Book Reviews

MEDIEVAL AND EARLY MODERN


Questions regarding the nature of women’s work in pre-industrial societies have preoccupied women’s and social historians since the work of Alice Clark and Ivy Pinchbeck in the early twentieth century. Marjorie McIntosh’s new book supplies both a lucid summary of the state of the field and important new research and interpretations of continuity and change in women’s work in late medieval and early modern England.

McIntosh’s source base is impressively diverse, both in terms of its regional scope and in the disparate types of sources she has mined. Poll taxes from 1377–1381 are a central source for quantitative information on the late medieval period, and equity court petitions, available from 1470, are revealing for qualitative and quantitative purposes for the later period. In addition, she has studied five market centers that are both economically and geographically distinct, making particular use of the “aletasters” reports that are available in some cases for the entirety of her period. (Aletasters reports list the names of each individual or head of household who worked in the food and drink trade for local court records.) Because McIntosh is also able to exploit her extensive archival research from her many previous studies, as well as a now substantial body of secondary works on her subject, we have here a fairly slim volume that rests on an almost unequalled amount and quality of archival evidence. Not surprisingly, then, her conclusions carry tremendous weight and will likely be considered authoritative for some time to come.

The book generally confirms the more pessimistic interpretation of women’s work that has come to dominate the field. Although women were present and active in a wide range of work activities outside the home—domestic and personal service, financial services and real estate, making and selling goods, drink work, and skilled crafts—they almost always worked predominantly at the lowest end and at the least remunerative jobs within these occupations. Important exceptions to this rule included women’s work in silk manufacturing and retailing in London, and women’s role in the drink trade before the end of the fifteenth century. Still, McIntosh includes many rich (and even entertaining) anecdotes and case studies, which render the generalized account of women’s work infinitely complex. The picture that emerges is of clear general principles applying to the majority of women—principles that served to deeply disable women from high status and high profit occupations—with infinite loopholes and exceptions. Women with exceptional initiative who had the wherewithal and strength of character could seize opportunities and overcome cultural, social, and economic disabilities to succeed in many aspects of the pre-industrial economy.

McIntosh explores with great subtlety the changes in women’s work experiences over the later medieval and early modern period. Overall, she finds a decline in women’s economic expectations from the early sixteenth century. Although she challenges the notion that there was any “Golden Age” for women’s work in the later medieval period, she does find substantial improvements in women’s opportunities in the post-Black Death era (mid-fourteenth to later fifteenth centuries), when social and demographic changes created more need for, and hence more acceptance of women’s economic activities. This state of affairs altered with the changing social, economic, demographic, and cultural climate of the sixteenth century. England’s growing popula-
tion meant more competition for employment. The expanded world of trade and credit worked to women’s disadvantage, as it necessitated more travel and privileged those who traded on the larger scale (i.e., men). Because it witnessed rising concerns regarding social order, sixteenth-century English society was less accepting of independent and uncontrolled women. For the time period 1300–1620, then, McIntosh demonstrates that women’s fortunes fluctuated significantly, despite the overall continuity of their lesser status and constrained experiences compared to those of men.

One of the most interesting and significant sections of the book delves deeply into the “historical puzzle” of women’s apparent disappearance from the drink trade between about 1490 and 1540. McIntosh uses the aletasters’ reports to demonstrate that women brewers had held a relatively high status in their communities; their evident abandonment of such roles is thus particularly striking. Here, as elsewhere in the text, McIntosh is in close dialogue with the work of Judith Bennett (e.g. Ale, Beer and Brewsters in England: Women’s Work in a Changing World, 1300–1600. New York: Oxford University Press, 1996). McIntosh subtly revises Bennett’s views, demonstrating that women’s transition out of the drink trade was more abrupt, and more tied to local demographic and economic factors, than previously believed. Although she convincingly argues that this was a real change in women’s economic experience, McIntosh also posits an alternative interpretation for this transition. It is possible that there was a change in recording practice, which would have meant that women’s roles in brewing were hidden by a new method of record keeping among aletasters, in which the head of the household was recorded as the brewer, rather than the wife who did the brewing. She dismisses this possibility quite quickly, without really looking into the context of other changes in record keeping, which might have helped to shed more light on this issue. Still, her clear method of laying out this historical puzzle, the posing of the alternative interpretations, and her incisive analysis make this section a model for historical research and analysis that even lower-level undergraduates will be able to follow and appreciate.

Indeed, while this volume is an important contribution to historical scholarship on the topic of women’s work in the later medieval and early modern period, it has an equally high value as an accessible, comprehensive and appealing text for undergraduate study. As someone who co-teaches a course on premodern women with a medieval colleague, I welcome this book with tremendous enthusiasm. It not only covers its topic well, but also it introduces students to larger historiographic debates in a way that is both concise and comprehensive.

SUSANNAH OTTAWAY, Carleton College


Christopher Dyer’s new book presents in published form the author’s Ford Lectures delivered at Oxford University in the spring of 2001. Invitations to give the Ford Lectures are reserved for Britain’s most eminent historians and provide a venerable and highly visible platform. Traditionally, the books that emerge from the Ford Lectures do three things well: they synthesize and summarize the leading secondary literature that has defined the contours of a particular field; they situate the author’s own contributions within that field; and they present an overview that makes these contributions accessible to a wider historical audience. Dyer succeeds admirably in all three areas,
and the result is a book that is sure to be of lasting value for specialists and non-specialists alike.

The book is animated by two general themes. The first is that the nature of late medieval society and the fortunes of its economy can be understood only by focusing attention on the lower orders of society, particularly peasants but also artisans, wage laborers, servants, and apprentices. Much of the argument in this regard is supported with evidence derived from the period’s material culture and relies heavily on the author’s deep and abiding interest in archaeology. A good example of his approach occurs in a chapter dedicated to consumption and investment in the medieval economy. Dyer argues against the view that consumer goods constituted a negligible part of medieval economic activity and buttresses his argument with a bevy of archeological site reports documenting findings of belt buckles, combs, professionally produced pottery, metalware, and various other inexpensive items acquired for household use and personal adornment. On the basis of these finds he challenges the now-orthodox view that a consumer-oriented economy emphasizing fashion and shopping as major components of economic life can be found only in later periods of history. His treatment of domestic architecture and related building activity takes a similar tack: contrary to those who consider medieval housing to have been cheap and ephemeral, Dyer shows that many medieval houses and farm buildings have survived into the present day, and argues that their design and quality indicate a greater concern with comfort and privacy than is generally attributed to the period. Dyer’s point in emphasizing material culture is two-fold: to show that the lower orders in society were active participants in forming an economy based on consumption, and to emphasize the contributions of the carpenters, metalworkers, potters, and other craftsmen who produced most of the goods that circulated through the economy.

The second general theme is that the “long” fifteenth century, encompassing the last quarter of the fourteenth century and the first quarter of the sixteenth century, should be viewed as a period of considerable innovation and economic dynamism. In this respect Dyer opposes the traditional textbook view that presents the period as a kind of dead zone between the more vibrant worlds of the high Middle Ages and the early modern period. He readily concedes the devastating influence of plague and depopulation throughout the period and recognizes that political instability, civil unrest, and urban decay cannot be ignored as components of the period’s history. But he suggests that too much emphasis has been placed on these negative attributes of the period and insufficient attention given to its positive accomplishments. In his view the period’s dislocations were actually the key to its positive transformations, because they forced all levels of society to grapple with a fundamentally new set of economic realities. In this situation the biggest rewards were reaped by those who were most nimble and best able to adapt and innovate, and Dyer argues that adaptability and innovation were much more characteristic of those at the base rather than at the apex of society. A prime example of such adaptability can be found in the management of larger agricultural enterprises, which were revamped to emphasize pastoral production at the expense of cereal cultivation, a process that was directed largely by entrepreneurial peasants who flourished by taking on leases of lands formerly managed directly by lords and their agents.

Neither of the themes Dyer develops will come as much of a surprise to specialists familiar with the author’s previous work. Indeed, in spite of Dyer’s claim to be contributing to the creation of a “new” Middle Ages (a term that is pressed into service as the title of the book’s first chapter), both of the positions he stakes out have long pedigrees. One can be traced back to Marc Bloch and the founders of the Annales move-
ment (on the importance of writing history from below), the other to James Thorold Rogers who, more than a century ago, described the late medieval period as a “golden age” for workers. What is new and important in Dyer’s book is the skillful bringing together of these themes in a relatively short and accessible work, one that ranges effortlessly over the country as a whole and situates the period’s major changes in a framework that stretches from the tenth to the eighteenth century. Dyer devoted his Ford Lectures to bridging the gap between medieval and early modern economic history, and his own willingness to journey back and forth across that bridge attests to the solidity of the structure he designed.

JAMES MASSCHAELE, Rutgers University

WESTERN EUROPE


*Keynes and His Critics* is the thirty-sixth volume of the British Academy’s Records of Social and Economic History, New Series. It is doubtful that the academy could have chosen a better collection or a better editor for the first volume in the series to cover the twentieth century.

