color-blindness is an application. But color-blindness, Loury argues, ignores history, stigmatization, discrimination in contact, and vicious circles. If one is sensitive to this history then color-blindness will appear morally bankrupt, especially so because the principle of color-blindness can not be used to correct deviations from it.

Unfortunately, Loury argues, the current manifestation of this past injustice is “foggy” because it is nearly impossible to say with any quantitative precision just how much of the current racial inequality is due to this sort of disadvantage. Because of this epistemological fog Loury can only assert that, in general, the past has been unjust. As an example, Loury uses Orlando Patterson’s hypothesis about the legacy of slavery on black family structure today. He calls the argument persuasive, but says it cannot answer the question “what would the family pattern look like among today’s blacks in the absence of these historical depredations” (p. 125).

To me, this part of the book is confusing. Loury begins his meditation with three axioms that together claim that all racial inequality is “socially constructed,” yet now he argues that one cannot tell how much of it is determined socially because we cannot measure its historical components and processes precisely. Which is it? When Loury argues, as on page 40, that “race predicts behavior only because, thinking it will do so, the observer uses the race-marker to discriminate, thereby inducing a statistical association between functionally irrelevant though easily observable markers and functionally relevant but unobservable traits,” I take him to mean that observed social inequality by these markings is due to the processes of racial classification and the resulting social dynamics—all of it, even if the processes and dynamics cannot be measured. To argue otherwise—that the marker itself is intrinsically relevant in some way—is racial essentialism, an argument that Loury rejects explicitly in his second axiom.

I believe Loury retreats from social determination at this point because he wants to distance himself from any hint of support for reparationists, preferring instead the bland view that past injustice is relevant in establishing a general presumption against indifference to present racial inequality (p. 126, author’s italics) . . . interpretive rather than compensatory, . . . an ethically indefensible past, . . . the right interpretation, . . . be open-minded” (pp. 126–27). There is nothing wrong with wanting to hold this view, but nothing in Loury’s argument leads inexorably to it. To me, Loury’s conclusion seems a bit contrived. The first two chapters emphasize information-hungry agents, racially biased priors, vicious circles, and stigma, and their role in producing enduring discriminations in contract and contact. One would expect, then, that the policy prescription would focus on interrupting these processes: multicultural education to weaken racially biased priors; calls for mass heroic deviations in behavior, or altered structures, on both sides of the racial divide for the purpose of disproving the prior (which often happens in times of war or civil unrest, but why wait for that?); engaging the political process across the board to establish a general presumption against indifference to racial inequality; and, yes, even class action suits for damages from southern states, the federal government and corporations for their participation in and gains from state-sanctioned racial oppression—a wonderful public exercise in that great American tradition of settlement and political compromise. What better way to teach “the right interpretation” than to demonstrate it?

Finally, on stigma: the dishonor of slavery is part of the story, but by no means the major part. American racism is more American and more contemporary than that. The color “black” was sharply stigmatized in England (as opposed to Iberia) long before the New World resuscitation of slavery. The rhetoric of the Revolutionary War era did
bring the contradiction between slavery and freedom into sharp relief—precipitating black freedom in the north while reconfirming black slavery in the south. But slavery was a well-known institution in western culture and posed no truly disturbing conundrum. Black freedom did. The very same vicious circles that Loury so eloquently describes produced the very “evidence” used to argue that blacks were inferior and that slavery was the solution to having them “in our midst.” America’s racial problem has always been one of segregation, with slavery being one of its many forms. Racial dishonor follows this ever-changing contour, not some meta-memory of a distant past. The more things change, the more they stay the same. Perhaps a fog prevents us from seeing this, but this particular fog can be lifted.

WARREN WHATLEY, University of Michigan, Ann Arbor


Robert Ball played an unusual and critical role in the history of the U.S. economy. Unusual because Ball was a bureaucrat and the stage in economic history is typically reserved for entrepreneurs, politicians, and an occasional labor leader. Ball, however, was among America’s quintessentially effective bureaucrats. What makes Ball’s role in history important is that he proved so effective in expanding and maintaining Social Security—by far the most massive federal economic program and the most important remaining legacy of FDR’s remake of the U.S. economy.

Edward Berkowitz’s fine “political biography” of Robert Ball is thus an important contribution to the discipline. As the need to accommodate the transition to a much older society is among the most important economic challenges the industrial world must face over the next quarter century, his biography is also quite timely.

Robert Ball was the chief policy maker within the Social Security Administration (SSA) from 1950 to his departure in 1973, and through much of that period SSA Commissioner. This was golden age of Social Security, when the program expanded to cover the bulk of the U.S. workforce, include disability and retiree health care coverage, and increase benefits to reduce poverty among the elderly to the level of the population as a whole. Ball also led the liberal camp during the retrenchment years of the past quarter century. The 1983 reforms did cut benefits for retirement at any given age. But as they left the basic design of the program intact, the reform was a success from Ball’s liberal perspective.

What’s truly amazing is that all but one of these milestones (Medicare) came during Republican Administrations: the expansion of coverage and the addition of disability under Eisenhower; the increase in cash benefits under Nixon; and the 1983 compromise under Reagan. The key explanation for this anomaly, according to Berkowitz and other close students of Social Security, is Robert Ball. Berkowitz persuasively shows how Ball’s charm, intelligence, discipline, and superb political skills expanded and then preserved the U.S. Social Security system in these politically hostile environments. Berkowitz’s biography should become the basic guide to the “within the Beltway” politics of Social Security. It should also become the indispensable primer for aspiring career civil servants.

