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Book Reviews

MODERN EUROPE

Urban Achievement in Early Modern Europe: Golden Ages in Antwerp, Amsterdam and London. Edited by Patrick Karl O'Brien, Derek J. Keene, Herman Van der Wee, and Majolein 't Hart. Cambridge: Cambridge University Press, 2001. Pp. xiv, 361. \$64.95.

This edited volume is the result of a series of interdisciplinary conferences and seminars sponsored by the Renaissance Trust between 1990 and 1995 to examine "Achievement in Intellectual and Material Culture in Early Modern Europe" (p. 3). Historians of science, culture, the economy, and architecture and urban design were brought together to reflect on the intersections between past achievements in their respective fields within urban centers, as well as on the transfer of those achievements from one urban place to the next over time. These scholars were also called upon to consider the connections between the findings of more traditional "case-study" urban history and the grand narratives of modern development and geopolitical conflict. All of the contributors to this volume agreed to address the same meta question: "Why do recognized and celebrated achievements, across several fields of endeavor, tend to cluster within cities over relatively short periods of time?" (p. 5). In a schema entirely consistent with the Braudelian paradigm of early modern development (Fernand Braudel, *The Perspective of the World*. New York, 1981–84.), three cities in particular were chosen as representative of these episodic peaks of early modern achievement: Antwerp, Amsterdam, and London in roughly the sixteenth, seventeenth, and eighteenth centuries respectively. The chapters of the book are thus organized in groups of three, with one chapter devoted to each area of endeavor in each of the three cities, beginning with their material bases in economic growth and ending with high culture as exemplified by the arts, books, and scientific research and discovery.

As Patrick O'Brien's introductory essay makes clear, the overall aim of the project is to think analytically and comparatively about the wellsprings of achievement, and not just to describe it, in however careful (or indeed loving) detail. This is a goal which will sit well with those economic historians who read this book. But it is a goal only partially achieved. O'Brien links the three urban histories by their common experience as "mercantile imperial" centers (p. 6), and not surprisingly, the most pervasive theme of the book is the importance of economic prosperity as the base upon which other achievements can be built. Thus golden ages turn out to be "path dependent"; they only emerge in those places already "prepared by histories of physical and human capital accumulation, actively engaged in regional, international and intercontinental trade" (p. 17). This is not an overly contentious claim, and as O'Brien himself notes, it is entirely consistent with the conclusions of "canonical social scientists" writing on these questions already long ago (p. 35). Indeed, each of the chapters that follows this introduction to either a greater or lesser extent acknowledges the essential role played by the free flow of men, money, and ideas in centers of economic vitality. Without this flow the consumer cultures which characterized Antwerp and Amsterdam in their golden ages and London over a longer, less narrowly definable period, would have been inconceivable. Everything from monumental architecture to hair ribbons and cotton garments, with domestic decoration, books, and telescopes in between can trace their roots to the changes wrought in these cities by their vibrant international economies.

Although all of the chapters of the book share this common theme, and the argument behind it is a entirely compelling one, it remains nonetheless a fairly limited conclusion. Once we turn to the task of explaining the specific features of the economic rise (and fall? —Amsterdam and London are after all still particularly centers of economic vitality and cultural excitement) of these cities, and specifically the waxing and waning of the endeavor-

ors examined in detail in the chapters of this volume, the advantages of the comparative framework are less clear. Common threads are quickly subsumed in the details of each particular urban experience. The methods of the descriptive historian take precedence over those of the social scientific. This is likely to be a source of frustration to any social scientists who are manifestly not interested in the shape of the trees within the forest. However, this reviewer would still encourage the analytically minded to take a look at this book. Not only is the introduction a terrific piece of comparative work, but the individual chapters, all well written and reflective of the most recent scholarship in their respective narrower fields, yield the kind of understanding of variety and detail which should always lie behind the meta narratives we tell about economic and cultural achievement in the modern western world.

ANNE E. C. MCCANTS, *Massachusetts Institute of Technology*

Merchants and Marvels: Commerce, Science, and Art in Early Modern Europe. Edited by Pamela Smith and Paula Findlen. New York: Routledge, 2002. Pp. ix, 437. \$ 27.95.

The Marxists had it right all along, they just got tripped up by their materialism. Early modern capitalism opened vast new worlds, particularly in the arts and sciences, only the traffic went both ways. Creative agents invented new markets and pushed commerce in directions that favored enterprises immensely cosmopolitan and innovative, often solely for the sake of beauty and display. Commerce offered a context but the nobility, and not an imagined bourgeoisie, had the edge when it came to exploiting the market for *objets*. Paintings could be traded for property, land, and houses. Princes could sponsor natural philosophers, and the fluidity in values meant that good investors, like good practitioners of the arts and sciences, took an interest in all aspects of learning. The interrelatedness of the representational arts and natural philosophy stands as one of the central themes in this tightly integrated collection of essays. We now have a vast historiography telling us that we should no longer teach early modern science without reference to the art of the time, and vice-versa. The point is beautifully illustrated by an exhibition recently held at the J. Paul Getty Museum in Los Angeles (spring 2002) on the art of Pieter Saenredam. Working in Utrecht in the 1630s, he used geometry to regularize and make precise the angles and corners found in the exquisite paintings he made of the city's churches. He knew as much about geometry as he did about chiaroscuro. At precisely the same moment, an hour or two away by barge, Descartes in Leiden put the final touches on his *Discourse on Method* (1637). In effect he explained to the world why precision and clarity of thought made possible the kind of beauty that Saenredam's paintings would come to embody.

Pamela Smith and Paula Findlen have written a superb introduction that explains how, once we include the practitioners of art and science "as part of the story of the emergence, or better, the *construction* of science, the connections between early modern natural philosophy and commerce jump out at us with striking clarity" (p. 17). Paula Findlen goes on to illustrate the point with an essay focused on the early modern cabinet of curiosities, and bluntly tells us that "knowledge of nature could not increase without the commerce in nature" (p. 304). Doctors and apothecaries played key roles as collectors and merchants of the exotic. Even other-worldly alchemists wound up contributing to these repositories of economic and spiritual capital. Just about every European country is represented in these essays although the Dutch, with Anne Goldgar writing on tulips both as painted and cultivated and Harold J. Cook exploring Dutch anatomy studies, receive particular attention. Deborah Harkness gives us a portrait of the arts and sciences in Elizabethan London that

shows how “natural science practitioners thus competed in a commercial world in which ideas and materials were quickly transmuted into merchandise” (p. 142). Francis Bacon was simply theorizing about the implications of a world that already existed. One of the points to be taken away from this collection concerns the relationship between the great theorists and the world around them. It permitted Bacon and Descartes to write as they did.

Every essay in this book is exceptionally good and space does not permit me to dwell on each of them. Rather I want to raise the issue presented by the book’s thesis. Assuming the interrelatedness of early modern art and science—a marriage effected in part by market forces—why and when did they diverge? By late in the eighteenth century we can see the art of Turner, among others, influenced by Newtonian optics, but we can find the dedicated circles of scientific practitioners, whether in Birmingham or Paris, displaying very little interest in the techniques of contemporary artists or even in art itself. There is a story to be written about the decoupling, but first it would have to begin with this book. When in the eighteenth century economic theorists begin to become seriously interested in the market, the pin factory and not the cabinet of curiosities strikes them as the place to look for understanding its underlying principles. As the division of labor spread so too did the gap open between the arts and the sciences. Where and when that process occurred remains to be seen.

MARGARET C. JACOB, *University of California, Los Angeles*

The Professions in Early Modern England, 1450–1800. By Rosemary O’Day. Harlow: Longman, 2000. Pp. xi, 334. €19.99, paper.

To review a work which cites one’s name in both acknowledgments and text is probably imprudent and quite possibly unethical. On the other hand, a rigorous self-denying ordinance would have drastic implications for the viability of academic book reviewing. Further justification for proceeding in the present instance is that Professor O’Day’s references to my own work are not wholly one-sided, either praise or criticism. The following assessment of her latest book will seek to adopt an equally balanced—if not “professional”—approach.

Despite the somewhat broader scope suggested by what is presumably her publisher’s choice of title, Professor O’Day deals primarily and almost exclusively with three learned professions—the church, law, and medicine—between the early sixteenth and the mid-eighteenth centuries. The core of her book comprises three clumps of survey chapters, covering “The Clergy of the Church of England,” “The Lawyers of the Common and Civil Laws,” and “Physicians, Surgeons and Apothecaries.” Her target audience of undergraduates, postgraduates, and the general reader is thereby provided with outline accounts of the size, structure, organization, and functions of each respective occupation and its various component elements. Flanking introductory and concluding chapters tackle the conceptual and historiographical issues raised by any attempt to treat the early modern professions as a coherent subject of historical analysis. They also serve to expound what readers of this JOURNAL are likely to find O’Day’s most interesting and controversial contribution, the claim that the occupational groups with which she deals first emerged as “learned professions” in sixteenth-century England, thanks to what she terms “Social Humanism” (p. 5). That phrase denotes a Renaissance-Reformation philosophy or ideology emphasizing commitment to the service of church and state as a concomitant obligation and justification of occupational and social privilege. Bonding all those with grammar school or university backgrounds, this altruistic and essentially religious ethos was, according to O’Day, what made the learned professions “truly distinctive” (p. 16).

Despite a smattering of evocative quotations, constraints of format and space prevent O'Day from developing this thesis as fully as we (and doubtless she) would have wished. The asserted uniqueness of the humanist-professional emphasis upon "service," and the extent to which this discourse of divinely-ordained vocation qualified, masked or displaced more mundane and self-interested motivations are two issues which demand fuller treatment than they receive here. O'Day may also exaggerate the novelty of her emphasis upon the cultural and intellectual baggage carried by early modern English professional persons, even if she is entirely right to insist that their history should not be reduced to a matter of "prosopographical analyses" (p. 5) or "sociological approaches . . . emphasising structure, neglecting contemporary meaning" (p. 8).

The outline chapters on the clerical, legal and medical professions synthesize much research published over the past quarter-century. Given the lack of other comparable surveys, these will be particularly useful to students. O'Day is at her most assured and incisive in writing about the clergy, her own primary research field, although an exclusive focus upon the Established Church regrettably precludes consideration of the emergence of Protestant Dissenting ministers as a distinct and influential socio-cultural fraction. Her account of medical men (and some women), their training and work practices, also deploys an impressive range of well-chosen individual examples to illustrate the growing diversity of practitioners, the blurring of vocational distinctions between physician, apothecary and surgeon, and their increasing reliance on "practical preparation and private courses rather than university study" (p. 250). Although better on the civilians than their common-law rivals, the treatment of the legal fraternity seems by contrast an outsider's account, excessively preoccupied with institutional structures and taxonomy while somewhat marred by factual slips, misunderstandings and omissions.

O'Day's intended student audience may be confused by inadequately glossed allusions to unfamiliar terms and concepts—such as "commonplacing" (p. 118) and "social medicine" (p. 199)—as also by her occasional indulgence in academic controversy of only marginal relevance to her main themes; many non-U.K. readers will also puzzle over the reference to a UCCA clearing house (p. 69). "Professional" early modernists are likely to deprecate an awkward system of in-text referencing, a bibliography which is neither consistently comprehensive nor entirely up-to-date, indifferent copy-editing or proof-reading, the lack of attention to other arguably professional occupations—such as school teachers and officers in the armed forces—and some sweeping, insufficiently supported assertions and generalizations. Yet when all is said and done, no-one should underestimate the difficulty and importance of the task which O'Day has tackled, or the value of the service she has performed in making a large and diverse body of recent scholarship more accessible to nonspecialists.