G. C. Peden, Professor of History at the University of Stirling, has spent the better part of three decades grappling with the details of Treasury history, and in particular with the influence of Keynes on that history. The experience shows. Accented by Peden’s excellent yet understated commentary, the 76 documents and document excerpts included in *Keynes and His Critics* take the reader from debates over Britain’s return to the gold standard, through questions of how to deal with the Great Depression and finance the war, to the days of Bretton Woods and Keynes’s death in 1946. They tell a tale of Treasury transformation. The Treasury of the late 1920s had fundamentally disagreed with Keynes’s new ideas on the connections between loan-financed public works and unemployment; by the end of World War II, however, even the opponents of Keynes’s ideas stood firmly upon his ground.

As *Keynes and His Critics* shows, however, transformation is a tale needing to be told carefully. It was not a simple triumph of the *General Theory* and a simple defeat of a pre-Depression “Treasury view.” It was a nuanced tale of Ralph Hawtrey, director of financial enquiries, struggling in 1929 with the *Treatise on Money* even as he anticipated the idea of the multiplier (pp. 62, 89–91), and of Hawtrey and other Treasury bureaucrats educating Keynes even as he educated them about liquidity preference. It was a tale of Richard Hopkins who, as controller of finance and supply services in 1929, stood firmly with fellow controllers Otto Niemeyer and Frederick Leith-Ross on classical ground and viewed Keynes as a “currency heretic” (p. 53), but who, as permanent secretary between 1942 and 1945, had become a willing advocate of Hugh Dalton’s cheap money policies (pp. 344–54). It was a tale of a Hopkins who, even though his own policy preferences had been transformed, still had to compete regularly for the Chancellor’s attention with Wilfrid Eady and H. D. Henderson on issues of capital budgeting and deficit finance. The transformation of Treasury was not so much the triumph of Keynesian policies as it was a transformation of the questions to be asked during the formulation of those policies. Treasury’s answers still resisted Keynes in 1945; the questions they asked, however, did not.
Keynes and His Critics is worth the investment just for its tale of Treasury. Yet the book’s value extends far beyond its story of bright, clever, and articulate Treasury officials whose names have fallen unremembered by most. It illuminates Keynes himself. By editorial design Keynes appears only indirectly, when reflected in the eyes of Treasury under-secretaries and controllers. Nonetheless, one regularly sees here not only the transformation of under-secretaries, but also how Keynes’ own ideas changed from the Treatise on Money through the General Theory to Bretton Woods. And because Peden provides no shortage of footnotes and cross-references to the thirty volumes of The Collected Writings of John Maynard Keynes (E. Johnson and D. Moggridge, eds., 1971–89), together with a very good bibliography, one knows where to look when the inevitable “Did Keynes really say that?” questions arise.

Keynes and His Critics is not just for graduate students and specialists in the history of Keynes and Keynesian policy. It is the kind of book everyone who teaches undergraduate macroeconomics should spend time with. Consider the decades’ worth of American undergraduates who have grown up to become leading journalists, politicians, and business executives: these policy-makers were weaned upon stylized versions of “classical” and “Keynesian” theories that relegated “international trade” and “open economy” chapters to a hurried end-of-semester survey, versions that suggest that the essence of Keynesianism and its alternatives can be captured by closed-world stories. Yet, as Keynes and His Critics makes clear, neither Keynes nor his critics argued anything approaching the stylized position. With the possible exception of chapter 6, which focuses upon specific responses to the General Theory, Keynes and His Critics provides a chronicle of competing open-economy visions.

As with any publication of “selected” documents, questions arise of the “Why include this and not that?” sort. One wonders, for example, whether there are no documents between July 1925 and August 1928 worthy of inclusion. More significantly, why are there no documents for the three years between April 1933 and March 1936? Is the absence of talk about trade and foreign loans in the documents of chapter six simply an artifact of that chapter’s focus on reactions to The General Theory, or was it the result of changes at Treasury during the intervening years?

Yet in the end these are mere quibbles. Peden has performed a great service for scholars and teachers with Keynes and His Critics. Scholars interested in the evolution of ideas, in the tension between intellectual innovation and the constraints of administration, in how civil servants deal differently with new ideas in the midst of a major crisis like the Great Depression or World War II, will all find the book valuable. And so will all who teach macroeconomics or macroeconomic history and wish their students to grow up and approach policy questions carefully. I am neither a fan of Keynesianism nor a fan of bureaucrats; but after spending time with Hawtrey, Hopkins, et al., I can safely say I will be teaching the economics of both differently.

Wade E. Shilts, Luther College

AFRICA


This book appears to have been ready to go to press when its author, Charles Feinstein, died on 27 November 2004. It is based on lectures he delivered at Cambridge
University earlier that year. Feinstein was born in Johannesburg in 1932. At the University of Witwatersrand in 1950 he took a course given by Helen Suzman in South African economic history and then had done nothing further on the subject for more than 50 years. His unpublished 1959 Cambridge University dissertation was on “Home and Foreign Investment: Some Aspects of Capital Formation, Finance and Income in the United Kingdom, 1870–1915.” Its aim was “more accurate or more complete measurements, to establish causal relations which others have assumed . . . .” His subsequent work on U.K. national accounts and on international capital movements represented major contributions. Throughout his career, he sought measures of economic activity. This economic history of South Africa is no exception. His arguments are documented by appropriate explanatory tables.

The book covers the people and economy before 1652 (when the Dutch East India Company set up its base at the Cape of Good Hope) and then the subsequent history to 1994 (with the end of apartheid and the transfer of power to the first democratically elected government). It has good (and essential) maps as well as an invaluable list of original and present-day location names. Feinstein has no sympathy for the European colonizers who seized land, through conquest and dispossession.

Initially, in 1652, the Dutch intention had been to set up an outpost at Cape Town that would allow the ships of the Dutch East India Company to restock themselves with vegetables, meat, and water for the remainder of the trip to the East Indies. Soon, the small colony was planting and growing food—wheat and other grains—and breeding cattle and sheep. The company employed Germans in its settlement. In the late 1680s French Huguenots settled in the Cape. This group of Dutchmen, Germans, and French became the nucleus of the Afrikaans-speaking Boer population (Afrikaners). Settlements took hold with huge farms for cattle raising. By 1806, when the British obtained control of Cape Town, the European colony numbered merely 27,000. As the white population expanded, political reorganizations took place, so that by 1865, 250,000 Europeans peopled the two British colonies of the Cape and Natal and the two Boer republics of the Orange Free State and Transvaal. Everywhere, the Africans (blacks) were pushed out of the way.

As the white population spread it clashed with different indigenous peoples, the Europeans always assuming that the land was theirs for the taking. At first it was the white farmers, who had plenty of land and already not enough labor. And then after the 1870s, as the diamond fields and the gold mines were developed, a large, regular supply of labor became essential. Blacks were brought in from outside the region. Feinstein argues that farmers (whether English or Dutch), British imperial administrators, and mining interests united “in their brutal denial of human rights to black people,” sustained by “their conviction that Europeans represented a superior civilization” (p. 35). Feinstein quotes a Britisher’s comments in 1877: “wherever a civilized power is brought in contact with an uncivilized, where law and order find themselves unavoidably neighbor to anarchy, civilization cannot stand still, it must for its own preservation be aggressive lest if law and order be not forcibly imposed anarchy may haply render both impossible” (p. 36).

The culmination was the Natives’ Land Act of 1913, which made it illegal for Africans to acquire or rent any land outside the existing reserves that had been set up for them (the reserves would later—after World War II—be renamed “homelands”). Policies toward land were designed to accommodate the labor needs of farmers and the mining interests. Much of the book deals with the genesis of and development of policies that kept Africans from developing skills: “black labour for white masters.” Feinstein explains the paradox of scarce labor and low wages. There is a chapter on creat-
ing the color bar: “formal barriers, poor whites, and ‘civilized’ labour” (Chapter 4). Feinstein defines the term “coloured” and throughout uses the word “black” as a collective noun for the African (Bantu-speaking) inhabitants, the heterogeneous group in the category of colored, and Asians.

Gold was not just any commodity, and Feinstein believes that it was responsible for the good growth rates in South Africa in the years 1913–1950. Manufacturing was slow to start. It got some encouragement during World War I, and then further governmental assistance from the mid-1920s. By the 1920s a poor white group was developing in South Africa. Feinstein argues that unique to South Africa were tariff and other policies to spur manufacturing designed specifically to create employment for labor of European descent. What emerged was low-paid unskilled African workers and relatively high-paid white workers (see p. 133 for international comparisons, 1938–1939). Feinstein explains how this was accomplished.