The major shortcoming of Berkowitz’s political biography is the narrow range of the politics it covers. His biography covers small “p” politics. But as Ball’s life is fundamentally about big “P” Politics, this is a serious deficiency.
Ball came of age in the 1930s and believed in the New Deal vision. He saw Social Security, or contributory social insurance, as the great alternative to widespread poverty and a demeaning means-tested social safety net. By tying benefits to work and worker contributions, New Dealers saw Social Security as providing protection against the hazards of modern economic life in a way that preserved the independence and dignity of American citizen-workers.

Ball’s great expression of this Politics came at the beginning of his career, when the choice between social insurance and means-tested welfare was very much on the table. Congress had not increased Social Security benefits in line with wages in the prosperous and inflationary post–World War II period. So Old Age Assistance, the federal means-tested piece of the Social Security program enacted in 1935, continued to provide more income to the elderly than Old Age and Survivors Insurance (the social insurance piece we now call “Social Security”). Ball was then a young and fast rising star and served as staff director of the 1947/48 Social Security Advisory Council. He elegantly laid out the Political case for social insurance (see, for example, his “Social Insurance and the Right to Assistance,” Social Service Review 21, no. 3 [September 1947]) and convinced the bipartisan Advisory Council, and then Congress, to restore the relationship between benefits and wages set in the 1939 Social Security Amendments, and to expand the program to cover the great majority of U.S. workers.

Had Berkowitz addressed Ball’s Politics, his book would be far more useful today. The impending demographic transition, and the rise of a new Politics opposed to the New Deal vision, is again forcing the nation to decide the future of social insurance. Social Security has a major funding shortfall. Reforms that bring the program back into balance could easily reduce benefits to the point where many if not most older Americans become dependent on means-tested programs. Substituting funded individual accounts for a portion of Social Security, the primary component of the alternate Political vision, would not eliminate the need to expand means-tested programs. Indeed, it would add thorny financial and moral hazards to our old-age income system. Had Berkowitz written a larger Political biography of Robert Ball, it would have been an especially useful guide to the debates that will shape the future of the U.S. economy.

STEVEN SASS, Boston College


For more than 50 years after the Second World War, the luxury end of the American automobile industry was dominated by two venerable domestic marques, Cadillac and Lincoln. Unlike Pierce-Arrow, Stutz, and Cord, Cadillac and Lincoln survived the 1930s, perhaps because they were parts of much larger corporations. Equally importantly, in contrast to Packard, the Cadillac and Lincoln marques were not degraded by the introduction of smaller, less luxurious cars that generated sales but ruined public perceptions. (Packard’s image was not helped by the sale of the dies for its 1942 model to the Soviet Union. For decades afterwards, viewers of television news shows were exposed to shots of Soviet leaders in what were apparently vintage Packards.) Since the early 1970s, Cadillac and Lincoln have been affected by severe shocks in-
including the two petroleum crises and the rising popularity of Mercedes, BMW, Lexus and other foreign luxury cars. These have led to frequent redefinitions of the meaning of luxury (for example, when it was decoupled from size beginning in the 1970s) and placed the American producers in a series of reactive situations. By the late 1990s, when the production of Lincolns temporarily overtook that of Cadillacs, sales of both marques had declined drastically from their peaks. The output of Cadillacs, for example, was halved between the late 1970s and the end of the century.

Thomas E. Bonsall traces these changes in his two companion volumes. But as the books are intended for automobile enthusiasts, a much larger and more lucrative readership than economic and business historians, the information is buried among hundreds of pictures and lengthy discussions of changing wheelbases and dashboards. Nevertheless, Bonsall, a journalist as well as an automotive historian, does make pertinent remarks about firm strategy and management. He believes that Lincoln has suffered from management instability since the death of its champion, Edsel Ford, in 1943. As a result, the nature of Lincolns and Continentals vacillated for decades, undermining attempts to create a general perception that they were comparable in prestige and luxury to Cadillacs despite the production of some of the most beautiful models of the period. Cadillac, however, benefited from its relatively secure position within the General Motors framework to achieve a consistency in design which, even if not always aesthetically pleasing, left the public in no doubt as what a Cadillac was. Bonsall is more critical of performance in recent years, when GM has changed its management structures and undermined Cadillac’s privileged position in the corporation. But given the uncertainty that GM has faced, its failure to keep on top of ever-changing conditions is arguably more justifiable than Bonsall contends.

Because the business part of his story is buried among detailed accounts of changing body styles and engines, these books are not efficient tools for business and economic historians. For readers who want handsomely illustrated accounts of the evolution of an important aspect of American aspirations, however, they are worth reading.

Paul L. Robertson, Griffith University


*Downsizing in America* is an illuminating and thought-provoking book on an extremely important topic. Since the bubble burst on the 1990s boom, unemployment has once again become a major policy concern. William Baumol, Alan Blinder, and Edward Wolff examine the related phenomenon of downsizing, a term widely used in the media to describe job cuts by firms, and which they define as declining numbers of workers employed per firm. Their goal, as the book’s title suggests, is to examine the causes and consequences of downsizing, and to compare the reality with the perception in the mainstream press.

