

Book Reviews

ANCIENT AND MEDIEVAL

Revolution and Consumption in Late Medieval England. Edited by Michael Hicks. Woodbridge, UK: The Boydell Press, 2001. Pp. x, 198. \$75.00.

This collection of studies is the second volume in the Boydell Press's series *The Fifteenth Century*, which has superseded the older series of fifteenth-century studies published by Alan Sutton. The 11 papers it contains were originally written for the ninth "junior" conference on fifteenth-century history at Southampton, England, in September 1999, though not all the contributors are "juniors." The title expresses both the wide range of topics included in the volume and also the two principal focuses of attention.

On the revolution side we have Alistair Dunn's study of the management of confiscated estates around the time of the Lancastrian Revolution (1397–1408), Shelagh Mitchell's study of Richard II's employment of household knights, Alison Gundy's examination of the relationship between Richard II and the earl of Warwick, and T. B. Pugh's discussion of Richard of York's problems and aspirations. Jessica Freeman's paper uses evidence from Middlesex to defend the idea of county community as a workable concept. These papers are often relevant to aspects of economic history insofar as they relate to aristocratic incomes and the politico-economic foundations of power relationships. Most directly pertinent to debate, in this latter context, is Pugh's analysis of the revenues of Richard of York, which effectively undermines McFarlane's claim that financial problems were irrelevant to his political actions.

The papers that fall into the consumption category are predictably more directly related to problems in economic and social history. Christopher Woolgar comments on various characteristics of daily diet in magnate households from two sets of fifteenth-century accounts; he confirms that, as in monastic households, the wellborn laity were overfed, and also comments interestingly on the extent to which visual display trumped considerations of flavor and texture in the preparation of many dishes. John Hare analyzes patterns of regional variation in the prosperity of fifteenth-century Wiltshire to show how the changing local fortunes of the textile industry had sufficient impact to modify Postan's simplified account of that century. John Lee shows, partly from college records, how the marketing of agrarian produce in the Cambridge region responded to shifts in demand during the fifteenth century, notably because of rising demand from the university and the growing influence of the London market. Miranda Threlfall-Holmes uses the voluminous Durham obedientiary accounts between 1464 and 1520 to contrast the demand schedules of the monks for different imported products—wine, spices, dried fruit and sugar—and comments on the overwhelming ascendancy of Newcastle merchants in supplying the priory with these commodities. Winifred Harwood examines the impact of the expenditure of St. Swithun's Priory, Winchester, on the local economy, estimating that in its later years about 14 percent of the priory's income went in local wages and a further 40–54 percent in purchases made locally.

Peter Fleming's paper, like Jessica Freeman's, falls outside the categories of revolution and consumption, but relates to other issues in late-medieval social history. He examines the rhetoric of power employed by urban elites, especially from Bristol evidence, but goes on to demonstrate how its premises were often falsified in other areas of urban culture, such as mystery plays; the elite view was not necessarily shared by all townsmen. These papers contain much new evidence relating to current issues in political, social, and economic history, and it is particularly pleasing to see such impressive work from younger scholars. It is papers such as these, demonstrating from first-hand research what market forces and

cultural values were really operating in the late Middle Ages, and how they affected political and social aspirations, that effectively test the older generalizations, enliven discussion, and allow us to remake the history of late-medieval England.

RICHARD BRITNELL, *University of Durham*

Origins of the European Economy: Communications and Commerce, AD 300–900. By Michael McCormick. Cambridge: Cambridge University Press, 2001. Pp. xxviii, 1101. \$60.00.

This impressive book sets out to examine two inter-related issues—the Mediterranean economy between c.300–900 and the celebrated Pirenne thesis—using a battery of archaeological and textual analyses. In sum, Michael McCormick successfully establishes a new benchmark for any understanding of the Mediterranean in late antiquity and the early Middle Ages. As for developing the Pirenne thesis, like many other recent studies, McCormick provides scale and chronology to the Belgian historian’s sketchy ideas published in the 1920s, without perhaps invoking a paradigmatic shift in thinking about trade and commerce in this era.

McCormick’s view of Europe from the Mediterranean is best illustrated by Map 20.4 “The Northern Arc, from Space.” Baghdad lies in the top left hand corner; Rome is in the right-hand corner; York is in the lower right-hand side, while Birka and Staraja Ladoga anchor the bottom center. It is a view charged by McCormick’s reading of his catalogue of 828 travelers’ movements, collected in appendix 4 and given additional substance by an invaluable catalogue (appendix 3) of Arab and Byzantine coin finds in Carolingian Europe. The result, not surprisingly, lays emphasis upon a cosmopolitan world in striking contrast to the postwar emphasis upon a Europe composed of regions. Calibrated by quarter-centuries, McCormick presents a canvas that is truly pan-European, but uses individuals as metaphors of the changing geo-politics of the continent.

“Hunting for commerce has proven illusory” (p.15) as far as the early medieval commerce in the Mediterranean has been concerned. So he quantifies the travelers’ accounts to illuminate the new archaeological data. The results are little short of extraordinary. Let me offer my choice conclusions (of the many dozens in this book). McCormick demonstrates that there is a clear seasonal change in communications between antiquity and the early Middle Ages. Summarized on chart 3.1, late Antique travelers journeyed between May and September, whereas their later counterparts traveled throughout the year. Why? Travel was primarily by sea before AD 700, and primarily overland afterwards. He shows that the domicile of a great number of early medieval pilgrims and travelers was within a 100 km radius of a major emporia (trading center) (p.158). He demonstrates that between 753–776 and 800–825 western travelers aiming at points beyond Constantinople abandoned ancient Byzantine routes and began to travel east through the Islamic world. The wealth of Islamic contacts and, indeed, the present of Islamic coins in Italy and elsewhere in Carolingian Europe is powerfully demonstrated. Chart 12.1 graphically illustrates the percentage of Arab coins in Sardinia, the Viking territories, the Adriatic region, the Iberian peninsula and the Rhône valley. The twin-peaks show a decline from 700–724 in the second quarter of the eighth century, then a sharp rise to c.800, then a fall to c.850–874. Taking this evidence with the data on travelers and the incidence of Byzantine coins, he concludes that “communications between the Frankish empire and the eastern Mediterranean world surged in the final decades of the eighth and the first decades of the ninth century . . . never again in the history of Europe did they come close to the low levels that prevailed before 750” (p. 442).

Many incidental sections provide substance for this grand narrative. Illuminating sections on traveling the Mediterranean, on the relic trade, on Venice, on the objects of trade, and on the slave trade are deployed to substantiate the central thesis. Pirenne is essentially vindicated, if shown to be wrong about the details. Islam played a crucial part in the thrusting adventure of Charlemagne's life, breathing new economic vitality into hitherto moribund places.

Archaeologists will welcome this window on a world of pilgrims and missionaries, and the splendidly illustrated quantification of the data. Historians, one can be sure, will be less complementary. After all, the countryside and its management, the traditional domain of postwar historians of regions, are barely mentioned. Added to this, the book might be read as further praise for Charlemagne's bold economic judgement, something that has grown steadily more unfashionable with historians as the European Union has established a prize in his memory as its greatest accolade. Historians, too, will be skeptical that this cosmopolitan world tells us anything new. Such a conclusion would be mistaken: in its detail McCormick has established a benchmark for what, as he rightly points out, has been a virtual world lost between those studying the East and West, and the North and South. Time will show what a massively useful work this is. Indeed, his book obviously invites a companion that looks at estate histories from Wessex to Palestine and treats them to the same audacious analytical scrutiny. Taken together, what a homage these would be to Henri Pirenne!

RICHARD HODGES, *University of East Anglia, Norwich*

Society and Economy in Germany, 1300–1600. By Tom Scott. Basingstoke and New York: Palgrave, 2002. Pp. xi, 313. \$69.95, cloth; \$21.95, paper.

Between 1300 and 1600 a number of significant developments in European social and economic history relate specifically to Germany. They include the formation of two dynamic urban systems, the Hanseatic League and the towns and cities of South Germany; the fifteenth-century introduction of technological innovations that sharply boosted silver production in Saxony, Bohemia, the Tirol, and elsewhere; the incipient crystallization within the German heartlands of the divide separating western Europe's free peasantry from eastern Europe's unfree; and the exceptionally high incidence of urban and rural revolts, culminating in the German Peasants' War, pre-industrial Europe's largest popular uprising. With empirically rich and analytically engaging discussions of these and other important topics, Tom Scott's magnificent survey highlights how recent scholarship has challenged and complicated older interpretations of the changes in Germany's society and economy during the Late Middle Ages and the Reformation. His volume represents a masterful account of the new concerns and directions that are likely to influence future research.

A noteworthy feature of the survey is Scott's meticulous attention to the regional, geographical, and even topographical dimensions of economic activity. The volume opens with a brief exposition of Germany's political geography. Later, Scott maps the different agricultural systems, inheritance practices, and zones of rural industry, mining, and metalworking within central Europe. He then considers commercial networks, the different functions (radial, axial, and portal centrality) of towns and cities within an urban system, and the process by which many of them gradually transformed their rural hinterlands into large, dependent territories. But the consolidation of such a territory, he emphasizes, did not assure a city's complete economic domination of the hinterland, for urban artisans, particularly in southwest

Germany and northern Switzerland, faced growing competition from rural craftsmen and markets. In addressing these issues plus neglected topics such as the formation of rural guilds after 1500 and urban policies of granting rural citizenship to nobles and peasants, Scott shows that the increasing commercialization of economic activity deepened and reconfigured the relations between town and country, particularly in South Germany.

His admittedly circumspect argument that South Germany's urban economy did not contract after 1550 will provoke debate, however. Much of the argument focuses on Augsburg, where, Scott suggests, a sharp rise in cloth production helped to offset the bankruptcy of several merchant houses. But the rise is actually an ambiguous measure of Augsburg's economic vitality, for severe overproduction plagued the cloth industry after 1550 and drove the city's weavers into grinding poverty. Scott also notes briefly that Augsburg may have reached its economic zenith only in the second half of the seventeenth century. That the population had imploded by then and, before 1800, never came close to regaining its earlier size, however, reinforces the impression that Augsburg's late-sixteenth-century economy lumbered because of structural imbalances.

A feisty and critical reassessment of how the late medieval agrarian crisis affected the nobility and lordship is another outstanding feature of Scott's survey. For western Germany, he doubts that seigniorial incomes eroded to the extent claimed by earlier scholars, and, drawing on Hillyar Zmora's recent work, he argues that many rich and powerful territorial nobles prospered by lending money to their princes and receiving, in return, lucrative administrative offices in the territorial bureaucracy. While commenting favorably on much of Peter Blickle's account of the reimposition of serfdom by ecclesiastical lordships in the Upper Rhine and Swabia after 1350, Scott also cautions that serfdom's recrudescence in parts of Bavaria, Westphalia, the Rhineland Palatinate, and elsewhere probably requires a different explanation. Regarding eastern Germany's second serfdom, he lucidly outlines how nobles, in the fifteenth century, acquired large seigneuries (*Gutsherrschaften*), which, in the sixteenth century, they exploited through a vastly expanded system of commercialized domanial agriculture (*Gutswirtschaft*) based on increased peasant labor services—a system that underpinned grain exports to western Europe. His sketch of the general patterns and the important regional exceptions dispels some lingering misconceptions about the evolution of east Elbian social and economic relations.

Regarding the many revolts and rebellions that roiled Germany between 1300 and 1600, Scott's most trenchant remarks pertain to Blickle's enormously influential work on the Peasants' War of 1524–1526. Blickle argued that the turmoil possessed a revolutionary character because the insurgents justified their demands by invoking godly law—ethical principles from Scripture that should govern social and economic relations. Lacking all local specificity, godly law could unite aggrieved peasants and discontented townspeople from widely different lordships and territories in a mass insurrection to overthrow the feudal order. Scott, however, rightly questions the strength of the peasant–burgher alliance and, in reviewing northern Switzerland's persistent rural unrest, notes that peasants could participate in large territorial rebellions without invoking religious justifications.

Given Scott's astute exposition of many complex social and economic issues, the short closing chapter, concerned primarily with deviancy and conformity, may disappoint some readers. Although the topics of witchcraft and the Jews receive sufficient treatment, he hardly addresses the recent scholarship on crime, the reform of marriage, and the imposition of new standards of sexual discipline during the fifteenth and sixteenth centuries. Nevertheless, Scott has written an extremely refreshing synthesis that scholars and serious students will greatly appreciate.

TERENCE MCINTOSH, *The University of North Carolina at Chapel Hill*

MODERN EUROPE

Die Baumwollgewerbe Nordwestdeutschlands und der westlichen Nachbarländer beim Übergang von der vorindustriellen zur frühindustriellen Zeit 1750–1815; Verflechtung und regionale Differenzierung. By Gerhard Adelman. (In the series: *Beiträge zur Unternehmensgeschichte; Bd. 11.*) Stuttgart: Franz Steiner Verlag, 2001. Pp. 184. \$34.00.

This broad survey of the Early Modern calico industry and its first steps to industrialization in North Western Germany and its Western neighboring regions gives a detailed portrait of the different branches of calico production, such as spinning, weaving, bleaching, and printing. The author has treated the development of these manufacturing techniques and the corresponding organizational conversions and economic changes separately for each of the numerous political and economic entities in this part of Europe, such as Flanders, Brabant, Twente, Berg, Minden-Ravensberg, and so on. The study covers the areas of the present German federal state of Nordrhein-Westfalen, the Northern Netherlands, and the Austrian Netherlands (today Belgium). It intends to analyze the involvements (*Verflechtung*) and differentiations (*Differenzierung*) between the many smaller regions contained in these areas. In this context, “involvement” comprises personal and corporative entanglements, transfer of technologies, and exchange of goods between politically separated regions. “Differentiation” refers to separation or disintegration, such as that caused by geographical or political borders (pp. 28–29, 38).

The author distinguishes three stages of industrial development: outwork craftsmanship (*handwerkliche Heimarbeit*, characterized by decentralized manual work, *Verlagssystem* or *Kaufsystem*, no mechanization), manufacturing enterprises (more than ten persons concentrated at a single production site, low mechanization, [pp. 48–54]), and factory production (by concentration of production, mechanization, use of horses or steam power, [pp. 56–57]). He clearly illustrates how fustian and calico production originally developed in some of the traditional linen regions, and then how this became an independent textile industry. He also explains the significance of major innovations—in warp yarn and weft yarn spinning, among others, which each caused characteristic changes in the organization of production and employment, and demanded further innovations, such as the flying shuttle. On the path to full industrialization, manifold forms of coexistence or combination of elements from all three stages of development can be found (pp. 154–55). The book serves as a most useful introduction to the interdependencies of technical and economic progress. As hardly any regional studies existed in this field of investigation, the author had to search for information on numerous small businesses, many of which are described in detail. In this way he has also contributed to an important chapter of entrepreneurial history.

The panorama offered here shows us that in the late eighteenth and in the nineteenth centuries, the most technologically advanced calico regions in this part of the continent were East Flanders with the industrial center of Ghent, and though on a more modest scale, the early industrial conglomeration of Wuppertal and Barmen, in the German duchy of Berg (pp. 141–42, 153–54, 164). The time-lag in the industrialization of English calico production was about ten years (p. 72). As the author concludes, even in the advanced regions of the Netherlands and the Rhineland, full industrialization took place only after 1830. To cover these decisive decades of industrialization, he refers briefly to his own work *Vom Gewerbe zur Industrie im kontinentalen Nordwesteuropa. Gesammelte Aufsätze zur regionalen Wirtschafts- und Sozialgeschichte.* Stuttgart: Steiner Verlag, 1986.

The analysis of the respective calico economies according to the concepts of *Verflechtung* and *Differenzierung* is not convincing. The author vaguely concludes that in Westphalia, there existed a close involvement with Holland in the exchange of goods and per-

sons (caused in part by religious persecution on both sides), whereas in Belgium differentiation was more dominant (pp. 38, 47). Further, he concludes that in general, political measures such as taxation, subvention, incentives, or customs duties had little effect on the initiatives of entrepreneurs (p. 31). Smuggling, for example, was widespread and even provoked by the elevation of duties. However, the analysis has not been carried on throughout the book, and no general results on this aspect are offered.

The book itself has its own history. It was submitted as a thesis for *Habilitation* in 1970, but for serious reasons the author was hindered to prepare it for publication. Therefore, the discussion on proto-industrialization, which took off in the 1980s, and abundant literature on this approach, has not been taken into consideration. The most recent title in the bibliography dates from 1974. It is regrettable that the author or the editor did not provide this publication with a brief bibliographical survey. Such contextualization with more recent literature (e.g. *Rheinland-Westfalen im Industriezeitalter. Bd.1: Von der Entstehung der Provinzen bis zur Reichsgründung*. Edited by Kurt Düwell and Wolfgang Köllmann, Wuppertal: Hammer, 1983, or *Von der Heimarbeit in die Fabrik: Industrialisierung und Arbeiterschaft in Leinen- und Baumwollregionen West- und Mitteleuropas während des 18. und 19. Jahrhunderts*. Edited by Karl Ditt and Sidney Pollard. Paderborn: Schoeningh, 1992) surely would have augmented the intrinsic value of the book, for it offers a complementary view to the approaches of social history, which explicitly were not those of the author. Furthermore, a map and an index of names would have been very useful, especially with regards to the value of these investigations for entrepreneurial history.

KLAUS WEBER, *Centre for the Study of Human Settlement and Historical Change,*
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Les Grains du Désordre: L'Etat Face aux Violences Frumentaires dans la Première Moitié du XIX Siècle. By Nicolas Bourguinat. Paris: EHESS, 2002. Pp. 9, 542. €42, paper.

Les Grains du Désordre contributes to recent efforts to re-introduce the state and cultural elites into economic and social history. This study of French food rioting in the first half of the nineteenth century puts the state, not the protesters or their targets, at its center. Nicolas Bourguinat examines not only the context for Napoleonic, Restoration, and July Monarchy economic and social policies and their enforcement, but also the ways each government actively produced new post-Revolutionary social identities through information gathering, social classification, administration, and police.