During 1950–1973 South African economic growth was better than in the years 1913–1950, but wanting when compared with Feinstein’s sample of 30 countries. In 1948 the Afrikaners’ National Party was elected, with a strong endorsement of apartheid. Rules on racial separation were strengthened; the African was left further behind. In 1970, 63.4 percent of Africans in rural areas had no education whatsoever. Moreover, following the dictates of apartheid, millions of blacks were forced off farms and also out of urban areas, making the reserves even more crowded. Industrialization went forward during the 1960s and early 1970s, encouraged by government protectionist policies. Though some skills were learned by Africans, on the whole the sharp division between white and black remained. The government, most of the white electorate, radicals, and Marxists all were convinced that apartheid was consistent with a successful capitalist economy. Feinstein shows how wrong they were.

From 1973 to 1994, in both relative (to Feinstein’s sample countries) and absolute terms, the South African economic record was abysmal. In the century prior to 1973 South Africa had been transformed from a country dependent on agriculture to a modern economy. After 1973 stagnation in real gross fixed capital formation, declining output per unit of capital, rising unemployment, high inflation, a sharp depreciation in exchange rates (after 1983), and serious balance of payments problems set the economy awry. The surge in oil prices hurt a country that did not produce this vital commodity. Gold, which had driven the economy, encountered soaring costs of production, which world prices did not offset after the 1980 spike. Low levels of efficiency and high costs of production in the industrial sector meant that nongold exports could not substitute “and launch the economy into a new orbit propelled by exports of manufactured goods” (p. 220). Low-income black workers provided no adequate domestic market for the goods produced by the new industries. Ineffective government policies could not cope with the economy’s woes. Added to all this, there was “seething political discontent and militant activity by black people in mines and factories. . . .” (p. 202). The failure of the economy, made worse by the international situation (from African governments in neighboring nations, to financial sanctions from abroad after 1985, to the end of the cold war), climaxed in the democratic elections in 1994. Although the change was sudden, over the past decades there had been a general awareness that the system of apartheid was flawed, that blacks needed to acquire skills, and that black access to education should be improved. Feinstein explores the mistaken notion that low-paid workers with low skill levels are compatible with successful development—the fallacy of “cheap” labor. Economic development, he argues, is achieved by technical progress, better human capital, and advances in productivity.
The book ends in 1994 with the downfall of apartheid. Feinstein does not deal with the performance of the South African economy under black leadership. This volume should be required reading for all students of South African economic history; other students of economic development will also find many lessons to be learned.

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UNITED STATES


Today, most Americans who live in metropolitan areas live in single-family detached homes and commute to work by automobile. New York City is America’s sole urban center where a significant fraction of the population lives in apartment buildings, works downtown, and commutes by public transit. We are choosing to live and work in the suburbs. But, this trend has triggered a “cost of sprawl” literature that posits that there are many unintended consequences of the pursuit of the “American Dream” that range from increased traffic congestion, urban air pollution, greenhouse gas production, farmland paving, to reducing center city tax revenues, and denying the urban poor access to employment opportunities.

In this book, Robert Bruegmann revels in playing the contrarian. In his opinion, sprawl opponents have too often dominated the debate. Bruegmann attempts to level the playing field by arguing that sprawl offers many benefits. “It has also provided millions of people with the kinds of mobility, privacy and choice that were once the exclusive prerogatives of the rich and powerful.” (front book flap).

The book is organized around three broad chronologies. The first five chapters describe “sprawl across the centuries.” In chapters 7 through 10, Bruegmann presents an excellent investigation of the intellectual history of the antisprawl movement. “Not incidentally, it was exactly at this point, when the automobile ceased to be a luxury item for the affluent and came into the hands of a large middle class, that the anti-automobile sentiment grew really strident” (p. 130). The final three chapters of the book offer a history of anti-sprawl policies ranging from London’s Greenbelt to recent Smart Growth efforts in the United States.

Bruegmann also seeks to explain why sprawl has taken place. A short chapter 6 is devoted to this task. This is the book’s weakest link. Bruegmann is a historian of architecture, landscape, and the built environment. He is not an empirical microeconomist. He engages in a peculiar form of hypothesis testing. If he can name a counter-example, he dismisses a potential cause of sprawl. Most economists would seek to set up an econometric test to run a “horse race” of alternative explanations. For example, do we see greater sprawl in cities that have had worse urban riots in the past? Do we see greater sprawl in cities that have built more highways? Bruegmann chooses not to pursue this strategy and in the end he declares “In the case of urban areas and sprawl, as in the case of virtually any vast and complicated human or natural system, there is very little simple cause and effect. Rather, there are innumerable forces, always acting on each other in complex and unpredictable ways” (p. 112).

Bruegmann presents an evenhanded overview of what are the benefits and costs of sprawl. But, the debate over sprawl’s merits hinge on how large are the benefits and costs of this trend. To answer this question requires some investigation of the existing empirical literature. As demonstrated by the smash success of Steven Levitt and
Stephen Dubner’s *Freakonomics* (New York: HarperCollins Publishers, 2005) statistical analysis can *increase* book sales! Permit me to offer two examples where insights from the applied economics literature would have informed this cost/benefit analysis. To measure the benefits of sprawl, we need some measure of the consumer surplus that home buyers gain from purchasing a new, spacious suburban home at a reasonably low price. To measure one piece of the environmental costs of sprawl, we need an estimate of how many extra gallons of gasoline do suburban drivers consume relative to similar center city residents.

Unfortunately, Bruegmann is unwilling to get down and dirty to help the reader digest the existing empirical evidence that has been generated by economists, urban planners, and geographers. For example, consider this vague quote concerning the environmental consequences of sprawl. “For our present purposes, the first and most important question is whether global warming is necessarily linked to sprawl. Although low-density living has undoubtedly been accompanied by more miles of driving and more energy use per capita and this in turn has led to production of more greenhouse gases, it is certainly not clear that sprawl itself is the culprit” (p. 149).

For broad readers interested in urban history, this book is fun and worth reading. I greatly appreciated the author’s highly engaging and clear writing style. The book’s photographs and endnotes are highly informative. For teachers, this book will be a useful supplemental reading in a variety of undergraduate courses ranging from urban history to urban economics. Many undergraduates interested in long-run trends will be captivated by this book’s ideas. This book highlights the importance of adopting a historical perspective when thinking about an important current policy issue.

MATTHEW E. KAHN, Tufts University


Michael R. Botson, a steelwork turned historian, has written a tremendously important book. Although the title, *Labor, Civil Rights, and the Hughes Tool Company*, would suggest yet another case study of the nexus between the civil rights and labor movements, Botson’s work has much larger historiographical implications. Botson does indeed illuminate an oft-ignored but consequential story about a group of Houston’s black steelworkers who successfully fought for their civil and labor rights. Botson argues that it was their activism with the support of the federal government that created the conditions to improve their lives. This history, however, is connected to much deeper historical questions about the relationship between black workers and organized labor and about the historic nature of racial discrimination in employment. Although in the end, Botson raises as many questions as he answers, his study offers fresh fodder for the continuing historiographical battles about race, employment, and the labor movement.

Founded in 1915, the Hughes Tool Company became one of the nation’s largest steel fabricating plants. When it was small, the factory, which was run directly by Howard Hughes Sr., had the air of a family business. But as the manufacturer of steel drill bits grew, labor relations changed dramatically. To fight the worsening working conditions, workers joined the Houston Metal Trades Council and went on strike in 1918. The protest was crushed. Afterward, the anti-union Hughes decided to use a modified version of the American Plan to forestall any future union activity. The
Hughes Tool Company’s managers fostered two company unions, an all-white Mutual Welfare Organization and an all-black Hughes Tool Colored Club (HTC). This set-up seemed to have near universal support. As Botson points out, company officials supported the segregated organizations out of their racist views and because it kept workers divided and wages low. Most whites believed that biracial company unions were proper and in line with the larger community standards. Finally, and importantly, the black workers assented to this design out of a rejection of the more mainstream—but still racist—labor organizations such as the American Federation of Labor (AFL) and out of a preference for mutual assistance. Segregation for these black steelworkers was not a product of some fringe ideas about nationalism or group identity. Rather it was a conservative response to America’s conservative labor movement.

