The current fascination with globalization compares with the first great era of globalization, from the middle of the nineteenth century through to August 1914. For the earlier period, the international gold standard is generally seen as the unifying money machine, with stable exchange rates that undergirded the huge expansion in world trade and capital flows. But gold only became the dominant international standard by the mid-1870s. Before that, conventional thinking (including my own before reading this excellent book) saw only a confusing mélange of bimetallic or inconvertible paper standards.

However, Flandreau shows that the era of bimetallism from 1848 to 1873, during which France owned almost half of the world’s stocks of both gold and silver, provided more stable exchange rates and better international macroeconomic stability than did the pure gold standard that succeeded it. Flandreau carefully documents his theory for the period from 1848 to the “crime” of 1873, when France suspended the free coinage of silver and moved towards a gold standard.

Gold and silver have extensive nonmonetary uses, both industrial and decorative. They are also subject to substantial supply shocks from new ore discoveries or innovations in mining technologies. To the casual observer, the natural market price ratio between gold and silver is dominated by these fluctuating nonmonetary demands and supplies. According to conventional wisdom, if any one monetary authority chooses to fix the relative price, say, at 15.5 ounces of silver to 1 ounce of gold as the French government did, the price would be naturally unstable. If the notional “market” price of gold rose to say 16.5, people would proffer only silver to mint and thus put the whole regime on a silver standard. Or, if the notional market price fell to 14.5, only undervalued gold would be proffered (Gresham’s Law), putting the whole regime on the gold standard. Reinforcing this conventional idea that bimetallism was subject to such knife-edge instability is the quaint story of Sir Isaac Newton. When master of the Royal (British) Mint in 1717, Newton “mistakenly” underpriced gold in terms of silver and thus accidentally put Britain on the gold standard—on which it remained (except for major wars) until 1931.

However, the core of Flandreau’s book is to show that this knife-edge instability is not a general characteristic of bimetallism. Once monetary stocks of gold and silver become substantial, he shows theoretically (chapter 1) that there can be a fairly wide variance in the ratio of an economy’s total stock of silver to that of gold consistent with the government fixing the price ratio at 15.5—as the French government did until 1873. Then, in subsequent chapters, he shows that French stocks of monetary gold and silver were indeed large, not only relative to nonmonetary uses in France, but relative to those of neighboring countries such as Britain as well as Germany, which was more or less on a silver standard before 1873. Austria and major Asian countries such as India and China were also mainly on silver. In France, monetary circulation outside the central bank was heavily metallic, unlike in Britain, where deposit money and banknotes had become more common. Because of the large stocks of (private) monetary gold and silver circulating in France, it became the center of arbitrage for fixing exchange rates between the silver bloc countries to its east and the more developed gold
bloc countries to its west—with little or no orchestration needed from the Banque de France. Indeed, before 1873, the world seemed to have more broadly stable exchange rates linking Asia and Europe than subsequently, when a monometal gold standard became dominant outside of Asia.

True, understanding the details of how this arbitrage worked requires mind-numbing knowledge of the interactions between gold points and silver points, through which Flandreau carefully guides the reader. The minting of coins was largely farmed out to the private sector. But the range of exchange-rate variation was small, with gold or silver specie being the automatic means of settlement between the major financial centers.

Surprisingly, this worldwide bimetallic system was resilient to macroeconomic shocks. For example, major discoveries in California and Australia in the 1850s into the 1860s flooded the system with newly mined gold, but the inflationary consequences were ameliorated by adjustments in private monetary circulation—largely in France. In one of the book’s most fascinating chapters, Flandreau documents how gold coins began to replace silver coins—starting first in Paris and then only slowly filtering out to the French provinces. Because gold and silver shocks were generally uncorrelated, and because the total monetary base of both metals was very large, supply shocks were naturally dampened better than they were under the pure gold standard that followed.

Why then did bimetallism come to an end? Flandreau recognizes but somewhat discounts the standard argument that it was more convenient to have just one metal as the unit of account—or that there was a natural progression from base metals or cowrie shells to silver and then to gold. Rather, he sees the sudden demise of bimetallism as an accidental consequence of the Franco-Prussian War of 1870 when France had to pay a huge indemnity to Germany. Germany then used it to build up its monetary base in gold in order to join the gold standard, but then attempted to unload all of its now-surplus silver in the Paris market. Worried about capital losses if they suddenly accumulated huge amounts of silver, the French authorities then restricted the free coinage of silver, and the open-market gold price of silver specie then suddenly fell, and with it Asian exchange rates. More generally, the European and American economies paid a rather heavy price from the demonetization of silver because of the consequent fall in their collective monetary base and the deflationary pressure from the 1870s to the mid-1890s—sometimes called the world’s first great depression.

Flandreau’s book, first published in French in 1995, is a must read for all serious students of international monetary history.

RONALD MCKINNON, Stanford University


Robert Allen’s monograph is a work of serious and deep scholarship that spans a century and a half of Russian economic history. Its aim is to re-evaluate the Soviet economic system under Stalin and his successors, correctly characterized by Joel Mokyr (on the back cover) as “the greatest failed experiment in the history of the human race.” Allen’s work synthesizes a vast literature, not only on Russia but on economic history and economic development generally, to which he adds his own original research on comparative crop yields, Soviet living standards, and policy simulations of
the 1930s. For those who wish to disagree with his findings, he provides careful documentation and explanations of methodology.

Allen’s major conclusions are that Russia’s (the Soviet Union’s) growth was accelerated by Stalin’s forced industrialization, which permitted Russia, along with Japan, to make a rare breakthrough from relative poverty to industrialization. Although imperial Russia had made considerable progress, a noncommunist Russia would not have fared nearly as well. Allen’s simulations show that the industrialization success was explained by Stalin’s decision to one-sidedly invest in heavy industry and by soft-budget constraints that promoted full employment in an economy with surplus rural labor. His recalculations of Soviet living standards show that the Soviet population, especially urban workers, shared in the benefits of rapid growth, thereby contradicting the famous calculation of Abram Bergson (The Real National Income of Soviet Russia Since 1928. Cambridge, MA: Harvard, 1951, p. 251), which shows stagnant or declining real wages and living standards in the 1930s. Collectivization does not perform well in Allen’s simulations due to the resulting loss of livestock and output in its early years. A noncollectivized Soviet system that preserved Stalin’s investment priorities and soft budgets would have yielded the same or better results. Allen’s take on the Soviet climacteric after the 1960s differs from most other explanations: Although factors such as diminishing returns, and disincentives to investment likely played a role, it was poor investment decision making—namely the emphasis on modernization of old facilities—that was the primary culprit. Unlike those who blame declining Soviet performance on a collapse of discipline, Allen argues (and I tend to agree) that the problem was more that bad orders were being obeyed, not the disobedience of good orders. Allen has set, with his provocative set of propositions, a broad research agenda. Under normal circumstances and given the importance of the subject, a number of researchers would enter the fray to test and examine Allen’s methodology and assumptions. I fear that this will not happen, given the limited number of younger scholars still active in the field and the language requirements for serious research.

Much of Allen’s research was written prior to the first fruits of economic research in the formerly secret Soviet state and party archives, most notably the works of R. W. Davies and Stephen Wheatcroft (The Years of Hunger: Soviet Agriculture, 1931–1933. Houndsmills: Palgrave, 2004), Paul Gregory (The Political Economy of Stalinism: London: Cambridge, 2004), and Paul Gregory and Valery Lazarev (eds.) (The Economics of Forced Labor: The Soviet Gulag. Stanford, CA: Hoover Press, 2004). Allen himself can perhaps himself reexamine his own conclusions over the next five years to see how they hold up to the emerging findings of archival research.

My own substantive remarks relate to this “open” research agenda raised by Allen’s provocative research, some of which can be framed in the light of this archival research.

Allen’s finding of substantial growth of living standards, especially those of urban workers, between 1928 and 1937, is generally supportive of my own conclusion (Po
litical Economy of Stalinism, chap. 4) that Stalin and the Politburo were acutely sensitive to the effects of real wages on work effort and had to balance increased investment against the reduced effort associated with falling living standards. This conclusion is buttressed by the investment cycle of the mid and late 1930s, which to a degree is the mirror image of consumption statistics. However, given the extreme care of Bergson (and his colleague Janet Chapman), it is difficult for me to decide which estimate of per capita consumption is “better.” Part of Allen’s argument relates to his preference for index numbers that average 1928 and 1937 price weights, while Bergson cites primarily 1937 price weights—a dispute that involves us in the intricacies of
index number theory, but I had felt earlier that Bergson’s case for “later” prices was convincing in that it measured the relative ability to produce the “modern” output mix in the two periods. The actual calculational differences between Bergson and Allen relate to the relative weights of state-fixed (and low) agricultural procurement prices versus “free market” farm prices. I fear we will never get a good fix on this issue because of the growing and extreme repression (including death and gulag sentences) of “speculators”—hardly a conducive climate for the gathering of statistics on sales to organizations other than the state. Allen’s firmest evidence is anthropometric; namely, the rise in life expectancy in the late 1930s, but this could have been a consequence of the educational revolution as well.

The heart of Allen’s book comprises his simulations of various policy alternatives open to the Soviet leadership in 1928, where his basic policy choices are collectivization versus noncollectivized agriculture, hard or soft employment constraints, and investment allocations to light or heavy industry. Allen explains his simulation models clearly for those wishing to perform sensitivity analysis, and he clearly explains his choice of parameters, many of which are drawn from the literature. Given his intense interest in investment strategy, most simulations are reported with different investment allocations between light and heavy industry. What Allen reports are his “best” estimates without much sensitivity analysis. Assumptions (from Appendix B) that caught my eye were that labor was allocated to light and heavy industry to equalize marginal rates of return (an assumption I would question given the administrative allocation of labor), that consumer goods markets were “cleared” by prices (if I understood correctly). Moreover, it would be enlightening to see sensitivity analysis on agricultural supply elasticities. I do not know whether Allen’s basic conclusions could be altered by such experiments, and I fear that other researchers will not take up this challenge.