Each regime interpreted differently the law of 21 prairial year 5 (9 June 1797) declaring free trade in grain. Authorities regularly steered between market and command economies and, during subsistence crises when vulnerable consumers rioted, early nineteenth-century governments expected a certain level of protest as a quasi-legitimate “mirror” of their responsibilities (p. 416). All this changed during the 1840s, when the government enforced free trade vigorously. Bourguinat attributes this shift to several factors. Improvements in transportation and technologies for the “grain-bread circuit” promised better provisioning. The July Monarchy increasingly defined French society in terms of class antagonisms, thus making it easier to align itself with property and the market. This delegitimized consumers’ “moral economy” (what Bourguinat calls the “social contract on subsistence”), restricted local intervention in the market, and installed more effective means to punish protesters.

Part 1 of Bourguinat’s book evaluates the changing geography of grain production and market formation, confirming that even by 1850, north–south, center–periphery price differentials remained. Then, it considers contemporaries’ knowledge of the grain trade.

Bourguinat argues that the era's obsession with accumulating economic and social statistics reflected the State's unstable location between the "rules of non-interference in the economy" and its "projects to assist development and to supervise [recovery from] accidents" (p. 53). It also contributed a new sense of economic space and a "consciousness of national unity" (p. 81). Finally, it relates the preponderance of Paris in the provisioning system to the changing geography of protest. Bourguinat confirms that, by 1846/47, state intervention and market integration had diminished tensions in Paris's traditional hinterland. Instead, new zones (especially the Center and West) emerged as hot spots.

Part 2 considers how nineteenth-century subsistence crises evoked from the state and cultural elites a new, national classificatory vocabulary for French society that rested, not upon the corporate idiom of the *ancien regime*, but on a relationship to resources and needs. Intended to avoid *ancien-regime* taxonomies of hierarchy and privilege, these new models ultimately produced a "modern" notion of class antagonisms, which, in turn, informed policy on food justice, the economy, and police.

Part 3 surveys the "repertory" of protest behaviors and symbols, the identities of participants (including gender), the responses of authorities, and their mutations over time. Bourguinat eschews statistical analysis of repertoires and identities, critiquing efforts to classify riot types because potentially they "infer artificial differentiations among subsistence related incidents" when "redistributive food justice formed a whole in the eyes of contemporaries" (p. 356). Instead, he analyzes the ways protesters and government negotiated issues of "justice" and "rights" (p. 311), transforming the "social contract" about subsistence from the *ancien regime* to 1848. He argues that the Revolutionary Maximum, with its *ancien-regime* roots and nineteenth-century resonances, lingered in popular and elite memories as "nostalgia" for the state as arbiter of food justice. However, when the July Monarchy condemned these memories, it unambiguously criminalized food riots, clearing the way for more "modern" forms of political discourse and protest (p. 444). The last years of the July Monarchy marked the last major wave of food riots and the translation of their demands to a formal political level that contributed to revolution in 1848. Bourguinat suggests that this period also marked the final rupture of already fragile relations between bourgeoisie and the lower classes.

Unfortunately, this book's efforts to introduce so many themes raise more questions than it can answer, even in 469 pages. In fact, his 1997 dissertation often offers clearer access to his evidence and arguments than this revision. For example, his analysis of market integration and riot geography would have benefited from the maps included in the thesis, especially because his reluctance to count or categorize riots or participants provides little substantiation for some of his claims. His work is strongest on the nineteenth century, where he has done considerable primary research, and weakest on earlier periods, where he relies on a smaller body of secondary works. Moreover, he has undervalued substantial collections in the *Services historiques de l'armée de terre* and in departmental archives. Despite these weaknesses, Bourguinat's richly documented work is significant for it enhances our understanding of the period and raises important questions.

CYNTHIA A. BOUTON, *Texas A&M University*

Women at the Gates: Gender and Industry in Stalin's Russia. By Wendy Z. Goldman. Cambridge: Cambridge University Press, 2002. Pp. xvi, 294. \$60.00, cloth; \$23.00, paper.

The author of this study accomplishes much. She considers the process by which women joined the Soviet workforce from 1917 until the mid-1930s and demonstrates how critical they were to the Stalinist industrialization. She demonstrates women's use of their new

opportunities and explains how their presence reshaped notions of the proletariat and gender. She begins with the Bolshevik feminists who promoted their cause until the party's women's section was abolished in 1930 and takes the story to the mid-1930s. She shows how the party abandoned its restrictive model of female participation in the labor force and provided for the addition of millions of new women. Most of all she chronicles the struggle of activists and advocates to bring women into the workforce during the first two five-year plans.

The result is an overview of women's changing role in Russian industry. There was a qualitative as well as a quantitative difference between Soviet and pre-revolutionary practice. Vastly more women were employed by the Soviets and they were employed differently, often in occupations previously restricted to men. She shows convincingly that the Stalinist industrialization depended on women. As she explains, "By 1935, 42 percent of all industrial workers would be women" (p. 1). Yet only the pressing need for additional labor led the government to abandon its prejudices. As she puts it, up until October 1930, "Policy was predicated on the need to protect the privileges of industrial workers, fear of 'nonproletarian elements,' low regard for the unskilled, and apathy toward women" (p. 139). Party leaders clung to the old model because they feared that new workers, women included, would undercut their base of support.

The author analyzes for the first time "The Five-Year Plan for Female Labor" and other plans in the files of the Council of Peoples Commissars, Gosplan, and the Commissariat of Labor. Although the objective, she argues, was to develop new, "low waged 'female sectors,'" opportunities for women grew (p. 175). Equally interesting is another chapter based on archival sources, "Gender Relations in Industry." Using the transcripts from the All-Union Meeting for Work Among Women in 1931 and other sources, including factory reports to the Council of Trade Unions, she documents women's struggle to enter the workplace and the opposition from men in authority as well as ordinary workers. This is perhaps the greatest contribution of the book because she recovers the voices of the activists and presumably of some ordinary women as well. No one who reads this account can forget their difficulties in confronting prejudice and harassment.

The shortcomings of this book are as remarkable as its strengths. Good social history is also economic and cultural. With respect to economic history, the author often uses statistics from the era without qualification. Citing contemporary published sources in a section on social services, she writes: "Between 1928 and 1933 the number of dining halls expanded by a factor of 64, to 387,000 facilities serving more than 25.5 million people. The number of children in day-care centers meanwhile increased tenfold, from 104,386 in 1928 to 1,048,309 in 1936" (p. 274). Such statements reinforce her conclusion: "The industrialization of the 1930s presented a model of development that continues to fascinate working people, students, union activists, peasant organizers, and feminists around the world. It laid the groundwork for enormous achievements, including a modern, globally competitive Soviet state; the victory over a powerful, highly mechanized Fascist army; higher living standards; urbanization; and democratic access to health care, literacy, and education" (p. 284).

The author's first book, *Women, the State and Revolution: Soviet Family Policy and Social Life, 1917-1936*, won the Berkshire Prize in 1993. It is a shame that she did not bring that critical reading to this story. Of her protagonists in the book under review she writes, "The voices here are strong and clear, the sources rich" (p. 209). That is true. Yet many working women were silenced by terror and repression in an era of draconian labor discipline and mass murder. Their voices, including those of workers sent to the camps for petty infractions, cannot be heard in this study, and they should be because this too was the experience of working women.

JEFFREY BROOKS, *The Johns Hopkins University*

Les hauts revenus en France au XXe siècle: Inégalités et redistributions 1901–1998. By Thomas Piketty. Paris: Bernard Grasset, 2001. Pp. 807. €30, paper.

Thomas Piketty has assembled and collated French tax data going back to the beginning of the century and has used them to describe the changing fortunes of the wealthiest 10 percent of the households in France. The results are illuminating and impressive.

Professor Piketty sets out to answer three questions: How have the realities and perceptions of wealth evolved during the twentieth century? Has inequality increased or decreased? How much is this change “natural” in a market economy? He aims this last question squarely at the work of Simon Kuznets and the famous “Kuznets curve.” Piketty was not able to examine the entire range of incomes because in many years before 1950 most households fell below the threshold for income tax payment. However, he believes that studying the highest 10 percent of incomes and the divisions in that group (P90–P95, P95–P99, P99–P99.5, P99.5–P99.9, P99.9–P99.99, and P99.99–P100) will reveal important truths.

For the lowest 99 percent of society, salary is by far the biggest contributor to income, but for the top percentile, capital revenues assume a more important function. Clearly the Depression and especially World War II dealt a huge blow to these capital fortunes as the average income of the “200 Families” (P99.99–P100) plunged 80 percent. Over the course of the century, the higher the family’s income, the less its real purchasing power has grown; that of the “200 families” has actually contracted a bit.

Although Piketty agrees with Kuznets that inequality fell during most of the twentieth century, he does not believe that it is a natural phenomenon of the industrial economy. It was due to the impacts of post–World War I inflation, the Depression, the devastation of World War II, and government tax policy. In the period after World War I, real estate prices (from which the most rich derived much of their income) did not keep pace with the general inflation. The progressive income tax and inheritance tax hampered the compounding of fortunes that creates gross inequality. He confirms this collapse of the great fortunes among the 200 families by using inheritance tax records to show that in real terms the average inheritance in 1994 was one-third that of 1902. Piketty therefore does not believe that growing equality is inevitable and ongoing. The last 20 years have seen a growth in inequality despite the efforts of socialist governments. The only postwar period that saw inequality narrow was the inflationary 1968–1983 era, when there was a rapid increase in the minimum wage.

Besides the decline and nonreconstitution of the great fortunes, the other great trend over the century has been the stability of salary hierarchies in France. The top 10 percent of salaries are 2.5–2.6 times greater than the average salary, the top 5 percent of salaries are 3.4–3.6 times the average, and the top 1 percent of salaries are 6–7 times greater than the average.

Piketty then puts the French experience into the context of other nations. Before 1914 French inequality was not very different from that of Britain or the German state of Saxony. American inequality was markedly lower. The wealthiest classes of the United States also saw a sharp fall in income in the 1930s and the World War II period, which suggests that the actual destruction of the war does not explain the fall in wealthy incomes. For Germany, France, the United States, and Great Britain, the share of income held by the top 1 percent seems to have been cut in half from 1914 to 1950. The shares remained steady until after 1980, when the United States and Britain began to cut taxes for the wealthy. In the United States, the share of income held by the top 1 percent now exceeds its 1914 level, an apparent refutation of the Kuznets curve. Piketty concludes that perhaps the shift to the service economy has seen a rapid accumulation of fortunes just as the turn from an agricultural to an industrial economy caused a rapid accumulation at first.

The main text is followed by 220 pages of appendices detailing the tax, price, salary, and demographic information used by Piketty. These appendices alone are worth the price of the book.

The data also reveal some fascinating details about modern France: population aged 60 and above grew 140 percent during the twentieth century, the number of households grew faster than the population, and women's employment followed a U-curve. Clearly, many more books can be written by those who have Piketty's patience in sifting through the French tax records. This invaluable and unique book will be of use to both general scholars of economic history and also specialists in French history.

C. EDMUND CLINGAN, *University of North Dakota*

British Imperialism, 1688–2000. Second Edition. By P. J. Cain and A. G. Hopkins. London and New York: Longman, 2001. Pp. xxiv, 739. \$24.95, paper.

P. J. Cain and A. G. Hopkins's massive volume on British imperialism is a unique and significant scholarly enterprise. This is the second edition of a work which, when first published in 1993, came in two volumes. In a field dominated by case studies of specific countries, epochs, and incidents, Cain and Hopkins have written a book which encompasses the full sweep of British overseas imperialism from its rise in the late seventeenth century to the end of the empire in the late twentieth century, covering every significant venture and most of the rest as well, all in 739 pages of text, tables, and maps.

The terrain of the volume is divided into four periods: 1688 to the late eighteenth century, the late eighteenth to 1850, 1850 to 1914, and 1914 to 2000. The most significant break is at the mid-nineteenth century; up to this point, commerce and finance have had to share imperialism's dynamics with landed capital; thereafter commerce and finance are the dominant force. For each of the four sub-periods, Cain and Hopkins describe and analyze the evolution of Britain's domestic and international economy, often over several chapters, followed by a half dozen or more chapters detailing the experience of the British presence in India, Canada, Australia, Africa, the Ottoman Empire and Persia, China, and South America. Although the combination of scope and detail make this volume unique in contemporary historiography, what makes this a significant volume is that it has a compelling hypothesis explaining Britain's imperial impulses.

According to Cain and Hopkins, the central engine driving British overseas expansion and its associated imperial endeavors was a "particular type of economic development, centered upon finance and commercial services, which was set in train at the close of the seventeenth century and survived to the end of empire and indeed beyond" (p. 648). The logic of British imperialism was thus, very importantly, a product of the specific evolution and shape of the British economy and less importantly the result of actions at the periphery. More specifically, the dynamic of British imperialism rested predominantly on the evolution and shape of its commercial and financial sectors, with smaller roles for agriculture and industry. Although earliest studies of British GDP found that the share of service-sector employment and product grew with industrialization, indeed continued to grow as agriculture's share declined and industry's plateaued, students of political economy have been slow to recognize the economic weight of the British service sector's growth and its political ramifications.

Of course, finance is a dominant force in Hilferding's conception of late nineteenth-century European imperialism but his theory and analysis locate the imperialist economic logic in the monopolistic tendencies of finance and industry, resting on the economic history of central Europe at the turn of the nineteenth century. The British case is rather different, certainly over the 300-year sweep of its imperialist activity. First, Cain and Hopkins make it clear that commerce, shipping, and insurance were as important as high

finance in Britain's imperial impulses. Second, although monopolistic overseas trading and settlement companies were an important feature of the early stages of British imperialism, by the early nineteenth century these monopolies were losing their power and influence and were replaced by a much more competitive overseas commercial and financial capitalism.

Within British imperialist historiography, giving commerce and finance pride of place across the full run of British imperialism represents a bold contribution. Support for the domestic economic weight of these sectors is easily marshaled by Cain and Hopkins. In addition to the GDP sectoral data noted previously, Rubinstein's research shows that the largest nonagricultural concentrations of British wealth, even at the high mark of industrialism in the mid-nineteenth century, were the result of commercial and financial enterprise, not industrial enterprise. And, studies of nineteenth-century employment and other data make it clear that commerce and finance enterprise was highly concentrated in London, giving their wealthy leadership much easier access to power than industry's leaders in the provinces.

In *Act 1, Scene 1* spanning the years from the late seventeenth century to the late eighteenth, a powerful commercial and financial sector emerged, strengthened by the more secure establishment of private property and the wider freedoms granted commercial and financial entrepreneurs after the political shifts of the Glorious Revolution. The imperial expansion of this period combined the ambitions of aristocrats and the country gentry with those of their new junior partners, the moneyed commercial and financial interests. One of the creations of this eighteenth-century partnership, as O'Brien and others have shown, was a very strong military-fiscal state. According to Cain and Hopkins the period's mercantilism protecting agriculture and industry was not a feudal residue; rather it was a set of regulations and institutions that were made to fit the needs of this century's agrarian and service-sector interests. War and aggressive regulation meant that by the end of the eighteenth century Britain had captured the bulk of the re-export trade linking the widening overseas markets with Europe. Thus, the "old colonialism" was importantly a product of contemporary domestic agrarian, shipping, and commercial impulses, not of the smaller, provincial industrial sectors or atavistic social forces.

Act 1, Scene 2 spans the late eighteenth century to 1850 or so. The American revolution and the French revolution, with its attendant wars, threatened property and created fears of invasion, temporarily strengthening the conservative and antidemocratic inclinations of the propertied classes, and generating a wider patriotism based on "the principles of godliness, social discipline and loyalty to the crown" (p. 649). After 1815, however, an era of reform emerged which "reduced the national debt, abolished Old Corruption, enfranchised a larger cohort of property-owners, and dismantled the machinery of protection" (p. 649). The move to freer trade was complex, according to Cain and Hopkins. Although increased food imports and the need for overseas markets for manufacturing had political impact, the view that Britain's comparative advantage was as a warehouse and banker of the world was more important. The trade and financial dislocations of the Napoleonic wars and the domestic marketing of the immense war debt had turned postwar London into the strongest European financial center and a commercial powerhouse. This meant, in turn, that an increasing share of Britain's import bill was covered by the positive invisible account. As in the eighteenth century, money earned in finance and services was far more socially attractive than industrial wealth; it was less visible. And it is this class of gentlemanly capitalists who produced the home and overseas leadership for Britain's overseas imperial ventures.

The period's expansionist impulses involved promotion of world trade and enlarged flows of capital to Europe and the United States. Imperial ventures led to a chain of naval bases from Aden to Singapore, which apart from their strategic value, promoted safer and larger ocean commerce. In South America British interests moved to draw the new republics into the British economic and political orbit by financing their nation-building activi-

ties, offering free trade treaties, and supporting the growth of liberal institutions. Settlement schemes and access to the London capital market were central features of Britain's support for its white-settler colonies. In several lengthy discussions of British policy in India in the nineteenth century, Cain and Hopkins offer considerable evidence that the British service sector had at least as much influence in Raj economic and political policy as did British industry, if not more.

Observing both the activity of the government and the service and finance sectors in developing new markets, Cain and Hopkins deny Platt's contention that mercantilist goals of self-sufficiency within traditional trading patterns shaped imperial activity in the first half of the nineteenth century. Cain and Hopkins argue for an emerging, freer world development scheme. The authors are also not satisfied with Gallagher and Robinson's views that an informal empire of influence supporting Britain's new manufacturing interests was created and that the important impulses for imperial expansion came from the periphery. With regard to the latter, Cain and Hopkins are much more impressed by the influence exerted by the service sector from London on their overseas business representatives and on the London government's foreign and colonial policy. With regard to informal imperialism, between the rise of manufacturing on the continent and the United States and the very slow growth of commerce with the constitutional empire and the independent regions of Africa, South America, China, and the Ottoman Empire, Cain and Hopkins are not impressed by the substance of informal empire. Yet, they do feel they can summarize the wider mission of the post-1815 economic impulses as "the world's first comprehensive development program" (p. 650)

Act 2 begins in the mid-nineteenth century. The power and influence of the landed interest in the gentlemanly order begins to decline, with dominance passing to the service and financial sectors. The economic weight of the latter sectors continued to grow in the late nineteenth century, concentrated in the Home Counties. There, the economic and political interconnections (and intermarriages) of the landed interest and the service sectors were most significant, centered on London and, above all, the City. Free trade and sound money became the core values for the region's propertied classes, the nation's governments and the wider polity. In the industrial provinces these core policies created both winners and losers, thus denying the industrial sector a unity of outlook to influence London. But much profit in the commercial and financial sectors comes from the sheer volume of transactions, clearly enhanced by free trade and stable money. Curiously, Cain and Hopkins do not emphasize that the free international movement of private short- and long-term finance was an equally important core value, so fundamental that it was almost never discussed. Governments did try to engage the City in various financial schemes to open the doors of the Ottoman Empire, Persia, and China but the City usually ignored such requests; the number of high-return projects in these regions were too few and the expected political and economic risks too high to draw City capital.