WILFRID PREST, *Princeton University and University of Adelaide*

Farm Production in England 1700–1914. By Michael E. Turner, John V. Beckett, and Bethanie Afton. Oxford: Oxford University Press, 2001. Pp. xii, 295. £45.00.

Michael Turner, John Beckett, and Bethanie Afton have assembled a data set based on a large number of English farm records (in the form of account books, wage/labor books, and memoranda books). Some of the fruits of this effort appeared previously in Turner, Beckett, and Afton's *Agricultural Rent in England, 1690–1914* (Cambridge: Cambridge University Press, 1997). This new book employs a subset of 979 of these farm records to cast new light on some important aspects of English farm production in the period 1700 to

1914. In addition to the introduction and conclusion, the book comprises five substantive chapters. Chapter 2 discusses the records as a source; chapter 3 considers some aspects of farming practice (notably cropping patterns and fertilizer use); chapter 4 presents new estimates of wheat yields; chapter 5 presents new estimates of barley and oat yields; and chapter 6 presents new evidence on livestock weights. The most important findings of the book are that the overall growth in wheat, barley, and oat yields was much lower than is commonly thought, and that the main period of yield growth occurred between the 1820s and 1850s. The authors argue on this basis that the Agricultural Revolution can be fixed firmly in the period 1800 to 1850. The great strength of the book is that the data on which it is based are almost entirely new. For that reason, it is a very welcome contribution to our stock of knowledge, and both the authors and their funding body (the Leverhulme Foundation) are to be congratulated.

The book suffers from two major weaknesses that limit how much we can learn about farm production and how much confidence we can place in the results.

First there is the problem of sampling. It is imperative to know whether the sample of farms is representative of the population of farms, and here the authors do little to reassure us. The 979 records are drawn from 281 archival collections—but it is not clear what this means in terms of sampling. Does each of the records refer to an individual farm (a sample of 979 farms)? Or does each of the archival collections refer to an individual farm (a sample of 281 farms)? Or is it something in between? Also, it seems likely that there will be positive selection in the sense that the bigger and better farmers are more likely to have kept records, and those records are more likely to have survived. The authors do not provide any information on the social background of any of the farmers or on the size of the farms in the sample. There is some discussion of the geographical and temporal distribution of the records (pp. 62–65), but this is rather cursory. The geographical coverage is very highly skewed amongst the 38 English counties. Each of 19 counties has ten or fewer records in total; whereas Cambridgeshire, Lincolnshire, and Norfolk comprise 27 percent of the sample (262 records); and Hampshire comprises 17 percent of the sample (165 records). We know that Cambridgeshire, Lincolnshire, and Norfolk are not representative of the national farm. And if the geographical coverage is changing over time, then this could easily impart false trends to the data series (as there was a great deal of regional variation in the level of yields and so on).

The second weakness of the book is the lack of depth in the statistical analysis. The book is replete with statistics, but in many cases it is unclear what we can conclude from them. For example, we are told that 75 percent of farmers who grew clover purchased clover seed on at least one occasion (p. 77). But, unless clover seed commonly changed hands as a gift, presumably 100 percent of farmers who grew clover purchased clover seed on at least one occasion? Most tables give the mean value of the variable of interest, but rarely give the confidence interval—so it is impossible to know whether the mean value is statistically significantly different from earlier or later years, or other sources. For example, table 4.6 provides two estimates of the mean wheat yield for each county—the new series, and those derived from the Board of Agriculture Reports. But we are not told whether the two series are correlated, and we cannot test whether the two estimates for any particular county are significantly different from one another. In the analysis of the new wheat, barley, and oats series (which will probably be the data series of most interest to most researchers) we are never told the value of the time trend for any of the series; and, judging purely by eye, the trends for wheat and barley are probably not statistically significantly different from zero.

In the long term, the work undertaken by Turner, Beckett, and Afton in this book will have a substantial positive impact on our understanding of English farming between 1700

and 1914. But it is not yet clear what we can conclude from the new evidence that they have brought to bear.

LIAM BRUNT, *Nuffield College, Oxford*

The Bouchayers of Grenoble and French Industrial Enterprise, 1850–1970. By Robert J. Smith. Baltimore: The Johns Hopkins University Press, 2001. Pp. xix, 247. \$42.50.

Ever since David Landes's seminal work on the French family firm and the interplay of culture and economics, French business history has wrestled with the question of French particularism and the role of family enterprise in determining business outcomes. For well over a quarter of a century, historians have challenged or qualified Landes's arguments, first by pointing to successful family enterprises in France or elsewhere, second by reassessing French economic performance in modern times, and third by identifying other factors to explain slower growth in macro or micro terms. Robert J. Smith's thought-provoking study of Bouchayer et Viallet, a medium-sized French firm that rose and fell on family leadership and culture, squarely confronts, once again, the issue of family influence on business success and failure. Combining access to family papers with an astute appraisal of personality and context, Smith has produced a first-rate inquiry into the dynamics of family business firms. Mindful of the fact that family firms still account for a predominant part of GNP, but that few family firms continue as such for more than several generations, Smith asks how family control and values contributed to the success of Bouchayer et Viallet yet also braked growth at a middling level and ultimately undermined the continuity of the company. Intended as a case study in the trajectory of family enterprise, Smith weaves together business, family, and cultural history in exemplary ways that will benefit practitioners of all three fields and that demonstrate the value of the first approach for studying and writing the second and the third.

Bouchayer et Viallet, in its heyday, was the leading French producer of pressure pipelines for the hydroelectric industry. The founder, Joseph Bouchayer, was the son of an artisan nail-maker. Typical of first generation entrepreneurs, he combined energy, ambition, and a shrewd business sense to construct a modestly successful company specializing in gas heating and ventilation. Instrumental to the growth of the firm was his association with Félix Viallet, a bourgeois with engineering experience, but, more crucial, local status and contacts to expand the clientele base of the company. In nearly every way the Bouchayers at this stage conformed to the Landes model. Firm and family closely intertwined. Family and personal networks accounted for investment capital and management recruitment. Patriarchy and paternalism defined company culture. All of these elements, at this formative level, were strengths that contributed to the success of the firm. Groomed to succeed his father, Aimé Bouchayer, Joseph's eldest son, foresaw the potential of hydroelectric production and led his company into the pressure pipeline trade. Abetted by three able brothers, Aimé built Bouchayer et Viallet in the years before the First World War into one of the premier business houses in Grenoble. Dying of pneumonia in 1928, he was succeeded by his eldest son, Jean, who lacked nearly every positive aspect of the first two generations of Bouchayers. Jean, as Smith portrays him, seems to have walked straight out of the pages of both Landes and Thomas Mann's *Buddenbrooks*. Able to capitalize on Aimé's legacy, but arriving at a difficult juncture in French economic and political history, Jean managed to keep the firm going but also steered it down a road of decline that his son, Robert, could not reverse. In 1971, four generations deep in history, the firm liquidated its assets.

Smith, as he tells this story, is always sensitive to the economic environment that influenced the fortunes of the firm. Even Aimé had a difficult time sustaining his earlier successes in the more difficult years following World War I, especially as hydroelectric production waned. Yet for Smith the family dynamics were everything. Where family connections and cohesion joined with drive and strategic insight, the family firm prospered and grew. Aimé was a family entrepreneur to the core, but he took risks and expanded operations. Networks offered family firms advantages comparable to those of vertically integrated industries. To a point, Smith challenges the perspectives of both Landes and Alfred Chandler. But he also identifies in Bouchayer history a certain fatal quality to family firm history. Although his portraits of family life are nicely drawn, he perhaps makes too much of the inevitable corrosion of wealth and wider perspectives on third and fourth generations. Robert failed not for lack of energy or commitment or family quarrels, but rather because he possessed poor strategic and business judgement. Still, Smith is right that in a firm where family leadership and identity dominated, entrepreneurship and culture were decisive in setting potential limits to future growth or sustained success. From Aimé's time on, the potential for seizing upon new opportunities depended on the ability to absorb these within the ethos of the sort of family firm the Bouchayers had constructed. Not even Aimé was always up to that task. Smith has written an engaging and insightful history of the rise and decline of a family firm. It can only be hoped that his publisher will broaden readership and teaching possibilities with a paperback edition.

MICHAEL MILLER, *Syracuse University*

Historia económica regional de España, siglos XIX y XX. Edited by Luis Germán, Enrique Llopis, Jordi Maluquer de Motes, and Santiago Zapata. Barcelona: Crítica, 2001.

This volume is a collection of nineteen essays, seventeen of which summarize the economic history of the individual autonomous regions established in Spain as part of the transition to democratic government that began in 1975. The last two essays are valiant efforts to synthesize some of the information in the first seventeen. The first of the concluding essays discusses the persistence of pre-nineteenth-century structures in Spain during the nineteenth century. The second examines the relationship of the various autonomous regions within Spain to the European Union.

This volume is both frustrating and rewarding. The numerous authors make a valiant effort to construct economic histories of their respective regions using a fairly consistent and conventional analysis of macroeconomic variables and cycles. The results, however, are uneven. The various contributors use differing combinations of historical and economic materials, and the aggregate data for the regions they study vary tremendously in their quality. To the historian, who looks to individual and cultural context for an understanding of outcomes, the results are often merely descriptive. At the same time however, it is refreshing to see the discussion of economic growth taken out of the nation-state context, which tends to homogenize local variations in behavior.

This, however, points to another frustration. Economic history is often written as the economic history of nation-states, when, in fact, economic life responds as much to regional geo-economic variables as to political boundaries. This is especially true before the era of effective bureaucracies in the twentieth century and in countries (such as Spain) where the centralizing capacity of the state long was relatively weak. From this point of view, the shift to a regional focus is welcome. It allows us to see localized responses to both foreign and domestic market conditions much more clearly. As a result, it puts another

nail in the coffin of the Black Legend that once told us that Spain and Spaniards somehow lacked the entrepreneurial drive so evident in Holland and England. The rapidity of response to market conditions in many parts of Spain throughout the two centuries studied is often remarkable.

Unfortunately, the economic dimensions of many of the new autonomous regions in Spain are as artificial as those of larger nation-states. They were the product of complex political decisions with uncertain connections to economic realities, past or present. Thus, while Catalonia is a relatively coherent regional economy, the separation of Madrid from Castilla la Nueva-La Mancha makes little geo-economic sense. The Autonomous Region of Andalusia includes parts of at least two distinct regional economies, whereas that of Navarre makes little economic sense without reference to Aragon and the Basque Country. Thus, although the shift away from analysis at the nation-state level is welcome, the limitations of a regional analysis based on historical and political accidents also leave many gaps in the story.

Subjectively, the two most interesting essays are those on Madrid (José Luis García Delgado and Miguel Carrera Troyano) and the Balearic Islands (Carles Manera). As political capital and primate city of Spain, Madrid has always functioned on regional, national, and international levels. The essay captures this well and tracks the changing relative importance of those levels. The Balearic Islands, meanwhile, constitute perhaps the most clearly defined economic region in the country. By exploiting locational advantages and maritime (later airborne) trade, the islands sustained a remarkably high standard of living despite a limited local resource base.