Allen makes use of Ragnar Nurkse’s and W. Arthur Lewis’s famed models of surplus agricultural labor, and his simulations suggest that a key ingredient of the success of Soviet industrialization was its ability through administrative methods and willingness to take enterprise losses on a massive scale to redeploy agricultural labor from the countryside where its marginal product was zero to industry and construction where it was, at least, positive. If a “capitalist” labor market policy had been used, simulations show that the transfer would have been much slower and successes more muted. My own research (*Political Economy of Stalinism*, chapter 4) suggests that Allen’s simulations overlook a negative side effect of soft budgets that proved to be the major labor headache of the Soviet leadership. With soft budgets and overfull employment, labor discipline deteriorated, labor turnover skyrocketed, and labor productivity collapsed. Official concern is reflected in the fact that repression was applied to the entire industrial labor force in 1940 and thereafter with laws that made absences, tardiness, and job changes criminal offenses. Given the extreme concern (and even panic) of the Soviet leadership over the discipline issue, this negative effect would somehow need to be grafted to Allen’s employment simulations, but it is unclear how.

Allen concludes that the partial effect of collectivization was, at best, neutral. He obtains this result by weighing the loss of livestock and grain output during the first three years of collectivization against the increase in grain marketings. Allen also recognizes the negative effects of collectivization in the form of the famine of 1932/33 that cost 5.5 to 6.5 million lives, the deportations and camp sentences of the most able farmers, and the organizational disruption of agriculture. Allen, therefore, can be interpreted as saying that Stalinist industrialization without collectivization would have been the preferred alternative. It is hard, however, for me to envision Stalin’s acceptance of a political solution that failed to impose strict Soviet rule in the countryside or
to extract surpluses by terror. I realize that Allen has taken upon himself the task of economic policy alternatives, but in this case economics and politics are too closely intertwined. In my view, the imposition of command in both industry and agriculture imposed an extreme price on Soviet society that must somehow be weighed against any economic successes. Allen dismisses the fractions of labor force in camps (5 percent of industrial labor and 2 percent of the total labor force) as “not large enough to be decisive” (p. 108), but the stock of forced labor at any one point in time is not the appropriate measure of the economic and social effects of repression. According to the forthcoming seven-volume History of the Stalin Gulag (Istoriia Stalinetskogo Gulaga, introduction. Moscow, Rosspen, 2004) 15 percent of the Soviet adult population had been in prison at the time of Stalin’s death in March of 1953. It is hard to conclude that the disaffection associated with such extreme proportions did not have disastrous economic and social effects.

Taken in total, Allen’s monograph could be interpreted as saying that relatively good economic decision making of the 1930s, backed by the clout of command and repression, allowed Russia to join the small club of advanced industrial nations but that this success was followed by bad decision making that resulted in the eventual collapse of the system. He interestingly argues that the collapse was not caused by the “failure of managers to act in accord with the plans. On the contrary the plans were implemented; the problem was that they did not make sense. The great strength of Soviet socialism was that great changes could be wrought by directives from the top. . . . By the 1970s, the ratio of good decisions was falling” (p. 211). I find myself in agreement with Allen’s conclusions, but he fails to answer the reasons for “the failure of imagination at the top” (p. 211). In my view, the failure was explained by F. A. Hayek and Ludwig Mises in the course of the socialist controversy of the 1920s through the 1940s. Lacking markets to provide relative valuations, Soviet leaders had no checks and balances to tell them their decisions were consistently wrong. Their mistakes were therefore not the result of human error—different Soviet leaders would eventually have made the same disastrous mistakes—they were the result of the economic system.

My final point relates to Allen’s discussion of my own work on the economic growth of the prerevolutionary Russian economy (Paul Gregory, Russian National Income 1885–1913. Cambridge: Cambridge University Press, 1982), the basic results of which he uses in his own calculations. Allen correctly characterizes my own work as belonging to the “optimistic” school, for it shows that Russian economic growth was rapid by the standards of the late nineteenth and early twentieth centuries. In this book I speculated that a noncommunist future would have produced a contemporary Russian economy not that far removed in affluence from its immediate European neighbors. Allen disagrees with this speculation and demonstrates that if Russia’s per capita growth rate for the 1885–1913 period were projected forward to the end of the Soviet period, Russia’s GDP per capita would have been only 78 percent of the USSR’s actual figure (p. 33). Only if a capitalist Russia’s growth had accelerated after World War I as did Germany’s could my speculative case be made. As Allen points out Russia’s prerevolutionary economic institutions left much to be desired (pp. 14–15). The political and institutional history of a noncommunist Russia can not be modeled, but if Russia had approached twentieth-century European institutions, its economic performance would have improved relative to its past performance. Allen emphasizes the deleterious impact of the Great Depression on a noncommunist Russia’s agriculture, but contemporary living standards of affluent economies with competitive agricultures (Australia, New Zealand, Canada, and to some degree the United States) are today unaffected by the remote events of the late 1920s and early 1930s.
Even if we accept Allen’s calculation that noncommunist Russia of 1989 would be 22 percent poorer than its Soviet counterpart, we could still question whether this difference was worth it. Irwin Collier (“The Measurement and Interpretation of Real Consumption and Purchasing Power Parity for a Quantity Constrained Economy: The Case of East and West Germany,” *Economica* 56 [February 1989]: 109–20, and “The Welfare Standard and Soviet Consumers,” *Comparative Economics Studies*, forthcoming June 2005) has shown that consumers in planned socialist economies would sacrifice 12–15 percent of their income to have a free market in consumer goods. Moreover, in the course of transition to a market economy, the disappearance of whole sectors of the planned economy revealed their inability to pass any type of market test in a market setting. As welfare-based measures of Russian GDP replaced Bergsonian production-potential estimates, the size of the economy shrank to a small fraction of its previous self. A noncommunist Russia would, in my opinion, not have suffered the massive waves of repression and terror of the Stalin years, which, in my view (Gregory, *Political Economy of Stalinism*, chap. 3) were not a historical accident of Stalin’s victory in the power struggle, but an organic feature of socialist planning as argued by F. A. Hayek (*The Road to Serfdom*. Chicago: University of Chicago Press, 1944) 60 years ago.

**PAUL R. GREGORY,** University of Houston

**LATIN AMERICA**


This book is dedicated *To the memory of Carlos F Diaz Alejandro*. Specialists on Argentine economic history are familiar with his 1970 publication, *Essays on the Economic History of the Argentine Republic* (Economic Growth Center, Yale University), an early application of econometric methods to historical study. He was part of a group at the Economic Growth Center inspired by Simon Kuznets and sponsored by the Ford Foundation. The brightest young development economists came together there in the early 1960s and went forth from New Haven to assemble country-specific data and write a number of path-breaking studies. Among them were R. Albert Berry, Werner Baer, Gerald Helleiner, Clark Reynolds, Shane J. Hunt, and Howard Pack. Carlos may well have been among the very best of those very best. He died prematurely in 1985. In recognition of his contribution to economic history, a number of books honor his memory and several awards are named for him. This book is thus one in a series to remind us of the special contribution Diaz Alejandro made in his career.

An Irish econometrician who worked for years on Argentine macro-economic policy for the World Bank, Desmond McCarthy, has said of that country’s economic policies that everything has been tried twice and failed both times. Data from Angus Maddison, *The World Economy: A Millennial Perspective* (OECD Development Centre, Paris, 2001, p. 195) indicate a growth of per capita product of 1.6 percent, 1870–1913. As della Paolera and Taylor write in their thoughtful introduction, “Argentina’s 1913 level was clearly in the world top ten, and almost the top five. Whatever its exact status in 1913, for all practical purposes Argentina was an advanced country” (p. 3).

What has happened since 1913? The cumulative per capita product growth rate, despite many ups and downs, was about 1.1 percent per annum (excluding the painful
downward adjustment in the last few years). Quite respectable, as such a growth rate can double average standards of living in 70 years or so. But Argentina’s 2002 IS$10,000 per capita product is now very far from the top ten. The high income countries of Europe and its overseas expansion now average more than IS$28,000 per capita (data from World Bank World Development Indicators 2004).

Australia, like Argentina, an antipodal country whose modern economy focused on primary product export, but one unencumbered by rent-seeking governments and leaders, grew its per capita product at 1.8 percent per annum, 1913–1998. Somewhat more than Argentina but enough to put its per capita product level over IS$27,440 by 2002. Both countries were primary product exporters; both suffered the vicissitudes of demand fluctuations in the center countries. They varied in the effectiveness of public policy.

The 12 chapters in this excellent collection are generally aware of policy failures and their linkage to the unfolding events in the several sectors. Perhaps most poignant is the chapter by della Paolera, Irigoin, and Bozzoli, “Passing the Buck: Monetary and Fiscal Policies,” pp. 46–86, which emphasizes the willingness of a succession of governments to pass difficult policy choices on to the next occupant of the presidency. “The once abundant economic rents of Argentina have been consumed bit by bit during the twentieth century, and this of necessity prompts society to take more seriously the simple but ill-understood phenomenon of economic scarcity. . . . [A] new constitutional and fiscal design could help the country escape the trap of discretionary rulers and diminish the incentives for opportunistic administrators to pass the buck” (p. 76).

In their epilogue, della Paolera and Ezequiel Gallo address the Argentine puzzle: How could a country do so well at the start of the twentieth century, a belle époque, yet do so (relatively) badly at century’s end? Their answer: Argentina “missed the opportunity to design the right institutions that would secure sustainable growth and insulate the society from the voracity of politicians and rent seekers” (p. 374). Perhaps inevitably, such an answer only stimulates the urge to seek some “deeper” explanation.