The terrain of the age of high imperialism in the late nineteenth century is well explored in chapters on the Ottoman Empire and Persia, Africa, and China, Cain and Hopkins making handsome use of the last 50 years of historical research. What is quite striking is how often by focusing on the links between Britain's commercial, shipping, and financial sectors and this era's imperial ventures, Cain and Hopkins find these sectors' economic imperatives as significant forces shaping events.

Having covered so many imperial developments and incidents with nuance and depth, it seems ungenerous to quibble about the distribution of the authors' efforts. Yet, somewhat more attention might have been devoted to the institutions that reproduced the gentlemanly capitalist class and its evolving interests. There is a sense in this volume that this class was evolutionarily successful, managing to reproduce and enhance its power and purposes well

into the twentieth century when a larger and faster growing rival power, the United States of America, began to limit its scope. Were the specific courses of study in the nation's secondary schools important, filled as they were with Greek and Roman imperial history? What were the roles of economists and historians who actively engaged in public debate about imperialism's course? For example, it certainly looks as if the losses in the American colonies *and* the Rio de la Plata at the turn of the eighteenth century had a profound impact on the willingness of the British foreign office and military to engage in grand imperial action abroad. How did these events enter the consciousness of the service sector's leaders and supporters, in and out of government, and how quickly?

The limits of this review prevent even a cursory mention the many solid contributions to twentieth-century imperial history that are achieved with Cain and Hopkins's hypothesis. It should be noted, however, that students of America's rise to international power in the twentieth century will ignore this volume at their peril. Cain and Hopkins argue with much force that the British imperial dynamic was alive and well after World War I and not much cooled by World War II either. The gentlemanly capitalists of the commercial and financial sectors remained strongly linked to colonial, foreign, and international monetary policy formation, however weakened the United Kingdom's international economic circumstances. The interwar scrapping between Britain and the United States revolved around these conflicts and this volume advances our understanding of the British economic and political imperatives considerably.

Focusing on the political and economic power and scope of Britain's service sector, Cain and Hopkins have written a very important contribution to British imperial and world history. The book offers a compelling hypothesis to encompass the complex and evolving arc of one of the most significant historical phenomena of the modern era. And, at the list paperback price, it must represent the economic history bargain of the twenty-first century!

MICHAEL EDELSTEIN, *Queens College and the Graduate School, CUNY*

Geschichte und Zukunft der deutschen Automobilindustrie. Edited by Rudolf Boch. Stuttgart: Franz Steiner Verlag, 2001. Pp. 290. €49.

This collection of articles on the history and future of the German automobile industry provides an excellent overview of its development over the last century with a special focus on the Auto Union AG, which was founded in 1932 in Chemnitz. Though largely forgotten today, the Auto Union AG formed from the merger of four Saxony automobile firms (Wanderer, DKW, Horch, and Audi) and was Germany's number two automaker and auto exporter in the 1930s, well ahead of Ford, Adler, and Daimler-Benz, but behind GM-controlled Opel. Audi's famous four rings logo stems from this merger. In addition, the Auto Union was also the number two motorcycle producer in the world. With the fall of the Berlin Wall, the archives for this firm finally became available and Rudolf Boch headed a research project to systematize the documents. On the occasion of the publishing of the archive guides, Boch organized an October 2000 conference that presented the preliminary findings by (mostly) young scholars. The conference ended with a podium discussion of contemporary luminaries including Carl Hahn, the former chairman of VW, and Ernst Reuter, former chairman of Daimler-Benz, among other politicians and academics. The collection of state-of-the-art research by young scholars, who summarize their findings and recent books, also historicizes and contextualizes the present juncture of "post-Fordism," whose challenges the German automobile industry still faces.

Unlike many conference volumes, this one is exceptionally well designed and executed. The volume ranges from the beginning of the German automobile industry, when automo-

biles were a symbol of excessive luxury often pelted by stones and needing to dodge strewn nails, through its development as a product of propaganda and mass desire under the Nazis, to present concerns about the future of the automobile in the age of globalization and environment. The last article by Andreas Knie goes so far as to suggest that the individual product “automobile” and its technical construction may dissolve into a concept of “value or brand construction” with automobile companies becoming “mobility providers” of “intermodal services” (pp. 258–64). The volume also includes a range of different methodological approaches for analyzing the history of the automobile: cultural–social (Barbara Haubner, Norbert Stieniczka), economic–business (Reiner Flik, Eva Pietsch), technological (Peter Kirchberg), and sociological (Thomas Haipeter). Here they appear less as competing or incongruous approaches than as necessary and complementary ones needed to analyze such a complex industrial artifact. As Boch stresses, such approaches provide a new horizon for business histories, especially as enterprises themselves had to come to terms with such mentalities, market to specific demand preferences, manage economic and social constraints, and even create an acceptance of the automobile itself (p. 11). Ironically, the wariness toward “mechanical carriages” at the beginning of the last century is reproduced in the environmental worries about the automobile at the beginning of this century. We have somehow come full circle.

Because of its historical perspective, the essays also place the recent post-1993 turnaround of the German automobile industry in perspective. Although automobiles are arguably the marquee German product today, this was not the case for most of the last century. Although Germans invented the automobile, few owned one until the late 1950s; until then they remained a snobby symbol of luxury in a divisive, class-conscious society. The (now) relatively unknown Auto-Union AG had a greater presence in export markets than did Daimler-Benz before 1939. One of the more impressive articles in the volume uses the new documentation available to detail Auto Union’s export push during the autarky years of the Third Reich. Not until the economic miracle of the 1950s could a political and economic foundation form that permitted a car to be developed “for the people.” In a nice contribution, Norbert Stieniczka, argued that the Third Reich “placed the automobile in everyone’s mind,” and the Federal Republic “finally put the automobile in front of the door” (p. 181); then cars actually helped ordinary Germans to drive away and escape from the Nazi past. This volume remains one of the few in German economic history that thematically integrate past perspectives on the present and present perspectives on the past.

If there is one salient theme that arises out of this volume, it is how industrial products and their technological trajectories are shaped by myriad environmental factors. Products embody them, rather than exist independently of them. The sociologist, Michael Schumann, reminds the reader that many predicted that the crisis of 1993—a “valley of tears”—signaled the beginning of the end of the German automobile industry because of its high-cost industrial location. Yet in just seven years German car companies have triumphed in global markets, with the number of employees and domestic production of automobiles actually rising. Although not without issues, few now would suggest that German automobile companies do not have a bright future. But this turnaround (as with past turnarounds) was a result of a new strategy, a redirection to new markets, a new concept of how the automobile fit into society, a new production concept, and a new arrangement between capital and labor (pp. 279–81). Automobile companies had to make sure to conform to all these dimensions if they were to thrive. As Thomas Haipeter also points out in his essay on the transition from Fordism to post-Fordism, the present crisis was only new insofar as worker factory councils helped to initiate this transition (pp. 230–31). Unlike previous transitions, this crisis was overcome with the active engagement of the factory councils, which created a new “deal,” allowing lean production to thrive. The multidisciplinary

approach of this volume reflects the multidimensional approach that businesses had to develop in order to remain competitive across the century.

Finally, and perhaps most importantly, these articles remind the reader that the history of the German economy has deep roots in its eastern areas, which should not be forgotten. Saxony was one of the heartlands of German industry as well as its automobile culture. And the future of the German economy depends upon the east as well. As such this volume provides a rare sort of intellectual integration of eastern and western Germany.

JEFFREY FEAR, *Harvard Business School*

Finance from Kaiser to Führer: Budget Politics in Germany, 1912–1934. By C. Edmund Clingan. Westport, CT: Greenwood Press, 2000. Pp. x, 255. \$67.50.

This book, which ambitiously covers the period between the last six years of the Second Empire and the first year and a half of the National Socialist regime, seeks to trace and make sense out of German budgetary politics. This is no easy task as the author has to deal with three political regimes, a world war, an inflation and hyperinflation, and the Great Depression. He tries to make sense of it all by concentrating on three themes. The first of these involves the efforts to centralize the budget, largely unsuccessful under the Second Empire, much more effective in the Weimar Republic thanks to Matthias Erzberger's famous tax reform of 1919, the stabilization of 1924–1925, and the emergency decrees under Brüning, and fully attained in the National Socialist regime. His second theme is the effort to control deficit spending, totally unsuccessful between 1912 and 1924, potentially attainable between 1925 and 1929, and somewhat more successful during the depression, according to Clingan, only because of a disastrous deflationary policy under Brüning that was finally abandoned by his successors who chose to fight unemployment instead. The third theme, which lies at the heart of the book and is closely related to the aforementioned problem of controlling deficits, is that the mid-1920s was the true period of stability between these otherwise unhappy periods of inflation and deflation and depression and that the clue, not only to understanding its success but also to why Weimar broke down was the lesson taught by the Finance Minister Peter Reinhold, whose budgetary policies of deficit spending and pump priming, according to Clingan, brought the depression of 1925 to an end and made possible Weimar's brief prosperity. Had Weimar continued to pursue such "Keynesian" policies and had they been used to fight the depression, there would have been a much happier ending.

As Clingan cheerfully admits, his argument challenges the past 20 years of scholarship on the financial and economic causes of Weimar's failure identified with the work, among others, of Knut Borchardt, Harold James, and Theo Balderston. These writers stress Weimar's inability to overcome the capital shortage that plagued postwar Germany, the fragility of Weimar prosperity with its heavy dependence on short-term loans from abroad, high wage and social costs, and the international obligations under the Dawes and Young Plans that severely restricted freedom of manoeuvre of Weimar's leadership. From this perspective, Reinhold's willingness to abandon the balanced-budget policy pursued by his predecessors in the stabilization of 1924–1925 intensified the regime's budgetary troubles and properly persuaded his successors that Reinhold's experiment should never be repeated. The trouble with Clingan's argument in favor of Reinhold's policies is that he probably overestimates the beneficent effects of Reinhold's measures and greatly underestimates the barriers to fiscal discipline in the Weimar Republic. The depression of 1925–1926 was, after all, an inevitable stabilization crisis arising from the excesses of the infla-

tion and hyperinflation, and there is good reason to argue that the British coal strike of 1926 was more important in bringing about the German recovery than Reinhold's pump priming. Furthermore, as Clingan himself notes, fiscal discipline in 1924–1925 rested on what he calls a "soft dictatorship" that imposed stabilization measures by decree, just as it later rested on the "hard dictatorship" of emergency decrees. Certainly it was splendid that the Reichstag actually supported Reinhold's measures in the short-lived euphoria of having attained a balanced budget and stabilization as well as a measure of international good feeling under Locarno, but Clingan himself admits that the Reichstag failed to rally behind fiscal disciplinary measures subsequently. Although the Reichstag could be won over to tax breaks, civil servant pay increases, unemployment insurance, and other social measures, it proved impossible under Reinhard or under his successor to rein in its spendthrift proclivities or gain support for fiscal discipline. In the end, one is left with the sense that Clingan's argument is based on too much wishful thinking and too little attention to the excessive spending of municipalities and states, the unbridgeable political divisions, and the role of reparations and by dependence on foreign loans. He also seems to forget also that the imposition of exchange controls in July 1931, the ending of reparations, and the establishment of authoritarian and dictatorial governments were key factors in allowing for a measure of reflation combined with low wages and fiscal discipline. Even then, as recent research has shown, it took rearmament to significantly change the unemployment picture.

While this review has concentrated on the problems with Clingan's arguments, note should be taken of the fact that the book is written in a very breathless manner in which one sentence does not always follow logically from another and which has an excessive number of minor and not-so-minor errors of fact that are too numerous to mention here.

GERALD D. FELDMAN, *University of California, Berkeley*

The Growth of the Italian Economy, 1820–1960. (New Studies in Economic and Social History, vol. 44). By Jon Cohen and Giovanni Federico. Cambridge: Cambridge University Press for the Economic History Society, 2001. Pp. viii, 139. \$40.00, cloth; \$13.00, paper.

This handy little book by two recognized authorities in the field contains eight brief chapters: the mandatory introduction and conclusion and, in between, two chapters on growth and structural change in the long run (the first discussing the data, the second their interpretation), three thematic chapters on the period to World War II (devoted respectively to agriculture, industry and services, and macroeconomic history), and a separate chapter on Italy's postwar "economic miracle." It also contains an extensive bibliography (running to some 300 items) and a brief subject index, but no author index.

The authors immediately advise the reader of the book's "emphasis on historiography instead of history": as the subject is excellently presented in Gianni Toniolo's *An Economic History of Liberal Italy, 1850–1914* (London: Routledge, 1990) and Vera Zamagni's *The Economic History of Italy, 1860–1990* (Oxford: Oxford University Press, 1993), we are told, the purpose of this work is rather to provide a "comprehensive, critical review of the material on which these accounts are based" (pp. 1–2).

This book therefore is not, and does not seek to be, what its perhaps overly brief title might suggest: a bland introduction to the conventional wisdom, or an essay in synthesis and interpretation, suitable in either case for readers new to the subject. Rather, it reviews the disparate, discordant corpus accumulated over the recent decades. This is a more diffi-

cult task, which Jon Cohen and Giovanni Federico accomplish remarkably well; but their material does not by itself yield, and on it they do not visibly impose, a coherent story. The knowledgeable reader will welcome their able compression of that material into a relatively short compass; the beginner may be left with six authors in search of a character.

As Cohen and Federico review the literature they exercise their right selectively to praise and to disparage, to highlight and to downplay, to cite and to ignore. Not every reader will feel comfortable with their evaluations, but that is by the bye: the authors' glosses are rarely intrusive, and whether one agrees with them or not the book's value as a stepping-stone to the literature is undiminished. From this perspective, indeed, its only major fault (and, in the circumstances, a frankly surprising one) is the lack of an author index; but a second edition can readily put that right.

What is comforting, and perhaps remarkable, is the very richness of the literature reviewed in this book. Economic history has long occupied a prominent place in Italian universities, and the subject was until recently a required component of the economics and business curriculum; but it was so stripped of analytical content that it was taught as a freshman course with no prerequisites at all, and with a few famous exceptions the guild was unanimous in its aversion to (and ignorance of) modern tools and techniques. The core of the discipline is still unreconstructed, but valuable contributions now abound: from proper cliometricians, from economists interested in history, from historians willing to take economics seriously, from a new school of Chandlerian business historians. Gone, mercifully, are the days when we few could meet in a broom closet and not get in each other's way.

Cohen and Federico's compact survey achieves its aim of complementing the recent English-language textbooks on the economic history of modern Italy with an English-language guide to the underlying material: if these works bear their expected fruit, Italy will no longer be conspicuous by its absence from the broader literature.

STEFANO FENOALTEA, *Università di Brescia*

Family and Friends in Eighteenth-Century England: Household, Kinship, and Patronage.

By Naomi Tadmor. Cambridge: Cambridge University Press, 2001. Pp. v, 312. £45.00.

Naomi Tadmor has produced a path-breaking and richly detailed book, in which she explores the history of the family in eighteenth-century England. Her original and stimulating approach is based on an analysis of contemporary language in the diaries of Thomas Turner; in conduct treatises by Samuel Richardson and Eliza Heywood; in three novels, Richardson's *Pamela* and *Clarissa* and Heywood's *The History of Miss Betsy Thoughtless* and in a variety of other provincial diaries and administrative and legal sources. It is by means of her painstaking attention to eighteenth-century usage of familial and social terms, asking what words people (specifically the "language community" of the middling sort [p.13]) used and in what contexts, that Tadmor offers a fundamental reassessment of the history of the family, the household, kinship, friendship, and patronage in the early modern period.

The first three chapters of the book analyze concepts of the family, focusing on contemporary usages of the word "family." Tadmor's research and new insights offer a critique of "the oppositional and anachronistic terms" (p. 273) of the historiographical debate on the early modern family which focuses on an analytical dichotomy between "extended" and "nuclear." Tadmor argues that these terms are unhelpful for our understanding of families in the past, because early modern concepts of family were so "complex, variable, permeable and flexible" (p. 23). They were also changeable. She shows that when people in the eighteenth century referred to families, they sometimes meant groups of kin living together,

or separately, as we do today. They could also refer to ties of blood, marriage, and ancestry. But more often “family” in their language meant a household unit, or in Samuel Johnson’s terms, “those who live in the same house” (p.19). Family members included all kin and nonkin who lived together under the authority of the household head. Servants as well as children, apprentices as well as spouses were described as “family.” There were also “families” which included no related dependants at all. When in April 1764 the childless widower Thomas Turner referred in his diary to “my family at home,” he meant a servant and a shop assistant (p. 33). Importantly Tadmor’s research also illuminates the fluid and changing character of the early modern “household-family.” Related and unrelated individuals could be “taken in” for a variety of reasons including poverty, sickness, bereavement, employment, or patronage. They also moved out of households for similar reasons.