The Great Depression and President Franklin D. Roosevelt’s New Deal upset the fragile equilibrium in the Hughes Tool Company’s labor relations. Angered by company managers who dealt with the economic disaster by firing workers and cutting wages while increasing productivity and seeing a chance for an independent union, many white workers left the old MWO to join the International Association of Machinists (AFL). Hughes’ primary supervisor, R. C. Kuldell, did everything he could to disrupt the IAM unionists as well as the Congress of Industrial Organizations’ steelworker organizers who were also making inroads on the shop floor. In fact, it was the CIO, and not the AFL, that posed the greatest challenge to the status quo at Hughes. The Congress’s commitment to organizing the unorganized on an industrial basis and to supporting civil rights threatened not only low wages but also southern mores concerning race and union power. Kuldell fought the CIO by reinvigorating the two company unions: the Employees Welfare Organization (a modified version of the MWO) and the HTC, which together merged into the Independent Metal Workers Union Local 1 (all white) and Local 2 (all black). The IMW won initial National Labor Relations Board elections against the CIO, but shortly after the United States enter World War II, the balance shifted to the Congress. The CIO defeated the IMW by altering its union strategy. The Congress’s organizers focused almost exclusively on bread-and-butter issues such as wage increases and maintenance-of-membership. Moreover, to attract white workers, it created segregated locals. To avoid alienating its black supporters, the CIO at Hughes promised that it would fight for civil rights once it was the sole bargaining unit. The tactics worked at first, but a resurgent IMW finished off the CIO for good after the war. It was at this point that Hughes’s black employees—stuck in IMW Local 2—decided to wage their own fight for civil and labor rights. Unsurprisingly, they went outside their union for support. They found an ally in the NAACP, which helped them file a discrimination case with the National Labor Relations Board. In 1964 the NLRB ruled in Hughes’s black steelworkers’ and the NAACP’s favor and against the IMW. This pathbreaking decision blazed a new trail for federal activism in the field of employment discrimination.

Labor, Civil Rights, and the Hughes Tool Company is a compelling read. Botson is an excellent historian and storyteller. The book is rich in detail, and he more than adequately recaptures the monumental struggle for justice at this Houston factory. But in some ways, the book’s strength is also its weakness. Botson does not always take the opportunity to link his narrative fully to larger historiographical debates. For example, recently some historians have argued that labor unions were primarily to blame for racial discrimination in employment and that managers merely responded to market conditions. Botson’s story is a clear illustration that shop floor racial politics were more complex, and at least in this case, reflected ideologies and compromises from all parties involved. Botson also offers a much more nuanced view of the relationship be-
between blacks and the labor movement. As much as their white co-workers, blacks wanted to belong to labor organizations. And, they were similarly suspicious of the large, national federations, which did not seem to keep local conditions in mind. Furthermore, Botson’s discussion of segregated unions complicates our view of the past. By accepting segregated unions, these African-American steelworkers were complicit in maintaining Jim Crow at the workplace. And yet, these unions also shielded black workers from some abuse by the white-dominated AFL and CIO unions. Finally, Botson’s book urges historians to examine more carefully the history of company and independent unions. Although the CIO and AFL dominated the national scene for decades, these smaller unions nonetheless played significant roles. In summary, Labor, Civil Rights, and the Hughes Tool Company is an exceptional case study that might act as a springboard for the further investigation of larger historical and historiographical questions concerning the race and the labor movements.

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Beginning with a couple of papers published in this JOURNAL in the mid 1980s, Robert McGuire and Robert Ohsfeldt returned to the once-dormant view of Charles Beard’s (An Economic Interpretation of the Constitution of the United States. New York: Macmillan Company, 1913) thesis that delegates to the Constitutional Convention of 1787 clashed over creation of the constitution, based primarily on whether they were merchants with “personalty” interests, or farmers with “realty” interests. In To Form a More Perfect Union, McGuire updates his joint work with Ohsfeldt and his solo-authored work, corrects some errors in the earlier data, and presents some new specifications and additional estimations. Although most of the original studies appeared in the mid to late 1980s, one chapter is adapted from a 1997 article. All told, this is an impressive volume that, while not without its share of questions, nicely synthesizes and surpasses the earlier work, pulling it all into a single coherent and impressive piece of scholarship.

The central theme of the book is a recasting of the Beard thesis to show that self-interest by the delegates played a fundamental role in influencing their decisions at the margin. This is an important distinction from Beard and from much of the previous analysis of Beard by historians and political scientists, which focused on whether individuals and groups at the Convention did or did not vote a specific way. Instead, multivariate regressions are employed to estimate the marginal impact on the probability of voting for or against.

Sixteen specific votes at the 1787 convention are analyzed. Thirteen of the votes are then pooled to determine the percentage of times a delegate voted in a pronationalistic stance. Using a variety of explanatory variables, multiple regressions suggest that personal interests and ideology, and constituency interests and ideology, matter.

Although the analysis is comprehensive, there are some methodological questions that suggest the results are not as straightforward as presented. First, the actual votes of the delegates to the 1787 convention are unknown. McGuire relies upon Forrest McDonald’s (We the People: The Economic Origins of the Constitution. Chicago: University of Chicago Press, 1958) inferences on 16 specific votes, which were based
on a review of the debates, correspondences, and some educated guesswork. McDon-
dald was not able to make any inferences regarding the eight Pennsylvania delegates for
any votes but conjectured they probably all voted the same. McGuire accepts this con-
jecture and also adds delegates who were not present on particular votes, so that he has
the same 53 delegates (two delegates who left the convention early were not included)
for each vote regardless of whether or not they actually voted. This makes interpreting
the results a little more difficult. Furthermore, assuming the Pennsylvania delegates
always voted the same may bias the state-level variable estimates. In chapter 3
McGuire presents a very nice detailed analysis for six of the individual votes. He also
discourts two of the votes because McDonald’s coding for these votes are not consist-
ent with how the states actually voted. This may cause concern for some readers re-
arding McDonald’s inferences for the other votes as well.

Second, from a great variety of regressions, the economic model is supported if any
of the variables are found to be significant, even when there is no consistency across
the votes. This also suggests pooling the votes may not be appropriate. Also, with so
few observations some of the variables do not contain enough variation to be included
in logit analysis. For example, there are only three debtors in the dataset, so this vari-
able could be included in a logit regression only when one of the three debtors op-
posed the other two in how they voted. And given that the debtor variable had some of
the strongest results, it is hard to reach a firm conclusion based on only three voters.
Similarly, only four delegates are considered farmers. This coding for farmer is also
apparently much different than Beard’s view, which suggested that farming interests
comprised a sizable coalition.

Finally, I might quibble with the interpretation of the importance of constituency in-
terest variables. McGuire argues constituency interests would constrain delegates from
acting in their own immediate self-interest in order to secure future votes for office.
This is questionable because delegate votes were kept secret. To the extent that con-
stituency interests matter, it might be that delegates wanted to benefit their constitu-
teens regardless of future political ambitions.

These methodological concerns, however, are absent from the last two chapters,
which focus on the 13 state ratification conventions, and are therefore much more
convincing. Here, the actual votes are known and only actual voters are included in the
analysis. And with observations for over 1,350 delegates, there is more variation in the
data. Most of the constituency variables are not found to be important but, surpris-
ingly, delegates from smaller states were more likely to support ratification. On aver-
age, delegates from the later conventions were more likely, at the margin, to oppose
ratification, until it became a moot issue after the first nine states ratified, after which
point delegates from the remaining states were much more likely at the margin to sup-
port ratification. The economic interests were also estimated to be much stronger in
impact during the nine conventions before ratification became moot.

Empirical analysis is also conducted on seven individual state conventions for
which more data exist, by recasting constituency interests to the county level. Finally,
three states for which McGuire could obtain data representing personal ideology (age,
ancestry, religion, etc.) are considered in greater detail. In both cases, the results vary
considerably as to which specific variables are found to be important. While many his-
torians believe different factors were at work in different states, McGuire’s analysis
often conflicts with which particular factors are or are not estimated to be important in
each state.

In sum, this book should help to reawaken the debate on construction of the U.S.
Constitution. The detailed summaries and reams of regression analysis will be fruitful
to economic, political, and legal historians. Although this may not represent the last word on the subject, it is hard to argue with McGuire’s contention that “Constitutions are the products of the interests of those who frame and adopt them” (p. 8, italics in original).

JAC C. HECKELMAN, Wake Forest University


In this thoroughly researched, thoughtful, and well-written work, Richard Follett explores the economics of the sugar industry of southern Louisiana before the Civil War while also examining the social, cultural, and ideological world of master and slave within this distinct agricultural and environmental context. His central contention is that Louisiana’s antebellum sugar industry exhibited characteristics of a capitalistic social order, such as the supremacy of the marketplace and acquisitive individualism, as well as those associated with a prebourgeois (or antibourgeois) social order, such as the principle of reciprocal obligations between superior and subordinate members of society. Moreover, Follett maintains that slaveholders employed a combination of coercion and incentives to induce slaves to acquiesce to the unrelenting demands of sugar plantation routine and to the hyper-exploitation of labor that sugar production required. Slaves, for their part, accommodated themselves to this demanding “agro-industrial” complex and to the sugar masters’ authority in order to improve their daily living conditions and, more importantly, to establish not only customary privileges but also supposed proprietary rights over their property and labor. Thus, the masters’ balancing of market-oriented incentives and force, along with the slaves’ agency, drove the antebellum Louisiana sugar industry toward maximum efficiency.