A valuable feature of the work is inclusion of a CD-Rom with data developed and used in the study. It will facilitate continuing work to explore for more profound answers to the Argentine puzzle so effectively laid out by the authors and editors of this very useful collection of papers.

WILLIAM MCGREEVEY, Futures Group, Washington, DC

UNITED STATES


The founders were sailors and shopkeepers, farmers and merchants, not a few of whom were disconcerted by the incivility of dumping tea into the Boston harbor. They certainly were not anarchists, so there is no question that their revolution was “in favor of government.” Rather, the open question concerns the extent of the powers that the various parties wished to bestow upon their new government. This book’s thesis is that, far from wanting to create a limited government as previous scholarship has claimed, the Federalists wished to create one that could mobilize the full resources of society.
In developing and supporting this thesis, Max Edling relies on the debates recorded in the *Documentary History of the Ratification of the Constitution*. His particular focus is upon two central issues of the time: military power and fiscal authority. For both subjects, the book presents background, the Federalist position, the Anti-Federalist response, the Federalist counter-response, and the outcome. This is a natural method of organization, and it suits the inquiry. Because of extensive treatments of political philosophy and the historical background, less of the text is devoted to the actual debates than might be expected and desired.

The value and limitations of the approach can be seen in the analysis of the debate over federal assumption of the public debt. Edling finds in the documentary history that Federalists made appeals to honor and restore the public credit. Such arguments are not what the previous literature on the subject had considered the reasons for assumption; thus the contribution of the documentary evidence. The interpretation of the evidence, though, is problematic. Edling finds it noteworthy that the reasons debaters gave for debt assumption were neither opportunistic nor based on economics. Not only can restoration of public credit be interpreted as an economic argument, it is not clear why one would expect arguments to be opportunistic.

Calls for government action are often made on the grounds of a public interest that, following Arrow, does not exist. Although it is reasonable to presume that the arguments made were appealing to contemporaries, it is not clear that one can learn from them the true motivations of the actors. Edling provides a thoughtful discussion of this problem, but the implications of the findings remain unclear. The interpretation of forthrightness is just one difficulty posed by the use of this sort of documentary evidence. Another significant problem is that views of the nonelites are very poorly known. Records of the debates show that the views of the elites varied in ways that are not simple to characterize either across time (notably, pre- and postratification) or across geographic regions. The full range of opinion would be expected to vary even more.

Edling’s argument relies on a claimed absence of federalist checks in the Constitution. He writes, for example, that there were no limits to the types of taxes that could be levied. How, then, could the Supreme Court have decided that the use of an income tax required a constitutional amendment? Edling also stresses that the national government could tax regardless of the say of the state legislatures. The point is of course true, but misses the role of state legislatures in selecting the Senate. The extent of the state legislatures’ levels of influence may be a matter of some debate, but should not be ignored. (Although it may be mere coincidence that the greatest peacetime expansion in the federal government occurred after the passage of the Seventeenth Amendment, it is hard to reject a causal relation out of hand.)

In the end, Edling sides with the Anti-Federalists in concluding that, “[i]n important respects, this government was not a limited government, as the conventional interpretation of the Constitution holds, but an unlimited government” (p. 226). With the explicit restrictions in the Constitution’s text, in addition to the states’ role in selecting the Senate, this characterization is strained. It also leaves unexplained the fact, acknowledged by Edling, that an unlimited government did not develop over time. A more reasonable explanation is that, even if the powers accorded to the federal government have been underappreciated in past scholarship, those powers were in fact limited. Edling writes that, “the Civil War marked the failure of the founders’ attempt to create a lasting union” (p. 229). On the contrary, the union endures.

GEORGE BESHAROV, Duke University

This interesting and carefully crafted book is a journey into terrains at once familiar and yet (it turns out) insufficiently investigated. Lawrence Peskin’s project here is to survey a singular line of support that percolated from the 1780s into the 1820s (with trace elements extending longer) on behalf of manufacturing. Although aspects of the story are surely well known, he manages to provide new leverage on the subject.

Committed to probing rhetorical strategies as much as underlying processes, Peskin is drawn into fine-grained exegeses of discourses attaching to the assorted associations that emerged beginning in the late eighteenth century to back manufacturing as a precious centerpiece of the early national political economy. Led by figures such as Matthew Carey, Tench Coxe, and David Humphreys, and drawing in some measure on Revolutionary Era homespun and nonimportation movements, these organizations reflected handicraft and agricultural interests along with the central influence of merchants. Put side by side, the associations might well evince different inflections (not least around the vexing issue of tariffs). But Peskin maintains that overall their articulated positions comprised “neomercantilism.” While melding liberal endorsement of acquisitiveness and economic innovation with republican distaste for luxury, reverence for productivity, and notions of common good, neomercantilism for Peskin also embraced received mercantilist conventions by urging national economic independence, intersectoral coordination, and a vigorous governmental role in orchestrating economic developments (pp. 5–6).

For Peskin, this cluster of ideas and leanings was the conceptual framework for manufacturing in the new nation: the “intellectual origins,” as he says, behind those arguing that manufacturing could and should be harmonized with commerce and agriculture and (the corollary proposition) that making things was potentially every bit as vital to the young country as trading goods or harvesting crops. Because, moreover, he characterizes such promanufacturing arguments as possessing substantial traction in the milieu, Peskin likewise casts these “intellectual origins” as enjoying a potent role in fostering the manufacturing in fact taking root in the new United States.

There is much that is fresh and freshly valuable in this discussion. Peskin’s early pages, for example, usefully underscore the subversive dimensions of Revolutionary Era nonimportation projects. He suggests persuasively that, to the extent these were tied not just to restrained consumption but also to calls for manufacturing and hence to the colonies’ own emergent mercantilist priorities, nonimportation directly challenged British policies that sought to constrain the American colonies to serving simply as markets and sources of raw materials. At the same time, Manufacturing Revolution helpfully highlights the central role of merchants in backing manufacturing during the period he covers, while simultaneously (through its handling of the opening years of the nineteenth century) reminding us of the flip-flop that occurred after 1800 in partisan Federalist and Republican support for this strand of the economy.

Above all, Peskin’s evocation of neomercantilism allows him to bring into clarifying focus a particular and consequential framework of ideas, a salient and significant leitmotif, arcing across the late 1700s and early 1800s.

Yet it is worth noting that some of the very reasons Peskin explores this intellectual structure—specifically, the coherence and persistence he ascribes to neomercantilism—also raise questions about his perspective. Thus, although Peskin occasionally cites the agricultural South as slotting into a regional division of labor compatible with
neomercantilism, the decidedly northern thrust of his analysis implies a geographic lopsidedness in the neomercantalist outlook that might have been more fully addressed. So too, there existed centrifugal tugs, intermittently invigorated by politics, among agricultural, commercial, and manufacturing interests, as well as their proponents, during these decades. By the same token, manufacturing itself harbored divisions across the faultline of wage labor.

Now Peskin notices the intersectoral tensions. And he certainly recognizes the swelling restiveness of wage laborers. But in this latter context, he fixes perhaps too heavily on the emergent import of large factories and too little on the friction-generating effect of the general striving for heightened productivity (within and without factories) that characterized the spread of industrial capitalism—and that arguably formed the heart of industrialization in the young Republic. Peskin thus tends to bypass the full extent to which the journeymen’s struggle to retain their independence conflicted with the campaign for national economic autonomy he is foregrounding; and he consequently declines to consider fully the related ironic possibility that industrial capitalism itself was actually at some odds with neomercantilism. At a broader level, and taking both sectoral and intramunufacturing divisive pressures into account, one might ask whether it might not have made sense for Peskin to pivot his narrative more around the fundamental bracketings of neomercantilism: around why it was in truth not more entrenched rather than around its reach. This might in turn have encouraged this student of rhetoric to dig even deeper than he does into the intentionally ideological character—the willful stress on ongoing lack of contradictions—attached to the language of neomercantilism.

Yet this remains an intelligent and substantial study. Affirming the place of ideas in the brew of factors fashioning economic practices, Manufacturing Revolution illuminates an intellectual trajectory that at the very least helps us understand the portentous commitments to manufacturing surfacing in the originating years of the Republic.

JONATHAN PRUDE, Emory University


While other historians have illustrated how the military provided such critical elements to modern industrialization and organization as line and staff functions (see, for instance, Walter Licht. Working for the Railroad: The Organization of Work in the Nineteenth Century. Princeton, NJ: Princeton University Press, 1983), Russell Johnson points to another notable military contribution. In his examination of Civil War–era Dubuque, Iowa and its soldiers, Johnson demonstrates how enlistees’ everyday Union army experiences encompassed numerous characteristics of life in an urban industrial society.

Johnson depicts 1850s Dubuque as “pre-modern” in that it was not industrialized. Only after the town’s economic decline following the Panic of 1857 did community leaders begin to build a manufacturing sector. Thus they set the community on its course of economic recovery and “modernization.”

The onset of the Civil War, however, precluded many of Dubuque’s men from experiencing the consequences of industrialization and modernization at home. Despite strong war opposition among Dubuque’s working class immigrant Democrats, the
community filled its military quotas without ever resorting to the draft. In a densely written chapter, which would have benefited greatly from the incorporation of his related charts in Appendix B, Johnson demonstrates that those enlisting were “most likely sons living in low-nonmanual and artisan households and independent men among the city’s artisans and unskilled workers” (p. 104). Because these were the people also suffering the greatest hardships as Dubuque struggled to restructure and rebuild its economy, they joined for both patriotic and economic reasons.