The last four chapters of the book focus on concepts of kinship, friendship, and patronage. Tadmor’s analysis of the language of kinship modifies a scholarship that has emphasized the “narrow and shallow” structure of English kinship relations and the dominance of the nuclear family. She shows that in several contexts kinship language was used not to separate or to differentiate the nuclear family from other kinships ties, but “to submerge it in broader kinship relations” (p. 274). “Brother,” in Thomas Turner’s diary, for example, might refer to his brother, half-brother, or brother-in-law (p. 141). The term “father” could be used not only for designating the father, step-father, or father-in-law; any close and senior related or unrelated male friend might be referred to in paternal terms (p. 157). Furthermore unspecific words such as “kindred,” “relation,” “connexion,” or “friend” were apparently deliberately used to obscure the exact degree of relation, and to incorporate the nuclear family in a broad kinship network.

Tadmor’s detailed investigation of the range of relationships designated in the early modern period as “friendship,” shows that, unlike today, a “friend” could be related, as well as a nonrelated employer, patron, or any person who could help in business, legal, or administrative matters. A whole chapter is devoted to the importance of friendship ties for the formation of political networks.

Tadmor’s discussions touch only briefly on issues of gender. She draws attention to the male-dominated character of Thomas Turner’s social world. She also looks at the important role of “friends” for literary and real women in the negotiations of courtship and marriage. Nevertheless her argument that the study of family, friendship, and kinship relationships is essential for our study of the ways in which early modern social, cultural, political, and economic relationships were organized, negotiated, and experienced, is extremely significant for historians of gender, as well as many other fields.

This is a stimulating and significant book. Combining formidable scholarship with lucid argument, it uses literary sources in new ways to offer new conclusions relating to the historiography of the family. It also establishes a new research agenda—proposing “new directions which a more connected history of the family might take” (p. 279)—directions others working in a variety of fields in early modern history will find hard to ignore.

AMANDA FLATHER, *University of Essex*

Women Workers and Gender Identities, 1835–1913: The Cotton and Metal Industries in England. By Carol E. Morgan. New York: Routledge, 2001. Pp. 224. \$27.95, paper.

Historiographies of women and work in England are plentiful. Carol Morgan seeks to shed further light on gender relationships in the Lancashire cotton industry and the Bir-

irmingham and Black Country metal industries. The excellent introductory chapter summarizes recent approaches to the study of women and work in England, since Edward Thompson's seminal work (*The Making of the English Working Class*. New York: Random House, 1963). It also introduces the subject of discourses, a central focus of the book. Morgan's goal is to illustrate the interrelationships between the economic realities of working-class life and the predominant, contemporary, understandings of gender. Two chapters follow about how gender discourses were negotiated in the Lancashire cotton industries, while the bulk of the book (four chapters) concentrates on gendered experiences of work in the Birmingham and Black Country metal industries.

Two key themes about gender run throughout the book. First, the long-established belief that women's responsibilities were as mothers and housekeepers. This is illustrated with evidence from the Parliamentary Papers for the period in question, although this view remained after 1914. The second key theme is closely connected with the first, with the belief that older male workers should be paid a sufficient wage to be able to support their wives and families. Although the first theme is clear throughout the book, the second would benefit from further support concerning how the idea developed in the different regions, as well as the extent to which it affected gender attitudes and relationships, especially in the workplace.

For both industries, Morgan argues that women's identities were shaped partly by the contemporary rhetoric of domesticity, but also by the more complex intertwining of women's own view of their waged labor and their participation in community and workplace struggles alongside male co-workers. During this period, ideas of gender and class and associated identities were continually negotiated and reshaped in both the home and the workplace. The structure of the sexual divisions of labor varied between individual places of employment. Although many men were not opposed to women's waged labor, the nature of what was appropriate work for women varied. Morgan concludes that working-class respectability came to be based on both women's subordination and on gaining some control over work time and family time, wages and working conditions; however, the process of class formation was continually re-worked, with regional variations (pp. 170–73). The primary gains for women were in terms of shorter working hours and better conditions, although enforcement through the Factory and Workshops Acts was initially limited, and the sweated industries remained largely unregulated before the 1909 Trades Boards Act. Even then, some employers evaded the act. Advances were also made towards the notion of equal pay for equal work. This belief was based on the principle that paying women less money for the same work as men would create unfair competition and could undercut men's wages, rather than any belief about equality between the sexes.

The book is based on a wide reading of the sources, particularly for Birmingham and Black Country. Those for the cotton industry are less diverse with the notable omission of the cotton operatives' newspaper, *The Cotton Factory Times*, which includes considerable debate about women's role as cotton operatives during the period under discussion. Overall, Carol Morgan provides a clear, carefully constructed narrative about the course of events in both industries. She does not provide a radically different interpretation of her key question about whether gender attitudes were refined or modified during this period, particularly men's attitudes towards women and work, concluding that attitudes did not change significantly. This is not surprising. However, by focusing on improvements in working hours and conditions in the different industries and regions, emphasizing the variations within and between communities, Morgan helps to shed further light on the many and varied experiences women had with paid labor during the nineteenth and early twentieth centuries.

Changing Family Size in England and Wales: Place, Class and Demography, 1891–1911.
By Eilidh Garret, Alice Reid, Kevin Schürer, and Simon Szreter. Cambridge: Cambridge University Press, 2001. Pp. xxiii, 526. \$90.00.

This is an extremely important study in the demographic history of England and Wales by a group of historians linked in one way or another with the Cambridge Group for the History of Population and Social Structure. The title of the work is somewhat misleading because the book is as much about declining infant and child mortality as about changing family size in the period under study.

The book is based on a unique source in that the Office of Population Censuses and Surveys, now part of the U.K. Office for National Statistics, allowed the authors access to abstracts of anonymized individual-level returns for 13 selected communities from the censuses of England and Wales for 1891, 1901, 1911, and 1921. Census data in the United Kingdom is usually closed to public access for 100 years. The data from 1911 were particularly important because the census of that year included questions on marital fertility. These 13 locales were then categorized into broad environmental types—agricultural, white collar, light industry, and heavy, staple industry—for subsequent analysis. Environment here is to be understood both in terms of the physical environment and the cultural predominance of the mores of particular social groups. The authors show exemplary scholarship in both their recognition of the limitations and possibilities of their sources, and in the use of the innovative statistical methods they deploy to analyze them.

The most important conclusion of the book is that the mortality and fertility transitions of the late Victorian and Edwardian periods, which saw marked reductions in infant and child mortality and family size, can only be understood properly in terms of environments. Where one is born, lives and dies, is of more importance in these matters than social class. This is not to say that class is not an important determinant of life chances but that it is so in that it affects how far families can choose to live in clean, healthful environments. In very crude terms, families headed by persons with low-status occupations that live in white-collar localities have lower mortality and smaller families than their peers in areas dominated by heavy, staple industries. The authors also undermine the argument that high infant mortality and low family size are caused by female employment. The causal relationship may be reversed, with small family size enabling women to work.

The research undertaken reveals the complexity of the demographic transition of the period, and that there were plainly numerous different transitions going on in differing social groups, generations, and localities. Given this complexity, any simple, mono-causal explanations of such changes are problematic. However, the stress on the importance of environment leads one to concentrate on some explanations of change rather than others. Certainly shifts of populations between environments become important in understanding changes in mortality and fertility levels, and in this *Changing Family Size in England and Wales* lends support to other recent work in this field such as Robert Wood's *The Demography of Victorian England and Wales* (Cambridge: Cambridge University Press, 2000). The important role of state intervention in improving the physical environment of the cities in the late nineteenth century is also stressed.

However, it is the idea of the environment as a space dominated by the conventions and expectations of particular social groups that may be the most fruitful and controversial contribution of this book to future research. How this concept of a “language community” works in practice is left rather vague and is plainly in need of further elaboration—how, for example, were white-collar notions as to the acceptable norm for family size inculcated in other social groups? Lurking here are perhaps some of the arguments, and some of the shortcomings, of earlier cultural approaches to the demographic transition, such as the work

of J. A. Banks (*Prosperity and Parenthood: A Study of Family Planning among the Victorian Middle Classes*. London: Routledge, 1954). However, *Changing Family Size in England and Wales* has the virtue of seeing cultural explanations of fertility change as merely a framework in which complex local and sectoral shifts can be approached. It provides an important starting point for further research, rather than a grand and definitive explanation of demographic trends.

EDWARD HIGGS, *University of Essex, England*

The Subterranean Forest Energy Systems and the Industrial Revolution. By Rolf Peter Sieferle. Cambridge: The White Horse Press, 2001. Pp. x, 230. £35.00.

When getting a new book to review I always hope to see a bold approach and a wide swath cut through the tangle of facts and circumstances (that is my interdisciplinarian background) to be combined with a homework well done and with a mastery of relevant, and enlightening, detail (that is my predilection for revealing particulars). Too often I am disappointed in what I get to read—but not in this case. I like this book and I am glad that its English translation (with a number of notable revisions) appeared nearly 20 years after the German original.

Rolf Peter Sieferle opens with a broad review of energy systems that ranges from prehistoric foraging societies to fossil energy in industrial economies. The second chapter describes Germany's preindustrial forests and the use of their wood in traditional industries and by households. This is where the author's contributions to our understanding of the grand energy transition from biomass to fossil fuels begin as he presents some interesting data on the use of wood in glassmaking, salt works, and iron smelting and forging, then draws the attention to considerable household demand for firewood and cites numerous examples of attempts to regulate wood harvests and to prevent increasing deforestation and impending wood scarcity.

But the book's greatest value is in contrasting the role of coal in England's industrial development (chapter 3) with the German wood crisis during the eighteenth century (chapter 4). The English story gets a fine survey based on many well-known comprehensive British accounts (Ashton, Cardwell, Fordyce, Nef, but, curiously, omitting the five-volume *History of the British Coal Industry* edited by Flinn and published between 1984 and 1993) as well as on some documents from the seventeenth and eighteenth century. But the most interesting part is Sieferle's detailed account of land that was gained with coal use, and quantifications of the country's high and early dependence on coal that could not have been remedied by imports of wood from Northeastern Europe. The second part of this chapter presents detailed calculations showing the impossibility of basing the country's rising pig iron output on anything else but coal-derived coke after 1800, and in the subsequent section the author looks closer at a counterintuitive development that paralleled the rise of the new railway age, at rising numbers of stagecoach passengers and expanding numbers of draft horses after 1840.

While most of these matters may be familiar to serious students of the grand nineteenth-century transition, it is Sieferle's description and analysis of Germany's wood crisis and consequences that holds most of the surprises. Of course, at the end of the eighteenth century Germany, unlike England, still had a great deal of land that was not suited for anything but to be left under forests, but much of that cover was badly mismanaged and devastated so that (much like the recent Chinese situation) some areas officially designated as forests had only a few trees left. Authorities tried to stave off complete deforestation,

forecast to be just decades ahead, by regulating harvests and by promoting more efficient combustion. In fact, one of the oldest pamphlets on wood-conserving stoves dates from 1618, and in 1763 an official contest was held in Berlin to design a stove that minimizes household wood consumption, and conservation efforts were also extended to industrial operations. (Again, these moves mirror China's post-1980 quest to ease the country's rural energy supply crisis.)

Under these circumstances it would seem obvious that a new, and a better, fuel would enjoy a rapid and enthusiastic acceptance, but Siefert documents that this was not the case. The German transition from wood to coal was driven mainly by state initiatives and pressures: Frederick II ordering all garrisons to burn coal (this order was later extended to various industrial establishments), and various German states providing instructions on how to adapt combustion chambers to coal, establishing experimental ovens, and paying for conversions from wood to coal. Siefert cites an iconic example of what one may expect when state intervenes in this way: a Breslau merchant setting up a bleaching oven fired with coal from Silesian pits, pocketing a state premium of 200 Reichsthaler—and then abandoning the oven. The only difference between that merchant and the companies that picked up the irresistible offer of the U.S. government to develop synthetic oil industry of the early 1980s was some US\$ 80 billion in expenditures.

Besides financial abuse there was also consumer resistance and prejudice, all begging the question: Was the wood crisis an energy crisis? Siefert argues, as did some analysts of the early English situation, that it was in the first place a timber crisis as societies so heavily dependent on wood found it increasingly difficult to get good merchantable trunks. Britain, with its limited forest base, was the first country to experience this shortage, yet its conversion to coal did not resurrect its forests. In contrast, looming lumber shortages in Germany led to the creation of a scientifically planned and governmentally regulated forestry whose widespread practice first stabilized, and eventually even enlarged, the areas of densely-stocked forests.

In 1693 a Brandenburg jurist Johann Philipp Bunting wrote about *sylva subterranea*, extolling the superb usefulness of the underground forest of mineral coal. German beginnings of coal extraction are at least as old as the English works, but it was only more than a century after Bunting's treatise that German industries and households were finally persuaded, by the price advantage, to embrace this new fuel. Siefert's account of this transition, its preconditions, and its lasting consequences is an excellent contribution to the still inexplicably sparse literature on energy in human history: well informed, well written, revealing in many ways, and also reminding us, as all good history writing should, that, in so many ways, there is *nihil novum sub sole*.

VACLAV SMIL, *University of Manitoba*

ASIA AND LATIN AMERICA

Population Change and Economic Development in East Asia: Challenges Met and Opportunities Seized. Edited by Andrew Mason. Stanford, CA: Stanford University Press, 2002. Pp. xxii, 503. \$85.00, cloth; \$29.95, paper.

At the close of World War II, East Asian economies were severely crippled and in grave disarray. The development achieved by many resource-poor East Asian countries in the

postwar period represents an unprecedented realization. Never before has history witnessed such spectacular growth in a very short time.

Among these East Asian countries, Japan has been a forerunner in the level of economic development. Japan's per capita GNP for 1950 was only US\$153, which was even lower than Mexico (US\$181) or the Philippines (US\$172). By 1990, however, Japan's per capita GNP had grown to a level comparable to that of the United States or Germany. Japan's miraculous postwar economic recovery and continued economic growth has been analyzed profusely by many economists. No less spectacular, however, was the unprecedented rapidity with which Japan moved through the final stages of her demographic transition. Yet, despite these important historical facts of postwar Japanese economic development, not much work has been undertaken to shed light on the interaction between economic and demographic changes in a theoretical and systematic way. This book, edited by Andrew Mason, is the first publication that has fully addressed the issues of both industrial and demographic transitions in postwar Japanese economic development.

This path-breaking book covers the economies of Japan, South Korea, Taiwan, Singapore, Thailand, and Indonesia in the analysis of East Asian industrial and demographic transitions. Using the best methodology and data available, a total of 16 well-known writers addressed key issues associated with East Asian economic dynamism observed in the postwar period, particularly between 1960 and 1990. These issues include economic growth policy, family-planning policy and programs, labor-force growth, savings and capital formation, health, education, income inequality, and the economic role of women.

The studies reported in this book attempt to account for how these East Asian countries coped with many formidable developmental problems arising from rapid population growth and utilized the demographic bonus in order to induce remarkable agricultural and industrial development over the past few decades. These studies also point out similarities and dissimilarities in the pattern of demographic and socioeconomic transformations among these East Asian countries, thus providing readers with a clear understanding of the common denominator existing among these East Asian countries.

Moreover, the lessons that can be learned from past experiences of these East Asian countries provide a useful base for other developing countries that are now in the process of undergoing the same kinds of demographic changes and facing similar developmental challenges. Also, these lessons from the East Asian experiences can be utilized for identifying the appropriate type of official development assistance that developed nations should offer to developing countries. In the recent past, for example, a number of important findings such as those obtained from chapters 10 (Child Health and Health Care in Indonesia) by Eric Jensen and Dennis Ahlburg, chapter 11 (Education, Earning, and Fertility in Taiwan) by Fung-Mey Huang, and chapter 15 (Population and Inequality in East Asia) by Harry Oshima and Andrew Mason have been frequently referred to as an instructive base for formulating the Japanese government's new long-term strategies for official development assistance in population and development for developing regions.

In addition, these East Asian countries are expected to undergo extremely rapid population aging processes in the years to come. Empirical analyses developed in chapters 5 (The Accumulation and Demography Connection in East Asia) by Jeffrey Williamson and Matthew Higgins, chapter 6 (Saving, Wealth, and the Demographic Transition in East Asia) by Ronald Lee, Andrew Mason, and Timothy Miller, chapter 7 (Savings, Capital Formation, and Economic Growth in Singapore) by Mun Heng Toh, and chapter 8 (Population, Capital, and Labor) by Andrew Mason show considerably different scenarios in terms of projected saving rates as the population aging processes of these East Asian countries advance. Although the differences in these projected results have not been reconciled in this book, the study in chapter 6, based on microeconomic data from Taiwan, presents a serious challenge to the

validity of conventional results derived from macro data as in the case of chapter 5, drawn upon a cross-section of time-series of aggregate saving and investment data.

In this book, the 16 writers have discussed economic and demographic changes among the East Asian countries prior to Japan's prolonged recession beginning from the early 1990s and the Asian financial crisis in 1997. It is curious to what extent demographic variables have been affected by such serious economic slow-downs in East Asia, although this falls outside the scope of this publication.

Because of its comprehensiveness in the coverage of topics, this book is highly valuable to a wide range of readers—not only to economic historians, demographers, and development economists, but also to scholars specializing in Asian studies and development planners in both developing Asia and other parts of the developing region.

NAOHIRO OGAWA, *Population Research Institute, Nihon University*

Taiwan in the Global Economy. Edited by Peter C. Y. Chow. Westport, CT: Praeger, 2002. Pp. xiii, 284. \$67.95.

A strong case can be made that Taiwan's socio-economic performance over the last half century exceeded that of any developing and more recently developed country. Furthermore, the development model and strategy followed by Taiwan provide useful lessons to presently developing countries. This is the message of this book echoing that of a number of recent studies. The book has three objectives: to recapture (less visible) lessons of Taiwan's experience relevant to development economics; to address the role of foreign investment in the process of structural transformation and globalization; and to examine Taiwan's economy in the global perspective. The book is divided in three distinct parts corresponding to these objectives.

The volume covers a very wide domain from global issues to micro and industry-specific issues. The reader is faced with a series of papers that at first glance appears to be only loosely and eclectically connected. Yet they combine to form a useful mosaic highlighting many of the success elements of the Taiwanese development experience.

As is usual in a collection of essays the quality and style vary but the very diversity of the contributions is likely to appeal to different groups, i.e., development economists, international finance and trade experts, industrial organization specialists, and economic historians.