The authors approach their topic systematically, and clearly explain their logic along the way. They posit six hypotheses that might explain downsizing, ranging from the character and pace of technological change, to foreign competition and even the “breakdown of the social contract between labor and capital,” which they call a “soft” hypothesis and “almost sociological” (p. 24).
They discuss the possible explanations within economic theory for downsizing and their relationship to their six hypotheses. They maintain that declining demand for an industry’s output might lead firms to cut labor in the short run, but that in the long run, average firm size in an industry should be unaffected by the total demand for the industry’s product. The primary culprit for long-run trends in average firm size is technology. They note that technological change can either increase or decrease optimal firm size. They offer the hypothesis that the growth of information technology might have caused upsizing in retailing but downsizing in manufacturing. Part of their analysis that is likely to raise many eyebrows is that they do not incorporate any role for imperfect competition in their analysis, adopting as their standard perfectly contestable markets. It would have been very interesting to read their assessment of the relevance of the work on technology and market structure, such as that by John Sutton, when discussing their theories of firm size. Nor do they discuss the potential importance for their question of the large literature on transaction costs and the firm.

Next, the authors present extensive empirical analysis of downsizing using Census of Manufacturing and Department of Commerce Enterprise Statistics for the period 1958–1997. They analyze the evolution of firm size in two-digit SIC code industries. Noting that the manufacturing sector as a whole employs a lower percentage of the workforce than before, they find that labor employed per firm (average firm size) also has been falling within most American manufacturing industries since 1967, and not surprisingly, that downsizing has been most prevalent during recessions. The pattern in retail and service industries, however, is one of upsizing.

Insofar as manufacturing industries, their measures of technological change and technology intensity are associated with smaller average firm size. They also examine investment in information technology, and they find that it does not affect average firm size but is a statistically significant variable explaining the reduced dispersion of firm size in industries. Overall industry employment is strongly positively associated with firm size, confirming the hypothesis that downsizing is at least in part cyclical. In some econometric specifications, unionization is associated with more downsizing, not less, which they interpret as evidence in favor of the hypothesis that there has been a breakdown in the social contract between capital and labor. They find that increased export intensity is also a large and statistically significant factor explaining downsizing, which they attribute to the need to cut costs in order to capture foreign markets. Interestingly, the intensity of competition from imports is often insignificant and has a much smaller coefficient. These provocative results bring a new twist to the debates on globalization.

The regression analysis of service and retail industries is less illuminating, primarily because of the paucity of data, particularly historical data, on those sectors. They are keenly aware of this shortcoming and are forthright and circumspect in their conclusions.

The authors present some of their most fascinating results in the chapter on the consequences of downsizing in manufacturing. Consistent with previous studies, they find that downsizing does not increase productivity. Yet they find it does increase profits, and reduce average employee compensation. That is, “downsizing firms typically increase their profitability by decreasing their unit labor costs” (p. 233).

In sum, the authors find that the primary causes of downsizing are demand fluctuations in the short run, and technological change in the long run. They advocate a “more determined and more effective” government safety net to assist those workers who suffer as a consequence (p. 267).

The book is a major accomplishment. The authors had to address a daunting array of data problems and a disparate, but vast literature. The findings are presented in nu-
merous tables and figures, which are almost all instructive and provocative. The book is highly relevant for many economic and business historians, especially those with a focus on labor markets and business-labor relations.

RICHARD SICOTTE, University of Vermont


Francis Gavin has written a study of U.S. administrations and their approach to dollar politics between 1958 and 1968, when he identifies the suspending of the London gold pool in March 1968 as the effective end of the Bretton Woods system. From then, the world was on a dollar standard, and there were two separate prices for gold, one for the private market and one for official transactions. Events up to the better known crisis of August 1971, when the official dollar price of $35 an ounce was also ended, are treated only in a summary chapter. The major emphasis of this book is on how dollar politics and security politics interacted in the Kennedy and Johnson administrations, and in particular how a continual worry about balance-of-payments deficits forced the U.S. government to think about the reduction of military expenditure overseas, especially in Europe. One major theme is American-German negotiations about the “offset,” i.e., German spending on U.S. military products to counterbalance the cost to the United States of stationing troops in Germany, and pressure on the Bundesbank to hold dollar reserves. This, incidentally, has also been the subject of a fine study recently by Hubert Zimmermann: (Money and Security: Troops, Monetary Policy and West Germany’s Relations with the United States and Britain, 1950–1971. New York: Cambridge University Press, 2002). The well-known tensions between the United States and France and de Gaulle and Rueff’s criticism of the United States’s “exorbitant privilege” of forcing its dollars and deficits on the world are also examined in depth. Great Britain’s sterling crises and the relationship between strains on sterling (at that time the other major world reserve currency) and strains on the dollar are handled conventionally. Japan, which became a major focus of U.S. concerns right at the end of the decade, and especially in 1970 and 1971, is not treated in any depth. And Canada, which had abandoned a fixed exchange rate regime, is not treated at all, presumably because Gavin found no evidence that anyone ever thought of Canada’s noncompliance with Bretton Woods as a problem. There is also little discussion of the places where multilateral financial diplomacy took place: the G-10, the OECD’s Working Party Three, or the International Monetary Fund.