Once in the military, the Union army exposed Dubuque’s sons to many of the “features that would become common in postwar industry, including specialization or functional differentiation, standardization of tasks to ensure uniformity, hierarchical organization, and a spirit of formality and impersonality between officials and underlings” (p. 147). Johnson’s examples, such as the division of artiller y tasks, the use of infantry manuals and formalized firing procedures, and the reliance on drum and bugle calls, readily demonstrate the military’s role in preparing soldiers for factory life.

The Union army also immersed Dubuque’s soldiers in critical characteristics of the urban environment. Johnson again skillfully exemplifies how soldiers gained first-hand experience with “population concentration, changes in mortality rates, the separation of home and work, widening social (class) distances, and the emergence of the neighborhoods as the most relevant socialization unit after the family” (p. 193).

Johnson does not ignore the home front or life after the Civil War. He examines how Dubuque responded to the charitable needs of the soldiers and their families and the postwar economic fortunes of its veterans. Johnson notes that initially town residents, particularly the women, attempted to provide aid to both the soldiers and their families. Yet because needs increased while resources and interest in offering assistance waned, aid focused solely on soldiers by early 1864. Just as many soldiers’ families struggled during the war, the years after the conflict did not assure Dubuque’s veterans of upward economic mobility. In another overly dense chapter that would have benefited from inclusion of the charts relegated to Appendix B, Johnson argues that Dubuque’s veterans “persisted in the city at a higher rate than non-veterans but were little more occupationally stable and were less successful at accumulating property. The veterans’ experience with a form of urban-industrial society in the military apparently helped them cope, though not necessarily excel, in postwar urban-industrial Dubuque” (p. 275).

Thus by the end of his narrative, Johnson leaves the reader with the sense that the Civil War was a watershed event, particularly in regard to socializing individuals for life in an industrialized society. Watershed is too strong a characterization. Rather than introducing them to industrial life, the Union army provided soldiers with experiences that expanded upon the industry-related working and living patterns that they had already encountered before the Civil War. Dubuque during the 1850s and early 1860s was not wholly deficient in the characteristics Johnson ascribes to life in an industrialized community. As he points out throughout his narrative, Dubuque’s population was growing, its residential and social patterns were increasingly class-differentiated, and its residents had been exposed to the diseases that came with urban living, such as TB and cholera. Further, as illustrated in Sean Wilentz’s Chants Democratic: New York and the Rise of the American Working Class, 1788–1850 (New York: Oxford University Press, 1984), the absence of factories did not equate to a lack of exposure to industrial working conditions. Even Dubuque artisans prior to the Civil War could have experienced deskilling and the collapse of apprenticeship. Johnson provides no evidence to the contrary.

Depicting the Civil War as a watershed and overstating the role of the Union army in modernization does not, however, negate the importance of Johnson’s arguments.
Particularly his chapters dealing with soldiers’ everyday army experiences amply demonstrate the vital role the military played in creating a modern urban industrial society.

CHRISTIANE DIEHL TAYLOR, Eastern Kentucky University


Few events in modern economic history have garnered the scholarly attention and controversy of the Great Depression of the 1930s. Even so, the overwhelming focus of this vast literature is on the causes of the slump itself. Comparatively little attention has been paid to the recovery process, nor has much consensus emerged as to the reasons for the unparalleled length of the crisis. For decades after World War II, in fact, the spirited debate between Keynesian and monetarist economists concerning the Great Depression focused almost entirely on questions of causation and the upper turning-point of the cycle.

Frank Steindl, the Regents Professor of Economics and Ardmore Professor of Business Administration at Oklahoma State University, makes a welcome intervention in the literature on the Great Depression with his new book—a study that resolutely focuses on the recovery process of the 1930s in the United States. He provides an overview of some prevailing theories of the aborted recovery in the latter part of the thirties and closely examines the role of money stock dynamics and bank credit mechanisms in accounting for the lower turning-point of the cycle.

Steindl parallels this discussion with an assessment of some of the explanations advanced by contemporary economists—most prominently Irving Fisher—who were perplexed by the fragility of the recovery process. Finally, he advances new arguments regarding bank credit to account for the anemic upturn of the late thirties in the face of sustained growth in the money supply. It is within this context that Steindl focuses on endogenous propagation mechanisms as important yet overlooked factors in the tendency of the economy to move back to trend rates of growth.

Steindl’s analysis of interwar recovery mechanisms is grounded in a monetarist perspective. He argues that increases in the money stock help to explain almost half of the adjustment to full capacity utilization by the end of the 1930s. In turn, he rejects the importance of fiscal policy. Indeed, Steindl’s survey of the professional literature in the wake of the crisis prompts him to conclude how ironic it was that such scholarship “so heavily emphasized the role and effects of fiscal policy when the empirical evaluations found so little. . . . [while in contrast] the literature on monetary influences on the economy . . . was meager, but the actual force of those monetary impulses appears to be quite important” (p. 134). Even so, monetary expansion due to international inflows of gold had limited effect, as Steindl admits, due to the decline in the provisioning of credit by banks eager to strengthen feeble balance sheets. The contractionary tendencies of Federal Reserve Board policy and the increasingly rigorous oversight of bank examiners also served to squelch credit expansion.

Nevertheless, Steindl notes there remains a substantial portion of the movement in the output data for which additional arguments must be found. A greater challenge in the evidence, in his view, is that prices and output moved in opposite directions during the upturn of 1938–1940. How might a monetary approach to one of the greatest crises...
in modern economic history account for such perversely conjoined movements in the data?

Steindl concludes that price deflation in the late thirties was the result of an increase in money demand. An unstable tendency was thus enhanced as further deflation worked to further depress entrepreneurial expectations. The anticipation of further price declines, Steindl argues, worked to reduce velocity—he thus invokes a kind of “liquidity trap” argument to account for the fragility of recovery in the late thirties in the face of gold inflows serving to increase the money stock overall. Yet, as we know, as the thirties drew to a close, money demand did not continue to rise and velocity itself turned around. Here Steindl invokes a more traditional argument concerning the anticipation of war as an explanation of the lower turning-point.

Having rejected Keynesian arguments regarding effective demand, and seeing no genuine explanatory value in fiscal policy, Steindl concludes his work by striving to explain the remaining evidence on recovery in the late thirties, above and beyond that accounted for by the quantity theory of money, in terms of the endogenous propagation of output toward trend values. He asserts that “the resilient mechanisms generating such behavior are rooted in the dynamics of an enterprise economy. . . . [such as] incentives, opportunities, signals, and flexibility, the effects of which increase productivity” (p. 182). With this invocation of “real business cycle theory,” Steindl closes his study by explaining the rising output and falling prices of the late 1930s by a combination of changes in money demand and of mean-reversion mechanisms at work in the economy as a whole.

By his own admission, Steindl has produced a frustrating book. He admits that the endogenous propagation mechanisms that stand at the center of his analysis of the recovery process in the 1930s “cannot be observed directly” (p. 183). Offering no concrete evidence as to the manner in which the mean-reversion in output performance in the interwar American economy might have revealed itself, Steindl is forced to offer a series of assertions about the explanatory value of his endogenous model of recovery. This is hardly the stuff of convincing argument and it leaves the reader unpersuaded. Although Steindl’s determination to shore up the weaknesses of the quantity theory approach to the macroeconomics of the 1930s is laudable, the resort to an unobservable and endogenous restorative mechanism begs for more documentation and empirical detail.

If indeed Steindl sees real productivity change as the key part of endogenous recovery mechanisms in the cycle, it would have considerably strengthened his book to have focused on factors affecting supply during the 1930s. To be sure, his primary focus in this work is on monetary dynamics, but it is nevertheless unfortunate that Steindl is left arguing for the salience of processes of technological change and productivity enhancement—aspects of interwar economic experience he does not examine in any detail. There is a small but important scholarship on these matters. It is this literature—two older examples of which are Gerhard Mensch, Stalemate in Technology: Innovations Overcome the Depression (Cambridge, MA: Ballinger, 1979) and Esther Fano, “Technical Progress as a Destabilizing Factor and as an Agent of Recovery in the United States Between the Two World Wars,” History and Technology 3 (1987): 249–74, and more recently Alexander J. Field, “The Most Technologically Progressive Decade of the Century,” American Economic Review 93, no. 4 (2003): 1399–413—that provides some of the disaggregated empirical detail that Steindl believes essential for improvement in the analysis of the dynamics of interwar economic recovery.

MICHAEL A. BERNSTEIN, University of California, San Diego

State of the Union: A Century of American Labor weaves the stories of the union-busting practices of corporations; the growing political opposition of anti-union zealots; the conservative, incompetent, and corrupt union leadership; the racism and sexism within many local labor organizations; the abandonment of the labor movement by left intellectuals; and the more recent manifestations of capital flight into a tragic epic which explains the losses in union membership and erosion of working-class political influence during the twentieth century. Nelson Lichtenstein’s depressing story deserves much praise. It offers a sophisticated synthesis and confronts its readers with a provocative thesis.

Lichtenstein argues that over “the last forty years a transformation in law, custom, and ideology has made a once radical demand for racial and gender equality into an elemental code of conduct.” He finds the upswing in federal legislation that attacks sexism and racism a positive development, but points out that the unintended consequence of these measures has been to push “the rights of workers, as workers, and especially as workers acting in an autonomous, collective fashion,” into “the shadows” (p. 3). He identifies this transition as a shift in political culture from a labor (collective) pluralism to an individual-rights consciousness.

The beginnings of this change occurred between the 1940s and 1960s. New laws and labor leaders’ insistence on fighting for gains at the firm level transformed collective bargaining from representing part of labor’s political and economic agenda to being the only mechanism workers had to contest the practices of their employers. In this same period, union leaders purged their locals of radicals (who had been their most dynamic organizers), they failed to organize African-American workers in the South, and they had blocked most women from entry into skilled trades. This was not all. Congress passed the Taft-Hartley Act, Barry Goldwater started to have success with his right-to-work campaign, and the McClellan Committee made Jimmy Hoffa’s foibles public. Before the working class could attend to all of these wounds, deindustrialization and globalization further exacerbated the collapse of their movement.