The closing essay on the impact of integration into the European Union (Jordi Maluquer de Motes Bernet) summarizes the tremendous economic change of the last third of the twentieth century. It reviews the evolution of population, employment, output, and investment, and the efforts of the European Union to encourage development in less developed, typically agricultural areas. Two things stand out in this analysis. First, the most dynamic response has been in a quadrilateral bounded by Bilbao, Barcelona, Valencia, and Madrid. Second, the regional distribution of Gross Product per capita at the beginning of the twenty-first century shows regional strengths and weaknesses remarkably similar to variations in regional activity that were already clear by the late eighteenth century. The most striking development of the last century has been the degree to which the relative stagnation of Spain has been replaced by growth. This has dramatically reduced the gap between Spain and the rest of Europe. Some of that dynamism appeared in the first third of the twentieth century, only to be suppressed by the Depression and the Franco Regime. It reappeared with great vigor in the 1960s and was reinforced by the political transition of the 1970s. This is in striking contrast to the eighteenth and nineteenth centuries, when regionally divergent, slow, and sporadic growth left an expanding gap between most of Spain and much of Europe.

DAVID RINGROSE, *University of California, San Diego*

Système Éducatif et Performances Économiques au Royaume-Uni 19^e et 20^e Siècles. By Vincent Carpentier. Paris: L'Harmattan, 2001. Pp. 295. Paper.

One strain of French structuralism has argued for the existence of long-run economic cycles driven by Marxist class and economic dynamics. Vincent Carpentier employs this approach in his study of the relationship between economic and educational change in the United Kingdom over the past two centuries. The basic framework was developed by

Carpentier's mentor, Louis Fontvielle of the University of Montpellier, who has used it to examine the same relationship for France. According to this framework, leading modern economies such as those of France and the United Kingdom over the past two centuries have experienced four Kondratieff cycles of 50 years duration each. During the first three cycles, the level of resources devoted to education tended to move counter-cyclically; in the fourth cycle, the relationship reversed to move pro-cyclically. Fontvielle's explanation of this pattern is that during the first three cycles, during expansionary phases, high returns to physical capital implied pressures to cut back on public educational investments while during downturns, social stresses and contradictions in the system led to expanding educational investments. However in the fourth cycle, since 1945, both the rise of the welfare state and the increasing contribution of an educated labor force to productivity due to the increasing importance of science-based technical change resulted in the level of educational investments moving pro-cyclically.

In applying Fontvielle's paradigm to the case of the United Kingdom over the past two centuries, Carpentier argues that the United Kingdom, like France, experienced counter-cyclical movements in educational activity during three putative Kondratieff cycles between 1800 and 1945 and then experienced a fundamental shift to pro-cyclical educational movements in a fourth cycle. After an introduction laying out the conceptual framework, the four substantive chapters of the book deal in turn with each of these four cycles. For each of the first three cycles, the factor explaining decreasing educational activity during expansionary phases is, following Fontvielle, the high returns to physical capital. However, the reasons Carpentier adduces for educational expansion during economic contraction vary according to the period in question. During the first cycle, 1800–1850, it was rising social unrest during the 1830s that spurred initiation of parliamentary involvement in elementary schooling. During the second cyclical downturn, during the last quarter of the nineteenth century, a perceived loss of English technological superiority relative to Germany and the United States due to educational deficiencies prompted English efforts to establish universal primary education. In the third cyclical downturn during the interwar period, similar concerns about the importance of an educated labor force for British industry to stay abreast of new technological developments led to efforts to extend access to secondary and higher education. Then after 1945, the increasing contribution of education to the demands of an economy driven by science-based technology and the impetus of the welfare state for improving access to higher levels of the educational system resulted in a fundamental shift to procyclical movements in educational expansion in England, similar to the pattern identified by Fontvielle for France.

The basic mode of argument and exposition employed is that of a synthetic narrative based on secondary sources supplemented with tables of statistics spliced together from various government publications. Consequently, the evidence adduced supporting the existence of Kondratieff cycles and counter-cyclical followed by pro-cyclical trends in educational expansion is based on plotting measures of economic and educational activity against time and observing the synchrony between the two types of measures. Many readers will probably not find such evidence sufficient to establish the causal mechanisms proposed. And the claim of a long-term underlying structure generating long cycles for a century and a half but then subject to fundamental structural change in the last half century seems problematic and difficult to verify. One also wonders about how reliably data series covering a century and a half have been spliced.

However, whatever reservations one might have about the explanatory framework Carpentier employs, one must admire his survey of British educational history and its relationship to economic change over the past two centuries. It is based on an extensive, careful, and sensitive reading of the secondary literature on both economic and educational

history. And whether or not Kondratieff cycles have actually been present, the hypothesis of an initial regime of educational expansion moving counter to economic prosperity followed by a reversal is an intriguing one. Moreover, the hypothesis of a shift in physical capital–human capital relationships over the last few centuries has been manifest in recent work on human capital in growth models such as that of Oded Galor. Thus both the hypothesis that Carpentier puts forward and the historical narrative he develops are worthy of attention. Indeed one would hope for both an English translation of Carpentier’s book and further comparative work on these issues by Carpentier, Fontvielle, and their colleagues.

DAVID MITCH, *University of Maryland Baltimore County*

Recent Work in Belgian Historical Demography, Nineteenth and Early Twentieth Centuries. Edited by Isabelle Devos and Muriel Neven. *Revue Belge d’Histoire Contemporaine / Belgisch Tijdschrift voor Nieuwste Geschiedenis* 31, 3–4, Antwerp, 2001. Pp. 311–647. € 34.

Ten articles and a rich selective bibliography demonstrate the vitality of Historical Demography research in Belgium. In the introductory article, the editors sum up the main progress of the discipline in Belgium since 1981 and present an updated impressive commented bibliography. Belgian researchers have broken down many stereotypes. For instance, the process of industrialization in mid-nineteenth-century Belgium did not affect the traditional urban network in a spectacular way. Old-established cities and towns like Ghent, Leuven, Verviers, and Charleroi—that receive a special attention in this volume—continued to be important urban centers as they were well before the Industrial Revolution. The stereotype of a massive rural exodus generated by the industrialization is definitively overcome. By adopting a micro-research approach, Katleen Dillen shows that migration was mostly a positive choice and less disruptive than usually considered because it took place in a dense and vivid social network (“From One Textile Centre to Another: Migrations from the District of Ghent to the City of Armentières (France) During the Second Half of the Nineteenth Century,” pp. 431–52). This absence of dramatic change in migration pattern during the industrialization—which is therefore opposite to the situation observed in the Ruhr during the same period—explains why there was no difference in fertility intensity and calendar between migrant people and the sedentary population of the industrial area of Charleroi. Interestingly Flemish migrants to Charleroi adopted the same demographic behavior as the native Walloon people. So, according to Thierry Eggerickx, the main determinant of fertility behavior is the living conditions at the place of arrival rather than the geographical and cultural origin. Eggerickx also emphasizes that the beginning of the demographic transition coincided with the economic crisis of 1873–1892. However, until now the relationship between changes in demographic behavior and economic upheaval remains unclear (“The Fertility Decline in the Industrial Area of Charleroi During the Second Half of the Nineteenth Century”). The social network should probably have played a key role during that period of economic crisis. Indeed, the importance of a dense social network clearly appears as far as the illegitimate fertility in Leuven during the economic crisis of the mid-nineteenth century is concerned. Jan Van Bavel demonstrates that the risks of pregnancy before age 26 and subsequent marriage chances did not result from isolation in town (Leuven), but that sexual activity of unmarried women of courtship age was, on the contrary, a sign of integration within the local community. However what was the role of the economic crisis on the behavior of these women? (“Malthusian Sinners: Illegitimate Fertility and Early Marriage in Times of Economic Crisis: A Case Study in

Leuven, 1846–1856”). Leuven’s urban society in the nineteenth century is also the place to explore the relation between age homogamy and the increasing importance of romantic love. Bart Van de Putte and Koen Matthijs question Shorter’s theory by demonstrating that romantic love did not involve the lower classes. The only clear cultural change in Leuven was the spread of what is today called “a conservative model of marriage life” in which the patriarchal tradition was mixed with new family centered values (“Romantic Love and Marriage. A Study of Age Homogamy in Nineteenth Century Leuven”). This model of marriage behavior seems to correspond to the Catholic Church’s doctrine on matrimonial matters. The Belgian Catholic Church managed quite well to adapt itself to social changes of the nineteenth century (Paul Servais, “The Church and the Family in Belgium, 1850–1914”). Mortality has attracted fresh research. Michel Oris and George Alter explore the relationship between migration to the city and mortality pattern. In industrial towns, migration had a positive impact on mortality in the short-term, because the newcomers were healthier than natives of the same age. However, the place of arrival—the new industrial milieu—rapidly affected the children of the migrants who were disproportionately exposed to urban epidemiological conditions. Alter and Oris stress the existence of a “epidemiological depression” between 1846 and 1880, which will need further investigation. Moreover, migration to the industrial cities was at the origin of a specific pattern of mortality: high level of infant and child mortality, lower level of adult mortality (“Paths to the City and Roads to Death: Mortality and Migration in East Belgium During the Industrial Revolution”). The persistent high level of infant mortality at the turn of the twentieth century is confirmed by Marc Debuisson’s enquiry covering the whole territory of Belgium (“The Decline of Infant Mortality in the Belgian Districts at the Turn of the Twentieth Century”), meanwhile Jeroen Backs observes an increasing discrepancy between upper classes and poor people in front of death. The inequality results from a growing infant and child mortality (“Mortality in Ghent, 1850–1950: A Social Analysis of Death”).

On a methodological point of view, this series of fresh research offers new methods applied in a critical way (Alter and Oris). The life-course analysis is critically used by Devrieze and Vanhaute of (Anouk Devrieze, Eric Vanhaute, “Working-Class Girls. The Life-courses of 33 Women Cotton Workers in Ghent Around 1900”); event history analysis (Van Bavel); cluster analysis (Debuisson). This special issue on Belgian Historical Demography has a strong coherence and offers new perspectives of research for the following years.

RENÉ LEBOUTTE, *University of Aberdeen*

Economic Change and the National Question in Twentieth-Century Europe. Edited by Alice Teichova, Herbert Matis, and Jaroslav Patek. Cambridge: Cambridge University Press, 2000. Pp. xvi, 433. \$74.95.

The connection between nationalism and economic development is an important subject, and the contributors to the volume before us are to be commended for tackling it. But the significance of contributions to edited volumes in humanities and the social sciences rarely extends beyond their symbolic function—of serving as memorabilia to the rites and ceremonies in which scholarly conviviality finds its chief expression. Thus they are forgotten almost as soon as they appear in print, similarly to the elegant menus of formal dinners and wedding invitations, and for all intents and purposes are lost to the world of learning. Their only chance to escape this sad fate in most cases depends on the clarity, originality, and persuasiveness of the editors’ vision, which may claim and hold the reader’s attention,

while creating a conceptual framework within which each individual essay acquires an added meaning. The editors of *Economic Change and the National Question in Twentieth-Century Europe* fail to provide such a framework, and the result is a collection of historical trivia with no more intellectual interest than any limited amount of raw data awaiting an interpreter.