The evidence offered by Gavin about the state of thinking about the U.S. deficits is interesting. There is almost universal agreement among U.S. officials and policy makers that something is wrong. On the other hand, all the figures from the Kennedy and Johnson administrations will only think about flexible exchange rates in order to reject the suggestion (as for instance W. W. Rostow did). It is surprising that Gavin does not look at greater length at the debates among economists at the time, and about the arguments for free capital mobility and flexible rates. Milton Friedman is mentioned only in passing, and Gottfried Haberler, who headed a taskforce for the transition to the Nixon administration, not at all.
Gavin pitches his account as being at odds with the conventional wisdom on the subject. This sounds like a peculiar claim about modern economists' discussion of the problems of Bretton Woods, where there is an almost universal consensus about the problems of the fixed-exchange-rate system and simultaneous capital flows (which started to develop on a large scale in the course of the 1960s). So most economists will say to this book: so what? On the other hand, he is probably right about many historians and political scientists, who have treated dollar politics under Bretton Woods as part of the exercise of U.S. hegemony and have essentially, since David Calleo and Robert Gilpin's work, taken over the contemporary French criticism of Rueff and de Gaulle. In the context of this literature, it is helpful to see what U.S. policy makers were saying and thinking, and how worried they were by the inherent instability of their situation and the eventual likelihood of a dollar collapse. The exercise of linking discussions of security issues and economics and finance is also a welcome one. And the archival evidence (which provides the core of the book) is fascinating and well handled. There are some fascinating novelties: that at the end of his administration, President Eisenhower suggested replacing gold as the major measure of value with uranium and plutonium (p. 49). The idea was not taken seriously, but it does demonstrate something important about the gold standard of the past: that an ultimate source of value was the use of gold in providing a military use (to pay soldiers), and that in the modern world the military role was taken more and more by nuclear weapons.

But the heavy dependence on archives makes, I think, for a greater sense of crisis about the whole decade than is really warranted by a comparison of the 1960s with other eras. Policy makers live and breathe in a world of continual problems and crises: that is how they carve out their influence. Reading this book in 2004 for this reviewer has a paradoxically reassuring effect: that the policy-makers and journalists can be very worried, but the problems are still fundamentally manageable. I believe many readers will be struck by the similarity of many of the historical views recorded here with very contemporary debates. De Gaulle’s phrasing was very striking: “The United States is not capable of balancing its budget. It allows itself to have enormous debts. Since the dollar is the reference currency everywhere, it can cause others to suffer the effects of its poor management. This is not acceptable. This cannot last” (p. 121). The German government concluded that the Germans “can easily develop neuroses that can be catastrophic for all of us. They did it before and they can do it again. A neurotic, disaffected Germany could be like a loose ship’s cannon in a high sea” (p. 136). As a result the discussions between President Johnson and Chancellor Erhard were envisaged by officials as “one of the most important decisions the U.S. has faced in the postwar period” (p. 148).

Such language raises the important issue of whether we should take the crisis rhetoric at face value. There was not in the end any major clash in this case, at least not something that deserves to go into the textbooks. Erhard reached an agreement on the offset and the Bundesbank agreed not to convert dollars to gold. Six American divisions stayed in Germany and there were no troop reductions. There is also, perhaps surprisingly given the overall thesis of the book, no evidence that the financial worries, however acute they were, actually constrained U.S. geopolitical decisions. Was there even one fewer U.S. soldier in South East Asia as a result of concerns about the dollar? In the end, in that kind of debate, the security concerns overrode the financial debate. When modern neo-Gaullists use the words and thoughts of the General and say that U.S. deficits are not acceptable and cannot last, are they right or wrong? And over what time period is the appropriate calculation about “cannot last”? The major contri-
The rise of online information services deserves attention not merely as a chapter in the history of computer technology, but as a noteworthy field in and of itself. Charles P. Bourne and Trudi Bellardo Hahn make clear that the topic is ripe for exploration by historians of various kinds.

The co-authors are both scholars and leaders in the field of online services. Their book offers a lavishly detailed examination of the early—although not earliest—years of the development of online services. Bourne and Hahn intend to “…capture all the relevant facts, interpret and synthesize them, and mold them into a coherent story” (p. 7). Indeed, the authors provide a thorough chronology of the development of online systems and information retrieval services.

The first four chapters explore the early research in online and other information systems that culminated in a multitude of university and nonacademic efforts to develop prototype or experimental systems. Subsequent chapters exhaustively trace the emergence and use of some particularly prominent systems. Before concluding, the authors devote two chapters to a meticulous examination of the rapid rise of operational online industry in the 1970s. These chapters are the book’s most useful and may be the best currently available treatment of the industry.

The series of questions that guide the authors’ research are reflective of those that economic historians might ask and merit repeating. “What role did hardware, software, telecommunications and database developments play in driving the progress of online systems telecommunications and online systems?” “What were the characteristics of the early online services? What were their purposes and functions, and how well did they accomplish them? What roles did formal evaluation play in the progress of online systems and services?” “What was the role of government and private funding?” “What were the characteristics, behavior, and attitudes of the online pioneers?” (pp. 8–9)

Bourne and Hahn demonstrate that information retrieval systems were constrained by existing computer technology, and show how adroitly the developers of online systems adapted to the speed and memory limits of the hardware. In fact, the features of some of these information services exceeded the aptitudes and needs of even the intended users. It is in describing the interplay between end-users and online systems and the cross-fertilization of ideas and systems features between researchers and prototype models that the book is at its best. Many early online systems were intended for use by discrete groups of end-users. More often, however, intermediaries—particularly librarians—proved to be the conduit connecting the ultimate user of information retrieval services with the results of an inquiry. Users contended with various shortcomings of the early systems and hardware and ultimately provided feedback that aided in making system improvements.
More broadly, the authors illuminate many instances of interaction and collaboration among the various online pioneers. Computer scientists, librarians, engineers, psychologists, and others used the lessons learned from the early, prototypical models to enhance the level of performance and range of functions available in subsequent online systems. Indeed, the period examined by the authors seems to have been one in which online information services advanced from the status of imperfect, experimental tools that were often used by searchers in specific disciplines or industries to become part of a robust online industry serving a much broader audience. The histories of emerging industries are often interesting, and the authors’ treatment of the creation of a viable online industry gives us a view of the challenges faced in establishing pricing strategies and creating a virtuous circle of systems, users, and databases.