Follett demonstrates how capitalism’s profit-maximizing ethos, the environmental constraints of sugar production in Louisiana, and the slaveholders’ paternalistic ideology shaped the experiences of both master and slave. Follett’s discussion of the slaveholders’ demographic manipulation of the slave labor force demonstrates that the sugar masters practiced an insidious form of slave breeding. His examination of slaveholding management and the organization of plantation routine likewise proves that sugar planters enthusiastically incorporated technological innovations and modes of organizing labor more akin to factories than to farms in order to optimize both efficiency and profits. And his exploration of the psychological world of both master and slave shows how the paternalistic character of the master-slave relationship existed alongside the slaveholders’ capitalistic ethos even as it allowed the slaves to pursue the goal of greater cultural autonomy. All of these points are made cogently and effectively, and they are firmly rooted in a meticulous examination of a wide array of primary source documents and in a comprehensive understanding of the secondary literature and scholarly debates.

Follett’s work, in fact, is one of the latest in a series of studies on slavery in the antebellum South that has attempted to bridge the historiographical divide between those scholars who emphasize the differences between a capitalist North and paternalist South, thus insisting upon the inevitability of the Civil War, and those who see the South as essentially capitalist and who thus deny the notion of an impending crisis. Follett makes an important addition to this welcome development. The paternalism-capitalism debate has often seemed stilted, and, even to the degree that it expressed legitimate scholarly differ-
ences, by the early 1990s it had reached something of a dead end. Advocates on both sides in recent years have pursued a historiographical middle ground, and works such as Follett’s have sought to reconcile the two perspectives as a way of providing a more nuanced understanding of the slave South and of the slaveholders.

That being said, however, the larger implications of Follett’s argument, especially as it pertains to the slaves, raises certain questions. Follett insists that not only did slaves on Louisiana sugar plantations use paternalism to create for themselves a host of customary privileges, but they also accepted their masters’ system of market-oriented incentives, and that by doing so they were able to negotiate with their owners over the selling of their labor power. In effect, slaves acted, at least to a degree, as free laborers. Indeed, Follett goes so far as to argue that slaves internalized the values of the capitalistic marketplace, including that of acquisitive individualism, and that the very process of internalizing these values enabled masters to wrangle out of slaves their maximal productive capacity. This argument resembles the one aspect of Robert W. Fogel and Stanley L. Engerman’s *Time on the Cross: The Economics of American Negro Slavery* (New York: W. W. Norton, 1974) that received perhaps the most withering criticism, and it begs the question of how slaves could have negotiated anything. Follett is careful to note that masters ultimately had total power over their slaves, and yet he refers to “the slaves’ ownership of their labor” (p. 201), “the slaves’ right to their own labor” (p. 217), and practices that “commodified their labor power” (p. 218). It is axiomatic that even the most brutally oppressive and tyrannical of regimes allows its victims some minimal social space, but the question then becomes what significance do scholars attribute to such space. The literature on slavery has established an element of reciprocity between master and slave, yet slavery was essentially a coercive labor system, rooted in violence and in the unquestioned domination of one person over another. There was nothing about slavery in the Old South that even remotely resembled noncoercive or voluntary labor.

Follett seems intent on establishing the continuity between Old South and New South by denying the significance of emancipation. Discussing emancipation as the great divide in southern history, he states that “no such rubicon existed” (p. 235). For years scholars have pondered the question of “how new was the New South,” with some arguing that the New South’s social relations were so oppressive as to make them indistinguishable from those of the slave South. Follett appears to contribute to this interpretation by adding as a corollary that certain features scholars ordinarily associate with the New South—the commodification of labor power, the bargaining between workers and capitalists over the value of that labor power, the emergence of a marketplace to mediate those negotiations, and the struggle by African Americans to reap the fruits of their own labor—can be identified in the Old South. Although Follett’s interpretation offers an important corrective to what is sometimes seen as a romanticized portrait of antebellum southern slaveholders, especially those in the Louisiana sugar region, its applicability to the slaves and to the master-slave relationship leaves one wondering what was so old about the Old South.

JOHN C. RODRIGUE, Louisiana State University


Scholars in American history have been accustomed to consider apprenticeship as a system by which, from colonial days to the late antebellum period, fathers could vol-
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untarily bind their children to a master, by means of a written contract, in order to pro-
vide them with the opportunity to learn a useful trade. And these fathers did not neces-
sarily belong with the poorest classes of society. As a contractual form of labor, ap-
prenticeship did not entail any loss of freedom for the individual, lest the observance
of the obligations specified in the contract. Although different from indentured serv-
tude, in that the contractor was not the same person who was bound to labor for a mas-
ter for a given number of years, voluntary apprenticeship shared a number of charac-
teristics with the former.

In her book, Karin L. Zipf shows instead that a much more widespread form of ap-
prenticeship did exist in America from colonial times that was of a compulsory nature
in that it was enforced by the courts. Until the early 1800s forced apprenticeship was
common in all American states, but it later declined in the North with the affirmation
of the industrial economy. In the South, on the contrary, it “evolved in the shadow of
slavery as another form of forced labor,” becoming stronger and stronger in the last
few decades of the antebellum period (p. 8).

Concentrating her analysis on the state of North Carolina, Zipf illustrates how
forced apprenticeship was always, indeed, a system of social control, constantly evolv-
ing as to reflect changing social perspectives on race, class, and gender, until its final
abolition in 1919, with the passing of the Child Welfare Act. During the colonial era,
forced apprenticeship was meant to limit the financial onus of local governments in
the support of indigent children. Reflecting similar attitudes entertained elsewhere in
the colonies, the North Carolina law defined an orphan as any child with no father,
thus denying women guardianship rights. In so doing, it not only implied that women
were by definition unable to support their children alone, but also that they lacked the
moral qualities to do so properly, no matter how economically self-sufficient they
were. In this way, poor widows and single mothers were also deprived of an important
source of financial support for the family. As a notable example, Zipf mentions the
case of future U.S. president Andrew Johnson who, when a boy, had fallen under the
forced apprenticeship law along with his brother, but later escaped that condition and
fled to Tennessee with his mother and brother.

The apprenticeship laws enforced by the courts were all the more designed to con-
trol free blacks and mulattoes. In such cases, even couples could see their children
forcibly taken away from them if the parents were not married, or did not have a
steady and honest occupation. With the passing of time and the increase in numbers of
the black population of the state, forced apprenticeship was further extended. This
practice continued, and even intensified, during Reconstruction as an attempt by for-
mer masters to regain control over at least a part of their lost slave labor force. The
Freedmen’s Bureau always disapproved of this practice, and became a reference point
for the freedpeople’s claims to have their apprenticed children back. However, as Zipf
rightly points out, while Freedmen’s Bureau agents “idealized free-labor principles”
(p. 73), their racist bias made them believe in the inferiority of African Americans and,
consequently, in their unfitness to enjoy full independence. So, they mostly endeav-
ored to restrict, rather than eradicate apprenticeship. At the beginning of Reconstruc-
tion, court practices had tried to be more flexible and generous toward white women.
However, the pressures exerted by the claims of freedwomen to equal treatment with
whites resulted in the passing of a new state law which gave the courts the power to
apprentice the children of all single or widowed women regardless of race or class.
This policy was inspired by the same fears which led to the approval of laws establish-
ing requisites for voting: although they were designed to curb the rights of African-
Americans, they unavoidably disenfranchised lower-class whites as well.
The changing of times was signaled, from the 1890s, by the emergence of a reformist spirit that placed new emphasis on children’s rights and welfare. Child-labor reformers began to attack the institution of forced apprenticeship, fighting a long battle against industrialists, who wanted to continue to exploit child labor, and staunch white supremacists, who maintained that the innate inferiority of blacks made it indispensable to subject them to some sort of bonded labor for the protection of white society. Southern reformers, however, were more interested in white children than they were in blacks, and embraced the expansion of state paternalism in favor of white children so that the 1919 law ultimately “reinforced southern hierarchies of gender, race, and class” (p. 152).

This interesting study has more than one merit. First of all, it adds further articulation to the history of the white laboring classes of a state, North Carolina, that has perhaps been one of the most widely studied from this perspective during the past two decades. Furthermore, the author’s utilization of legal sources allows her to successfully achieve the always auspicated but not so often achieved interconnection between the categories of gender, race, and class in the writing of southern social and labor history. In this regard, Zipf’s book is a notable example of how the history of the southern laboring classes—either black or white—should be approached.

Although convincing in her arguments and in the documentation brought in its support Zipf, however, does not tell us—apart from a few cursory references to other states—how the North Carolina apprenticeship laws compared with those of the other slave states. We can only surmise that they were pretty much alike, although I suspect that Tennessee, for example, either had milder laws for the apprenticeship of free colored female heads of families or their application was more lax. The case of Andrew Johnson that she makes and reiterates could have been an appropriate opportunity to describe differences, if any, either between the apprenticeship laws of Tennessee and North Carolina or in their application. This fact notwithstanding, Zipf’s book is a valuable piece of original and well researched historiography.