In the first part of the book, devoted to the development path and lessons of Taiwan's experience, Gustav Ranis provides an excellent and balanced account of Taiwan's success in moving from a very poor agrarian economy in the middle of the nineteenth century to a mature industrial power presently. Among the elements contributing to its success, Ranis mentions favorable initial conditions (e.g., an early land reform, appropriate rural institutions, an early emphasis on increasing agricultural productivity, and export promotion), continuous attention to education, a flexible exchange-rate system and large public sector R&D support, including setting up industrial parks to attract foreign firms.

A second essay by Alice Amsden and Wan-Wen Chu argues that the conventional view of Taiwan's success being based on small and medium-sized enterprises and reliance on market forces needs to be qualified. They provide evidence of increased concentration in a number of industries and the importance of the government (the visible hand) in fostering networks.

The second section of the volume explores foreign-investment, multinational, and boomerang effects. Steven Lin shows that as Taiwan's economy became more globalized, it

shifted from a recipient to a provider of foreign direct investment (FDI). One of the conclusions reached by the author is that the small size of many Taiwanese firms is a handicap in the private research effort. Accordingly, he argues that the government needs to continue encouraging the formation of larger and more modern enterprises through acquisitions and mergers. Based on the success of relatively small innovative entrepreneurs and firms in coming up with major technological breakthroughs in the United States, this last recommendation appears debatable.

The other two chapters in this section, on offshore sourcing strategies of multinational firms in Taiwan (by Tain-Jy Chen and Ying-Hua Ku) and on the boomerang effects of FDI invested in the agricultural sector of mainland China on Taiwan's domestic economy (by Jiun-Mei Tien), provide a very detailed analysis of the direct and indirect effects of Taiwan's outward FDI and the danger of relying too extensively on a limited number of industries in mainland China.

The final part of the book, on Taiwan's economy and global perspective, is perhaps of greatest interest to economic historians. Henry Wan, after carefully reviewing the comparative performance of Taiwanese firms and the industrial policy of the government, provides a convincing case that the importance of Taiwan is significant beyond its size and that "its most profound role is to serve as a role model for development" (p. 156).

Frank Hsiao and Mei-Chu Hsiao explore Taiwan's role in the global economy from a very long-term historical perspective starting at the beginning of the seventeenth century. A most revealing historical fact in understanding the recent development strategy of Taiwan is that the Dutch and the Spanish used the island as an entrepôt between China and Japan as early as the first half of the seventeenth century. Over its subsequent history entrepôt trade became a significant characteristic of the island and it is therefore not surprising that the government in the 1970s relied heavily on industrial parks containing all necessary infrastructure to attract multinational firms. A final essay by Peter Chow reveals clearly the extent to which Taiwan followed an essentially eclectic approach, climbing the product cycle ladder one rung at a time consistent with dynamic comparative advantage on the world market.

In conclusion, this book provides additional evidence to the existing literature that a number of lessons relating to the development path and the strategy followed by Taiwan during the last five decades are potentially transferable to presently developing and semi-industrialized countries.

ERIK THORBECKE, *Cornell University*

Argentina: A Short History. By Colin M. Lewis. Oxford: Oneworld Publications, 2002. Pp. x, 260. \$12.00, paper.

For historians interested in the question of "what went wrong" with societies and economies, Argentina is frequently used as an exemplar. Rich in resources, with a highly literate population, with plenty of promising starts, and not a few bonanzas, Argentina is nonetheless in a recurring mess. Monumental booms very often lead to great busts. Recently of course, the former "emerging market" darling has become practically a basket case, with unemployment nearing 20 percent and a bleak future. So, what went wrong?

There have been many efforts to answer this question. Was it some institutional pathology, the wrong kind of resources, bad policy—or just plain bad luck? Colin Lewis, a distinguished economic historian at the London School of Economics, has written a clear, balanced synthesis of nineteenth- and twentieth-century Argentine history, accenting the political and economic cycles and events. Lewis does not advance any single theory or

interpretation. But he does have a principal theme: that Argentina has been caught in an unresolved tension between its “place in the world” as an international source of staples and a destination for world migrants, while at the same time struggling to create national, integrated institutions. His book is written for nonspecialist readers in search of a sophisticated tour and analysis of modern history. In my view it is the best single work of this sort.

Over the course of two centuries, Argentina has wavered back and forth between its “international” and “national” orientations. The nineteenth century was the heyday of the former, which gave way by the interwar years to the latter. By the 1970s Argentina was clearly swinging, if forcibly, back to its international dimension. Lewis deals with each phase by laying out the international conjuncture, the policy mix adopted by leading statesmen, and effects on economic growth. The best of this book is Lewis’s close analysis of Argentina policy making in its different eras.

Of course, some readers might find that the heavy emphasis on policy exaggerates the role of the state. This is not to say that Lewis leaves out social actors—he does not. But he does argue that Argentina had options, and these are explored through the ways in which social actors and state leaders interacted to tilt the country in internationalist or nationalist directions. What is consistent about policy making in Argentina, however, is how each policy mix delivered some spectacular results at first—but outlived its utility, and indeed began to throttle economic transformations when they became more and more urgent. It seems that Argentina is a good case in which stakeholders in the status quo manage to monopolize policy making, or at least become strong enough, thanks to state support, that they can neutralize efforts at making economic transitions. The recurring logjams therefore often require, or provoke, jarring ruptures—sometimes even very violent demarches—to shift the economic trajectories. And this in turn means that the losers lose their own place in public life or access to the economic pie. Thus for all the wealth in Argentina, it became a more and more polarized society.

The final chapter’s subtitle is called “the politics of frustration.” The book is, in many ways, about the frustrations of policy making in a country in chronic search of policies that bring the international and national trajectories if not into harmony, then into balance.

Readers looking for a smart, up-to-date, useful introduction to Argentine historical political economy should start with this book.

JEREMY ADELMAN, *Princeton University*

UNITED STATES AND CANADA

A History of Household Government in America. By Carole Shammas. Charlottesville: University of Virginia Press, 2002. Pp. xv, 232. \$55, cloth; \$19.50, paper.

This is an important book about families in American history not only because it synthesizes a vast literature, but also because it introduces an entirely new perspective on the subject by substituting the term “household” for the “family.” Doing so illuminates the crucial component of dependency in American family law. As Carole Shammas puts it so tersely, yet so meaningfully, the dependent household member is not free to leave. From colonial times until the Civil War, households in the United States consisted of a single head, usually a male aged 25 to 64, and all those living with him were his dependents including wife, children, widowed parents, other kin, apprentices, servants, and slaves. The head of the household was a master to whom all other members had to submit in exchange for his protection. Legal institutions brought over from England at the founding of the

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colonies defined all these dependent relationships (except slavery) and powerful religious traditions sanctified them.

Yet the transplantation of household institutions from the old world to the new could not proceed unaltered, as Shammass, professor of history at the University of Southern California, points out. The ocean barrier attenuated governmental power and the availability of land undercut the dependency of sons on their fathers. Except among the Puritans in New England and the Quakers of the Middle Colonies, neither church nor state could muster sufficient authority to command the obedience of dispersed settlers. Colonial governments therefore enhanced fathers' powers over wives, children, apprentices, and servants. The expansion of slavery and high marital fertility further extended that body of legal dependents, the total proportion of which reached some 80 percent of the colonial population compared to just 64 percent in Britain. Although Shammass emphasizes the need of colonial governments to have powerful masters and fathers in order to keep the peace, she finds little evidence that fathers effectively managed their children's marriages, underscoring the rocky underpinnings of patriarchy where land was cheap and labor dear.

Despite high-flown rhetoric concerning liberty, Shammass finds nothing revolutionary about the American Revolution insofar as dependents were concerned. She is particularly effective in contrasting French reforms with those of the United States. Yes, she acknowledges the decline of slavery in the North and the tweaking of inheritance laws in the South, but in both cases, masters and fathers found their hands strengthened rather than weakened. Slaves worked for their own emancipation, after all, and many northern owners simply sold their chattels to the South rather than see them gain their freedom. Changing the inheritance laws in the South did not alter practice, since fathers had always been remarkably free to dispose of their freehold property as they chose simply by writing a will. What needed to change, from most men's point of view, was the ability of fathers-in-law to prevent a husband from managing his wife's property in land and slaves through the use of entail, a perquisite that prevented the upwardly mobile male from achieving his business goals. Shammass demonstrates this wittily through fascinating case studies of the marriage alliances made by favorite Founding Fathers.

Republican ideology shored up family patriarchy only temporarily. Female literacy and religious evangelism in the nineteenth century encouraged an expansion of women's rights that directly threatened male privilege. Relaxation of divorce laws enabled more women to exit their husband's household, ending dependency. Likewise, the opening of the West and the expansion of wage work enabled children, servants, and apprentices also to leave. Shammass argues that these reforms provoked a civil war within the household that rivaled its military counterpart in historical importance. But the post-Civil War era inaugurated a counter-attack on dependents' rights through a continual redefinition of their legal status, designed to shelter them from the "pain and the privilege of full equality," as Shammass dryly puts it (p.145). Of particular interest to the nonspecialist is her description of the reaction of Protestant reformers in the North to the competition by Roman Catholic parochial schools and orphanages, staffed by many thousands of nuns and priests. Suddenly these liberals began attacking the institutionalizing of poor children as bad for the children and bad for the country. They thereby reversed direction on orphan care and argued for shipping the children of the urban poor to farm homes in the Midwest and West where hard work and a wholesome family environment would produce healthy, useful citizens. Shammass compares the orphan trains and their passengers to the epic trans-Atlantic journeys of indentured servants in colonial times. The richness of the book's contents is further enhanced by her stunning use of census samples to demonstrate the long-term decline in household size since the end of slavery, the small scale of institutionalization in the nineteenth century, and the rise of households headed by single mothers.

As a writer, Shammass is feisty and often flip although occasionally her text loses clarity. A great strength of the book, however, is her ability to place American practice in an historical and international context. In sum, Carole Shammass has very usefully revisited an enormous and eclectic literature on “the family” extending from colonial times to nearly the present, and brings to bear critical census data on a scale we have never seen before. The book’s long-term perspective will make this work exceptionally valuable to specialists and nonspecialists alike.

GLORIA L. MAIN, *University of Colorado, Boulder*

American Exceptionalism, American Anxiety: Wages, Competition, and Degraded Labor in the Antebellum United States. By Jonathan A. Glickstein. Charlottesville: University of Virginia Press, 2002. Pp. x, 361. \$39.50.

Capitalism promises that hard work leads to success. Yet when this axiom fails, committed conservatives, opportunist politicians, and fearful unionists assign much of the responsibility to the economy’s most exploited: unskilled workers. Blamed for their own hardships, emigrants from Ireland in the early nineteenth century, Asian and eastern and southern European migrants at the turn of the last century, and Mexican immigrants in more recent times, all faced denunciations in the press, physical assaults from nativists, and varying degrees of political repression. According to Jonathan A. Glickstein—in his *American Exceptionalism, American Anxiety*—fear of “degraded” workers is as central to American economic thought as is the belief in opportunity.

American Exceptionalism, American Anxiety focuses on the development of capitalist thought during the Market Revolution. Glickstein presents the voices of conservative and moderate Whigs, abolitionists, Utopian Socialists, labor radicals, Jacksonian Democrats, and proslavery Southerners. These debates shaped the mythology of American exceptionalism, or the belief that citizens of the United States had “widespread access to abundant opportunity” if they were willing to work hard (p. 5). Exceptionalism was a myth, according to Glickstein, because its proponents championed this “ideology of success” regardless of real economic conditions.

Glickstein’s examination of the contradiction between economic realities and exceptionalist hopes captures a history largely ignored. He points out that although real wages increased in the nineteenth century, better conditions happened unevenly. For instance, the tracts of some theoreticians ignored depressions, and others favored almshouses that differentiated between the deserving and undeserving poor. Most continued to promote the spiritual and economic reward hard work provided.

Always present, however, was what Glickstein refers to as “Gresham’s law,” suggesting an analogy to the well-known proposition from monetary economics. Glickstein interprets Gresham’s law to mean that “the rules of economic competition are set by the morally least reputable participants, who were commonly held to include both the servile labor force and the interests that controlled them” (p. 10). Essentially, proponents of exceptionalism hoped that free-market competition would produce a nation of independent farmers and entrepreneurs, but feared that a race to the bottom both in character and in wages would be the actual result.

Social realities and adherence to Gresham’s law led Utopian Socialists to advocate safeguards for high wages, which represented the conduits to economic independence. Whig theorists, whose view of exceptionalism became dominant, chose a different direction. They blamed the victim. The poor, under this rubric, were identified as morally

diseased. Surprisingly, those most likely to take an anticapitalist position, the journeymen whose opportunities to become small shop owners decreased in these years, moved closer to the Whiggish position. Glickstein illustrates this point when examining unionists' opposition to prison labor. These workers accepted the idea that poverty was contagious. Convicts who learned a skill in prison were not only cheap laborers driving down wages, but carriers of this social illness. Unionists demanded an end to prison labor because when released, convicts would likely contaminate their future workmates.

Although Glickstein masterfully deconstructs the myth of American exceptionalism, his dense style tends to obscure his argument. Most importantly, one voice was lacking in this study: "cheap labor." Glickstein discusses the plight of these groups but largely ignores their perspective. He rightly points out that these sources are hard to find. Yet the realism of examples such as letters of Lowell girls or slave narratives would have enhanced his critique of exceptionalism's promise. Overall, he successfully demonstrates the symbiotic relationship between economic realities and cultural perceptions in *American Exceptionalism*, *American Anxiety* and provides scholars of political economy an invaluable work.

JOHN ENYEART, *Stanford University*

A History of the Federal Reserve: Volume 1, 1913–1951. By Allan H. Meltzer. Chicago: University of Chicago Press, 2003. Pp. xiii, 600.

Allan Meltzer's *A History of the Federal Reserve: Volume 1, 1913–1951* relies primarily on archival materials and manuscript collections to construct a sweeping narrative of the founding and operation of Federal Reserve monetary policy during the first 38 years of its existence. With the exception of an initial chapter on central banking theory and practice, the book chronologically recounts the founding of the system and its early years (chapter 3), Federal Reserve monetary policy in the 1920s (chapter 4), the Great Depression (chapter 5), the influence of the Treasury and gold inflows in the 1930s (chapter 6), and its operation in the years following World War II (chapter 7). Because the major chapters are long (averaging around 150 pages each), many scholars will find the book more accessible by focusing on individual episodes. The useful concluding sections at the end of each chapter are helpful for those wishing to move forward in time and still maintain some of the continuity of the author's argument.

Of course, Meltzer's tome is not the first history of U.S. monetary policy. There are notable precursors including Milton Friedman and Anna Schwartz's *A Monetary History of the United States 1867–1960* (Princeton, NJ: Princeton University Press, 1960). (In only 60 more pages, they cover an additional 55 years of monetary history.) Nor is this the first book to describe the birth of the Fed and its early years of operation. Because Meltzer's book follows well-trodden ground, one may well ask what is new and different about this account.

One of its significant contributions is the second chapter, which provides a lucid and thorough account of the evolution of thought on monetary policy. This chapter explains how eighteenth- and nineteenth-century thinkers such as Henry Thornton and Walter Bagehot had already developed ideas that are regarded as main tenets of modern central banking (i.e., operating as a lender of last resort, the effects of money on prices, interest rates, and exchange rates, and the distinction between real and nominal interest rates). But this material is also crucial to one of Meltzer's central arguments: during the first four decades of its existence, the Fed deviated in important ways from the prescriptions and

wisdom offered in these early tracts, and as a result, committed many policy mistakes. As Meltzer emphasizes, a unique brand of U.S. central banking emerged in the first few decades of the twentieth century that was in part a response to the peculiar institutional design of the Fed (and the resulting awkward power-sharing arrangement between the board and the reserve banks), and the authorizing legislation (which led Fed officials to develop monetary policy around the quality rather than the quantity of credit—the so-called real bills doctrine).

Much of what readers encounter in this book has a tenor similar to Friedman and Schwartz's book, but there are notable differences, perhaps none more important than Meltzer's observation that Fed policymaking relied on a particular strand of the real bills doctrine developed by Winfield Riefler and W. Randolph Burgess. According to their view, credit should grow at the same rate as output in the long run (as dictated by the real bills doctrine), but open market operations should be used to control the volume of member bank borrowing and market interest rates. Meltzer argues that the Riefler-Burgess doctrine permeated Fed policymaking beginning in the early 1920s, and influenced their decisions through the 1950s. Adherence to this view led the Fed to conclude, for example, that lack of member borrowing and low nominal interest rates in the early 1930s meant that additional purchases of government securities were unnecessary and would only expand credit on speculative assets. Viewing Fed decisionmaking through the lens of the Riefler-Burgess doctrine helps to demystify Fed behavior (such as why the head of the New York Federal Reserve Bank, George Harrison, did not advocate expansionary open market operations during the Depression). However, there are points in the narrative where alternative views are perhaps brushed aside too quickly. For example, Meltzer relies primarily on statements by U.S. policymakers to downplay the importance of international cooperation during the Depression, and he dismisses the role of the stock market in the Depression without reference to research emphasizing how variability in stock prices affected aggregate spending (Christina Romer, "The Great Crash and the Onset of the Great Depression," *Quarterly Journal of Economics* 105 [1990]: 597–624).

As the title suggests and as the first sentence of the book states, Meltzer views his primary task as writing "the biography of an institution." Perhaps the biggest disappointment is that the book focuses narrowly on Federal Reserve monetary policy and does not provide a historical account of the Fed's other functions. In particular, the Fed's role as a supervisor and regulator of the banking system is not thoroughly investigated, and as a result, an important part of the early history of the Fed is largely ignored. Regulation is mentioned in passing (as it relates to monetary policy or to crises such as the bank failures of the 1930s), but the treatment is often cursory. For example, the Fed's power struggle with the Office of the Comptroller of the Currency and state banking systems over the expansion of the Fed's role as a regulator and supervisor receives little attention. Moreover, the Fed's influence on the structure of the banking system (via prudential regulations that discouraged membership during the system's early operation and then in the 1930s when it gained control over bank holding companies) is largely neglected. A more thorough treatment of its role as a supervisor and regulator in the early years would also shed light on why the Fed is still an active participant in regulating the banking system today (especially since the creation of the Federal Deposit Insurance Corporation). Nevertheless, because this is the first of two volumes, there is certainly scope to remedy these omissions in the next volume, which, if as carefully constructed as this one, will further deepen our understanding of this critical institution.