Bourne and Hahn make clear that “the rapid developments of the 1960s were a direct result of the plenitude of government support for information storage and retrieval research, experimentation and development” (p. 408). They rigorously detail the extent to which numerous public and private funding sources supported research efforts that yielded a rapid advance in online systems. Hence, the remarkable degree of university, defense-related, and private efforts to create workable online systems becomes clear.

This book may well serve as a definitive reference on a decisive period in the development of online information retrieval. It is not, however, a history that will provide a useable introduction to the field. Bourne and Hahn assume of their readers a degree of knowledge that seems unrealistic. The level of detail often overwhelms a narrative that is laden with acronyms and abbreviations. Although meticulously researched, the authors’ insights are not always easily discerned. There is, for example, no sufficient analysis of the conditions that proved most conducive to establishing commercially viable online systems or firms; neither is there a clear examination of how efficiently the large sums of public money were spent.

Perhaps this is too much to ask. The authors broadly achieve their goal of providing a thorough record of the rise of online information systems, but they have circumscribed their audience in doing so.

JOHN S. NADER, State University of New York at Delhi

*Book Reviews*


This book is, in my view, the best one-volume history of computing in print. An updated version of the 1998 original incorporating one additional chapter on recent events and a few minor editorial changes, it surveys major developments since 1945 in clear, straightforward fashion. Its coverage, including many useful illustrations, is both wide-ranging and thorough. Paul Ceruzzi looks hard at every significant aspect of computing technology—logical design, electronic components, networking, and software—while also considering the economic dynamics of the industry and its major firms. Whether discussing technology or business, his analysis is consistently balanced. He exhibits no obvious bias toward establishing a particular overarching interpretative lesson. His book makes no grand claims about the importance of government, the wonders of free markets, the pervasive influence of defense, the centrality of entrepreneurship, or the preeminence of any technician or region. Yet Ceruzzi is not afraid to make clear and forceful assessments about such matters when addressing the particulars. Discussing the rise of early semiconductor manufacturers, to cite one notable example, he declares unequivocally that commercial customers, not the military,
drove the pursuit of standard products of high reliability. This point, like many others sprinkled throughout the text, constitutes an original and compelling interpretation that will interest specialists as well as general readers.

Several other examples of this sort stem from Ceruzzi’s extensive treatment of minicomputers and their suppliers, especially the Digital Equipment Company (DEC). Too often, histories effectively divide computing into two grand ages, the first dominated by IBM mainframes and the second by the networked personal computers of Intel and Microsoft. By taking time to examine DEC, Wang, and other suppliers of minicomputers, as well as their customers, Ceruzzi provides a much richer picture. Cognizant that microcomputers such as the PC emerged from a distinctive path and were not merely “smaller minis,” he nevertheless demonstrates how minicomputers provided a vital bridge between the two dominant forms by fostering new applications and opening new avenues of competition and innovation. We see clearly how DEC at once cut into the IBM empire while also providing many of the key figures in the PC realm, including the extraordinary creative team at XeroxPARC and a young Bill Gates, with vital experiences in the emerging world of interactive computing.

Insights such as these may not jump out readily for all readers. With its thoroughness and care for detail, this book has something of an encyclopedic quality, with all that entails. (Students in my classes persistently describe it as dry, though I just as persistently keep assigning it.) Ceruzzi packages his material in small bites, with frequent subheadings. His chapter breaks combine topics with chronology in a manner that further fragments the material and undermines the narrative line. At points the book seems unnecessarily repetitious. Such is the price we pay for comprehensiveness.

Readers looking for rigorous economic analysis of the computer industry may be disappointed. This book is essentially a business and technical history of computing, not an economic one. Ceruzzi offers little summary data about the diffusion of computing technology and makes virtually no effort to assess its impact upon users. Nor is he primarily concerned with the dynamics of competition among producers. Still, his careful assessment of key technical innovations, together with his insightful treatments of the individuals and firms that created them, make this work of great value to anyone seeking to comprehend the first half-century of this vitally important technology and the industry it spawned.

STEVEN W. USSELMAN, Georgia Institute of Technology


This fine collection of ten papers, with an overview introduction by Marc Flandreau, spans how foreign advisors proffered advice to countries suffering financial crises during the classical gold standard before 1914, the more chaotic interwar period of the 1920s into the early 1940s, and then the postwar doctrines of the International Monetary Fund (IMF) when it was the world’s chief monetary physician.

The three periods are more remarkable for their similarities than their differences. Recurring bouts of countries’ losing monetary or fiscal control, often accompanied by international overborrowing followed by threatened devaluation, is depressingly familiar. Foreign advice, if it were to have a chance to be effective, carried with it the (implicit) promise of international bridging finance—often associated with debt consolidation—to help the country surmount its crisis with less painful internal retrenchment.
In the first two periods, however, this bridging finance was usually provided by some major international investment house—such as Rothschild or J. P. Morgan—with the money doctors being remarkable individuals. One such was the Princeton professor Edwin Kemmerer, who promoted monetary reforms throughout Latin America and some parts of Asia from 1903 into the 1930s. Immediately after World War II, however, the task fell to government agencies such as the U.S. Treasury with its Marshall Plan and various development assistance programs. Since then, the IMF has been the principal international crisis manager—with less scope for colorful freelance money doctors.