The vision of the editors is confused, vague, and at the same time strikingly unoriginal. They claim to address “a large gap in our understanding of the historical relationship between the ‘national question’ and economic change” (p. i). This presupposes on their part at least a rough idea of what the “national question” is. However, their use of the phrase suggests that they have no idea at all. They write that “during its history, nationalism [was] intrinsically connected with the national question” (p. 1), which, one assumes, could only be written if the authors were unaware of the obvious tautological nature of such a statement and instead believed that nationalism and the “national question” were two independent empirical phenomena. They claim that “in countries of western Europe the national question has, generally, not been accompanied by frequent eruptions of violence” (p. 2), which, unless the three historians forgot about the French Revolution (and a few more revolutions following upon its heels) that accompanied the formation of national consciousness and identity in France, must mean that they consider the “national question” to be independent also from the phenomena of national consciousness and identity. But it appears that they identify the “national question” (or nationalism, or nationality?) with ethnicity or race; otherwise it is impossible to understand their characterization of Germany as “nationally largely homogeneous” (p. 4). Unfortunately, ethnicity and race are not discussed, so we remain in the dark.

“While there are a good many publications on nationalism, including its political, cultural and religious background,” the editors write (p. 1), “the economic dimension [i.e., background?] of the national question has been little examined.” This statement—unless, again, we assume that nationalism and the national question are two altogether different phenomena—suggests a less than adequate familiarity with the “good many publications” mentioned previously, for the dimension dominating that literature is economic. Indeed, its central argument is a variety of economic determinism: it claims that, in the final analysis, nationalism (and the national question) is a product of economic change, specifically the development of capitalism as a result of the growing pressure of market forces. And, lo and behold, this is the position to which the editors of the volume also—though somewhat unconsciously—subscribe.

How firmly they believe in it may be gauged from their commentary on individual pieces in the collection. They characterize papers in line with economic determinism as “convincing,” but simply report on the findings that go against the argument, as in Joerg Roesler’s study of post-World War II Germany, which found no reflection of economic differences in the national consciousness in the East and West. It seems that the editors are not certain what such findings mean. They are quick to point out that “many authors show that the pressures of the market economy tend to sharpen national conflicts and bring them to the surface where they latently exist” (p. 3), but raise no questions about the essay of Bruno Fritzsche on Switzerland, where economic pressures do no such thing, as a result of which the author, instead of reconsidering the hypothesis, renames issues that in other contexts are called “national” “linguistic” (and thus removed from the volume’s discourse). The reason for such incongruities, possibly, is that this hypothesis is taken for an axiom, i.e., on faith before it is ever tested by evidence. This adds to the ritualistic character of the collection.

The aim of the volume, state the editors, summing up their introduction, “is to stimulate study and debate” (p. 7). Frustration is a stimulant, and there is no doubt that “a good

many” readers chancing on this book will feel frustrated. So, perhaps, the aim will be achieved, after all.

LIAH GREENFELD, *Boston University*

Von der Autarkie zum Wirtschaftswunder: Wirtschaftspolitik und industrieller Wandel in Italien 1935–1963. By Rolf Petri. Tübingen: Max Niemeyer Verlag, 2001. Pp. 534.

Italy’s transformation in little more than a century from a backward, agricultural periphery to one of the world’s leading countries is one of economic history’s success stories. Especially successful were the “economic miracle” years of 1950–1963, during which Italy maintained rates of growth second only to those of Japan and Germany and largely completed the structural transition to a modern, industrial economy. Rolf Petri argues that this success was built on a foundation laid in the 1930s and ’40s by the fascist policy of *autarchia*. Autarchy policy identified and gave a decisive push to precisely those industries that proved most dynamic in the economic miracle. Employing a wide range of tools, from macroeconomic policy, through administrative control of foreign trade and the allocation of credit, to direct participation in the form of state-owned enterprises, the fascist regime created and nurtured firms, supported research and development activities, and encouraged investment in new physical plant and licensing of foreign technology. While the author eschews any statements about whether *autarchia* was necessary, good, or efficient and does not hide its shortcomings and failures, the flavor of this account is nonetheless positive. This contrasts with most other assessments, which have deemed autarchy policy contradictory and *ad hoc*, the cause of enormous waste and gross misallocation of resources.

Petri argues for a new definition of the policy. Regardless of rhetoric about self-sufficiency for a coming war, *autarchia* was in fact the context-specific expression of an enduring Italian policy tradition, which Petri dubs neo-mercantilism. A common thread running through disparate analyses of politicians, entrepreneurs, economists, and “technocrats” over many decades was the imperative of endowing the country with a full range of modern industries in a situation of resource poverty, technological backwardness, and capital scarcity. If this was the true goal, then *autarchia* cannot be judged by Italy’s obvious failure to achieve anything like self-sufficiency. If this goal, rather than specific instruments such as tariff protection, was the constant, then the international opening of the 1950s (in the context of a changed economic context at home and abroad) was not such a policy revolution. If this was the common diagnosis, then the 1950s consensus among economic liberals and interventionist “technocrats” on the absolute priority of capital accumulation, with corollaries of wage moderation and fiscal restraint, is easier to understand.

The bulk of the book is a detailed account of the formulation of the autarchy plans, their implementation, and the evolution of the firms, technologies, industrial zones, and products they begot from the 1930s through the 1960s. The narrative focuses on autarchy policy’s targeted sectors: energy, chemicals, and metallurgy. This is a prodigious work of scholarship, based on original research in the archives of private firms (both Italian and German), of business associations, cities, and chambers of commerce, and of course of state-owned enterprises and government agencies. In addition, the author displays a confident grasp of the scientific principles underlying technical progress, and of an enormous body of literature spanning fields including business history, chemistry, political science, and the history of economic thought. The material is presented in chapters that are structured thematically, rather than chronologically or by sector. This organization does not facilitate the assimilation and retention of the profusion of detail provided, and may limit the book’s usefulness

as a reference. Individual chapters cover: the formulation of plans and policy instruments available; macroeconomic performance in the 1930s and the disastrous course of the war; efforts to secure supplies, processing, and distribution of energy; critiquing the critics of autarchy; measuring capacity expansion achieved; firms, institutions, products, and industrial locations developed under autarchy policy; industry-specific details of technological improvements introduced; and finally, the continuity of planning, policy, and personnel into the economic miracle.

Petri's argument contains two subplots that resurface periodically, but are never accorded a lengthy and explicit treatment, which leaves their importance to the overall argument somewhat uncertain. The first is the notion of "technocrats," whose knowledge and skills put them in positions of great influence, and who shared a group identity and ideology that shaped their actions. The second theme is corporatism. Petri claims that the proliferation of committees, consortia, cartels, institutes, secretariats, and the distinctive fascist *corporazioni*, despite their unclear, overlapping, and contradictory responsibilities, provided forums in which private interests could compete outside the market, resolve conflicts, compensate losers, and exercise influence over policy making through a bottom-up flow of information. Above all, consensus could be built and maintained through these institutions. The same interest mediation had to be carried on in different ways after the war.

Petri discusses methodological issues at length, rejecting equilibrium-based economic modeling, econometric efforts to quantify the determinants of growth, and counterfactual inquiry in general. Offering detailed critiques of specific pieces of literature, Petri asserts that these methods are inherently incapable of illuminating the nature of irreversible, unpredictable, path-dependent development. Schumpeter is invoked as offering a more appropriate analytical method. Consistent with this stance, the author confines himself to describing what actually happened in the defined areas of study. He notes whether the results seem to have been good or bad in an absolute sense, but refuses to be drawn into speculation about whether autarchy altogether was a good or a bad thing, whether there are lessons for other times and places. Some readers may find that frustrating, for this book demonstrates that if anyone has the breadth and depth of knowledge to address these important questions, it is surely Petri.

BRIAN A'HEARN, *Franklin and Marshall College*

The Effects of Competition: Cartel Policy and the Evolution of Strategy and Structure in British Industry. By George Symeonidis. Cambridge, MA: MIT Press. Pp. x, 542. \$55.00.

This book provides a detailed investigation of the impact of the 1956 Restrictive Trade Practices Act on the intensity of competition in the United Kingdom. In my experience, American economists are usually traumatized to discover that the 1956 Act allowed firms to register restrictive trade agreements, including explicit schemes to fix prices and that by the end of 1959, firms had registered 2,240 agreements in the clear expectation that the newly established Restrictive Practices Court would take a relaxed view about these restrictions on competition. In fact, the Court generally took a tough pro-competition stance and after a few landmark cases where restrictive agreements were judged to be against the public interest, many of the registered agreements were abandoned voluntarily or modified substantially. This episode thus provides the raw material for assessing the impact of an intensification of competition on economic performance. George Symeonidis uses the theoretical framework of John Sutton to assess the effects of this intensification of competition. The main focus is on the upshot for concentration, although the effects on advertising

intensity, innovation, and profitability are also considered. Disappointingly, perhaps, there is no discussion of the effects on productivity.

After a brief introduction, Symeonidis sets out the details of the 1956 act in the context of the evolution of cartel policy in Britain. He then explains how industries were classified as collusive, competitive or ambiguous during the 1950s, largely on the basis of the information collected by the Restrictive Practices Commission. Symeonidis insists on classifying resale price maintenance as having no significant effect on competition, but apart from this, the classification seems appropriate, and is shown to be broadly in line with an earlier classification produced by myself and Nick Crafts (*Business History*, 2001). Use is also made of information on collusion during the 1970s, so that it is possible to construct a dummy variable for the change in the competitive regime between the two periods. This dummy variable plays a key role in the econometric analysis that forms the core of the book.

Using Sutton's framework, it is important to distinguish exogenous and endogenous sunk-cost industries. In the former, firms are seen as playing a two-stage game where in the first stage, firms decide whether or not to enter, paying an exogenously given sunk cost of entry, which can be thought of as the fixed capital stock. In the second stage, firms that have entered compete by setting prices or quantities. In exogenous sunk-cost industries, an intensification of competition such as that following the 1956 Restrictive Practices Act, is expected to lead to a rise in concentration. Using a detailed data set from the *Census of Production*, exogenous sunk cost industries are identified by excluding industries with high levels of endogenous sunk costs in the form of advertising expenditure and R&D spending. Symeonidis then uses econometric analysis to demonstrate the existence of a positive relationship between the change in competitive regime and concentration. His results suggest that the intensification of competition after the 1956 Act increased the five-firm concentration ratio (CR5) by 6 to 7 percentage points in exogenous sunk-cost industries.

Endogenous sunk-cost industries are analyzed in two chapters dealing respectively with advertising-intensive industries and with R&D-intensive industries. Firms in these industries are seen as playing a three-stage game. As before, the first stage is the decision to enter. The second stage involves the decision to incur an endogenous sunk cost in the form of advertising or R&D spending, and in the third stage, firms set prices or quantities. In advertising-intensive industries, Symeonidis finds evidence of an increase in concentration following the intensification of competition. However, this is shown to be consistent with a reduction in advertising expenditure. In R&D-intensive industries, there is also a positive effect on concentration following the intensification of competition. However, there is no evidence of intensified price competition having an effect on innovation, as measured by the number of innovations in the widely used SPRU data set.