KRIS JAMES MITCHENER, *Santa Clara University*

Reflections in Bullough's Pond: Economy and Ecosystem in New England. By Diana Muir. Hanover, NH: University Press of New England, 2000. Pp. 324. \$15.95, paper.

This book is a two-fold tale of environmental transformation and economic change, dual processes that Diana Muir sees as integrally related. The pond in the title is a small body of water near her home that was made and manipulated by humans over four centuries. On a somewhat larger scale, Muir argues that all of New England has been made and remade as human populations introduced various approaches to living off the land (literally and figuratively). In attempting to interpret the interrelated histories of two very complex subjects, however, Muir sometimes falls short of the task. There are multiple, not-so-minor problems with her book, including lack of cohesion, inadequate attention to various secondary sources, and a failure to fully integrate social conflict into the narrative.

Although she attempts to present environmental and economic change as integrally related, Muir does not always succeed in this goal. The larger connections are clear (e.g., industrial production required great quantities of natural resources and polluted waterways) but the book typically fails to go beyond these to wrestle with more complex, specific links. Many of the chapters are either environmental or economic history, rather than a nuanced blend of the two. Muir also sometimes contradicts herself. At the very start, for example, she suggests that hunting-and-gathering people would not have made the shift to "distasteful labor in exchange for a diet of ever lessening quality" unless forced to it by population growth (p. 11). In the next few chapters, however, she makes it quite clear that natives who made the Neolithic shift lived quite well on plentiful food of great variety. Also problematic is her periodic effort to liven up an academic subject with folksy charm and imagined conversations (such as one between Indian elders and young lovers). The irregularity of this makes it seem contrived, an approach to writing that does not come naturally to Muir. Passages such as the imagined conversation are conjectural history, which can be misleading.

Many of the book's arguments are well supported by primary and secondary sources, yet this area too is not without its deficiencies. At first glance, Muir seems to be writing a synthesis, cobbling together familiar interpretations, but her references are often to primary materials and omit the secondary works where these interpretations might be found. In other cases, she is selective in the way she uses sources but without any explanation or apparent justification. She gets some of her insight on mill dams from Ted Steinberg's *Nature Incorporated: Industrialization and the Waters of New England* (Cambridge and New York: Cambridge University Press, 1991), for example, but casually overlooks the social conflict that is central to that work (more on this in what follows). Then, too, Muir seems either unfamiliar with or unwilling to recognize a whole host of books and articles that deal with the history of New England's environment and economy. Carolyn Merchant's *Ecological Revolutions: Nature, Gender, and Science in New England* (Chapel Hill: University of North Carolina Press, 1989) does not appear in any endnote and Richard Judd's *Common Lands, Common People: The Origins of Conservation in Northern New England* (Cambridge, MA: Harvard University Press, 1997), which updates Merchant's understanding of nineteenth-century agriculture, also is not cited. Both of these books offer interpretations that clash with the one Muir presents, and perhaps that explains their absence. But they are among a number of standard, respectable sources that she needs to engage at some level, if only to call them into question.

Finally, *Bullough's Pond* only hints at the social conflict that was inherent in the transformation of the natural world and concomitant economic development. At times, Muir seems to assume that everyone in New England responded to land scarcity by becoming successful inventors or entrepreneurs, but of course most eventually ended up as wage laborers and did not much like it. The common people of the region also did not much like

what the new, larger mills did to fish runs and farmland along waterways either. Yet, according to Muir, if doing without anadromous fish “was the price of progress, it was a price New England was eager to pay,” as if all were of one mind on the matter (p. 163). This passage, in fact, is meant to explain Chief Justice Lemuel Shaw’s important ruling in a dispute over the Lawrence dam, only one of many suits brought against mill owners in the antebellum period. Some people were also upset about the industrial pollution plaguing the region’s waterways, which Muir misses as well. She draws on Barbara Rosencrantz’s earlier work to claim that “industrial effluents” were thought to be purifying. But, as John Cumbler makes clear in his book *Reasonable Use: The people, the environment, and the state, New England, 1790–1930*, (Oxford and New York: Oxford University Press, 2001) this was not everyone’s understanding. Folks such as Henry Bowditch, at one time head of the Massachusetts Board of Health, did understand the danger these pollutants posed to public health and they campaigned for government controls. They were defeated, however, and Bowditch was pushed off the board, because such regulation interfered with profit-making. So perhaps the history of mill owners’ efforts to provide good water to the people of Lowell and Lawrence is not an uncomplicated “tale of virtue” (p. 218).

Muir’s interpretation is a laudable attempt to bring nature into the story of industrialization, but it is built overtop of an old tale of Yankee ingenuity and triumphant achievement. At the outset, she explains that *Reflections in Bullough’s Pond* is no jeremiad, but rather a “paean” to the human ability to face economic collapse and resources dearth and “overcome daunting odds” (p. ix). Because she takes this approach, Muir fails to offer a full and complete understanding of both the environmental and economic history of the people of New England. The book is still a worthwhile read, offering countless facts and anecdotes, but it leaves room for another try at writing a synthesis of the ever-changing relationship between people and nature in the region.

CHAD MONTRIE, *University of Massachusetts, Lowell*

Dividing Lines: Municipal Politics and the Struggle for Civil Rights in Montgomery, Birmingham, and Selma. By J. Mills Thornton III. Tuscaloosa: The University of Alabama Press, 2002. Pp. xi, 733. \$59.95.

This remarkable work contains three separate studies of communities in the Civil Rights movement. Although connections between the communities are made occasionally, J. Mills Thornton treats each one’s history as unique and autonomous. His central point is that Civil Rights activism emerged in each place when city politics developed “cracks” in the entrenched order and presented African Americans with opportunities to seek improvements in their conditions. Out of the dynamic of white political fracture and black political opportunism grew the movement that ended segregation and disfranchisement. Thornton’s method is a kind of thick, chronological description of developments that reveal the contingency of events—how one occurrence in a particular circumstance begat another set of actions in a slightly altered context until, after many such cycles, the community had been transformed. Readers must make a commitment to the book, because it is well over 300,000 words of text and contains extensive detail about each place (More than 1,300 individuals’ names appear in the index.) The author assumes that the reader knows the larger narrative—the “Eyes on the Prize” story line—and he mostly ignores historiographical debates in the text, even those authors he flatly contradicts. (A product of Episcopal Sunday School in Montgomery, the author took seriously the admonition, “If you cannot say something nice . . .”) But *Dividing Lines* does, in my view, provide the literature of the Civil Rights

movement with the most sophisticated explanations for activism in arguably the three most important places for understanding what the movement accomplished, and what it did not.

Several distinctions set this work apart from the existing literature. Better than anyone else, Thornton explains the crucial relationship between black activists and the posture of the individual federal courts in Alabama and reveals how the emerging symbiosis between street protests and particular judicial rulings represented one of the most important contingencies in the Civil Rights story. Certainly no history of the movement has dealt more seriously with white conservative attitudes toward Civil Rights activism, which until now has been a glaring lacuna in the literature. He explains how white attitudes toward blacks hardened when challenged but also how splits among segregationists emerged as white violence threatened economic progress. Nor have the so-called moderate whites been so precisely and sympathetically characterized. Similarly, he demonstrates sensitive understanding of the black middle class in the three cities, especially when they were at odds with Martin Luther King Jr.'s national strategy for change. Readers familiar mainly with the King-centered narratives of David Garrow, Taylor Branch, and Adam Fairclough will find here far more hard-nosed criticism of King's motives and actions, at least as they pertained to these cities. A fiercely independent mind, Thornton does not defer to King's heroism, explores his failures as a leader and the inconsistencies of his decision making, and frankly does not give him his due until the end of the book. Fred Shuttlesworth, the courageous Birmingham ally of King, is at the center of Birmingham story, but he also is held to much stricter account than he was in Diane McWhorter's 2002 Pulitzer-Prize-winning *Carry Me Home: Birmingham, Alabama—The Climactic Battle of the Civil Rights Revolution* (Waltham, MA: Adobe Systems, Inc., 2001). Notwithstanding this great admiration for the work, two notes of dissent must be made. Thornton argues so vigorously for a *political* explanation for why activism emerged in these three cities that he does not acknowledge that some of his evidence supports alternate explanations. For example, at crucial points in all three cities, white businessmen challenged the harshest forms of white supremacy as a detriment to future economic progress. Thornton provides the facts but does not entertain analytically the possibility that economic motives had overridden political concerns. Nor does he address the sociologists' "mobilization" argument—that the movement followed the accumulation of blacks' resources in improved education, communications, and group institutions—for which Thornton also provides abundant support.

Similarly, the focus on the internal political developments in the three cities is not connected to King's powerful ideological appeal. His invocation of American democratic values and Judeo-Christian morality moved national politics and the federal government to pass the Civil Rights Act of 1964 and the Voting Rights Act of 1965. Events in Montgomery, Birmingham, and Selma were crucial in making King's ideological appeal. Thornton probably assumes we know that already, but having accomplished so much in improving our understanding of the Civil Rights struggle, the book would have been better by connecting what we have already been told to the news he brings.

ROBERT J. NORRELL, *University of Tennessee*

Saving International Capitalism during the Early Truman Presidency: The National Advisory Council on International Monetary and Financial Problems. By Kevin M. Casey. New York, London: Routledge, 2001. Pp. xi, 252. \$70.

In 1945 Congress established a cabinet-level committee, the National Advisory Council on International Monetary and Financial Problems, to monitor the lending institutions to

be established under the Bretton Woods agreement (the World Bank and the IMF) and more generally to coordinate U.S. foreign economic policies. The members of the council consisted of the Secretary of the Treasury (chair), the Secretary of State, the Secretary of Commerce, the chair of the Export-Import Bank, and the chair of the Federal Reserve Board. The point was to assure a conservative Congress that lending by U.S. agencies and to the extent possible by the World Bank and IMF would serve U.S. interests, not some vaguely defined international interest. As might be expected, the council still exists (with "Problems" changed to "Policies" in 1966), but Kevin Casey covers just the important early years. The book began life as a dissertation, but it is more mature than is usual with a book based on a dissertation. The author avoids focusing on institutional minutiae simply to prove how much he read. Instead, he makes a successful effort to place the council in a larger economic and political context.

One of the main findings is that, as might have been expected, the containment of communism quickly became the central goal of American lending policies. An early request by Poland for a World Bank loan is a case in point. Poland applied for \$600 million in the fall of 1946. Soon this was scaled back to a proposal for \$75 million to finance increased coal production, much of which would probably go to Western Europe, which received support from the council staff. But by the beginning of 1948 Poland's commitment to communism and the Soviet Union made even this small, western-oriented loan impossible. A second finding is that the council pressured the IMF to require that countries receiving loans first get their fiscal houses in order. The policy of making IMF loans contingent on domestic reforms, in other words, goes back to the very beginning of the IMF and at least in the United States reflects deep-seated political instincts.

The title *Saving International Capitalism* is interesting and suitably ambiguous. It could refer to the use of U.S. lending power to contain communism. But this is probably not what the author had in mind. The author seems to accept the view that the system of fixed exchange rates backed up by international lending agencies envisaged at Bretton Woods was crucial for the development of a successful liberal economic order after the war. The proposition is debatable, witness the long debates about fixed versus flexible exchange rates and about the role of the IMF. But in any case, the title accurately describes what some of the founders of the council believed they were doing.

This book is too specialized to recommend to someone who simply wants to get a sense of how American foreign economic policy evolved in the war and postwar era. And the quantitative economic historian looking for data on trade flows, exchange rates, and so on will have to look elsewhere. But for an economic historian doing research on this period *Saving International Capitalism* will be indispensable for understanding the political and bureaucratic environment that shaped policymaking after the war.

HUGH ROCKOFF, *Rutgers University*

GENERAL AND MISCELLANEOUS

The Sword of Damocles: U.S. Financial Hegemony in Colombia and Chile, 1950–1970.
By Jon V. Kofas. Westport, CT: Praeger, 2002. Pp. xix, 238. \$64.95.

Using a rich body of primary sources from the IMF, the World Bank, and the U.S. and British governments, Jon V. Kofas analyzes the role of these global financial institutions and governments in the political and economic development of Chile and Colombia between 1950 and 1970. According to Kofas, the loans provided by the IMF in the post-

World War II period gave great leverage to this institution, forcing the local governments to pursue economic policies that did not benefit the masses, but only the multinational corporations, the local elite, the international banking sector, and the United States. The IMF conditions for providing loans to, or alleviating the debts of, these two countries sought macroeconomic stability over social development and resulted in a loss of economic independence. This reinforced these countries' roles as exporters of primary goods, thwarted the industrialization process, further concentrated income in the hands of the traditional elite, and reinforced traditional and backward social relationships in the countryside. All this was possible, Kofas argues, through authoritarian regimes that repressed the labor movement—a process that counted on the endorsement and support of the U.S. government.

In his section on Colombia, Kofas argues that the repressive Gomez and Rojas regimes of the 1950s were the first governments to facilitate the economic adjustments that were required by the IMF as conditions for new loans. These two rulers borrowed heavily to cover the gap created by falling coffee prices, high defense spending, and their reluctance to increase taxes on the upper classes. The civilian governments of the National Front that came after Rojas did not improve the situation much: they increased foreign debt dramatically (therefore increasing the IMF influence), followed the austerity plans proposed by the IMF, increased the country's dependency on coffee, and created an exclusionary and undemocratic system controlled by the political elite. The creation of the Alliance for Progress was not of much help either, given that the agrarian reform promised by the government never materialized because of lack of political will. This situation, argues Kofas, did not leave the lower classes many options besides joining the growing left-wing guerrillas or the political party created by former dictator Rojas.

With the Chilean case, Kofas emphasizes the role of the foreign copper multinationals. In the early 1950s the Chilean government accepted the IMF recommendations to qualify for foreign loans that included a decrease on social spending and an increase on the participation of foreign multinationals in the mining sector. The IMF considered social spending and wage increases as inflationary measures that needed stronger governmental control. Government officials made a first attempt at fighting inflation with economic growth financed by foreign loans. This model eventually failed, inflation skyrocketed, and the government had no choice other than the austerity programs proposed by the IMF. These programs imposed wage controls and lower social spending together with more generous terms that favored the copper multinationals. As a result, social unrest grew and increased the popularity of the left-wing political parties. The rightist government of Alessandri (1958–1964) attempted to follow a more independent path through an ambitious economic plan to promote exports of copper and agriculture. This plan was financed by debt and only benefited the traditional rural upper classes and the multinational corporations and could not be sustained in the long-term. Eventually, Alessandri was forced to follow the IMF austerity program. The last opportunity for an economic reform came with President Frei (1964–1970) who pursued the “chilenization” of copper: the purchase of half of the copper companies' stock by the Chilean government. Frei's attempts to stop the growing popularity of the left-wing groups through reforms eventually failed because of the pressures from the IMF for austerity and debt payment. Thus, the living conditions of the working class continued deteriorating, increasing the popularity of the left-wing movement that eventually elected Allende for president in 1970.

It is worth pointing out Kofas's assumption that since before World War II the Latin American primary good exports have faced deteriorating terms of trade in the international markets. This view, one of Kofas's main points, developed by Raul Prebisch has been successfully challenged by Victor Bulmer-Thomas; his calculations of the region's terms

of trade show that they improved during the war, Colombia's coffee and Chile's copper being among those that benefited the most (*The Economic History of Latin America Since Independence*. Cambridge: Cambridge University Press, 1994: 218–24). Not taking into account the work of Bulmer-Thomas in a substantive economic history of two Latin American countries can be considered problematic. Kofas's book, however, is a good source for scholars interested in understanding the details of the relationship of the Colombian and Chilean governments with the IMF.

MARCELO BUCHELI, *Stanford University*

On Borrowed Time? Assessing the Threat of Mineral Depletion. By John E. Tilton. Washington, DC: Resources for the Future, 2003. Pp. v, 158. \$55.00, cloth; \$21.95, paper.

John Tilton intends “to provide a concise primer” for the layperson on concepts and data relevant to his titular query (p. vii). Given the book's success in accomplishing this goal, it behooves me to remark at the outset that you should forthwith cease reading this review—your return per unit time would be higher from turning directly to Tilton's elegant summary of theory and fact.

There you will find a progression that in many ways mirrors the evolution of resource economics as a field. The first chapter portrays the problem of mineral depletion as a race between new uses, higher living standards, and population growth on the one hand and continued exploration and various forms of technological advance on the other. The second chapter presents theoretical developments—from those of Malthus, Ricardo, and Hotelling, to more recent frameworks—succinctly and in historical context. The third describes physical and economic measures of long-term resource scarcity, as well as advantages and disadvantages of each, whereas the fourth draws on previous work in analyzing trends in these measures over the previous century and concludes that minerals scarcity has not increased across this period. The fifth enumerates the geologic and economic factors that will shape resource availability in the near and distant future. The sixth provides a standard introductory treatment of externalities. The final chapter restates the book's major conclusions (the bottom line: we are not in immediate danger but the long-run is less certain) and includes a discussion of current policy issues. An appendix by Peter Howie presents long-run (from 1870 to 1997 in most cases) time-series graphs of real prices of selected minerals in the United States (a handy resource in light of the fact that single sources of continuous data are scarce).

Several of the book's strengths are readily obvious, chiefly its brevity (123 pages excluding the usual literary extremities), clarity (Tilton's knack for seamlessly integrating concepts, figures, examples, and data makes for a comfortable read), and accessibility (the book imparts substantial quantities of theory and yet is almost equation-free). Others are more subtle. For instance, Tilton pays consistent attention to potentially significant nuances (from a relatively detailed discussion of deflators in chapter 4, to repeated reminders of the reasons that past data may not be the best predictor of future scarcity) and avoids offering conclusions without appropriate qualifications. Indeed, it is from the murky waters of these qualifications that many new avenues for research are likely to arise.