Has the quality of monetary medical care improved with its greater bureaucratization? A loaded question to be sure. However, in each period, institutional arrangements in the international economy and economic doctrines, which cycle through time, predominated. Under the gold standard, the traditional conservative advice was to consolidate errant public finances and tighten domestic monetary policy in preparation for a return to full convertibility at the traditional mint parity, if possible. Insofar as a new or revamped central bank was set up as in the Kemmerer missions, the central bank’s future flexibility was to be severely limited by the gold convertibility constraint and the “real bills” doctrine: that discount loans could only be made on the basis of fully collateralized commercial paper, with no direct lending to the government or its designees. Some of the international bridging loans, which would be conditional on these reforms being made, could provide gold backing for any new currency issued.

Rather than accepting a permanent devaluation, the postcrisis appreciation back to a traditional parity would make it easier for domestic private debtors, as well as the government, to repay their (restructured) foreign currency debts. And, of course, the international banking house, which was the lead manager for the bridge loan, was mainly interested in having it and its syndicated co-lenders be repaid. Unlike the modern period, poverty alleviation and “development” were not explicit objectives. But perhaps this tough love worked better, at least until the implosion of the international gold standard in the 1930s.

With the collapse of the gold standard and the advent of Keynesian economics, the ideology of monetary reform for developing countries changed dramatically. In a fascinating chapter, Eric Helleiner shows how, immediately after World War II, money doctoring, led by Yale University’s Robert Triffin together with U.S. Federal Reserve and Treasury officials, turned away from the traditional Kemmerer fixed-exchange-rate orthodoxy of gearing monetary policy to respond to changes in the balance of payments. Instead, economic development became the focus: the government was not discouraged from leaning on the central bank to direct subsidized credit to the private sector or to government-controlled agencies. In addition, capital controls and tariffs were considered complementary to a more nationally oriented monetary policy—particularly in Latin America where Robert Triffin and Raul Prebisch made common cause.

An intriguing subtext in Helleiner’s paper was the pressure from the new money doctors, often American but including the IMF and IBRD, to hasten the dissolution of currency boards in ex-British colonies in East and West Africa, the Indian subcontinent, and Malaya, in favor of national central banks. Partly to preserve the sterling area as a potential financial resource for Britain, but also because of the suspicion that newly independent governments might well abuse their money-issuing powers, the British Treasury lobbied hard to keep currency boards—particularly those spanning contiguous countries—intact. But anti-imperialist money doctors, notably Thomas Balogh of Cambridge University, made common cause with left-of-center native politi-
cians in the newly independent countries to force the breakup of regional currency boards into national central banks. France resisted this change in ideology much more effectively, and maintained its CFA zone in Africa more or less intact with only one or two defections. Subsequently, the French African countries have had a much better record of monetary stability than the rest of Africa.

The irony in all of this is that, by the 1990s, the IMF had returned to trying to impose “hard” money systems, including the occasional currency board or outright dollarization, on its struggling clients in Africa and Latin America, but with indifferent success. After recognizing that the gold standard is passé, a resurrected Professor Edwin Kemmerer might well feel vindicated—although somewhat saddened—by the monetary chaos in so many of his former clients.

RONALD MCKINNON, Stanford University


1 Nations tend to develop myths that attribute positive qualities to their founders and uniqueness to their political institutions. However, there is a quip attributed to Bismarck, “People will sleep better not knowing how their sausage and politics are made.” In this book, Alberto Alesina and Enrico Spolaore deconstruct the Bismarckian sausage factory in order to show us how states are made. They present their theoretical argument in eight steps, each beginning with a verbal outline followed by a formal model. Asterisks guide the nontechnical reader around the algebraic danger zones. The result is a remarkable set of propositions that leaves little place for individual leaders, heroism, uniqueness, or even a subtitle.

The authors begin by discussing the simplest kind of law making factory, a unitary state with pluralistic political institutions. They argue that in deciding where to place their political boundaries, voters trade off the advantage of a larger state in providing public goods at lower cost against the disadvantage of increased heterogeneity of preferences that would result. The authors show that total welfare is maximized when the world is divided into an optimal number of identical states.

Subsequent chapters add complexity to this initial core. A first modification allows for unilateral secession of border regions. Because residents of peripheral areas are more distant from the capital, where the public good is assumed to be produced, they receive fewer benefits for their taxes and therefore have an incentive to secede. As a result, under these more realistic assumptions, the equilibrium number of states is greater than the number that maximizes total welfare.

A second modification of the initial structure permits transfer payments that compensate potential secessionists in peripheral regions for the low benefits that they receive. If such schemes are feasible, then the equilibrium number of regions approaches the optimum. However, as the authors demonstrate in an elegant algebraic proof, if a border region agrees to join a larger state, there is a time inconsistency problem. Once unification has occurred, since the median voter of the new state pays in taxes more than she receives in transfers, she will approve a decision to eliminate the transfer payments.

As Frederic Lane argued in a seminal paper (“Economic Consequences of Organized Violence.” This Journal 18, no. 4 [1958]: 401–17), the vast majority of histori-

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cal states do not fit the democratic model preferred by Western political economists. Rather, actual political decision making has tended to be monopolized by a minority of citizens who provide protection in return for tax revenues. Alesina and Spolaore therefore devote a chapter to states ruled by a monopolizer of power. Their Hobbesian “Leviathan” is a ruler who need consider the welfare of only the fraction delta of his subjects. They demonstrate that when delta is equal to one half, the result is the optimal number of states. More generally, with delta smaller than one half, the outcome is a set of large states that are too few in number.

So far, the model predicts that actual democratic states will be too small to maximize the welfare of their citizens. How then can they account for evidence that the wealthiest states in terms of per capita GDP tend to be small in terms of population? The authors devote a chapter to commercial policy, arguing that if trade barriers are lowered, it is easier for small states to be viable because they can reap scale economies by selling in foreign markets. As the authors recognize, however, trade policy is itself a function of country size, because smaller states will tend to favor freer trade.