The final substantive chapter shows that despite the reduction in the number of firms accompanying the growing concentration after the intensification of competition, there was no long-run effect on profitability. This is consistent with the findings of an earlier study by Denis O'Brien and others (*Competition Policy, Profitability and Growth*. London: Macmillan, 1979).

This book is clearly aimed primarily at industrial economists seeking to establish some regularities in the new intellectual landscape following the unravelling of the old structure-conduct-performance paradigm. Nevertheless, there are aspects of the book that will also appeal to economic historians, such as the use of case studies, the detailed appendix on collusive agreements by industry and the extensive data sets drawn from the *Census of Production*.

ASIA, LATIN AMERICA, AND PACIFIC

Sugar and Society in China: Peasants, Technology, and the World Market. By Sucheta Mazumdar. Cambridge, MA: Harvard University Press, 1998. Pp. xx, 657. \$49.50.

This thoroughly researched history of sugar in Qing China (1644–1911) was published at a fortuitous time. After nearly two decades of “China-centered” history, scholars are again situating China within a global context. In the early 1980s, when Sucheta Mazumdar began her research, many scholars were turning away from questions relating to China’s contact with the outside world in order to concentrate on developments within China. Reacting against an established body of scholarship that portrayed late imperial China as technologically stagnant, isolated, and impervious to change, historians set out to document the myriad social, political, and economic transformations underway in China prior to the “Western impact” (Paul Cohen. *Discovering History in China*. New York: Columbia University Press, 1984). The new comparativists build upon these insights. With a wealth of local and regional histories to draw upon, they are now returning to the old question of why China failed to experience its own self-induced industrial revolution (See, for example, the recent exchange of views in the *Journal of Asian Studies* 61, no. 2 [May 2002]: 501–662). Published on the upstroke of this reinvigorated debate, this book combines “China-centered” and comparative approaches to analyze why China, “universally acknowledged to be one of the most developed economies up through the mid-eighteenth century, paused in this development in the nineteenth” (p. 10).

Mazumdar’s reinterpretation of this “fundamental question” (p. 10) is carefully laid out in seven substantive chapters. The first details the history of sugar consumption from antiquity to the nineteenth century. Encyclopedic in scope, the chapter argues that consumption in China was low relative to nineteenth-century England and suggests that there were structural limitations on the expansion of a domestic market. The second chapter examines China’s active participation in global trade both prior to and after 1500, and the third reviews the literature on the technology of Chinese sugar manufacturing. These chapters demonstrate that there was no lack of investable capital in China nor was there technological stagnation. Indeed, Chinese sugar cultivation and refining techniques changed considerably over time, through both indigenous invention and selective appropriation of foreign technology.

Having undermined notions of Chinese isolation and stagnation, Mazumdar moves on in the next three chapters to present her central thesis. Following Robert Brenner, she argues that the unique structure of Qing property relations inhibited development. Sugar in southern China was processed on small plots by farmers who mixed various cash crops with subsistence agriculture. This small-holder economy arose in the seventeenth century when late Ming (1368–1644) peasant rebellions combined with early Qing legal policies to bring about a new trilateral relationship between the state, landlords, and tenants that privileged stability over innovation. To guard their tax base, the Qing essentially protected direct producers from the threat of expropriation, a security that was buttressed by cultural strictures against land sale enforced by strong lineages. Landlord incomes were assured by the state-supported practice of fixed rents, which also reduced landlord interest in managerial agriculture. Merchants dominated the terms of trade, exploiting peasants’ need for cash (to pay taxes and rent) by imposing a contract system that set purchase prices well below what the world market would pay, leaving merchants with no incentive to undertake the difficult task of changing property relations to take direct control of production. This fettered market, in turn, gave farmers no opportunity to maximize commercial production, and reinforced their tendency to maintain a hedge in unproductive subsistence agriculture.

On the individual level, all actors acted rationally to maintain or improve their economic positions but in the aggregate their behavior did not lead to economic development over the long term.

Although Mazumdar emphasizes the historical contingencies that gave rise to this unique peasant economy, she sees it as remarkably enduring once in place. Despite the many changes underway in nineteenth-century China, she argues that this structure persisted into the twentieth century. Chapter 7 details these transformations: the expansion of world trade; the weakening of the Qing state; unimpeded population growth; the rise of new types of elites, including strongmen and bandits; and the subsequent intensification of violence. Yet, Guangdong farmers continued to produce sugar for export until the early twentieth century when competition from industrialized producers forced them to withdraw from the world market. In contrast, the Japanese colonial regime on Taiwan successfully reorganized its sugar industry by sharply curtailing the independence of peasant producers.

Mazumdar's fusion of "China-centered" and comparative history is more successful for the early Qing than it is for the end of the dynasty. The discussions of China's place in the early modern world economy, its participation in trans-Pacific trade networks, and the attention paid to both local and global processes in the construction of the small-holder economy are among the best elements of this fine book. Overlooked are transformations in the triangular relationship between the state, landlords, and peasants brought on by the fiscal and political crises of the nineteenth century. For the Lower Yangzi region, Kathryn Bernhardt (*Rents, Taxes, and Peasant Resistance*. Stanford, CA: Stanford University Press, 1992) has found that the social relations of production shifted substantially after the Taiping Rebellion (1850–1864) when the late Qing intensified its involvement in rent relations and allowed taxes to rise. These alterations in state policy, continued by Republican-era governments, worked to the disadvantage of landlords and ultimately resulted in the collapse of landlordism altogether. In Mazumdar's account, such internal political transformations appear to have had no effect on property relations, although Japanese imperialism did. In the end, despite her laudable effort to transcend the "artificial dichotomy" of global and local history (p. 407), Mazumdar's contention that China "paused" on its way to modernity as a result of deeply entrenched and fixed indigenous social structures that required external shocks to break through to industrial development comes perilously close to resurrecting the earlier "Western impact" view of nineteenth-century Chinese history.

CAROL BENEDICT, *Georgetown University*

The Thai Village Economy in the Past. By Chattip Nartsupha. English Translation by Chris Baker and Pasuk Phongpaichit. Chiang Mai: Silkworm Books, 1999. Pp. viii, 131.

This charmingly old-fashioned little book was first published in Thai in 1984, and now appears in an elegant English translation. The two major intellectual influences that gave it birth are rather older, dating from the 1950s, 1960s, and early 1970s. The first developed from the intersection of the academic pre-eminence of varieties of Marxist thinking about the "Third World" and the struggles of anticolonial peasant-based revolutionaries and produced a high age of romanticism about the Southeast Asian village and the unfortunate victims who inhabited them. The origins of the second are more uncertain, but probably represent, paradoxically, a Thai appropriation of those Western social-science constructions of Thai cultural uniqueness which were especially popular in the 1950s and 1960s.

Together, these two influences led Chattip Nartsupha to produce a work of powerful simplicity. Claiming to “adopt the angle of vision of the ordinary villager” (p. v), he argues for the primacy, both morally and temporally, of the Thai village. The subsistence-oriented village seems always to have existed and, and it went about its central task of growing rice, to have enjoyed autonomy, economic and social self-sufficiency, harmony, solidarity, and mutual helpfulness—“a small community in which people help one another in a spirit of common humanity” (p. 73). The members of this unmodulated peasant society all seem to speak with one voice, so that when Chattip commits their thoughts and views to paper, he is attempting “to recount the life history of the Thai people” (p. vi). Until the middle of the nineteenth century, this little paradise of innocent self-sufficiency survived relatively untroubled, able to absorb and domesticate the intrusion of Buddhism, and even to come to terms with an emerging Thai state that sought to tax its produce and labor but which refrained from more structurally disturbing assaults.

Western capitalist imperialism, which developed from the middle of the nineteenth century, proved an entirely different and much more destructive force, because it gradually but relentlessly changed the nature of (rice) production from its hitherto subsistence plane into a commercial activity. In so doing, it unleashed the unstoppable forces of modernization and its attendant evils—private property in land, wage labor, differentiation and specificity of function, indebtedness, greatly accelerated landlessness, the loss of self-sufficiency, and the breakdown of the sustaining values of the village community. Even under this onslaught, however, the Thai village proved resilient, even if in severely attenuated form, because the “parasitic form of capitalist development” (p. 58) it faced employed a “backward mode of production” (p. 56).

Much work done over the last few decades in Southeast Asia raises doubts about the accuracy and usefulness of this account. In Chattip’s primordial villages, there seem to be, generally speaking, no classes, no functional division of labor, no antagonisms, notwithstanding Chattip’s muted acknowledgment of the existence of slaves (“accepted as part of the family” (p. 73)) and landlessness. In his account, history happens because the “foreign and unnatural” (p. 74) forces of state and capitalism bear down on the unfortunate village, break its modes of life and its spirit, and leave it confused and dysfunctional. More recent scholarship, while acknowledging the deeply transformative impact of Western-sponsored capitalism and the modern state, is strongly inclined to dismiss romantic concepts of the unchanging, socially undifferentiated village as a later and convenient fiction. No matter where or how far back scholars look, they tend to find classes and clashes and winners and losers, rather than harmonious self-sustaining organisms. As well, they tend to discover incoherent or easily mutable senses of place, identity, and community, deeply entrenched trading networks, credit dependency, individual forms of landholding, specificity of function, tightly organized and jealously self-regarding labor exchange systems, and predatory, individualistic, and anti-social forms of commercial behavior. More generally, they tend to emphasise the role of both state and capital not in destroying “the village” but, rather, in consciously constructing and consolidating the village-as-entity to serve their particular needs.

Perhaps, as the Scott-Popkin debate of two decades ago demonstrated, the most interesting aspect of the problem is not so much what we find, but rather what predisposes us to search for and comprehend things in the ways we do. Just as Chattip’s book encapsulates the regnant discourses and theorizings of an earlier time, contemporary scholarship inevitably does the same. Which, of course, is not to say that it is not true.

R. E. ELSON, *Griffith University*

Institutions and Investment: The Political Basis of Industrialization in Mexico Before 1911.
By Edward Beatty. Stanford, CA: Stanford University Press, 2001.

After a long decay, in the last decades of the nineteenth century the Mexican economy experienced a process of accelerated growth mainly associated with the export sector. As the latter developed and diversified, new opportunities for investment opened in agriculture, livestock rising, and mining. Starting in the 1890s, this process was also accompanied by an early phenomenon of import-substitution industrialization, which would continue to unfold until the Mexican Revolution broke out in 1910. Although industrial growth was “limited in scope and fraught with inefficiencies” (p. 187), it appears as an uncommon experience in an era dominated by export-led growth in Latin America and as one that has attracted less attention than it deserves in the historiography on Mexico. This is the subject of Edward Beatty’s work.

The author tries to find out how this phenomenon was possible within a context that offered scant advantages for industrialization, and in spite of the many other investment opportunities that were available to entrepreneurs. According to him, the answer to this question lies in the institutional field, and more precisely in the adoption of a series of formal institutions that government consciously and consistently used to promote domestic industry as an important component of national development and modernization. The book departs from the more accepted views on this issue in at least two ways. First, it denies that industrial growth was only a byproduct of export-led growth and that it would have happened independently of the economic policy adopted by the regime. Second, it shows that, at least in the economic field, the long presidency of General Porfirio Díaz (1876–1880, 1884–1911) was not dominated by preferential treatment and discretionary decisions intended to benefit the small clique of his favorites and allies. Far from that, policy makers devised an array of policy institutions with the clear purpose of fostering industrialization, and then made a clear effort to administer these institutions in a reasonably equitable fashion. The exceptions to this rule were neither frequent nor decisive in terms of the final outcome.