Lichtenstein argues that we continue to live with the consequences of labor’s collapse. For example, he pointed out that in the 1990s when the country experienced a 3 percent per year growth rate, most median incomes continued to decline. By 2000 “real household income for young families (breadwinners under age thirty) stood at one-third less than their counterparts in 1973, even though their total working hours were longer and the educational level of the head of household higher than a generation before” (pp. 13–14). Although rights consciousness, State of the Union argues, can provide workers protections against sexual harassment and racial discrimination, it cannot produce the class-based actions necessary to alter wage rates and the larger social structure.

The implications of Lichtenstein’s work are well worth noting. The protections granted to workers by the 1964 Civil Rights Act, and enforced by the Equal Employment Opportunity Commission, fail to challenge current corporate practices because they only protect individuals in a limited set of instances. If an individual challenged his or her employer over the right to set wage and hour rates, or in some cases, such as Wal-Mart, over organizing a union, he or she would likely be fired. Lichtenstein is right to place much of the blame for this current state of affairs squarely on the shoulders of labor leaders. They somehow forgot unions are supposed to be agents of social democracy not simply intermediaries between workers and their bosses.
In hopes of not sending the prolabor reader away too depressed, Lichtenstein concludes with some suggestions for rebuilding the labor movement. They include a new and improved craft unionism, which would eliminate the racism and sexism of the past, and a new political militancy. These points of advocacy were the least creative and weakest part of an otherwise brilliant work.

J O H N P. E N Y E A R T, Bucknell University


Educational institutions have always been central to American visions of democracy and the public good: the Northwest Ordinance of 1787 declared that “knowledge, being necessary to good government and the happiness of mankind, schools and the means of education shall forever be encouraged”; and George Washington’s inaugural address to Congress in 1790 recommended the founding of a national university. The American conception of academic excellence had a strong pragmatic component, as exemplified by many of the “public Ivies” that were created through the Morrill Act of 1862. Such land grant universities were hardly ivory towers, but were designed as red-brick institutions that would contribute to advances in agriculture, industrial growth, and innovation. As such, the key question was not whether the promotion of education and social welfare was consistent with ties to industry, but the extent and nature of the collaboration.

This book is concerned with one type of collaboration: the transfer of technology between universities and industry. The Bayh-Dole Act of 1980 was designed to facilitate patenting and commercialization of innovations by universities, and the growth in academic patenting and licensing that has since occurred has commonly been attributed to its effects. The Economist (14 December 2002) goes further and argues that it was “possibly the most inspired piece of legislation to be enacted in America over the past half-century,” and even “helped to reverse America’s precipitous slide into industrial irrelevance.” The authors refute such grandiose claims after a careful examination of the quantitative evidence and several case studies.

The volume comprises an array of articles produced over the past decade. As a result, the evidence is not presented in an entirely seamless fashion: for instance, the data in the chapter that deals with the period up to 1980 are not fully comparable with the information presented in the chapter that deals with the aftermath of Bayh-Dole. However, the introduction and conclusion are extremely effective in setting out and recapitulating the main themes from the essays, and each chapter concludes with a useful summary statement. The first three chapters of the book survey historical developments in the twentieth century. Economic and legal historians of the nineteenth century have documented the details of changes in the academic backgrounds of inventors and in work for hire doctrines that affirm the authors’ conclusions regarding the existence of academic-industry links long before the Bayh-Dole Act. The fields of electricity and mechanical engineering illustrate early connections to industry, such as MIT faculty and graduates who engaged in consultancies, or formed enterprises to commercialize their discoveries. More recently, pioneers in computer and medical science have likewise been drawn from American universities. The federal government
financed 24 percent of academic research in 1935 and 70 percent in 1970, with the ex-
pectation that universities would produce findings that yielded practical benefits, espe-
cially for national defense and biomedical science. University patenting was debated 
early in the twentieth century, although policies were not well articulated prior to the 
1940s. Many institutions outsourced patent management to the Research Corporation 
(founded in 1912), and until after the 1960s chose not to administer patent portfolios 
themselves.

Subsequent chapters discuss the political rationale for the Bayh-Dole legislation, 
and empirically assess its effects. After an analysis of the patent and licensing data for 
three leading research universities (UC, Stanford, and Columbia), the authors consider 
the role of entry and experience in the patterns of patenting for a larger sample of acad-
emic institutions. They find little evidence that Bayh-Dole triggered changes in the 
culture of patenting for experienced universities. Institutions that began patenting after 
1980 were initially likely to receive less important patents but over time benefited 
from a generalized learning process that enabled them to produce more valued inven-
tions. The book’s overall conclusion is that the Bayh-Dole Act did not significantly al-
ter the commercial orientation of academic institutions and, in the absence of the legis-
lation, their patenting and licensing would still have increased. Higher rates of 
patenting and commercialization owed to separate factors including the overall 
strengthening of patent rights and growth in a few patent-intensive fields such as bio-
technology and pharmaceuticals. Five case studies are used to highlight the complex-
ity of university-industry technology transfer. A large part of the interaction between 
university researchers and corporations is “social” in the sense of research collabora-
tions, the diffusion of knowledge through publications and conferences, and job 
mobility, as opposed to “economic” licensing or sales of property rights in ideas. The au-
thors conclude by expressing concern about the current extension of property rights to 
areas formerly considered part of the public domain such as basic materials and re-
search tools, and the need for licensing policies that pursue more encompassing social 
goals than narrow profit maximization.

The book would have benefited from a more detailed examination of the factors be-
beyond Bayh-Dole that influenced collaborations between universities and industry, in-
cluding the National Cooperative Research Act of 1984 which encouraged research 
consortia among academic and industrial researchers. However, the authors raise im-
portant issues and make a key contribution to the debate about the role of universities 
in society. Their monograph deserves close attention from policy makers, students of 
technological change, and ivory tower scholars alike.

B. ZORINA KHAN, Bowdoin College and NBER

Territories of Profit: Communications, Capitalist Development, and the Innovative 
Enterprises of G. F. Swift and Dell Computer. By Gary Fields. Stanford, CA: Stan-
ford Business Books, 2004. Pp. xviii, 281. $60.00, cloth; $24.95, paper.

Many of us in teaching and casual conversation draw analogies between events oc-
curring at widely different moments in time. Such comparison can draw out funda-
mental processes underlying economic change and help us get our bearings in the face 
of rapid technical and economic transformation. The dramatic emergence of the inter-
net and the world wide web has sparked a burst of such analogy-making in the press 
and trade publications. The exercise seems to have yielded a consensus opinion. The
most promising candidates for comparison are the railroad and its oft-neglected essential complement, the magnetic telegraph, dubbed by one author as “the Victorian internet.”

In this interesting and thoughtful book, Gary Fields pursues the analogy with exceptional sophistication and rigor. His approach melds the case-study method of business historians with recent theoretical work in organizational behavior and the dynamics of innovation. At its core, the book presents case studies of the two firms identified in the subtitle. Fields shows how Swift and Dell, though supplying vastly different products, each succeeded by seizing upon opportunities opened by changes in transport and communications. Both developed new “demand-pull” distribution systems, in which information flowed directly from final customers back through a network of assembly (or, in the case of Swift, disassembly) facilities and suppliers. These novel exercises in logistics yielded economies of speed and coordination, largely in the form of reduced inventory and waste. Returns of this sort were especially large in these cases because both meat and personal computers diminished rapidly in value, due to spoiling in one case and technical obsolescence in the other.

Fields draws several implications from these studies. One is the critical role of firms as innovators. Both Swift and Dell were start-ups that found ways to displace established firms and compelled others to imitate its methods or exit the business. Another is the importance of distribution as a driver of change. In both cases, these pioneering firms innovated first in the realm of distribution. Other features of their enterprises, such as massive slaughterhouses and automated assembly plants, followed from the crucial breakthrough in marketing channels.

What most interests Fields is how the cascade of change triggered by the critical innovations in distribution ultimately worked their way back through the supply chain and reshaped the distribution of economic activity among firms and across space. He characterizes this process as expansive, yet concentrating. Swift and Dell coordinated activities across a much larger geographic reach but in the process bypassed many intermediaries and concentrated activities in a few facilities and regions. These are the “territories of profit” to which the title refers. Significantly, Fields argues that Dell has not utilized the internet to spark a bidding process among an expanded pool of suppliers. Rather, it has narrowed its supplier base and leveraged its market power to extract significant concessions from those suppliers, much as Swift did in the late nineteenth century with stockyard operators, drovers, and grazers. Dell’s suppliers must deploy information technology compatible with Dell’s and supply components on short order to supply centers that are ostensibly independent but in fact captive to Dell. Contracts require suppliers to keep larger inventories than those of Dell and to delay receipt of payment for more than two months, even though Dell itself collects from its customers upon order. Far from serving to promote market interactions at the boundaries of the firm, the internet has actually enabled Dell to exert administrative authority up the supply chain, without incurring the large risks associated with ownership of facilities and inventories. The “new” knowledge and information economy of the new millennium thus strongly resembles the old.