The book's weaknesses are few and forgivable. No work of this length could be as comprehensive as might be desired and some (especially more recent) contributions to the area are absent, although the author's 30 years of perspective are evident throughout. Tilton occasionally lapses into unnecessary philosophical prose. The environmental chapter could be more focused on problems specific to the minerals sector. Perhaps most glaringly, the

dose of history contained in the book is thin and for the most part deals only with the broadest of contexts. In reviewing the state of knowledge in a field that has generally neglected to develop rich histories, Tilton is largely a captive of those who have gone before. Still, some of Tilton's own work recognizes the importance of location-specific knowledge in expanding a country's resource base and altering comparative advantage. Many of the improvements in knowledge that have enhanced minerals availability have occurred locally and therefore minerals scarcity cannot be fully understood by looking only at global trends. A greater emphasis on the stories of particular countries and industries within those countries would have been complementary to the book's themes.

Functionally, *On Borrowed Time* is a gateway to the resource economics literature that would work well as an undergraduate text or in the introductory section of a graduate level reading list. The resource economist will find this book a refreshing review of what we know and a useful inventory of areas not yet fully explored. Economic historians will benefit similarly (and indeed may be better equipped than the resource economist to dig beneath the surface of at least one of the primary areas of open inquiry, resource-augmenting technological change).

Ultimately, the author's modest aim of educating the layperson belies a remarkable coup. In a few short pages this book not only explains in simple terms the major conceptual touchstones of resource economics and sifts through the data most relevant to its title, it also turns on its head that branch of theory rooted in the assumption that minerals can (in an economic sense) be appropriately viewed as endowments. The arguments and evidence presented here as a mere introduction to the field display a cognizance of the complex forces determining minerals availability. Such awareness is remarkably absent from much, if not most, of the theoretically advanced literature that has followed Hotelling in treating the "fixed stock" assumption as innocuous. The fact that it does so convincingly and without apparent bias leaves the pre-indoctrinated reader with the promising feeling that resource economists have for too long been covering old ground. In the end it is the title's punctuation mark that looms largest, and rightly so. New frontiers await.

JESSE CZELUSTA, *Stanford University*

How the Dismal Science got its Name: Classical Economics and the Ur-Text of Racial Politics. By David M. Levy. Ann Arbor: University of Michigan Press, 2002. Pp. xv, 320. \$55.00, cloth; \$21.95, paper.

How did economics become the "dismal science"? David Levy answers this question with a potent brew of literary texts, neologisms, economic modes of reasoning, and philosophical explorations of utilitarianism and calculus. Levy is even more interested in exploring the extent and durability of the attack than in identifying its originator. In 1849 Thomas Carlyle's "Occasional Discourse on the Negro Question" bitterly assailed economists as partners with antislavery evangelicals in a coalition against white planters in the colonies and whites in Britain. In 1865 a major political dimension was added to the literary attack of 1849. There was lengthy public agitation over the brutal suppression of a Black uprising in Jamaica. A formidable cohort of men of letters joined Carlyle in defending the Jamaican governor's brutal suppression of the rioters. The imperial literati in Carlyle's camp were opposed by an alliance of evangelicals, radicals, and economists, led by John Stuart Mill.

Levy relentlessly tracks this collective and long-term literary assault on British economists as destroyers of both cultural and social cohesion. The attackers invoked language

of genocidal virulence against a number of low-status outsiders, including blacks, Jews, and the Irish. Levy abundantly documents the ways in which twentieth-century cultural critics, both left and right, developed a tradition of silence about the ethno-racial slurs embedded in Carlyle's writings in order to perpetuate his heroic stature as a literary pioneer in the cultural critique of early capitalism.

Levy argues that Carlyle had good historical reasons to link "Exeter Hall Philanthropy" and the perpetrators of the "Dismal Science." This evangelical-Utilitarian alliance mobilized over generations to emancipate black slaves in particular and humanity in general. The coalition grew logically out of the functional equivalence of the Christians' Golden Rule Principle and the Utilitarians' Greatest Happiness Principle.

Levy's answer to the original question has a striking Carlylian clarity: "We became the dismal science as a result of our classical predecessors' role in the abolition of *British* racial slavery." . . . "It's a matter of the historical record," i.e., Carlyle's accusation in "The Negro Question." It does not seem to occur to Levy that this source's "history" may be as tainted by gross inaccuracy as was Carlyle's characterization of economists and economics in the same document.

For three generations after Adam Smith, British abolitionists got virtually no overt support from British political economists in their successive campaigns against the African slave trade and West Indian Slavery. Here, it is the *Dismal Science* that fails to probe a tradition of silence—in the history of economics itself. If, as Levy puts it, British economists had but one reaction to American racial slavery—"END IT," why did their predecessors avoid similar agitation against *British* racial slavery? The short answer was supplied by Herman Merivale, holder of Oxford's Chair of Political Economy at the moment Britain's colonial slaves entered into full freedom as wage laborers. Emending Adam Smith's universal dictum, Merivale declared: "Slave labour is dearer than free, *wherever an abundance of free labour can be procured.*" "No economical cause," he continued, "can be assigned on which we may rely for the extinction of slavery" (Merivale, *Lectures on Colonization* [London, 1840/1861]: 300–08 [emphasis in the original]). Five years later the respective parliamentary leaders of both the administration and the loyal opposition acknowledged J. R. McCulloch, the pillar of British economic orthodoxy, as an outspoken authority on the economic rationality of slavery.

To put the argument in Levy's preferred methodological terms as well, there was therefore a good rational-choice explanation for why the rhetoric of early British Abolitionists statistically favored moral over economic arguments. There is equally good reason why their opponents favored economic over moral arguments by about the same ratio. See S. Drescher, *The Mighty Experiment: Free Labor versus Slavery in British Emancipation*. New York: Oxford University Press, 2002.

Given this historical record of evasive hedging on the issue, it seems difficult to attribute the economists' fluctuating intensity of opposition to two variants of racial slavery to their constant loyalty to the market ideal.

In one way, Carlyle's essay does provide a clue to the formation of Christian-Utilitarian activism in favor of American slave emancipation. As is usual with prophets, Carlyle, in 1849, was pretending to have known for an eternity what he had learned only recently. In forging his conjuncture between abolitionists and political economists he was reacting, as did many others, to current events. During the early 1840s antislavery evangelicals and supporters of free trade were on opposite sides in a bitter struggle over the protection of the British sugar market against foreign slave-grown sugar. The end of sugar protection in 1846 plunged the British sugar colonies into deep depression. Free trade in 1846 intensified the crisis of free-labor production that had begun with the end of "Apprenticeship" in 1838. Abolitionists and economists, whom Carlyle had detested separately, he could now detest

collectively. For their part, economists were no longer encumbered by their own fight for commercial liberalization. Many, such as Mill, may well have been jarred by Carlyle's cascade of racial slurs and genocidal fantasies into joining the coalition against American racial slavery. In this sense "The Negro Question" served as a midwife for the "wide-coiled monster" that would result from the union between evangelicals and economists. Thus began a new phase in what Levy calls the "Katallactic moment" in the final section of his book. I recommend this intriguing work to students of literature, economics, and history alike.

SEYMOUR DRESCHER, *University of Pittsburgh*

Technological Revolutions and Financial Capital: The Dynamics of Bubbles and Golden Ages. By Carlota Perez. Cheltenham, UK and Northampton, MA: Edward Elgar, 2002. Pp. xix, 198. \$65.00, cloth; \$30.00, paper.

Two years after the collapse of the Internet stock bubble comes a bold interpretation that situates that event—and much, much more beside—within a framework of technological revolutions interacting with financial markets. The author argues that a common pattern of technological change, entrepreneurial activity, financial activity, and social-institutional change has recurred in half-century "long wave" cycles throughout the capitalist era. We are currently in the midst of the fifth such cycle.

According to Carlota Perez, each distinct technological revolution or "surge of development" comprises four stages: irruption, frenzy, synergy, and maturity. The "irruption" stage brings a cluster of innovations, while "frenzy" involves an overreaction in financial markets, culminating in a crash. This crash leads to an "institutional recomposition" that facilitates the subsequent "synergy" phase of exploring and widely implementing the productive potential of the cluster of innovations. By the "maturity" stage, these possibilities are largely exhausted, and entrepreneurs and financial markets are ready to consider the productive possibilities of a new set of innovations. Perez's discussion of the interaction of "financial capital" with technological change expands on a neglected theme of Joseph Schumpeter's analysis.

Perez identifies five surges of development: the Industrial Revolution, beginning with Arkwright's mill in 1771; the Age of Steam and Railways, beginning with Stephenson's "Rocket" locomotive in 1829; the Age of Steel, Electricity, and Heavy Engineering, beginning with Carnegie's integrated steel plant in Pittsburgh in 1875; the Age of Oil, the Automobile, and Mass Production, beginning with Ford's Model T in 1908; and the Age of Information and Telecommunications, beginning with the microprocessor in 1971. For each of these ages, Perez identifies a range of characteristic technologies, industries, and infrastructures for transport and communications. Perez also identifies each age's characteristic "techno-economic paradigm." By this phrase she means the principles of innovation that are "common sense" in each age, for example with respect to the organization of production and energy use, scale, scope, materials, and the role of information.

A fundamental feature of Perez's interpretation is that technological and economic change occurs in a series of coherent revolutions, each of which takes about half a century to complete its work of transformation before the next revolution can begin. She explains this feature as the result of the dominance of each age's techno-economic paradigm, likening this to the dominance of scientific paradigms in Thomas Kuhn's interpretation of scientific revolutions. A paradigm remains dominant for as long as it promotes technological and economic progress. When it ceases to do so, during the "maturity" phase of each

surge of development, entrepreneurs begin looking outside the paradigm for new sources of progress and profit, leading to a new technological revolution and a new surge of development.

Perez believes that she thus has an answer to the conceptual arguments that have been raised against the “long wave” schemes proposed by Kondratiev, Schumpeter, and others. She answers potential empirical objections through a detailed interpretive narrative. This approach fits a large range of technological developments, financial innovations, panics and crashes, and other events into her scheme. She also acknowledges a number of historical developments as exogenous influences that modify the timing and other features of her scheme.

Fairly enough in a conceptual book of such broad scope, Perez does not offer formal hypothesis tests in support of her thesis. Indeed, she argues against the drawing of inferences from the difficulty of finding evidence for long waves in macroeconomic data. Long waves are not, she says, a matter of overall growth rates but rather of the changing composition of an economy, and evidence for or against them must be sought at that level.

In my view, it is doubtful whether this or other features of Perez’s scheme are either empirically testable or clearly distinguishable from alternative hypotheses. Perez’s own account acknowledges a fair bit of technological and other change within each of her ages, as well as a fair bit of continuity between one age and the next. How then does one then distinguish one age from another? If one does so by the clustering of innovations, then how does one decide which innovations are the fundamental ones that ought to be clustered? The argument that entrepreneurs give attention only to those potential technological innovations that fall within the reigning techno-economic paradigm seems to boil down to the more commonplace observation that innovations are profitable only if they fill a need within the existing context.

Perhaps, in the end, the existence of distinct technological revolutions and surges of development, as opposed to a series of overlapping movements of technological change, is in the eye of the beholder. I remain a skeptic. What remains, in any case, is a series of provocative ideas on interactions among technological innovations, financial markets, and social institutions.

DOUGLAS J. PUFFERT, *University of Warwick*

Statistics on the Table: The History of Statistical Concepts and Methods. By Stephen M. Stigler. Cambridge, MA: Harvard University Press, 1999. Pp. ix, 488. \$48.95, cloth; \$19.95, paper.

This volume serves up a buffet of delectable and nourishing essays in the history of statistics. Stephen Stigler, perhaps the most eminent modern historian of statistics, has here collected 22 previously published papers, revised the essays to stand in more nearly appropriate relationships one to another, and defined his historical concerns in a fine introduction.

From the essay that gives the volume its title, a wonderful piece on Karl Pearson and his controversies with Cambridge economists on the use of statistical evidence, to the often quoted essay on *Stigler’s Law of Eponymy* (“no scientific discovery is ever named after its original discoverer”), the book is historically sophisticated scholarship. Early papers on Jevons and Edgeworth present the emergent interconnection of the social sciences with data based argumentation, whereas the five chapters on Galtonian ideas take us through such issues as fingerprints and regression toward the mean. For instance, Stigler’s chapter 8 is a tightly reasoned study to defend the proposition that mathematical statistics as a discipline “began” in 1933, and this defense engages arguments about dating in the history of science more generally.

One of my own favorites became chapter 11, “Apollo Mathematicus,” which begins like Philip David’s *The Thread* (Boston: Birkhauser, 1983) with the author, Stigler, perusing the catalog from an Oxfordshire dealer in antiquarian books. From that beginning, through the book of the chapter’s title on mathematical medicine printed in 1695, we have a rollicking tale of Harvey’s methodology to convince the skeptical about the circulation of blood, Pitcairn’s sociology of invention, mathematical theories of secretion, and the cure of fevers. These diverse elements are all finely knit together by a reexamination of the similarities between this complex episode in which mathematics moves into a previously unmathematical area, and all kinds of modern complaints about the problems of mathematizing field x or subdiscipline y .

Part 4, on questions of discovery, takes up what would appear to be traditional questions such as the origins of the ideas associated with Bayes Theorem, maximum likelihood estimation, least squares, and degrees of freedom. For instance, chapter 17 on “Gauss and the Invention of Least Squares” revisits, using two new pieces of evidence he develops, earlier studies sorting out the set of claims and counter-claims for priority—among the champions of the Frenchman Legendre, the German Gauss, and the American Adrain—in the invention of the method of least squares. The issues of discovery, and utilization, of new ideas are treated with sophistication. Indeed, these essays are uniformly informed by a wonderful sensibility: the author has an eye for nuance, irony, and humor, and the wit and energy to see matters through. That Stigler writes so clearly and brings the reader in so easily makes these essays quite special.

The final Part 5 collects studies about standards. My favorite here is “The Trial of the Pyx,” which is the name of the British Royal Mint ceremony “to ascertain that the coinage issued by the Mint met the Crown’s specifications” (p. 383). Of course, such an event is a sampling procedure, and the complex ceremony is examined from the perspective of whether its arcane processes are coherent from a modern perspective—does the trial make statistical sense? Because Newton, among other noteworthy figures, was Master of the Mint, there is a great deal of lovely material that can be developed in telling the story.

As the various pieces originally appeared in such widely disparate places, Harvard University Press is to be congratulated for putting this special volume together and making these papers available to the general reader. Anyone who works in an area in which statistics plays an important role will be entertained and delighted by this volume.

E. ROY WEINTRAUB, *Duke University*

Entrepreneurship and Organization: The Role of the Entrepreneur in Organizational Innovation. Edited by Michael J. Lynskey and Seiichiro Yonekura. Oxford: Oxford University Press, 2002. Pp. xiii, 390. \$93.25.

This volume, which is the fifth in the Fuji Business History Series, had its genesis in a series of papers presented at the twenty-fifth International Conference on Business History held in Japan, 5–8 January 1998. At the heart of the book are three themes around which the very-wide scope of the papers are organized: entrepreneurs and the creation of new businesses and industries; aspects of the institutional and organizational environment which determine entrepreneurship and innovation; and, finally, the role of the entrepreneur in periods of transition.

Part 1 contains as diverse a range of case studies as it is possible to imagine for the twentieth century. Thus, for example, we find studies on the development of German department stores between the 1920s and 1980s, the Spanish motor industry 1930–1975,

the explosion into the home video game business by Nintendo, and a review and analysis of Bill Gates and the growth of Microsoft. Although this range of studies appears disparate there are, nonetheless, important unifying themes. These are, principally, the role of new technology and innovation, the importance of hierarchies and the setting-up of formalized management structures, the relationship between production and distribution, and the ways in which entrepreneurs have both shaped, and responded to, the wider economic and political environment in which they operated.

The second part of the volume contains three case studies, which revolve around the theme of the institutional and organizational factors that helped to drive entrepreneurship and innovation. The first of three chapters in this section focuses on the role of venture capitalists and the emergence and growth of the local area networking (LAN) industry in Silicon Valley. A particularly important conclusion that emerges from this case study is that venture capitalists were, initially at least, very reluctant to provide funding to firms in this industry. Chapter 2 examines the “hive-off” or outsourcing characteristics of large Japanese companies and compares them with major American, British, French, and German companies. Juro Hashimoto finds that the outsourcing characteristics of large Japanese companies were particularly important in facilitating diversification. Finally, Dong-Sung Cho’s detailed analysis of the Sony and Samsung companies explains how they both managed to determine the external environment in which they operated and generated the necessary internal resources vital to their pursuit of industrial leadership.

The final collection of chapters concentrates on the theme of entrepreneurship during periods of transition. Sally Horrocks discusses the research and development (R&D) efforts of British companies in the transition to peace following the Second World War. Using case-studies of Glaxo, Courtaulds, and Unilever, Horrocks demonstrates that expenditure on R&D was not in itself sufficient to guarantee industrial success, especially on a global stage. Much more important, argues Horrocks, in determining the success of R&D spending, was the ability of British companies to relate this to the marketing function. The varying successes of these companies in the postwar period provide ample testimony to the validity of this observation. From an entirely different perspective, Seiichiro Yonekura examines the transition of the Nippon Electric Corporation from a domestic telephone equipment manufacturer to a major player in the global telecommunications market. The role of Koji Kobayashi, NEC’s entrepreneurial chief executive officer, is given particular prominence in this transition. The problems that family firms encounter when the founder dies and succession problems arise have frequently been ignored in much of the business history literature because of the emphasis that has been placed on the creation and maintenance of professional, salaried, bureaucratic management structures. Mary Rose’s analysis of the leadership-succession issues confronting a wide range of British manufacturing industries in the 1950s, provides a fascinating account of the means by which some, but not all, of these problems were rectified.