Among a broad set of policy institutions designed to support economic development, Beatty chooses three that were directly concerned with industrial growth: the tariff, patent legislation, and an industrial promotion program called *Industrias Nuevas*. In addition to analyzing the formal aspect of these regulations, the author focuses on their administration and execution, as well as on the entrepreneurial response to them. Furthermore, he attempts to determine the link between these policies and industrial growth, that is to say whether, and to what extent, the latter was a result of the former, or if it should be considered an independent phenomenon. This is not easily achieved for these three cases of policy institutions, as it is always difficult to disentangle the many contributing factors leading to a particular outcome, or the specific role played by a single one. Nevertheless, the author offers an impressive array of quantitative evidence, historical reconstruction, and economic analysis that lives up to the complexity of this task.

The findings of this research are somewhat unexpected. Although the three policy institutions analyzed were expressly created to promote industrial growth and were administered in a relatively efficient and impersonal fashion, they did not always achieve their intended goals. The most successful among these institutions, and the one that can be more clearly associated to the development of particular industrial activities, was tariff policy, which awarded protection to import-competing industries. A second instrument, embodied in patent legislation, was costly and inefficient, as it fostered monopolistic impulses and slowed the process of technological diffusion within the Mexican economy. A third strategy, a set of tax and tariff exemptions conferred through the so-called *Industrias Nuevas*

program, was simply irrelevant; most industries were, in fact, founded outside the scope of this program, and many that were supported by it did not become viable businesses.

Despite the uneven effectiveness of these policies, the author suggests that industrial growth was the result of the proactive vision by Mexican policymakers, who acted ahead of demand in providing incentives and institutional conditions for investment in domestic industry and, by these means, fostered industrialization (pp. 70, 187). The assertion is daring because the author finds only one of the three industrial policies proved to have a direct link to the creation and development of industries. With this in mind, just how important were policy institutions in promoting industrial growth in late nineteenth-century Mexico? Why did two of the strategies aimed at achieving industrialization fail? Was it because they were ill conceived or badly executed? Or, as the author suggests, because the obstacles to industrialization were still powerful and hard to overcome? However, if this is the case, why, then, did some degree of industrialization take place anyway? In sum, beyond its originality and its important findings, this book is an invitation to continue exploring the role of policy institutions on economic development, particularly in those late-developing countries that had to overcome serious obstacles in their pursuit of industrialization.

SANDRA KUNTZ FICKER, *Universidad Autónoma Metropolitana-México*

The Emergence of a National Economy: An Economic History of Indonesia, 1800–2000.

By Howard Dick, Vincent J. H. Houben, J. Thomas Lindblad, and Thee Kian Wie. Crows Nest NSW, Australia: Allen and Unwin, 2002. Pp. xvii, 286. \$38.00.

Written by four leading economic historians of Indonesia from three continents, this book is an excellent account of the emergence of the Indonesian economy in the twentieth century from what was a cluster of disparate economic regions at the beginning of the nineteenth century. Using an innovative and, in the context of Indonesia, highly appropriate theme, the authors identify three fundamental forces that shaped the emergence of the Indonesian national economy: successive waves of globalization (and dislocation), state formation, and economic integration. The book is admirably successful in fulfilling its claim, not an easy task given the volume of literature that had to be mastered and put into perspective in order to comprehensively describe this process.

A fundamental problem in the study of Indonesian economic history is the concept of the Indonesian economy. To this day, economists draw a distinction between the now densely populated and natural-resource-poor Java and the sparsely populated and resource-rich Outer Islands, which include, among others, Sumatra, Sulawesi, Kalimantan (Indonesian Borneo), and Irian Jaya (Indonesian New Guinea). Statistics produced by the Central Bureau of Statistics of the Indonesian government still reflect this dichotomy. When did these two broad regions coalesce into the Indonesian national economy, if at all they did? What forces contributed to and shaped this process? How did politics and economics interact with each other during the emergence of the Indonesian economy? These important questions are systematically approached and answered in this book.

Organized into seven chapters in a manner that is logically consistent with the overarching theme, the book begins with a discussion of notions of the state, the nation-state, and the national economy. Following this is a discussion of the pre-modern economies of the Indonesian archipelago. These chapters form the entree to the core of the book, which includes, in succession, chapters on the economies of Java in the nineteenth century, the economies of the Outer Islands in the nineteenth century, and the late colonial state and

economy under which the two sets of economies coalesced into a national economy. Although each author brings his own style to his chapter(s) and these styles are noticeably different, the book has an engaging quality that will stand it in good stead as (one hopes) it is adopted as a standard text on the economic history of Indonesia.

Like any ambitious work of this kind, the book does have one or two flaws, but these seem somewhat trivial in comparison to the task that the authors have accomplished with such success. In the hopes that there will be future and more finely tuned editions of this fine book, these nevertheless merit mention. The first is the disproportionate level of attention paid to subjects that other scholars have considered relatively more or less important to the study of the economic history of Indonesia. One that jumps to mind is the series of papers and books in economics, history, and demography on the growth of the population of Java, a subject to which are devoted only two pages of systematic discussion (pp. 61–63), and which likely played a fundamental role in the later linking of Java with the Outer Islands on the basis of comparative advantage, specialization, and interisland trade, with Java focusing on labor-intensive production. The second is the almost-complete omission of U.S.-based sources on Indonesian economic history and political economy, such as the journal *Indonesia*, in which dozens of potentially relevant articles have been published, and leading mainstream economics and economic history journals such as the *Journal of Political Economy*, this JOURNAL, and *Explorations in Economic History*.

Although this book has deliberately been written to appeal to a nonquantitative readership, it nevertheless sets an interesting agenda for quantitative research for the quantitative economic historian. Questions that await analysis include, first, the quantitative definition of a national economy. This requires the measurement of the degree of integration of regional economies, both with each other and with the outside world, with a statement of and argument in support of some criterion that can justifiably be used to determine when a group of economies crosses the threshold from being disparate to being a single “national” economy. In the context of Indonesia, this quantitative analysis could fruitfully be supplemented with analysis of the political and economic factors that determine the location in time of this threshold. An exercise of this kind should be of interest to economic historians in general (one can apply this to the analysis of any economy) and to scholars of Indonesian economic history and economics in particular.

It is no coincidence that it has taken the work of four of the best minds working on Indonesian economic history to pull such a project off successfully. This is a contribution of tremendous significance for the study of the economic history of Indonesia, and it should be welcomed and read by scholars and students of economic history, Indonesian and other, alike.

SIDDARTH CHANDRA, *University of Pittsburgh*

Domestic Service in Australia. By B. W. Higman. Melbourne: Melbourne University Press, 2002. Pp. xvi, 358.

When I first arrived in Australia as a backpacker in the mid-1980s my job possibilities included negotiating a job as a governess on a remote station through an agency, and, when a café proprietor offered me a job, just a few minutes later finding myself alone in her house confronting a vast mound of laundry and other housework with no terms discussed and no prospect of lunch. I had applied to be a waitress but I felt like a slave. I did not know much about Australia or service jobs at that stage and neither of these positions stuck. I never worked in a bar although the Aussie barmaid might best illustrate the Australian stereotype of female service as Dianne Kirkby has shown in her recent work (*Barmaids: A History of*

Women's Work in Pubs, Melbourne: Cambridge University Press, 1997). It is interesting to place such experiences in the context of Barry Higman's excellent new book. For all the male and macho impressions of the pioneering male conquering the alien landscape of the outback, in fact colonial Australia had a very high proportion of women in the workforce. Yet the concept of service somehow sits oddly with the egalitarianism of Australian culture.

There has been a recent revival of interest in domestic service in other parts of the world. For eighteenth-century England, for example, both Tim Meldrum and the late Bridget Hill have recently published books on the subject (*Domestic Service and Gender 1660–1750*, London: Longman, 2000; and *Servants: English Domesticity in the Eighteenth Century*, Oxford: Oxford University Press, 1996) and Leonard Schwarz has challenged our ideas about sex ratio of servants in nineteenth-century England ("English Servants and their Employers during the Eighteenth and Nineteenth Centuries," *Economic History Review* 52, [1999]: 236–56). Higman, better known for his research and writing on slaves in the Caribbean, has now turned to this under-researched topic in Australian history and presents us with an authoritative, well-written, and very substantial history. The book is exhaustively and systematically researched and there are numerous tables and graphs to back up his analysis. Near the outset, Higman explains that he has favored such analysis over use of the case study. Yet certainly in the later chapters of the book—and especially the one on deference and defiance—there are a number of case studies and they effectively enlighten the text. Otherwise at times the investigation is a little too methodical and "objective" to be lively. There are, however, interesting juxtapositions between the nineteenth-century situation and the contemporary resurgence in service opportunities.

By the nineteenth century Australia seemed replete with servants, yet their history is very different from service in the United Kingdom or the United States as it does not closely follow the trajectory of economic development and far greater numbers of males are employed. In a comprehensive series of figures and tables Higman gives us numbers, distributions, unemployment patterns, age structures, immigration trends, and several illustrations including floor plans. Indeed the interior geography of service and the location of service within the developing homesteads is fascinating.

This book is an interesting intervention at a time when several of the issues it raises engender lively debate. Egalitarianism and the traditional Australian "fair go" are under review in an age of boundary protection and concern about refugees in a country still widely believed to be underpopulated and to need more skilled workers. Higman also looks at aboriginal service in detail. This is linked with the recent arguments about the "stolen generations" in a situation where, throughout the nineteenth and twentieth centuries, many aboriginal girls were taken from their families and placed in institutions to train for service-type jobs as necessary stages on the road to racial assimilation. This book is therefore welcome not only for the wealth of historical detail that it presents but also for the timely aspects of the material that it contains.

PAMELA SHARPE, *University of Western Australia*

UNITED STATES AND CANADA

The Deadly Truth: A History of Disease in America. By Gerald N. Grob. Cambridge, MA: Harvard University Press, 2002. Pp. 349. \$35.00.

Despite its somber title, *The Deadly Truth* is a very lively account of American disease history from prehistory to the present. So much research is summarized in this compara-

tively short book, that it becomes the best introduction to the subject currently available. The chapters are chronologically arranged; they focus on those diseases that have been leading causes of sickness or death over the last four centuries. Not surprisingly the epidemic and insect borne diseases receive the lion's share of attention; but the occupational diseases are given a chapter of their own. The chronic diseases, which dominate the twentieth century, are reviewed in chapter nine. The author's general conclusion is that although a certain amount of progress has undoubtedly occurred in the treatment and management of specific diseases, there will never be a "final victory" over disease and death. In the present context few would disagree.

As a traditional historian, Grob is better at describing than explaining the past, partly because he sees disease history as a choice between social construction (in which perception and interpretation are privileged) versus "the biological reality of disease" (p. iv). Grob chooses reality, which to him means tracking morbidity and mortality trends over time, and relating them to changing social, environmental, and behavioral factors. This choice is somewhat misleading. It implies that some aspects of disease history are free from problems related to the perception of what is biologically real, and thus the influence of perception on the creation of data about causes of disease and death, not to mention their interpretation for policy purposes.