Susanna DelFINO, University of Genoa, Italy


Central banking or, at least, effective central banking relies on the central bank having command over an appropriate set of tools, a clear set of objectives, and the independence to operate in accordance with best practice. Throughout the history of central banking, John Wood informs us, one or more of these conditions were routinely violated.

Wood notes that when the bill to establish the Federal Reserve system was debated in Congress, it became clear that there was little sense of appropriate tools or targets. As debate continued, Oscar Callaway, a Democratic representative from Texas bemoaned that: “We are told to ask no questions; have faith, simple faith . . . faith in man, fallible man, swept by all the passions, prejudices, and ambitions, mental misgivings, shortsightedness, and misconceptions of man” (p. 172). In reviewing the early histories of the Federal Reserve and the Bank of England, Wood skillfully highlights the many instances when misconceptions and shortsightedness ruled.

Two themes emerge over and again throughout the book. First, Wood believes that we will not understand the actions of central bankers or, at least, the actions of central
bankers prior to 1960 or so, without appreciating that they saw the world differently than monetary economists. Second, he explodes the myth that central banking ever was or ever will be politically independent.

Wood notes that observers of the banking system and, especially, central banking discuss the actions of central banks as inherently monetary. Banks, after all, provide money. But bankers themselves do not see the world in this way and do not concern themselves so much with money as with credit. Incoming Federal Reserve Chairman Ben Bernanke’s (“Nonmonetary Effects of the Financial Crisis in Propagation of the Great Depression,” American Economic Review 73:3 (June 1983), 257–76) now classic article highlights how these differences in focus lead to different policy implications, and Bodenhorn’s State Banking in Early America (New York: Oxford University Press, 2003) reinterprets early U.S. banking history from the credit rather than the monetary perspective. The credit approach is enlightening because it captures the essence of banking from the viewpoint of everyday bankers. Wood correctly maintains that this approach also provides insights into the machinations and motivations of central bankers (p. 182).

Wood marches us through the history of central banking at a brisk pace. In chapter 2 he highlights the debates between the Bank of England’s directors and David Ricardo during the 1810 Bullion Committee hearings. Ricardo and the bankers were clearly speaking two different languages. Chapter 3 discusses the debates over resumption in 1821, during which the Ricardo debate reemerged, mostly unresolved. Chapter 4 focuses on the Currency School–Banking School debate. Chapter 5 introduces Bagehot’s rule, but the Bank of England balked at viewing itself as a central bank. It remained firmly committed to stability and convertibility and little else. Chapter 6 discusses the nineteenth-century U.S. experience with central banking, such as it was. Chapter 7 introduces the Federal Reserve, which brings us, in chapter 8, to the Fed’s most spectacular failure: what Friedman and Schwartz (A Monetary History of the United States, 1867–1960, Princeton, NJ: Princeton University Press, 1963) labeled the Great Contraction.

Wood’s focus on motivation leads him to argue that, given the makeup of the Board of Governors, the Fed’s actions were predictable a priori. The Fed acted like a group of bankers; that is, commercial, not central bankers. The board focused on financial rather than money markets, and many members believed that only mass liquidation would quell the speculative frenzy. Recession and unemployment simply did not enter into their calculus. From Wood’s description of the Fed circa 1929, Bagehot may as well have never written anything, much less the prescription for central bankers facing the predicament the Fed then faced.

Later chapters highlight the Fed’s and the Bank of England’s learning, but these chapters emphasize the second of Wood’s themes, namely shattering the myth of central banking independence. As all monetary historians are acutely aware, the monetary authority of any country ultimately reverts to the sovereign, whether that sovereign be a noble or a democratically elected government. The sovereign defines money and can alter its definition almost at will. Anyone who doubts this should refer to the U.S. Currency Act of 1862, the Specie Resumption Act of 1875, the Silver Purchase Act of 1890, the Gold Standard Act of 1900, Roosevelt’s dollar devaluation and demonetization in 1934, and the United States’s 1971 abandonment of Bretton Woods. “The job of central banks,” writes Wood, “is to facilitate [monetary] regimes established by governments” (p. 267).

At a Federal Reserve Bank of New York conference in 1982, J. S. Fforde, an executive director and later historian of the Bank of England, explained that Great Britain
was a parliamentary democracy in which macroeconomic policy was established by the executive branch. It was the duty of the treasury and the central bank to carry out the executive’s policies. Thus, the Bank of England was best viewed as a central banking component of a centralized macroeconomic executive (p. 386). But former central bankers were not the only ones who thought this way. In Congressional hearings in 1964, several academic luminaries, including Paul Samuelson, recommended the subordination of monetary policy to the executive branch (pp. 344–46).

Central bank independence emerged only in the 1980s when electorates around the world, but most notably the in United States and Great Britain, grew increasingly intolerant of inflation. President Carter appointed Paul Volcker as Fed chairman not because he was the same sort of team player Arthur Burns was for Nixon, but because he was Wall Street’s candidate who would bring some credibility to the fight against accelerating inflation. The new contract between the polity and the central bank stipulated that central bankers pursue policies consistent with financial and price stability. Wood argues that this new contract brought us back to the beginning. The sovereign originally entered into a contract with the Bank of England because he needed financial stability to reduce funding costs. The convertibility clause imposed a measure of price stability. The new contract has replaced gold convertibility with convertibility into a broad basket of goods. We shall see how long this particular contract lasts.

Wood has produced a fine volume for those with a passing familiarity with the history of central banking. By its nature the book faces competing demands of breadth and depth. It sacrifices the latter for the former, but usually to good effect. This is fine so far as the target audience is not specialists, who may legitimately inquire into the value added. But for all us nonexperts, the book is a reminder that central banking remains more art than science, and like all art forms it has had more pedestrian practitioners than true trailblazers.

HOWARD BODENHORN, Lafayette College


The aim of Margaret Garb’s social history is to discover how the owner-occupied, single-family house “came to embody the American ideal of the family home” (p. 2). Her contention is that “struggles over property rights in housing fundamentally transformed living conditions and social relations in American cities” (p. 2). Garb does a superb job of reporting how working-class families and social service reformers confronted issues involving property rights and housing.

City of American Dreams begins with the protest over the proposed post–Chicago Fire ordinance prohibiting frame construction; it ends with the race riots of 1919. The former threatened the ability of immigrant wage laborers to afford a home; the latter, “in important ways was a tragic result of the reorganization of residential property relations that began in the final decades of the nineteenth century” (p. 1). That reorganization involved the health concerns aroused by homes that remained unconnected to Chicago’s sanitation system.

Garb’s research poses an important question for economic historians: what should be done about families who cannot afford new technologies, such as indoor plumbing,
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with significant net social benefits? It marshals impressive evidence illustrating the problem, but fails to provide an answer. There well may be no answer.

In 1871 European immigrant workers were anxious about their ability to afford homes built of brick and hopeful of limiting Chicago’s “fire limits.” In the first chapter, Garb discusses how owner-occupiers used their property to produce income and household necessities. She contrasts this to the apartment’s emergence as preferred housing for white-collar workers. A small frame house cost $800–1,000, two to three times the annual income of an unskilled worker. Rent for a middle-class apartment ran between $15 and $25 per month. Under reasonable assumptions, the monthly payment on an $800 mortgage would be in the neighborhood of $15–20. As Garb intimates, the distinction between frame homeowners and apartment dwellers was more than economic.

In her second chapter, Garb tells the stories of several working-class families who make “skillful and strategic” uses of property, a process facilitated by deflation and the emergence of more formal lending agencies (e.g., building and loan associations). Garb refers to August 1879 as a time of “soaring inflation” (p. 50) and asserts that “Low wages enabled capitalists to distribute ever-growing profits to investors . . .” (p. 52). Yet, the stock yield index, like the CPI, was falling. Nonetheless, Chicago real estate prices were rising as in-migration raised demand faster than housing could be supplied.

As time passed, small frame cottages filtered down. Two decades later, they were subdivided into multifamily residences; some were moved to the rear of the property to make room for tenements. Both cottages and tenements generally lacked indoor plumbing; Chicago generally did not supply sanitary services to these areas.

Health concerns enter in the third chapter as tenements come to house a growing proportion of the working class. Chicago’s first housing ordinance was passed because the lack of indoor plumbing created a fertile environment for infectious disease. Garb shows the link between health and housing was taken one step further: “An analysis that linked disease to race or national origin of tenants permitted propertied Chica
goons to avoid investing capital in improving tenement dwellings and provided them with (pseudo)scientific justification for regulating the immigrant poor” (p. 63). The balance of the chapter looks at the work of sanitarians, particularly the head of Chi
cago’s Health Department, who understood the analysis above involved one link too many.