The inherent risk in such comparisons is that they lead us to accentuate the common elements while overlooking other influential factors at work in each individual case. Fields is alert to the problem but does not duck it entirely. His treatment of Swift, especially, at points seems strained to highlight logistics at the expense of production. Fields exaggerates the importance of custom orders from the field, for instance, while downplaying the mass marketing of byproducts made from parts of the animal that previously went to waste.
Much like the firms he examines, Fields concentrates his focus on certain elements of the two enterprises, while broadening his reach by connecting their stories to a large body of theory. The essential comparison at the heart of this volume might well have been presented in a single scholarly article. Fields fills out the book with lengthy summary chapters and a long preliminary chapter in which he attempts a grand theoretical synthesis of thinking pertaining to technical change, the firm, and the geographic distribution of economic activity. Chapters preceding each of the case studies analyze the rise of the critical communications infrastructures through the lens of such theory. These nicely executed primers, though resulting in considerable repetitiveness, might well prove very useful for readers looking for accessible introductory treatments of these subjects.

STEVEN W. USSelman, Georgia Institute of Technology


Forests in Time is an edited volume of scientific studies and related essays that rely on observations made mainly at the Harvard Forest (established in 1907) near Petersham, Massachusetts over the past century. Delving further back into history using tools ranging from fossil records to personal journals, much of the book is dedicated to determining changes in the scientific characteristics of New England’s forests over the past millennium. The research is empirically solid and pertinent to broader questions about climate change, land use, and natural and human-induced ecosystem evolution. The book is divided into five sections. The first three sections focus on the broader environmental impacts of human-induced change, and the latter explore detailed consequences of ecosystem change from natural events, including infrequent but severe hurricanes, on nutrients and nutrient cycling. The early chapters of the book are perhaps of greater immediate interest to the economist and economic historian, as they speak more directly to human-induced environmental impacts. The latter chapters may, however, in patient hands, help improve economists’ ability to parse out characteristics of forests that generate value through ecosystem services and to design policy that more optimally matches scientific processes with economic values generated from these processes.

New England forests experienced human-induced change from Native American populations that was accelerated by European settlement 400 years ago. Both direct and indirect forces have influenced forest quality and quantity: the arrival of Europeans brought not only increased resource use but also new species, new pests, and new patterns of settlement that led first to extensive deforestation to make way for agriculture. This shift toward agricultural land uses reversed toward reforestation in the mid nineteenth century as agriculture in New England dwindled. The characteristics of this lengthy and sizable reforestation are one of the most interesting results of the book. The scientific evidence makes clear that the forests of today reveal strong residual impacts due to the human and natural-induced changes of years past, regardless of any initial visual similarities that may appear to exist. Several of these findings, such as the discovery of a long-term increase in nitrogen cycling on former agricultural lands and the roles of soil and atmospheric warming in the carbon cycle can help policy makers and scientists alike understand more completely the implications of human-induced changes ranging from logging to global warming.
The book highlights some useful distinctions for economic historians studying environmental impacts and change. Path dependency of environmental quality can be measured by several different sorts of scientific parameters, including soil nutrients, species composition, biomass, and nutrient cycling in soil and air. The scientific studies indicate that as human interventions and multiple uses of the forest over millennia have changed the structure and composition of the New England forest in visible as well as less direct ways. The forests are not in a long run steady state equilibrium; nor does restoration to a “pre-human” influence appear a feasible, or even possibly desirable, goal.

The Harvard Forest has conducted a number of experiments described in the book that highlight the complications of managing nature, even when we are attempting to manage it as nature would have done in the absence of human influence (such as prescribed burning to mimic natural fire disturbances, which has been a controversial goal of many public land managers in recent years). The reader may not absorb all the scientific details of phosphorus uptake, nitrogen fixing, or ecosystem respiration, but she should come to appreciate the ways in which long-run forest dynamics are inextricably entwined with human decisions regarding land use.

The book’s insights into the evolution of species composition over time may prove most useful as we face increased disturbances from pests and invasive species that have already removed the American chestnut and elms as dominant species in our Eastern forests and now threaten the eastern hemlock significantly as well. Although the research may not provide solutions that allow these species to thrive, it may serve two other important roles: to enhance our scientific understanding of their ecology, giving us the information needed to create the best chances for survival; and to enhance our economic understanding of how the patterns of human resource use drive long-term ecosystem change so that future forests and their users might benefit from more knowledgeable decision making. The founders of the Harvard Forest had foresight, and the authors of these pages make admirable use of a unique and rich data source to provide today’s decision makers with better information about our past impacts.

Brooks Kaiser, Gettysburg College

General and Miscellaneous


The process of global financial integration has attracted more than its share of controversy. While many point to the potential economic benefits of free trade in capital, others argue that the benefits are outweighed by the risk of international financial crises. This book, which summarizes almost a decade of work by two distinguished international economists, examines the history of global financial integration from the late nineteenth century to the present with a view to resolving this controversy and extracting lessons for policymakers today. Combining a wide range of hard data with qualitative historical analysis, the authors present a nuanced view of the costs and benefits of international financial integration. One conclusion, albeit carefully qualified, is certain to be controversial: many developing countries would gain from the reintroduction of some form of capital controls.
Book Reviews

The book is divided into four parts. After a preamble, Part 2 begins by charting the evolution of international capital market integration over the past century and one-half using a variety of quantitative measures. Chapter 2 examines both stocks and flows of international capital, while chapter 3 presents evidence on the relative absence of arbitrage opportunities in both asset and goods markets. Both chapters paint an overall picture in which international financial integration increased at the end of the nineteenth century before collapsing due to the combined effects of two world wars and the Great Depression, only to re-emerge with increasing speed throughout the latter half of the twentieth century.

The core of the book is contained in Part 3, which examines the inter-relationship between international financial integration, economic conditions, and politics. The organizing theme for this part is what the authors refer to as the “fundamental macroeconomic policy trilemma.” Familiar from the Mundell-Fleming model, the trilemma asserts that a government can attain no more than two of the three objectives of stable exchange rates, monetary policy independence, and international financial integration.

From this perspective, the history of international financial integration is a history of the evolution of choices about how to resolve this trilemma. Chapter 4 takes up this theme and presents a qualitative history of international capital markets, which divides the period into four separate regimes in which the trilemma was resolved in four distinct ways. Under the Gold Standard, countries surrendered monetary policy independence in order to attain the benefits of free capital flows and exchange-rate stability. In the interwar period, with the world moving towards autarky, the benefits of capital mobility were reduced, and many countries were concerned about the role of the fixed exchange rates in transmitting international financial crises. The resulting breakdown in the policy consensus led some countries to float their exchange rates while others closed their capital markets in order to retain domestic policy autonomy.

Chapters 5 and 6 present a range of evidence that is useful in assessing the tradeoff implied by the trilemma. Drawing on work co-authored with Jay Shambaugh, Chapter 5 examines how the relationship between interest rates across countries has varied with the form of capital controls and exchange-rate regimes. The evidence is in line with the trilemma hypothesis: absent capital controls, countries with pegged exchange rates lose monetary policy independence and their interest rates follow movements in the “world” interest rate. Chapter 6 examines whether the decision to give up monetary policy autonomy delivers benefits in terms of lower borrowing rates on international capital markets. For the prewar period, the authors find that adherence to the gold standard alone was sufficient to lower borrowing costs. This effect remains even after controlling for membership of the British Empire, default, war, and strong economic and fiscal conditions. This pattern was reversed in the interwar period: a return to gold was less important than alternative explanations of spreads, possibly reflecting the lower credibility of the gold standard.

The finding of Chapter 6 contains a potentially important lesson for developing countries: credible commitments to fixed exchange rate regimes have a payoff in terms of lower borrowing costs. However, the lesson needs to be tempered by the fact that they require a credible commitment, which in the light of the interwar evidence is difficult to manufacture. Moreover, the gains may be small, amounting to less than 20
basis points in some specifications. There is also the possibility that the authors’ specification may overstate the gains. For although the authors control for contemporaneous defaults, wars, and economic conditions, yields also reflect expectations of future events. And if countries abandon gold in expectation of adverse future shocks, or if domestic political institutions simultaneously make adherence to gold more difficult and future default more likely, the benefits to fixing exchange rates might be illusory.

Part 4 of the book attempts to draw lessons for the future of the international financial system from the historical evidence, and in particular asks how the trilemma might be best resolved today. Chapter 7 documents the fact that capital flows are much more concentrated amongst the rich countries today than they were during the gold standard. What is different about the modern period? One possibility discussed in Chapter 8 is that, in contrast to the nineteenth century in which high returns could be found in newly settled countries, the benefits to investing in poor countries today are low because of a host of factors that manifest themselves in low returns.

But if this is the case, the benefits to international financial integration are low. And the costs, in terms of an increased propensity for financial crises may be severe. The authors are careful to point out that the evidence on the costs of crises, and their relationship to international financial integration, is weak but present some evidence that they may be larger than the benefits of international financial integration in the first place. The conclusions the authors draw are suitably nuanced, but certainly provocative: unless developing countries can reform themselves in ways that make the returns to foreign borrowing large, these countries may benefit from restricting capital flows.

This conclusion will, no doubt, spark some controversy of its own. But even if we accept the argument that capital controls are desirable, are they feasible? Recent experience with the imposition of capital controls in countries such as Brazil (see, for example, the excellent review in Marcio Garcia and Marcus Valpassos “Capital Flows, Capital Controls and Currency Crisis: The Case of Brazil in the Nineties.” in Felipe Larrain, ed. Capital Flows, Capital Controls, and Currency Crises: Latin America in the 1990s, University of Michigan Press) suggests that the private sector is often quick to find ways around capital controls. If so, are we left with the pessimistic conclusion that developing countries are doomed to live with costly crises? The authors give us reason for optimism: if crises are the result of policy markers failures to respect the tradeoff implied by the trilemma, the first step in avoiding crises is to educate policy markers about this tradeoff. That education should start with this book.

MARK L. J. WRIGHT, Stanford University


Because the function of a book review is to direct busy readers towards good books and away from bad ones, a good book review and a kind book review are often incompatible. This is one of those unfortunate occasions. Even though Richard Day notes promisingly in the preface that “I felt my mission to have been that of providing a better characterization of economic change” (p. x), no economic historian is going to benefit from delving into this book.