Death comes as close as possible to being an aspect of reality that is minimally influenced by perception. In any time and place the living have found it relatively straightforward to distinguish themselves from the dead. But the history of sickness and disease among the living is a topic notoriously subject to perceptual influences that reflect religious convictions, the level of scientific knowledge, and the social and economic interests of the living, sick or well. Moreover, since the author repeatedly argues that we do not really know much about the "etiology and physiology of disease", choosing biological reality over perception does not seem to leave us with much. Consistent with his pessimism, Grob modestly insists that his generalizations about the past are necessarily tentative and probabilistic" (p. x).

Fortunately, as he reviews the best research available, the author overcomes his pessimism; on the whole he demonstrates how much we can and do know with a reasonable degree of biological certainty about the past. But because Grob's focus is primarily epidemiological, he pays relatively little attention to the economic side of disease history.

There are two exceptions. Chapter 7, "Threats of Industry," is a valuable summary of the history of occupational diseases, most particularly those which disabled or killed adults during industrialization. Grob describes how factory work created new health problems, and how those problems were either ignored or addressed by manufacturers and physicians in different contexts, given their similar or different material interests. Nevertheless, he concludes that no definitive verdict can be reached about the impact of industrialization on the health-related welfare of those directly involved in it (p. 179). As some things got better (higher wages) some things got worse (working conditions) resulting in no net effect on welfare.

Earlier in Chapter 6 ("Expanding America, Declining Health") the author provides an explanation for what economists regard as the paradox of development. It is now generally accepted that as the United States began to develop in the early nineteenth century it became a wealthier but less healthy country. From circa 1820 to 1860, mortality increased and mean adult height fell (pp. 122–23). Grob's explanation starts with the fact that eighteenth-century Americans were already exceptionally tall and long lived because environmental conditions protected them from frequent exposure to disease (p. 123). As markets expanded and transportation improved, the relative isolation between communities in settled areas broke down; as a result infectious diseases broke out more frequently,

spread more rapidly, and took more lives, even in rural environments. In addition, children were sent to school and adults worked in larger scale industries, both of which increased exposure. On the frontier migrants were struck down by the same diseases that had attacked the first Europeans who began to settle the East Coast in the seventeenth century, including dysentery and malaria. (See chapter 3.)

Because some infectious or vector-borne diseases damage the fetus or kill the mother, infant mortality was bound to rise: (p. 151). Malaria is an insect-borne disease that is particularly harmful to both pregnant women and babies. Grob stresses that because malaria was spreading rapidly in many frontier areas during the nineteenth century, health was bound to suffer, even if standards of living were high. Among the very young, frequent infections retard growth by investing calories in fighting infection and repairing the damage done. Meanwhile, the loss of appetite induced by sickness compounds nutritional problems among growing children. In some cases young hearts can be damaged and lung capacity reduced, which can lead to premature chronic disease in adulthood. Grob is aware that although some evidence suggests that there may have been a decline of meat consumption in the antebellum period, he believes that real wages were rising, not falling, at this time (p. 150).

Whether or not readers agree, they will find valuable information and stimulating ideas in every chapter of *The Deadly Truth* about disease history and its relationship to the settlement and development of the United States.

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

La Harpe's Post: A Tale of French-Wichita Contact on the Eastern Plains. By George H. Odell. Tuscaloosa: The University of Alabama Press, 2002. Pp. xx, 369. \$29.95, paper.

In 1719, French commander Jean-Baptiste Benard, Sieur de la Harpe, together with nine men, ventured into the Prairie Plains, a region previously unvisited by Europeans, to establish trading alliances with the Plains Indians. His journey took him to what is now eastern Oklahoma, where his men spent ten days at an Indian village interacting with peoples of various tribes who later merged into the Wichita tribe. The village was the principal home of the Tawakoni, the most powerful of the Wichita-related tribes. In *La Harpe's Post*, George H. Odell documents the initial contact between La Harpe's party and these peoples, thereby potentially adding a crucial chapter to the history of the Plains Indians during the early eighteenth century. The information regarding the native population is considered the most significant consequence from this event. Odell takes much of his information from findings obtained during an excavation of the presumed site of the Tawakoni village. Odell ambitiously uses this information to write a reevaluation of the general history of the eastern Plains during this period. The technical aspects of the archeological research, such as the excavation data and statistical analyses, are located in 11 appendixes, as his book is intended for a wide audience.

Despite Odell's attempt to write a general narrative on the protohistoric period in eastern Oklahoma, most of the book's conclusions involve the specific characteristics of the Tawakoni village. This locale was a self-sufficient village inhabited throughout the year by several groups of Plains Indians. The recovery of charred corn kernels indicates that these Indians were at least part-time farmers. Other features of the site (such as large storage pits, fire pits, and shell caches) suggest that they were semisedentary: the Indians ventured onto the Plains mainly to hunt for bison. The chert used in stone tools production as well as the clay attributes in the Indians' pottery point to contact with far-away peoples. The establishment of long-distance trade is

further supported by the abundance of European trade goods alongside the Indian artifacts. Still other remains, including smashed metates (stones used to grind corn), suggest that the Indians left this village under duress sometime around the year 1750.

The contributions of this book to the literature on early European contact are mainly aimed at professional archeologists. Professor Odell adds much to the debate between archeologists and ethnohistorians concerning the precise location of La Harpe's terminus. Instead of relying solely upon directions given in La Harpe's travel diary, as past researchers have done, Odell relies on artifacts located within the probable region of initial contact. The excavation findings, along with an occasional reference to the documented history of this period, create his interpretations on the activities of the inhabitants: Odell states, "I cannot think of another archaeological site I have investigated that contained so much information at this degree of specificity" (p. 139). Still, the conclusions drawn from the site report do "appear pretty mundane to the uninitiated" (p. 139), in that gender roles, trends in domestic trade, and institutional frameworks cannot be established using such findings.

The author's decision to refrain from using primary documents in his analysis is an obvious yet crucial shortcoming. He explains his desire to avoid engaging in ethnohistory, but his reasons for taking this stance are not persuasive. For example, on page 13, Odell writes, "Our modern mentality induces us to compartmentalize past people into mutually exclusive units . . . The process of fissioning and fluidity of social groups makes definitive classification difficult." Of course social groups are continually changing; however, this does not mean such classifications are not meaningful. Most ethnohistorians appreciate that culture changes over time, and that one must apply cultural data from a later date to earlier times only with great caution. For example, Charles Hudson's analysis (*Knights of Spain, Warriors of the Sun*. Athens: The University of Georgia Press, 1977) of the initial contact between Spaniards and Southeastern Indians successfully gleans historical and archeological sources to connect ethnicity and material culture. Because Odell is convinced that the Tawakoni were Wichita-related, such an application appears worthwhile: after all, it is not as if the Wichita suddenly reinvented their culture.

As for Odell's writing, it suffers from redundancy and needless digressions. The tale of the Nasoni interpreter's affection for a local Indian is mentioned on three separate occasions. The discussion concerning the use of a belly load operator in excavation and the history of the property seem pointless. As a member of the "uninitiated," I take the author's word that the scope of this analysis far exceeds that of a traditional site report; however, the focus is still too narrow to appeal even to those doing research on Native American economic history.

MATTHEW T. GREGG, *University of Georgia*

Money, Trade, and Power: The Evolution of Colonial South Carolina's Plantation Society.

Edited by Jack P. Greene, Rosemary Brana-Shute, and Randy J. Sparks. Columbia: University of South Carolina Press, 2001. Pp. xiii, 400. \$49.95.

The "problem" of South Carolina has long fascinated historians of the antebellum period, particularly political historians. Why were Palmetto State politicians always so fiery, confrontational, and eager to come to blows? Many fine scholars have attempted to answer such questions over the years, and, as a result, we know more about the politics of South Carolina than we do about the politics of any other state in the antebellum South.

Scholarly interest in earlier periods of South Carolina's history was much slower to develop. Indeed, until recently, historical scholarship on both South Carolina specifically

and the Lower South in general suffered in comparison to that on other parts of British North America, and arguably, even the British West Indies. Since the 1980s, however, things have changed considerably. A number of important studies have appeared on colonial South Carolina, and, if we still do not know as much about the colony as we do about places such as Virginia or Maryland, we know a lot more than we did 25 years ago.

We know, for example, that slavery was extremely important in South Carolina, and that the colony was the only one with a black majority in British North America. We know that by the middle of the eighteenth century South Carolina had become an exemplary mercantilist colony, exporting agricultural staples and deerskins in significant quantities to the mother country in exchange for imported manufactures and shipping services. We know that on the eve of the Revolution South Carolina was by most standards the wealthiest colony in British North America by a wide margin. We know something about wealth distribution in the colony, and perhaps even more about consumption patterns, especially in Charleston, the capital city. If some questions about South Carolina's early development remain unanswered, the main contours of the colony's history are now a lot clearer than they used to be.

With the publication in 2001 of *Money, Trade, and Power*, such contours are clearer still. This volume, published under the auspices of the College of Charleston's Carolina Lowcountry and Atlantic World Program, showcases the work of 15 scholars, most of them quite young, who represent what might be called the "second wave" of modern scholarship on colonial South Carolina. If the authors' findings do not fundamentally reshape the scholarly foundation built in the 1980s and 1990s, taken together, they contribute much to the construction of a stronger scholarly edifice, and, in so doing, fill in many gaps.

Unlike the case with many essay collections, this volume's thrust is actually captured pretty well in the title. Money, trade, and power—along with slavery, the taproot of the same—are what this collection is all about. In his tight, little introduction Jack P. Greene offers a useful breakdown of the volume's contents: three chapters on the early settlement of South Carolina and the establishment of the colony's political-economic order; three chapters on trade; five chapters on various aspects of slavery in South Carolina, and four chapters on social and cultural developments. If there is some unevenness to the volume, most of the essays are well executed, some exceptionally so.

For my money (trade and power!), the best essays included are those by Bertrand van Ruymbeke, R. C. Nash, and S. Max Edelson. The first two provide important new information about Huguenot migrants and mercantile life respectively, and the third lays out a sophisticated and eminently plausible approach to understanding the behavior of skilled slaves in the colony. A fourth essay, by Robert Olwell, rescues an important political actor—the Justice of the Peace—from undue scholarly neglect, highlighting the JP's role in the control of slaves in the colony. A number of essays contain every piece of useful economic data—Stephen G. Hardy should be singled out in this regard—and others treat important topics in innovative ways. Matthew Mulcahy's stimulating essay on the social and cultural effects of natural disasters in South Carolina—fires, earthquakes, hurricanes, epidemics, and the like—would fall into the latter group.

On balance, this is a very good collection of essays on the development of an important part of British North America. To be sure, some readers of this JOURNAL may find the essays a bit too empirical, atheoretical, and *ad hoc* for their tastes: almost all of the contributors are traditionally trained historians and their contributions reflect their training. This said, almost any open-minded reader can learn a good deal from this book. Greene, Rosemary Brana-Shute, and Randy Sparks have given voice to a group of talented young scholars, who, for their part, have justified the editors' faith in their work.