Chapter 4 discusses indoor plumbing technology, but one of the most important questions is not addressed in any detail: how much? Indoor plumbing made a house healthier, but also more expensive, both privately and publicly. A Chicago ordinance that required indoor plumbing in all new construction “was hardly enforced along the streets lined with multifamily frame dwellings” (p. 106). For a homeowner, adding mains and sewer pipes to an existing structure involved major construction. For the city, the installation of sanitation works was a major capital cost. It was one of the ma-
jor expenditures of American cities during Garb’s study period. It put most cities up against the 5 percent of assessed valuation constitutional borrowing limit, which Garb fails to mention. Cities had to choose where to construct new lines.

In Chicago, such decisions were complicated by the fact that annexation elections (1889) increased the city by 125 square miles. Garb mentions this annexation only in passing, but it was clear that most of the suburban areas seeking to join Chicago did so to gain access to the city’s sanitation system. The water systems of villages such as Hyde Park had to be integrated into Chicago’s. The expansion of the sewerage system
was so expensive that it required the creation of a supragovernment, the Sanitary District of Chicago, with its own borrowing limit.

Did Chicago fail to supply sanitation services to poor neighborhoods because it felt there was no demand, or was there no demand because Chicago offered no supply? Chicago had little reason to believe they were demanded. Where facilities were supplied, many residents chose not to pay the connection fee; many of those who did pay had no indoor plumbing. Garb suggests that clean water and efficient waste disposal were “privatized” (p. 116), but water mains and sewers are still supplied through city government. There was excess demand. Those forced to go without complained about how the limited supply was rationed. The potential of alternate arrangements to supply more Chicagoleans at an acceptable cost remains unexplored.

Chapter 5 discusses contractors such as S. E. Gross who supplied inexpensive homes with indoor plumbing. Garb notes the inclusion of indoor plumbing increased Gross’s costs by $500. It is not clear to what that number should be compared. A few years earlier, the asking price for a Gross home without indoor plumbing was $950–1,300. Thus, it appears that indoor plumbing could have increased costs by 50 percent or more. Incomes did not rise that quickly, so how was the cost increase met? Garb notes that “women’s remunerative labor was a critical component in the realization of home ownership” (p. 140).

The sixth chapter focuses on the University of Chicago Settlement House, an endeavor in which university researchers and others sought to reform the family home and improve neighborhood life. Indoor plumbing was one such improvement. Garb terms the increasing cost of home ownership an “unintended consequence.” The final chapter examines the housing market for blacks circa 1919. Garb does not explore the paradox between the belief that the presence of African-Americans depressed real estate values and the fact that rents were higher inside the “color line” because of housing scarcity. The fact that many of these neighborhoods still lacked sanitation services means to Garb that Chicago remained “unwilling to invest city funds in working-class neighborhoods” (p. 201).

The book concludes with a brief epilogue that effectively summarizes the social, but not the economic, issues. This is a fascinating book, but economic historians will find it raises as many questions as it answers.

LOUIS CAIN, Loyola University Chicago, Northwestern University, and the University of Chicago

GENERAL AND MISCELLANEOUS


Most economic historians would agree that nation size plays an important role in economic growth, but they typically treat it as an exogenous variable. The Size of Nations suggests that this assumption may not be as innocuous as one might think. Alberto Alesina and Enrico Spolaore try to explain where national political boundaries are drawn. Their premise is that larger size leads to economies of scale in the provision of public goods and policies, but also exacerbates heterogeneity in the preferences for public goods and policies. An example is national defense: it is less costly to pay for an army when the costs are spread over a larger number of people; however, there will be more people who prefer their tax dollars to be spent on something else.
One of the main achievements of the book is to show how the costs and benefits of nation size influence the formation of political boundaries. For example, the authors use economic theory to explain what type of boundaries could emerge under various political regimes. In chapter 3 they consider a model where individuals (or regions) choose whether to secede from a political unit. They show that individuals with far different preferences from those of the center will want to secede because the benefits from economies of scale are outweighed by the loss in utility from consuming too much or too little of the public good. An implication is that voting may not yield the “optimal” nation size, because partition raises the cost of providing the public good. The authors extend their theory to consider how transfers from the center can keep individuals from seceding. They also consider the case where a dictator has to ensure that some fraction of subjects do not prefer to secede. They show that as this necessary fraction increases, dictators limit the extent of their territory, and thus nations will be smaller.

Alesina and Spolaore use their theoretical framework to develop two propositions. The first argues there will be more small countries when there is higher world trade. The idea is that the benefit of a large internal market diminishes when trade barriers are low; moreover, small countries have a greater incentive to promote trade by lowering tariffs. Therefore, free trade and small nations reinforce one another. The second proposition is that there will be more small countries when there is greater peace in the world. Their notion is that enhanced security is an important benefit of size only when there are threats of outside invasion. Thus, as the world becomes more peaceful, regions with different preferences over public goods will find it less costly to secede.

How does Alesina and Spolaore’s theory fit with the history of nations, trade, and conflict? The authors address this question in chapter 11, where they compare the predictions of their model with the historical record, beginning with the rise of European city states in the twelfth century and ending with the breakup of the Soviet bloc around 1990. They make a convincing case that their theory is broadly consistent with history. For example, they draw on the work of Douglass North and Robert Thomas (The Rise of the Western World: A New Economic History. Cambridge: Cambridge University Press, 1973) and point out that city states had difficulties surviving between 1500 and 1750 because warfare was more frequent and defense costs rose substantially (pp. 178–83). As another example, they build on William Riker (Federalism, New York: Little Brown, 1964) and interpret the German Unification of 1871 as a response to external threats and rising international tariffs (p. 185).

Alesina and Spolaore admit that their historical analysis only scratches the surface, and stress that “historians could do more and better” (p. 223). In my view, there are a number of areas where historians could add to the discussion. For example, Alesina and Spolaore argue that the Dutch Republic managed to survive in the early modern period by taking advantage of trade opportunities in the Indies and America (p. 181). Was this the case, or did internal factors within the Dutch Republic matter more? They also argue that colonialism represented a “brilliant” solution for Europe, because it provided military and economic benefits while the denial of representation to the colonies reduced the costs of heterogeneity (p. 191). How did Europeans address the costs of heterogeneity? Was it simply through force, or were other devices employed?

Alesina and Spolaore also provide evidence for a positive correlation between the number of countries and the share of trade in world GDP. Perhaps Cliometricians can go further by establishing the causal relationship between nation size and trade. Lastly, the authors acknowledge their theory is silent on dynamics, or transitions in nation
size (p. 222). Historians could add much to this discussion by analyzing the collapse of empires and the unification of countries.

_The Size of Nations_ should be accessible to a broad audience. It is well written and it limits technical discussion to appendices and labeled sections. The theoretical relationships identified here could influence a number of topics in economic history, particularly the relationship between state formation, trade, and conflict. The book will, one hopes, produce a fruitful interaction between economic theory and economic history.

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Professor Alfred Chandler’s work, spanning many decades, has had a simple yet powerful message. In technology-intensive industries, successful firms (and by extension, countries) eschew unrelated diversification and reinvest earnings into new plants, distribution and marketing channels, and research to exploit economies of scale and scope. Although critics may carp that the basis for such prescriptions is thin, that defining success as large size and longevity makes the argument chase its own tail, it is the chemical industry, on which Professor Chandler leaned heavily in his argument, that created a more powerful challenge. After the oil shocks of the 1970s, the chemical industry entered a period of long and painful restructuring; business portfolios were shuffled and many well-established incumbents exited. Though some of this merely undid the excessive diversification of earlier decades, questioned remained. Whither the economies of scale and scope? What of the formidable advantage that established incumbents and their well-oiled corporate planning machinery?

This book is an attempt to make sense of it. It contains the story of two industries, chemicals and pharmaceuticals, once closely related and now split asunder. The story is told through thumbnail sketches of a diverse set of firms from each industry. For the chemical industry, we have the “core” firms, which consist of the six leading American chemical firms at the end of World War II that produced a diverse range of chemical products, and ten focused firms, including Lubrizol, PPG, Rohm and Haas, and Air Products. In addition, Chandler discusses the European entrants: Bayer, BASF and Hoechst, the major Swiss firms, as well as ICI, Akzo and Rhone-Poulenc. Finally, we have 24 other firms (“the challengers”): oil companies with chemical aspirations, three firms producing chemicals initially mainly for their own use, five conglomerates, and four spin-offs, byproducts of the restructuring.

For pharmaceuticals, Chandler distinguishes between firms that started as producers of advertising intensive, over-the-counter (OTC) medicines, candy and chewing gums, and those that began with more research-intensive, prescription medicines. As in chemicals, World War II marked a watershed, with the antibiotics heralding the “therapeutic revolution,” which is still continuing as evidenced in the advances in genetics and molecular biology. This increased the pressure on the firms to develop their own patent-protected drugs. In turn, this implied developing or acquiring large in-house R&D capabilities, where many firms stumbled, especially many of the OTC firms. Following this, there is a chapter on European and American challengers (including Johnson & Johnson, Proctor & Gamble, as well as the big German and Swiss
chemical firms), and another chapter on the leading American biotech firms such as Genentech, Amgen and Biogen.