Overall, this volume provides an interesting and scholarly evaluation of the interplay between entrepreneurs and the organizational and institutional environment in which they operated. A particularly attractive feature of all the chapters has been the efforts of the authors to relate their specific case studies to the wider debates on entrepreneurship, either from the perspective of business history, or from the theory of managerial–organizational structure, or from the theory of organizational competence more generally. This has been especially noticeable in the chapters focusing on Japanese firms. The overall standard of production is very high, and the editors are to be congratulated for their efforts.

DAVID M. HIGGINS, *The University of Sheffield*

The Mighty Experiment: Free Labor versus Slavery in British Emancipation. By Seymour Drescher. New York: Oxford University Press, 2002. Pp. vi, 307. \$50.00.

Seymour Drescher is a seasoned historian of abolitionism, with published studies dealing with the economic state of the British Caribbean in the late eighteenth and early nineteenth centuries, with the reasons for the abolition of the British slave trade, with the relationship between abolitionism and capitalism, and with the mass petitioning that was so effective as an extra-parliamentary tactic pursued by antislavery campaigners. *The Mighty Experiment* makes deft references to these areas of Drescher's scholarly output but ranges beyond this previous work to provide a full-scale investigation of the ideas of the emerging human sciences in relation to the development of abolitionism, primarily, but not exclusively, in Britain from the time of the American Revolution until the American Civil War. The book's theme is to show how abolitionists and proslavery supporters sought scientific validation for their views. The title refers to the process of slave emancipation in the British Empire, something that Drescher regards as underappreciated as a particular blueprint for abolitionist goals. It also underscores the book's emphasis on abolitionist schemes in the Atlantic world as examples of an ongoing spirit of experimentation linked to an Enlightenment belief in human progress.

The human sciences considered in relation to abolitionism include political economy, demography, and racial and epidemiological ideas. Drescher refers to key thinkers who wrote about the need to reform slavery, such as Adam Smith, and to others who offered proslavery views, such as Edward Long; but he also deals with the ideas of relatively obscure figures such as the Russian political economist H. F. von Storch. The coverage of unfamiliar writers is welcome but so, too, is the dissection of better-known texts. In a chapter on the *Wealth of Nations*, Drescher argues that Smith had a very partial view of the range of labor systems available in the Americas, displaying, for example, no interest in the bound labor of convicts and indentured servants. He shows that the *Wealth of Nations* is as important for its silences and evasions over slavery and its reform as for its emphasis on the inefficiency of slave labor (on which he notes that Smith offered no cost analysis of colonial slavery). It was a text that, despite its iconic status, had little influence on abolitionist campaigning or the proslavery response: leading second- and third-generation political economists in Britain (Ricardo, Malthus, Mill) had little to say about slavery to extend Smith's arguments. As for Long, Drescher quotes the passages where the Jamaican plantocratic writer expresses his racially biased views on the inferiority of black slaves but shows that before c.1840 this racial condemnation was not often pursued by other writers on slavery, except for early-nineteenth-century anthropologists who advocated a particular view of the mental capacity of Africans.

In chapter 8 Drescher argues that the ideas of the human sciences on slavery had different degrees of impact on the debates over British slave emancipation. Demographic commentary on the poor reproductive capacity and heavy mortality of the British Caribbean slave population played little part in the abolitionist campaigns of the early 1830s. Arguments emphasizing the racial inferiority of West Indian slaves were not pursued by proslavery diehards. The mighty experiment involved a gradual transition from slavery to freedom in which both sides of the abolitionist struggle wanted to secure a peaceful, gradual freedom for blacks in the British Empire, through the transitional phase of Apprenticeship, while attempting to maintain the productivity of staple crop output. The Whig government that passed the Emancipation Act was not just concerned with the immorality of slavery but with protecting property; they wanted the agricultural productivity of the British West Indies maintained under a protectionist framework for the benefit of the mother country.

The Mighty Experiment points to failed experiments in operating colonies with free black labor in the several decades before British slave emancipation. Thus there are detailed considerations of the Sierra Leone colony from 1787 onwards and of the independent black republic in Saint-Domingue that replaced French overlordship in 1791 and lasted for over a decade. Drescher extends his discussion beyond British slave emancipation to investigate how the mighty experiment affected abolitionist discourse in the United States and South America. He covers the debates that abounded in the 1840s and 1850s about whether the peaceful results of Apprenticeship could be replicated in other slave societies. But he also shows that the problems of ex-slaves in the British Caribbean, and the struggles faced by planters, led to a gradual loss of interest in repeating Britain's mighty experiment while at the same time racialization theories gained more influence. All these themes are sifted and probed in a readable style. For historians of slavery and antislavery, but also for demographers, sociologists, and students of political economy, there is much fresh material to ponder in this well-researched book.

KENNETH MORGAN, *Brunel University*

Bertil Ohlin: A Centennial Celebration (1899–1999). Edited by Ronald Findlay, Lars Jonung, and Mats Lundahl. Cambridge, MA: MIT press, 2002. Pp. xvi, 546, \$60.

This volume celebrates the one hundredth birth anniversary of Bertil Ohlin, though it is also dedicated, somewhat awkwardly, to the memory of his daughter, Anne Wibble, who co-authored a chapter in the volume describing life in the Ohlin household but did not live to see its publication. With 22 excellent essays by some of the most eminent economists including Paul Samuelson, Robert Mundell, and Paul Krugman as also Ohlin's prominent friends, associates, and family members, the volume is an appropriate tribute to the great master whose place in international trade, along with that of his teacher Eli Heckscher, is second only to that of David Ricardo.

Interestingly, the gem of the gems in the volume turns out to be a contribution by the master himself. When we think of Ohlin, the contribution that immediately comes to mind is *Interregional and International Trade* published in 1933 by the Harvard University Press. But in fact Ohlin had given a more or less complete statement of his theory in Swedish at just 23 years of age in his licentiate thesis submitted in 1922. In chapter 9 of the present volume, Christina Lönnblad offers the first English translation of that remarkable document. Only 36 pages long, the chapter systematically develops the core of almost all of what we include in the standard treatment of the Heckscher-Ohlin theory today plus a lucid statement of how economies of scale constitute an alternative explanation of trade and specialization.

In terms of the method of analysis, Ohlin already recognizes in the thesis that one can solve for the international equilibrium by either starting with the integrated world equilibrium and then dividing it into two economies with immobile factors internationally or by starting with two economies in isolation and then allowing them to trade. He develops only the latter approach, however, leaving the task of developing the former to Samuelson and Avinash Dixit many years later.

Assuming identical technologies internationally with flexible technical coefficients, Ohlin carefully shows how differences in factor prices due to differences in factor endowments give rise to differences in goods prices across countries and hence trade. He explicitly states what we call the Heckscher-Ohlin theorem, recognizes that trade substitutes for factor mobility and anticipates the tendency for factor-price equalization: "*In other words,*

interregional exchange tends to create a uniform price structure of the factors of production" (p. 151, italics in the original). The main lacuna left by him and filled many years later by Samuelson once again is the statement of the precise conditions under which factor prices would equalize internationally. But to his credit, he recognizes large differences in the factor proportions leading to complete specialization as one of the key conditions precluding complete factor-price equalization.

In the final substantive section, Ohlin offers equally original discussion of how economies of scale constitute a separate explanation of international trade. He poses the question: "If the factors of production were perfectly mobile, so that each difference in their relative scarcity could be equalized by transfers, would no division of labor then occur?" He goes on to answer, "The answer must be negative. Division of labor is also due to another factor, that is, increasing profits [to be read as increasing "returns"]. Even if the relative scarcity of factors of production were exactly the same in the two regions, there would still be division of labor; if each area were to produce all goods it required this would suppose production on a very small scale" (p. 163).

Apart from the introduction, the volume has five parts. Part 1 describes Ohlin as a man, Part 2 offers commentaries on his contributions during early years of his life, Part 3 discusses his writings on macroeconomic issues, Part 4 focuses on the evolution of the Heckscher-Ohlin theory during the past 50 years, and Part 5 assesses the applicability of the Heckscher-Ohlin theory to economic history. Space limitations forbid comments on all the contributions but they all make superb reading and a learning experience. Of particular interest to students of international trade and history of economic thought are likely to be the contributions by Samuelson, Rolf Henriksson, Harry Flam and June Flanders, Mundell, Krugman, Ronald Jones, Donald Davis and David Weinstein, Douglas Irwin, Kevin O'Rourke and Jeffrey Williamson, and Ronald Findlay and Mats Lundhal.

Having contributed to the literature on external economies of scale in international trade myself during the late 1970s and early 1980s, I found Krugman's essay particularly enjoyable. Displaying his trademark parsimony, here Krugman offers a formalization of Ohlin's idea on how increasing returns can lead to trade within the two-factor Heckscher-Ohlin model. He then goes on to offer his own interpretation of how increasing returns as a basis of trade disappeared from the scene while the formal models inspired by Samuelson's reformulation of the Heckscher-Ohlin theory came to dominate the profession and how his own contributions in the late 1970s and early 1980s placed economies of scale at the forefront.

In sum, economists specializing in trade, economic history and history of economic thought will all want this volume on their bookshelves. And they will scarcely regret the money and time spent on it.

ARVIND PANAGARIYA, *University of Maryland at College Park*

The Dissemination of The Wealth of Nations in French and in France, 1776–1843. By Kenneth E. Carpenter. New York: The Bibliographical Society of America, 2002. Pp. lxiii, 255. \$45.00.

When the Emperor Napoleon left France for the last time, in 1815, he paused on his way to the Atlantic at the Chateau of Rambouillet. He went into the library, in the account of his valet Marchand, and chose a number of books to take with him to Saint Helena. One of these was the five-volume translation by Germain Garnier of Adam Smith's *The Wealth of Nations*, published in 1802. It is now in the Reserve Section of the Bibliothèque Na-

tional in Paris, having been given by the faithful Marchand to Mme. Dosne, the mother-in-law of Thiers, the statesman who presided over the return to France of the Emperor's ashes.

The volumes of Napoleon's Smith betray little sign of their oceanic *aller-retour*. They bear none of the marginal notations which are present in at least some of the other volumes from Saint Helena; only one page is dog-eared, in the section on taxes in volume four. But Napoleon's interest in economic policy was attested to by many of his final interlocutors. "The Emperor turned to several points of political economy consecrated by Smith in his *Wealth of Nations*," one of them wrote, of a particularly "rich" discussion in November 1816; "he avowed them to be true in principle, but demonstrated that they were false in their applications."

Kenneth Carpenter's outstandingly interesting new book is by far the best study yet published of translations of the *Wealth of Nations*, in France or elsewhere. It recounts "an extraordinarily complex history," and one which illuminates much in the political, cultural, and economic history of revolutionary Europe. Carpenter describes his study as "the organized presentation of source material for others to use," and it is this, as well as much more. He provides a detailed account of 30 translations and editions of the *Wealth of Nations*—of which 26 were published between 1776 and 1810—preceded by a long essay on "French transformations" of Smith's book. The successive editions, he shows, with their introductions and omissions, their varying sizes and appearances, their explicatory (or exculpatory) notes, their "modifications" and "clarifications," amounted to a continuing reconstruction of Smith's text. It was at first a work of the moment (by the Englishman "Guillaume Smith," or "Schmitt"); it then became a revolutionary cult, a "canonical" study, and a "scientific" classic.

The French history of the *Wealth of Nations* began even before the first publication of the work in English, in early 1776, when an aspiring translator, the abbé Morellet, complained that his translation of Smith has been seized by the police and burned. No translation was published in France in book form until 1788. But in the course of the 1790s, the *Richesse des Nations* "became the most frequently published economic treatise of the French Revolution." Even the Jacobin-dominated Committee of Public Safety, in May 1793, agreed that copies should be given to each of the commissioners being sent to various provinces as "observers" of public opinion. "It was read with fervency, it was learnt by heart, it was cited as an authority," a reviewer wrote in 1800.

There are rival abbés, in Carpenter's history (Morellet and the abbé Blavet, the translator of the 1788 edition), and rival poets. The poet Jean-Antoine Roucher published a new translation in 1790, which promised "elegance" as well as enlightenment, and a volume of notes (never published) by the philosopher Condorcet. Blavet responded with a further translation, in which he spoke of his own veneration for Smith ("almost my cult.") An advertisement for the Roucher edition described the Blavet edition as "unintelligible and almost barbaric"; Blavet accused Roucher, "who did not know English," of "literary larceny."

Germain Garnier, the author of the 1802 translation, was one of Napoleon's prefects, and was also a poet. He was so eminent, a reviewer wrote, as to be in a position, frequently, to modify Smith's ideas. He "moved" certain passages which interrupted the train of the argument, and added a continuation of Smith's history of the East India Company. He managed to bring some "clarity," another commentator wrote, to a work which was little more than a "chaotic heap of uninteresting facts, of misplaced dissertations, and of digressions without plans, without connections, without method."

Smith's odd renown in the revolutionary period, Carpenter shows, was associated with the expectation that everyone, without exception, would soon participate in the public discussion of policy, including economic policy. As the philosopher Volney wrote in 1788, in a commentary on Smith, in earlier times it was only the middle classes who read: "now

everyone reads, the duchesses, the seamstresses. Everyone began with novels, and everyone has gone on, or will go, to works of administration and legislation.” (Volney added that he had even observed one footman lending another footman Condorcet’s pamphlet on the corn trade.) Smith’s work was particularly suited to this new epoch, another commentator observed; it was a work in which everyone could recognize themselves, or could see themselves as its object.

One of the essential defects of the work, another commentator, E.-F. de Sénover, wrote in 1818, “is to be dangerous for a large number of readers. It is impossible to read Smith without deriving from it an immoderate disgust for existing regulatory laws, of whatever sort; a lively desire for reform, and the intimate sentiment that one can do better.” It was this intimate and unconservative sentiment which was at the heart of Smith’s revolutionary renown.

Kenneth Carpenter’s book is a splendid contribution to the continuing enterprise which a reviewer of 1801 described as “the art of understanding *Smith*.” It is a contribution, too, to the history of an epoch in which, as a young Prussian wrote in 1810, Smith and Napoleon were identified as “the two most powerful monarchs on earth.” It provides some insight, even, into Napoleon’s own choices, in which the “economic” and the “political” were intimately interrelated. The Emperor was preoccupied, on Saint Helena, with the details of his own transformation of Smith. One of his interlocutors reported him as commenting on the English as a *nation de “boutiquiers”*—as in Garnier’s translation of Smith’s “nation of shopkeepers”—and as a *nation de marchands*. But he was also interested, in November 1816, in the revolutionary consequences of freedom of commerce. It was the modern system, he said; it “affected all classes, agitated all imaginations, shook an entire people; it was entirely identical with equality, it led naturally to independence.”

EMMA ROTHSCHILD, *King’s College, Cambridge University*

War and Welfare: Europe and the United States, 1945 to the Present. By Jytte Klausen. New York: Palgrave, 2001. Pp. 341. \$18.95, paper.

This is an ambitious, wide-ranging book in comparative historical political economy. It covers Britain and Sweden in great detail, the United States and Germany in somewhat less, while also having interesting points to make about France and Austria. The central theme is the importance of the legacy of the Second World War for the subsequent development of the welfare states of all the countries covered. By welfare state is meant, not the institutions of social policy, but the state’s assumption of responsibility for economic security and expansion, and for social and economic fairness. This assumption is in turn linked to the idea of economic planning. Jytte Klausen argues, first, that severe wartime economic shortages in all belligerents (except the United States) led to controls that underpinned the perceived efficacy of planning. Second, she argues, although much of the belief in economic planning in a strong sense evident in the immediate postwar years quickly evaporated, it left an important legacy in tethering economic and social issues to each other. Diverse economic and political circumstances meant that this legacy was deployed differently by different postwar national actors, but nevertheless commonality of wartime experience did provide important elements of similarity in peacetime evolution. In this account the war acted as a major source of societal innovation, even in the “outlier” case of the United States, where the much more favourable macroeconomic circumstances of the late 1940s provided notably less incentive for a new “social settlement” between major interest groups, especially labor and employers.

Klausen places her work firmly in the “state capacity” approach to policy making, emphasizing the growth in capacities of the wartime state as a major contributor to the possibilities for the creation of the welfare state. But this perspective does not lead to the neglect of either the role of interest groups or politics. On the former, there is a great deal of discussion of the roles of organized labor and capital, and in particular the impact of the wartime state in reconstructing the representation of these groups. Particularly innovative is the parallel discussion of the agricultural interest, and the war’s effects in most cases of enhancing the importance given to maximizing national agricultural production. On politics, perhaps the most interesting discussion is of the highly contingent manner in which it became the CDU rather than the SPD that dominated the politics of the new German Federal Republic. It is convincingly argued that the incapacity of the SPD to reformulate its ideas, largely unchanged since Weimar, and its inability to form any kind of positive relationship with German farmers lost the party an opportunity to seize the leadership of postwar West Germany.

The overall theme of “diversity within similarity” in transwar developments is made persuasively. It offers a useful counterweight to excessive concentration on national exceptionalisms. More particularly, it rightly emphasises the continuity between wartime and postwar institutions in all major belligerent countries, in contrast to the work of, for example, Mancur Olson which suggested that war was much more destructive of those institutions in defeated counties than now seems justified. Perhaps inevitably in a book of this scope errors of fact creep in, mostly minor. However, the suggestion (p. 276) that the British Labour Party achieved only 7.5 percent of the popular vote in 1931 (the true figure was 29.3) is perhaps indicative of a broader interpretative issue. Klausen focuses attention on the continuity between war and postwar ideas and institutions, and thereby plays down the forces making for the state’s acquisition of new responsibilities in the 1930s. Not only was the British Labour party much stronger than suggested in the 1930s, but the National government of those years, partly in response to this strength, was groping, however blindly, for ways of managing the economy more effectively. The Swedish Social Democrats were doing so in less opaque fashion, as of course were American New Dealers. In Germany, surely one aspect of the Nazi regime was that, in however perverse a fashion, it took on responsibility for the employment and welfare of the population to a quite unprecedented degree. Perhaps the persuasive story told by Klausen about the continuities across the end of hostilities needs to be seen in the light of important continuities also between the emergent forces for national economic management of the 1930s and the wartime development of the state.

JIM TOMLINSON, *Brunel University*