The first problem of the book is that it is a book only in the sense that its pages have consecutive numbering. The volume is composed of 12 essays, based mainly on pre-
viously published papers and essays written between 1967 and 1998. These essays cohere about as well as the collection of objects you find in the typical California yard sale. Here you can read, in no particular order, essays on both technological changes and sharecroppers in the Mississippi Delta, 1940 to 1957, and on global human development since the origin of the earth. Had Cambridge University Press mixed up the order of the chapters on the way to the printer, only the author would have noticed. Had one of the chapters been deleted by mistake, no purchaser would have been angrily demanding their money back at the bookstore.

The second problem of the book is that even if the reader decided to try applying Day’s methods of economic dynamics in economic history, after reading the book they would not have any idea how to do that. It is not a cookbook for the methods of economic dynamics. The examples are laid out at too abstract a level. There is page after page of diagrams, all with time on the horizontal axis, where curves swoop, dive, arc, and ascend like dolphins at play. But where exactly these curves came from is not laid bare. The analogy would be trying to teach people to cook haute cuisine by showing them a list of ingredients and pictures of the final dishes. A model of “Economic Development and Migration” (chapter 8) may highlight the “complexity and interdependence of the building blocks of the economic system and the complex multimode, multiphase structure of development that evolves” (p. 156). But how that complexity is modeled will remain a mystery to the average reader. Even if the mission stated in the preface succeeded and you got the religion, no reader could figure out how to worship the new dynamic god in future academic work.

But average readers, I think, will not be persuaded by the material presented here to abandon their static friends and family and go off in search of dynamism. They will not get “teched up” in Recursive Programming and the other tools of the Economic Dynamist that the new god demands, because Day never actually demonstrates the value added from such techniques. He may be able to predict labor inputs in the Mississippi delta in 1940–1957 using a dynamic model of production (chapter 4), but has he done so any better than alternative static models? That question is not addressed. He models the green revolution in the Punjab in dynamic terms (chapter 5), but again with no reference to any alternative ways of modeling these changes. As with calibration methods in macroeconomics, or computable general equilibrium models in economic history, showing that your model can roughly reproduce the paths observed in the data is not proving that you have produced the final, best description of reality. Theories can only be judged relative to other, competing theories, and this book never considers such alternatives.

Thus sadly while economic dynamics and economic history have many of the same interests and issues, this book largely fails to speak to historians.

GREGORY CLARK, University Of California, Davis


This book is the second of a three-volume series, which, in the author’s words, addresses “the role of certain forms of change in human population history.” Specifically, the author’s aim is to establish that demographic phenomena are subject to regular growth patterns. The most important pattern, by far, that he identifies is a constant
proportionate decrease in the growth rate leading eventually to zero growth (an improved version of the Lotka logistic curve). The author calls this the G curve. His position is that most historical demographic phenomena have followed the G form or occasionally, what he calls the G¹ form, which is the first derivative of G. The alternatives, the E curve where the growth rate accelerates; the F curve where the growth rate is constant and the H curve where the growth rate decelerates more slowly than in G are, according to the author, rarely encountered, and never before 1500. The first volume aimed to demonstrate that natural increase followed this pattern in most cases. This volume attempts to demonstrate that the same pattern applies to migration.

In order to demonstrate the point, Harris examines a very large amount of data from an impressive number of publications. He demonstrates that growth rates followed a G pattern in the growth of variables as diverse as emigration streams, emigrant populations, many of the different slave trades, and the growth of urban populations. More controversially, for example, he also shows that the share of children in emigrant populations, the gender composition of slave cargos, and the time taken by slave voyages also followed a G curve pattern.

This approach raises several issues. The fundamental question is obvious. Like trees, no variable can grow indefinitely; the G pattern must be true by definition because the growth rate must slow down or decline. Hence, the observation that this occurs in a large number of cases may have little historical value. Secondly, Harris argues that the pattern is determined by demographic growth patterns (which he discussed in the first volume of the series). In other words, Harris argues that history, including, for example, technical change and the development of the international economy, does not matter. This seems difficult to reconcile with what we know about the causes of emigration streams and other variables—including those variables mentioned previously. To take one example, the author explains the common timing of emigration flows from Britain to Australia, New Zealand, and the United States in the late nineteenth century by demographic developments in Britain. (p. 40).

There is also a problem with the G pattern itself. Many of the variables show, not G pattern growth, but a series of G pattern growth curves—i.e., alternating periods of faster and slower growth. This pattern intuitively fits historical reality, but it is not clear if it is compatible with the author’s position that a G pattern dominates.

Hence, we have a book that is not historical but structured around the shape of the growth curves of variables. It seems at $99.99 a rather expensive way of discovering these patterns.

DUDLEY BAINES, London School of Economics


In Affirmative Action Around the World: An Empirical Study, Thomas Sowell, a well-known critic of affirmative action in the United States, extends his critical lens to four other countries around the globe: Malaysia, Nigeria, India, and Sri Lanka. The book is organized into seven chapters. The first chapter provides a broad overview of affirmative action and related policies in different countries and advances the central thesis of the book—that these policies, though varied, produce similar negative outcomes. The next five chapters focus on individual countries—some in greater depth than others. For example, the chapter on the United States is 50 pages long whereas
the chapters on Sri Lanka and Nigeria are each less than 20 pages. A final chapter
summarizes the author’s conclusions. Despite the book’s subtitle, the chapter on Ma-
laysia is the only one with any tables, charts, or graphs and the reported data are not
original to the book.

A text on affirmative action in five nations is an ambitious undertaking, particularly
if one strives for a balanced and thorough examination of the available scholarly re-
search. But Sowell does not bind himself with such constraints. Instead, he presents
only the unfavorable evidence. His arguments are familiar: affirmative action leads to
the admission at universities of less qualified students or the hiring of less qualified
workers than would have happened in the absence of these policies, affirmative action
increases racial and ethnic animosity, and affirmative action benefits only the most
privileged in the targeted group and thus exacerbates income inequality within that
group.

These are potential costs of affirmative action, but a sound economic analysis re-
quires that these costs be compared with potential benefits. One potential benefit is the
improvement in the allocation of resources that might result from the reduction of dis-
crimination against the targeted group. Although Sowell admits to the existence of his-
torical discrimination, he does not discuss its contemporary manifestations either in
the United States or in other countries. Thus, a reader might easily think that a prob-
lem does not exist. In the case of India, there is ample evidence to the contrary as
summarized by Ashwini Deshpande in a recent essay “Recasting Economic Inegal-
ity” (Boundaries of Clan and Color: Transnational Comparisons of Inter-group Dis-

Potential benefits of affirmative action such as greater social capital among the tar-
geted groups, greater integration of a society’s elite, and greater ability of all students
to function in a multicultural society are not discussed. Sowell emphasizes the cream-
skimming features of affirmative action policy—that the beneficiaries tend to come
from the upper income strata of the targeted groups—without acknowledging any
spillover benefits to the larger community.

In contrast, a recent book by Thomas Weisskopf Affirmative Action in the United
States and India (London and New York: Routledge, 2004) accumulates evidence on
both costs and benefits of affirmative action policies citing empirical studies not refer-
enced by Sowell. The case of reserved seats at Indian universities for scheduled castes
(SC) and tribes (ST) offers an illustration of the differences between the two books.
Citing a 1986 paper by Suma Chitnis (“Measuring Up to Reserved Admissions,” in
Reservation: Policy, Programmes and Issues, edited by Vimal P. Shah and Binod C.
Agrawal. Jaipur, India: Rawat Publications, 1986), Sowell writes, “None of the stu-
dents preferentially admitted to six highly selective engineering schools progressed
through these schools on schedule and most did not maintain a high enough grade av-
erage to continue in these institutions” (emphasis added, p. 31). Citing a 1999 study
by Kirpal and Gupta (Equality Through Reservations, Jaipur, India: Rawat Publica-
tions), Weisskopf reports that the graduation rate for preferentially admitted students
at the prestigious Indian Institutes of Technologies (ITT) was 84 percent compared
with 94 percent for general entry students and concludes, “Clearly, and not surpris-
ingly, SC and ST students lag well behind general-entry students in their performance
on ITT examinations. Moreover, those who graduate as a rule take more years to do so
than those general-entry peers. Still, the fact that graduation rates for SC and ST stu-
dents are now over 80 percent at the elite ITT suggests that these beneficiaries of re-
served seats—and the institutions that are admitting them—are achieving important
successes” (emphasis added, pp. 171–72). Weisskopf acknowledges both the positive and negative information about affirmative action, whereas Sowell uses an old statistic to present a particularly bleak perspective on affirmative action’s impact.

*Affirmative Action Around the World: An Empirical Study* might have been more accurately titled *The Case Against Affirmative Action Around the World*. The book succeeds in establishing the costs of the policy are similar around the world, but it fails as an overall assessment of affirmative action. It is sure to provide fodder for conservative pundits, but it offers little to advance economic scholarship on this issue.

CECILIA A. CONRAD, Pomona College

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Ten years in the making and keenly anticipated, the five-volume *Oxford Encyclopedia of Economic History* is now available. The casual reader of sales brochures may by now feel that the present spate of historical encyclopedia-writing is excessive, an attempt to squeeze a few more dollars from the university library budget before the system collapses. But anyone who has lapsed into this cynical state of mind should make an exception for this one. The edifice that Joel Mokyr and his team have constructed is indeed a monumental achievement, justifying the publisher’s claim that it is “a truly remarkable work of scholarship.” The members of the editorial team, who deserve to share in the glory, are Maxine Berg, Loren Brandt, Erik Buyst, Louis P. Cain, Jan de Vries, Paul E. Lovejoy, and John H. Munro.