These sketches of firms are preceded by an introduction that introduces the three key themes, namely the importance of being an early mover, of developing barriers to entry through scale, and the need for developing an “integrated learning base,” which seems to consist of learning how to produce and market related products. Time and again Chandler points to the severe consequences to firms that fail to follow the “virtuous strategy,” either by failing to invest adequately in research or by reaching out to unrelated products. He returns to this theme in the conclusion, now fortified by references to the computer and electronics industries (the subject of a companion book, Inventing the Electronic Century: The Epic Story of the Consumer Electronics and Computer Industries, Alfred D. Chandler, New York, The Free Press, 2001).

This is familiar stuff, as is the conclusion that early entrants are hard to challenge after they have successfully established themselves. What is new is discussion of what happens when the science base of a high-technology industry becomes less fertile, as happened when polymer chemistry could no longer produce new breakthrough materials. In order to survive, firms must change, giving up the search for fundamental research in favor of incremental advances and focusing more narrowly. This is exactly what the stock markets (and their shock troops, the despised corporate raiders) wanted chemical firms to do, and what the CEOs of chemical firms resisted. The integrated learning base becomes less useful and may even hinder the firms in moving from innovation to merely efficient large-scale production, but the book seems to ignore this issue, seeking shelter in discussion of “defining the strategic boundaries of the firm.”

For his admirers, including myself, this book will disappoint when measured against its predecessors. For one, it is far too lean. Over 40 chemical firms and some 30-odd firms in pharmaceuticals are covered in a mere 240 pages, an average of three pages per firm. Fifty or more years of history are reduced to a litany: acquisitions of other firms, and (frequently) subsequent divestitures, followed by a reiteration of the virtues of integrated learning bases and the perils of unrelated diversification.

What is missing is rich detail to make the argument. For instance, the book notes that Warner-Lambert (an OTC firm) acquired Parke-Davis, an innovative firm, but ended up destroying Parke-Davis’s innovative abilities, apparently due to poor management. But what form did this poor management take? Did they promote incompetent research managers? Did they underinvest in R&D? Did they underinvest in marketing? Did talented scientists leave because they chafed under managerial practice ill-suited for research? Were the compensation contracts unsuitable? Or, was it simply bad luck? In other words, even if one agrees that candy and antitoxins do not share an integrated learning base, what sorts of problems arise if a firm tries to encompass both?

Similar questions arise through out. To take another example, Allied Chemicals, the second largest American chemical company in the 1930s, was quickly overtaken by the likes of Dow and Union Carbide because it failed to develop an integrated learning base. As the book tells it, Eugene Webber, the reclusive CEO, set strict financial targets for the various divisions, which otherwise had great autonomy. By design, there was little communication among the divisions. But why was this structure fatal? Did managers in one division spot opportunities that other divisions could have profitably exploited? Or was it simply a failure to invest in research at a time when polymer chemistry and chemical engineering offered rich opportunities? In other words, even if one accepts that Allied represents a “failed strategy and structure,” one looks in vain for how and why the failure took place.
The book is a useful introduction to the firms, including some that have disappeared, that made up the American chemical and pharmaceutical industry. From a book by Professor Chandler, one expected so much more.

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Paolo Baffi, a governor of the Bank of Italy, was originally enlisted to write this history of the Bank for International Settlements (BIS) but died before completing the research and writing. He left a text, however, The Origins of Central Bank Cooperation: The Establishment of the Bank for International Settlements (Bari: Editori Laterza, 2002), in which he chronicled the preparatory work leading to the creation of the BIS and the first two years of operation. Professor Gianni Toniolo took over the job in 1999 with the assistance of Piet Clement (Head of Archives and Research Support at the BIS) and has produced a careful and balanced account of the institution from its beginning to the end of the Bretton Woods monetary system. In comparing the two book titles it is worth noting that Baffi identifies central bank cooperation with the BIS and thus downplays a long series of ad hoc cooperative arrangements in the nineteenth and twentieth centuries (e.g., 1825, 1836, 1839, 1890, 1906, 1907, 1909–1911 and the 1920s), whereas Toniolo tells the reader that the BIS did not invent central bank cooperation but institutionalized it.

This history is no easy Sunday afternoon reading. The 488 pages of narrative, structured into 12 chapters and an epilogue, are enriched by 2,189 endnotes and five long appendices ranging from BIS statutes and balance sheets to biographical sketches of eminent people involved with the institution. The book starts with the issue of cooperation, which for the BIS served the objectives of stabilizing exchange rates and smoothing business cycles (chapter 1). Cooperation was to be effected by “frequent meetings, visits, incessant exchange of information, common consultation and joint discussion . . .” (p. 3). The roots of the BIS, however, are to be found in the transfer of German reparations to the European Allies after World War I (chapter 2). The problem was complex and politically charged, with the Germans wanting to link the size of the obligations to capacity to pay, the French to securitize German payments and ultimately to thwart German industrial development, the British to secure enough payments to settle their debt with the United States, and the United States to delink reparations from war debts. Thus, the BIS was born as much a reparations bank as a locus of central bank cooperation.

The new institution was not lucky with the timing of its launch, as much of the industrialized world was sliding into economic depression. German war reparations certainly did not help. In 1931 the Credit-Anstalt crisis propagated to other parts of Europe. The BIS acted as a crisis manager and lent to the central banks of Austria, Hungary, Germany, and Yugoslavia, but it was too little to do any good (chapter 4). Central bankers, including those who met in Basel, failed to understand that feasible cooperation would not have been enough to sustain the combination of a gold standard and high resource utilization (chapter 5). It was an error in judgment for which central banks paid in reputation and loss of power after World War II. Toniolo is ambivalent on this point. On the one hand, he claims that central bankers
could not do more without the full support of their governments. On the other hand, he concedes that they “. . . were not entirely able, intellectually and professionally, to comprehend the need for and the implications of wholesale policy coordination” (p. 109). Then, in very short succession, the two main pillars upon which the BIS edifice was built—the gold standard and war reparations—collapsed.

The blow was huge but not fatal. Members of the club continued to see one another in Basel, once a month, and discussed in private what they could not voice in public (chapter 6). In fact, the BIS barely survived World War II. The institution was accused, especially by the Americans, of having sided with Germany during the conflict. The specifics involved claims that the BIS had carried out transactions in gold looted by the Nazis and transferred to Germany gold belonging to Czechoslovakia (chapter 7). The inclination at the Bretton Woods conference was to do away with the BIS. Leading the charge was the U.S. Treasury, which, beside the already noted hostility to the institution, saw no role for the BIS in the architecture of the new international institutions. After all, what could the BIS do that the IMF could not also do? In the end, the BIS was saved by the persistence of the European central bankers and by a special charter that gave the institution a great deal of resilience (chapter 8).

After the Second World War, the BIS was out of step with the prevailing economic paradigm and policy prescriptions. The economics of the BIS emphasized budget discipline, sound money, free trade, and international monetary cooperation; there was little sympathy for mechanical application of the standard Keynesian model. To make matters worse, central bankers, the BIS clientele, had lost much of their luster and prestige because of their unrelenting support of the gold standard in the interwar period. Power shifted to ministers of the Treasury, who made central banks subordinate to government (chapter 9). Despite the unfavorable winds, the BIS acquitted itself well with the European Payments Union that led to currency convertibility in 1958. To be sure, the underpinnings of the union were not in line with BIS thinking that would have prescribed an earlier date for convertibility, freer trade, and a less accommodative monetary policy. But Toniolo concludes that “an early rush to convertibility would have met with insurmountable political obstacles” (p. 345).

International monetary cooperation reached the high point in the 1960s, during the heyday of the Bretton Woods system (chapter 11). The BIS expanded and refined its tool kit of financial engineering in support of the U.S. dollar but could not undo the fundamental flaw of the gold-dollar standard first noted by Robert Triffin.

On paper, the end of Bretton Woods should have made the BIS, along with the International Monetary Fund, redundant. Instead, it was rejuvenated by a different agenda: European monetary integration—which is now complete—and financial regulation—which is evolving (chapter 12).

To sum up, is the world a better place because of the BIS? The Epilogue, an unnumbered chapter, was written with that question in mind. Toniolo tries to be impartial but it is clear that his feelings are for the institution. The fact that members of the club keep coming back to Basel month after month speaks for its value. These folks are voting with their feet. By all accounts, they cherish these secretive meetings with neither agenda nor minutes. They exchange information and points of view, but most of all they feel part of a small and homogeneous group that can make important decisions quickly without the tiring delays and uncertainty of open fora. In the original design the Basel club was set up to distance itself from politics. But politics regained the upper hand after World War II and many central banks
became mere agencies of the Treasuries. Now the pendulum has swung back to central bank independence, vindicating the early BIS design. The pendulum has also swung from the Keynesian orthodox paradigm to issues of policy and institutional credibility. By sitting relatively still through the years the BIS has become modern again.

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