The last three steps in the discussion deal with conflict, generalized war, and federalism. The reader learns that there is also a two way relationship between country size and conflict. For a given probability of conflict, a large country offers advantages to its citizens because it can produce the public good of protection more cheaply than a smaller country. However, because larger countries are less dependent on trade than smaller ones, a world of larger states can afford to have more frequent trade disturbing international conflicts. As for the degree of decentralization, it depends on the extent of democracy. The higher the percentage of the population to be considered in political decisions, the greater is the optimal degree of decentralization.

An empirical chapter puts some of the authors’ principal hypotheses to the test. Are actual states too small for the efficient provision of public goods? Evidence that the public share is larger in small states than in large states (with size measured by population), suggests that the cost of public goods is higher in the former. Does having too many states lower individual welfare? The higher per capita GDP and faster per capita economic growth in large states compared to small states in the postwar period again supports the theoretical result that the number of states is great than optimal from a welfare standpoint.

Finally, in an historical chapter, the authors apply their theoretical model to explain systematic changes in state size since the mosaic of small political units in medieval Europe. After 1500, they argue, the number of states declined, because an increase in trade restrictions and a greater frequency of international conflict made small states nonviable. Outside Europe, the movement to large empires was excessive, owing to the dictatorial nature of political power in these regions. In the late eighteenth and nineteenth centuries, rising external threats and a desire to have more weight in commercial disputes led to unification in the United States, Italy, and Germany. Then, increasing protectionism in the late nineteenth century caused the industrialized countries to search for colonies as a vent for surplus production. Finally, a movement to free trade after World War II favored the breakup of colonial empires, including that of the Soviet Union, into a large number of small, open economies.

This book is a tour de force, offering a highly original synthesis of political science, economics, contemporary statistics, and historical facts. The argument is crystal clear. And the authors succeed in building an inexorable momentum as they proceed step by step from the simplest model to the complexities of the real world. Their thesis that first, voters trade off the lower cost of public goods against increased heterogeneity in determining their state’s borders and second, this decision is sensitive to the probabil-
ity of conflict and the cost of exporting to foreign markets, provides a powerful tool for explaining historical change.

But do the primary forces that determine state size lie on the demand side? For William McNeill (*The Pursuit of Power*, Chicago: University of Chicago Press, 1982), it has been primarily military technology—a supply side factor—that has determined state size. When military scale economies increased, he argued, it became less costly for centralized rulers to put down revolts in outlying regions. Although the authors cite McNeill’s work, they fail to take into account that it implies a completely different vision of international conflict from the one they model. When military technology is stable over long periods, state borders will tend to remain in equilibrium and major conflicts should be the exception. Such was arguably the case for long periods of Chinese and Roman history, for the first part of the nineteenth century and for the last half of the twentieth century. Systematic conflict will then arise when the cost of providing protection changes, as occurred in the early modern period and between 1850 and 1950. In short, if we drop the assumption that the probability of conflict is exogenous, we need a theory that deals explicitly with changes in the cost of providing protection.

Moreover, on the demand side, can we assume that the willingness to trade, too, is essentially exogenous? In his 1948 Beit lectures at Oxford University, Harold Innis (*Empire and Communications*, Oxford: Clarendon, 1950) argued that the size of nations was determined by the relative costs of storing, decoding, and transmitting information. With the diffusion of standardized Latin and Carolingian script in the centuries preceding the high Middle Ages, he suggested, the cost of storing information in multiple copies fell. As a result, European states declined in size, and there arose considerable trade across political boundaries. In the early modern period, the diffusion of printing in the vernacular led a movement in the opposite direction, with the formation of large nation states and growing barriers to trade across linguistic borders. More recently, with a return toward the medieval pattern in many parts of Europe, can we ignore the decentralizing effects of the microelectronics revolution?

These considerations of military and informational technology suggest an alternative interpretation for the authors’ statistical results from the most recent period. The negative correlation between country size and the public share could reflect not the cost of public goods but the formation in the past of linguistic zones of different sizes. Voters today in large, heterogeneous states may simply be less willing to share their income than those in small, homogeneous states. If so, the negative correlation between the public share and country size need not be a sign that small states are inefficient.

The positive sign of country size in the income and growth regressions is also problematic. Here the effect of low income on the diffusion of military innovations is important. In the nineteenth century, the poverty of Africa and Asia made these regions easy prey for the colonial powers that conquered them in bits and pieces. When these powers subsequently retreated under pressure from nationalist movements, they left a large number of poor, small states whose boundaries bore no relationship to voter choice as modeled by the authors. Yet these states arguably dominate the authors’ regressions. Might causality not go from poverty to state size?

In short, by the end of the tour of the Bismarckian sausage factory, the reader will have satisfied her immediate hunger to understand how states are formed. Nevertheless, she will likely put the book down with the feeling that there is more to the question of the size of nations than the standardized frankfurters cranked out by Alesina and Spolaore. Of course, provoking the reader in this way may be precisely their in-
tention. They have provided us with what will surely become a standard reference point for speculation about historical change.