The volumes contain nearly 900 articles written by 800 experts. The entries are not structured according to any one historical schema or framework, which would have been quite constraining. Instead, a set of eight major topic groupings generated a heterogeneous mix of titles. The list of these Category 1 headings conveys a sense of the whole: Geography; Agriculture; Production Systems, Business History, and Technology; Institutions, Governments and Markets; Macroeconomic History and International Economics; Labor; Natural Resources and the Environment; and Biographies, a category with subheadings for Inventors and Writers on Technology; Entrepreneurs, Bankers and Labor Leaders; and Economists and Economic Historians. The articles are richly illustrated, and intended to be accessible to general readers as well as to specialists, and supplemented with generous bibliographies and cross-references. The fifth volume contains more than two hundred pages of reference material, including a directory of useful internet sites.

A seven-page preface by Mokyr reviews the project’s guiding philosophy, emphasizing the effort to escape “Western-centric biases” in the choice of topics, and highlighting the coverage of institutions—broadly defined—as a distinguishing feature of economic history. In this respect the work is true to the editor’s word. Inevitably the majority of the detailed contributions deal with European and American topics, reflecting the research specialties of the field. But clearly a conscientious effort has been made to extend the geographic coverage to the far corners of the earth, primarily in useful surveys of national and regional economic histories, but also in novel topical entries such as the *Diandi* (a Chinese term for pawning land) and the *Waqf* (an Arabic form of property). These articles will make valuable references for students, and for scholars seeking information outside of their own specialty. Also informative are articles that address relatively abstract concepts with historical applica-
tions, such as Custom, Entrepreneurship, and Taboos, to say nothing of Feudalism and Capitalism.

It hardly need be said that such an encyclopedic work poses a challenge to the reviewer. The volumes are meant to be consulted for reference, not read from start to finish. If the overall construction represents an implicit commitment to a particular theory or conceptualization of economic history, such a subtext has thus far eluded this reviewer. Readers of this JOURNAL may be assured that anyone in our field can while away many happy hours pleasantly and profitably by browsing through the articles and following the trail of clues suggested by the numerous cross-references.

My one concern is how well the volumes serve those who are not in our field. In an effort to gain the perspective of an outsider, I undertook a small experimental spot check. Before examining the Table of Contents or the Index, I asked a colleague in British history to come up with a short list of questions on which historians might want to consult an encyclopedia of economic history. The result was the following list:

1. Did British imperialism pay?
2. Is the Industrial Revolution still a useful concept?
3. Has the Standard of Living controversy been resolved?
4. What were the causes and effects of the “Great Depression of the Nineteenth Century”?
5. Did the British economy decline between 1950 and 2000?
6. What is the present status of Keynesian economics?

These seem to be informed and intelligent questions, possibly somewhat dated; but it is reasonable after all to expect an encyclopedia of history to update readers on older concepts and controversies. How well does the Oxford encyclopedia do by this standard? The batting average is about 0.500. (A similar exercise with an Americanist yielded similar results.) Here is a rundown:

1. Good. There are no entries for Empire or British Empire, but a search under Imperialism leads the reader to an informative 11-page essay on Economic Imperialism by Gareth Austin, covering the past 500 years. This includes a paragraph on the Davis-Huttenback thesis and subsequent debate.

2. Again Good. Clearly the Editor in Chief thinks the Industrial Revolution is a useful concept, because he assigned the article to a leading expert, namely himself. The essay concludes: “The significance of the Industrial Revolution in economic history cannot be overestimated” (Volume 3, p. 55).

3. Here my informed outsider might come away disappointed. The index entry for “living standards” contains just one subheading for Industrial Revolution, which leads the reader back to Mokyr’s essay and a paragraph on human suffering caused by industrial restructuring. The index refers the reader to Volume 4, p. 48, for material on Wages and Labor Income by Charles Feinstein. But p. 48 is actually only the last page of a Feinstein essay on Investment and Savings (pp. 41–48). The essay on Wages and Labor Income by Leandro Prados de la Escosura and Joan R. Roses (pp. 48–52) surveys trends in several countries, beginning in 1856. Another essay by Peter Scholliers on Wages (Volume 5, pp. 203–10) discusses limitations of real wages as measures of living standards. But the venerable debate about real wages and living standards during the Industrial Revolution itself does not receive direct attention.

4. Good. I think it is accurate to say that these questions are not directly covered. To be fair, the expression Great Depression of the Nineteenth Century is obsolete, and Mark Casson’s essay on Entrepreneurship does include a good review of the debate on British entrepreneurial decline (Volume 2, p. 214). But the subject matter of Barry Supple’s essay on British decline as myth does not seem to be represented, and I could
find no attention to the trajectory of Keynesian economics as an aspect of economic history—though there is a biographical entry on Keynes by D. E. Moggridge (Volume 3, p. 218).

Can we learn anything from this hasty exercise? Perhaps only that historians and economic historians have different primary interests, something we probably knew already. One must also recall that my two consultants and their questions are distinctly Western, whereas the editors were at pains to extend their reach into non-Western topics. Perhaps most basically, one can hardly ask contributors to survey the state of present knowledge in subject areas where little economic history scholarship exists. Yet when all these mitigating factors are acknowledged, it might still be reasonable to suggest that more market research among potential nonspecialist consumers could be useful in designing future encyclopedias of economic history.

Let me return in conclusion, however, to my theme of celebration. We should all be grateful to the editors, and especially to the 800 contributors who have performed a valuable service for the collective profession, for which they will receive precious little professional reward. By all means urge your library to purchase this landmark reference. If you can get a good discount, you might want to invest in a set for your personal library. Otherwise, you can hold out either for the paperback or the on-line edition. Some sample chapters are already available at the Oxford University Press web site.

GAVIN WRIGHT, Stanford University


The importance of sugar in the creation of the Atlantic world can scarcely be overstated. Soaring production and surging demand underpinned the so-called hot beverages revolution in eighteenth-century Europe, as sugar sweetened the coffee and tea drunk by ever-widening groups. The abundance of studies of sugar in its Caribbean heyday has sometimes led to the assumption that the exigencies of sugar cultivation had imposed a common structure on production from the beginning, typified by the capital-intensive latifundia employing African slaves. This assumption, however, comes from reading back, and this new study of the formative first two centuries of Atlantic sugar production wisely cautions against such an approach.

The nine contributors to this volume, edited by Stuart Schwartz, focus mainly on early sugar production—dominated by the Iberian kingdoms—in Madeira, the Canary Islands, Hispaniola, Cuba and Brazil. These essays are particularly valuable because they often convey research previously inaccessible to nonreaders of Spanish and Portuguese. Also commendable is the acumen of these scholars who have creatively pieced together what are often very spotty archival records for this period. Consequently, much of the information offered here will be new to many readers. For example, it may come as a surprise to some the extent of cane cultivation in sixteenth-century Hispaniola and seventeenth-century Cuba, even though these early periods of Spanish florescence in colonial sugar production could not be sustained for longer than a few decades each.

But the greatest overall strength of this volume is that it shows how ad hoc the process of sugar expansion was in the Atlantic world. Far from determining forms of
production, sugar cultivation called forth a host of responses to the problems of land, capitalization, and labor. Iberian monarchs typically offered land as incentive, but no law required sugar plantations to be large. A variety of sizes could be found throughout the region in this time period, with a similar complexity in the terms of use or ownership of land on which sugar was grown. A common difficulty was the acquisition of capital to erect a mill, the most expensive element in sugar production. Initial success in production favored further capital accumulation and expansion of mills, but planter debt was everywhere and at all times a problem.

Also, African slavery was only one possible solution to the labor requirements of sugar production, which at various times and places also employed native Americans slaves and European peasant sharecroppers and wage laborers. If African slaves eventually came to dominate the sugar labor force, it was because of increased availability and low cost through the intensification of the Atlantic slave trade. That said, a clear pattern does emerge from these essays: Iberian peasants were not likely to meet the labor requirements of the sugar complex as it expanded ever further away from Europe. Coerced labor—in one form or another—appeared to be a necessary ingredient for the success of production, and the New World indigenes in the growing zones were ill-prepared to take up this burden. Consequently it is hard to imagine the continued expansion of the Atlantic sugar economy without the inputs of enslaved African workers.

Another challenge presented here to the conventional narrative of the spread of sugar concerns the introduction of sugar to Barbados, supposedly the beginning of a “revolution” in sugar production. An essay contributed by John McCusker and Russell Menard shows that when sugar cultivation eventually leapt to the British Caribbean, Barbados was already well poised to receive it. By the time colonists here turned to sugar cane, they had already gained valuable experience producing other agricultural commodities for the export market. It is doubtful, then, that Dutch capital fleeing Pernambuco was decisive in starting up production in Barbados. Also, the authors show that the movement away from tenant farming in Barbados and towards larger “integrated” estates was gradual, and small farmers producing cane never disappeared completely.

Although most of the contributions to this volume focus on sugar production, two address the African slave trade and European sugar consumption. Herbert Klein provides a useful synthesis of the most recent research on the trans-Atlantic slave trade. And in a memorable essay, Eddy Stols describes the rapid addiction of Europeans to sugar, which began to dominate elite banquets and sweeten holidays in the sixteenth century, even though it was still not an item of consumption for ordinary people. Stols also describes the inter-European trade in sugar, but lacking from the volume is any detailed description of the trade on its Atlantic routes.

This volume may not achieve the status of revisionist history, but it should go a long way towards dispelling ignorance about the origins of sugar in the Atlantic world, especially for scholars reading only English. The quality of scholarship here is excellent and the emphasis on contingency in the creation of an Atlantic sugar economy is apt. This volume also serves to emphasize the pioneering role of the Iberians in creating the Atlantic economy, even though the English and French eventually came to dominate it.

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