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There are fundamentally two types of comparative studies. The first intends primarily to shed light on select cases; the second intends to generate theoretical or historical propositions through illustrative case studies. Machiavelli’s Children is best understood and appreciated as belonging to the second genre. In the tradition of Barrington Moore, though challenging Moore’s own structural argument, Richard Samuels has written an equally bold book that will attract, and justifiably so, considerable attention. Moore and many other contemporary comparative scholars have stressed the determinative role structures play in historical development; structures both constrain and capacitate historical action. Samuels, by way of contrast, seeks to privilege agency, and more particularly, the role of leadership. “We have seen how often leaders transcend options that seem thoroughly limited, and how leadership itself becomes the contingency that shifts the trajectory of historical change.” Great leaders are those who “stretch their constraints” by creatively deploying a repertoire of historical reinvention, inspiration, bullying, corruption, bricolage and, above all, Machiavellian virtuosity. The book’s very title signals a profound indebtedness to Machiavelli whose analysis and very spirit pervade every page of Samuels’ text.

Machiavelli’s children, in fact, are those leaders in Japan and Italy whose choices enabled these two late developers to flourish despite less than favorable objective circumstances. Samuels’s analysis pivots around decisions made by selectively paired Japanese and Italian leaders at comparable moments of political and economic modernization. Focusing on questions of identity, wealth, and power, Samuels relates how Japanese and Italian leaders confronted problems of state building, economic organization and legitimation. He provides readers with a conceptually rich, lively narrative that not only accounts for Japanese and Italian development separately, but also how experiences from one case inform and deepen an understanding of the other. Given the fact that Japan and Italy are often treated as exceptional Asian and European cases, Samuels’s comparative attempt is both unprecedented and commendably daring. Beyond deep cultural differences, one confronts obvious asymmetries: the military in Japan, the Church in Italy, liberal hegemony in pre-Fascist Italy, its absence in Japan, to name a few. Still more striking, the genesis of distinctively fascist order in Italy, and the fact that Mussolini had no Japanese counterpart, no leader with whom he could be paired. Generally speaking, Samuels avoids forcing the particularities of the separate cases into an artificial comparative mold, but admirably demonstrates how such differences led to different choices and constructs by leaders in roughly comparable circumstances, how they seized distinctive opportunities and sorted through contingent alternatives.

In the final analysis, however, this book will be valued less for what the author has to say about Japan and Italy, than the theoretical and historical points he makes about human agency and the role of leadership. These will be savored both by seasoned scholars who will reflect upon their work in new ways, as well as students newly initiated into the complexities of comparative historical analysis. In this regard, the intro-
ductory and concluding chapters are absolute gems that can almost be disassociated from the Japanese and Italian cases which, appropriately read, serve less as rigorously controlled comparisons than as selective historical illustrations.

Read alternatively as a rigorous comparative history, *Machiavelli’s Children* is not without flaws. Richard Samuels is a well-regarded authority on modern Japan who only learned Italian and began to conduct research on Italy in 1996. Mastering only one of the three selected historical periods (liberal Italy, Fascism, postwar Italy) in so short a period of time would have been a major accomplishment. In seeking to cover Italian history from the mid-nineteenth century to the present, Samuel’s ambition tends to outrun his competence. While in most instances, certainly those most central to his analysis of selected leaders, Samuels is on firm ground and gets things right, he nevertheless commits surprising mistakes of fact (for example, misattributing the encyclical *Non Expedit* to the progressive Leo XIII rather than to the reactionary Pius IX) and offers occasional judgments which either have no empirical basis at all or are so misinformed that one wonders why they were not flagged by Italian specialists who read the manuscript before it went to print. Citing no sources, for example, he claims that Fascist attacks against newspapers, socialists clubs, and peasant leagues “endured him [Mussolini] to industrialists” (p. 135) and that “industrialists employed the fascisti to bully unionists” (p. 164). Elsewhere, however, he cites a recent study on industrialists and Fascism that not only dismisses such folktales, but documents the industrialists’ preoccupation that Fascist violence against workers might rekindle another wave of factory disruptions, after they had successfully defeated general strikes and factory occupations on their own, never asking nor receiving Fascist assistance. Most incredibly, Samuels credits Fascism with “the best economic performance in Italian history” (p. 166), at the time Mussolini made the fatal misstep of entering the war on the side of Nazi Germany. One can only wonder what set of indices or what authority Samuels had in mind, considering the long-standing consensus among Italian economic historians that Fascist policy (particularly ruralism, maintaining an artificially strong lira for reasons of prestige, and autarky) undercut economic performance. In any case, the Italian economy did far better during the first decade of the twentieth century and during the initial, residually liberal stage of Fascism (1922–1927) than during the thirties, when a fully articulated Fascist state was put in place.

One wonders also why such formidable *Confindustria* leaders as Gino Olivetti and Angelo Costa are never mentioned, given the relations the former had with Giolitti and Mussolini, and the latter had with De Gasperi, relations which in consecutive historical periods struck at the very heart of national economic policy. *Confindustria*, after all, was not merely the industrial peak association, it was the undisputed *interlocutore primario*. In the contemporary period, one also wonders why so much attention is devoted to the *Lega Nord* leader Umberto Bossi and so little to the *Alleanza Nazionale* leader Gianfranco Fini, who not only transformed a marginalized, former neo-fascist party into a major center-right one, but occupies the second most important position in the Italian government. Unlike Bossi, Fini remains one of the most popular politicians in Italy, and unlike the self-destructive *Lega Nord*, the *Alleanza Nazionale* might prove to be the most durable member of the ruling coalition.

These are the criticisms of an Italianist that may or may not be of great significance to nonspecialists. Just as *Social Origins of Dictatorship and Democracy* elicited harsh reactions from area specialists without seriously undercutting its comparative significance, the same might be said of *Machiavelli’s Children*, very much a companion volume. In the best sense, this work is an important conversation between Barrington
Moore and Richard Samuels on the nature of history and the comparative method; as such, it commands our respect.

FRANKLIN HUGH ADLER, Macalester College