PETER A. COCLANIS, *University of North Carolina-Chapel Hill*

Neither Lady nor Slave: Working Women of the Old South. Edited by Susanna Delfino and Michele Gillespie. Chapel Hill: The University of North Carolina Press, 2002. Pp. 324. \$55.00, cloth; \$19.95, paper.

Each of the 13 essays in this volume considers an aspect of female participation in the paid or unpaid labor force of the antebellum American south. The work of ordinary women is the theme that unites the essays, work the editors in their introduction note was often unacknowledged due to prevailing and evolving attitudes about women's proper work and the role of the male head of household as the family breadwinner. The essays vary widely in their scope, but share a search for ingenious sources of information, a search necessitated by the invisibility of women in official and more conventional sources. The topics range from Native American female makers and sellers of baskets to antebellum female iron manufacturing workers, from coastal Savannah slave women participants in produce markets to western Virginia businesswomen, from Richmond prostitutes to New Orleans nuns.

Several of the essays make extensive use of newspaper sources. Stephanie Cole's "A White Woman, of Middle Age, Would Be Preferred: Children's Nurses in the Old South," uses ads seeking nannies in the urban south as evidence about how evolving attitudes concerning motherhood affected the age and race composition of these workers. In "Patient Laborers: Women at Work in the Formal Economy of West(ern) Virginia," Barbara J. Howe combines ads for new businesses with census and credit agency ratings in four cities of that region to determine the types of businesses that women established and ran.

Susan Barber, in "Depraved and Abandoned Women: Prostitution in Richmond, Virginia, across the Civil War," also relies on newspaper as well as court records to uncover information about this oldest of occupations. She finds a mixture of racial relationships, considerable wartime activity, and a range of financial outcomes for the participants. In stark contrast, two essays depict the work of antebellum religious orders. Using convent archives, Emily Clark examines the financial strategies pursued by the New Orleans Ursulines, especially those occasioned by the order's ownership of slaves. This order's members were white and came mostly from affluent families, bringing with them dowries of property when they entered the convent. Members who joined Baltimore's Oblate Sisters of Providence also brought dowries to the order, but the Oblate Sisters were African, and often Caribbean, American women. Diane Batts Morrow's, "Faith and Frugality in Antebellum Baltimore: The Economic Credo of the Oblate Sisters of Providence," describes how this order, founded in 1828, supplemented income from its limited dowries with tuition from its school for free blacks and from its sewing of vestments for other religious orders. Among many other sources, Morrow derives information from the financial records of the order and from advertisements for educational and sewing services.

Three essays examine industrial work. Bess Beatty's "I Can't Get My Bored on Them Old Lomes: Female Textile Workers in the Antebellum South," depends primarily upon limited company records and diaries to determine that North Carolina textile firm operators tried various labor sources. She also finds the female labor force less docile and immobile than past stereotypes suggest. Michele Gillespie finds a more deliberate strategy by mill owners of hiring low-wage women for work in Georgia textile mills. She concludes that they used prevailing assumptions about race and gender to construct a working class to their benefit. In "To Harden a Lady's Hand: Gender Politics, Racial Realities, and Women Millworkers in Antebellum Georgia," Gillespie presents evidence of considerable experimentation as firm owners sought the lowest cost combination of inputs. In the textile industry, she finds that slaves were not the lowest cost inputs. In contrast, Susanna Delfino's "Invisible Woman: Female Labor in the Upper South's Iron and Mining Industries," notes considerable use of slaves in antebellum southern iron production. Although

census data do not indicate female workers, Delfino finds female names on the employee roles for various iron manufacturers. Her essay provides an interesting and detailed description of iron production in the charcoal fuel era, a description that clarifies the seasonality of production and the variety of labor skills required.

The essays in this collection are informative, interesting, and persuasive about the specific cases they examine. In attempting to erase the boundary between the public sphere of women's lives and the (socially constructed) private sphere, the essays necessarily find only limited information. Thus they are descriptive and their analysis is primarily qualitative rather than quantitative. The conclusions sometimes seem strong compared to the evidence, however ingeniously derived. *Neither Lady nor Slave* raises many interesting questions as it adds intriguingly to the evidence about women, work, and the antebellum south.

ANN HARPER FENDER, *Gettysburg College*

Selling Yellowstone: Capitalism and the Construction of Nature. By Mark Daniel Barringer. Lawrence: University Press of Kansas, 2002. Pp. viii, 238. \$29.95.

In this well written book, Mark Barringer provides an interesting and detailed history of commercial enterprises in Yellowstone National Park. The book has great value to scholars concerned with the management of public lands, the roles that interest groups (park employees, concessioners, tourists, and environmentalists) have played in the history of Yellowstone, and the difficulties in designing contracts for the private provision of goods and services on public lands.

The book proceeds chronologically, beginning with a brief account of the Yellowstone region's history from the early 1800s to the founding of the national park, the world's first, in 1872. It was in the late 1800s, when rail transportation made the previously difficult-to-reach park more accessible, that the park's commercial enterprises expanded from those of a few entrepreneurs to larger enterprises with hotels and stagecoach services. In the 1890s and early 1900s railroads and concessioners built grand hotels and elegant restaurants to serve their growing and predominantly affluent clientele. As the author explains, the park's administrators and concessioners, along with the railroads, marketed the park as a place for enjoying recreation, nature, and frontier history—all in relative luxury. Then, the growing popularity of automobiles brought a major transformation, including a large increase in the number of campgrounds, which appealed to the new wilderness-seeking, middle-class clientele. Following the Depression and World War II, the park experienced a dramatic increase in visitors, serious congestion, and, for the first time in over 50 years, generally poor service from the concessioners. In the 1960s, following a failed attempt by the park administration to force an expansion of concessioners' facilities, the family-owned enterprises that had dominated the park's commercial activities were bought out and replaced by larger firms. In recent years, the park administrators and commercial interests have continued to adapt to changing and sometimes conflicting demands, as illustrated by the prominent controversies over wolf reintroduction and snowmobiling.

Perhaps most interesting to economists are Barringer's detailed accounts of concessioner-government relationships. In economics terminology, one obstacle to designing efficient contracts arises because private firms making investments tied to public lands run the risk of a holdup (for example, the government may allow increased competition, demand additional investment, or not renew franchises). Another arises because the government cannot perfectly monitor firms granted monopoly franchises and, hence, cannot

ensure efficient levels of investment or output. From Barringer's accounts, it is clear that these problems have long hindered efficient policy in Yellowstone.

Although the book is carefully written and rich in facts, its repeated references to popular "mythology" and the "construction" of nature may strike economists as more distracting than illuminating. Barringer explains how the park's administrators and concessioners marketed the park in ways that fit the popular western, frontier, and nature mythology that they themselves helped to create. That the administrators and concessioners responded to changing demands is clear, but the importance of mythology is less so. Some myths to which the author refers (for example, that Yellowstone is unaltered wilderness and has been protected from commercialism) seem too obviously false to need much refuting. Although the park's literature has perhaps exaggerated the wildness of the park, this may merely reflect an effort to reduce the frequency with which visitors attempt to swim in boiling water or pet dangerous animals. Similarly, some of the Old West myths, which Barringer credits with contributing to the "constructed image" held by park visitors, are obvious fiction, yet others appear reasonable if viewed as stylized facts about the occupations and lifestyles of the people who moved to the Rockies in the 1800s. Furthermore, it is at least plausible that simple economic principles, independent of mythology, could explain many of the changes in the marketing of the park. Notably, over time incomes have risen, transportation costs have declined, and the country has developed more of its land. Thus, because the park provides normal goods (for example, viewing scenery, learning about animals and geology), because transportation is a complement to those goods, and because other undeveloped land is a substitute for the park's undeveloped land, the demand for the park's goods has increased over time. These shifts in demand have increased the economic rents generated by the park and, hence, the potential for ex post opportunism in contracts and the conflict between lobbyists for preservation and lobbyists for development. In my view, the existence of a simple economic explanation leaves the central importance of a constructed mythology debatable. Nevertheless, the history presented in the book is well done and, overall, the book's strengths greatly outweigh its weaknesses.

ROBERT K. FLECK, *Montana State University*

Regulating Railroad Innovation: Business, Technology, and Politics in America, 1840–1920. By Steven W. Usselman. Cambridge: Cambridge University Press, 2002. Pp. xv, 398. \$65.00 cloth; \$25.00, paper.

As its title suggests, the focus of this work is on the process and context of railroad innovation rather than its economic causes and consequences. The book reflects a wide reading in both primary and relevant secondary sources and the author's extensive use of the corporate records of the Pennsylvania and Burlington railroads.

Usselman chronicles "the prolonged attempt by Americans . . . to seize control over the most profound technological innovation of their lives: the railroad" (p. 1). As the quote suggests, he equates railroads with railroad technology, which leads to an extremely broad, sometimes diffuse, focus. The first chapter, for example, deals with antebellum railroad politics as especially reflected in the career of Abraham Lincoln. Regulating innovation, the author asserts, is a contradiction in terms, and he emphasizes the tension between managing existing railroad technology and further innovation. In nine chapters divided into three sections he traces this tension. Section One, "Assembling the Machine," focuses on development; this is followed by "Running the Machine," a period he characterizes as "engineering enshrined." Section Three, "Friction in the Machine," evaluates difficulties

arising from the triumph of the ideology of engineering efficiency over inventiveness. In short, railroad technology was strongly path dependent.

I found some of the generalizations confusing or not consistent with available facts. The shift from “development” to “management” that the author places in the 1870s is overdrawn given the enormous expansion of new road in the 1880s. There never was a time when the carriers were unconcerned with such management issues as engineering, testing, and so on. The author argues that by institutionalizing innovation in the late nineteenth century the railroads shunned “innovations that threatened to disrupt . . . rules,” but that they “attained far more impressive improvements in productivity than ever before” (p. 142). There was, however, a price to pay. Elsewhere he claims that the path taken “served to impede or even to foreclose alternative developments” (p. 6), that “the deadening inertia of the system threatened to freeze rail design” (p. 225), and finally that “techniques that had facilitated steady improvements in productivity . . . no longer generated returns so readily” (p. 330). This is an interesting argument. Unfortunately, the author fails to make a convincing case for any technological paths not taken that should have been, and the productivity evidence assembled by Albert Fishlow and John Kendrick does not reveal either sharp breaks or stagnation during the period covered by this book. Edwin Mansfield’s work does suggest that twentieth-century innovations diffused comparatively slowly, however (“Innovation and Technological Change in the Railroad Industry,” National Bureau of Economic Research Special Conference Series *Transportation Economics* [New York, NBER, 1965], pp. 169–96.)

Usselman also sees persistent dichotomy between engineering and economics that I find dubious. It is certainly true that engineering became enshrined about 1900 but not that the carriers “buffered engineering experts from the market” (p. 240) or that “engineering values . . . [substituted] for . . . market competition” (p. 241). In 1842 the civil engineer John Trautwine criticized the use of double tracking on a few lines claiming that “our engineers should construct their roads with a view to *paying* well instead of *looking* well,” and he argued that efforts to imitate English practice “must necessarily bring ruin.” (“Remarks on the Injudicious Policy Pursued in the Construction and Machinery of Many Railroads in the United States,” *Journal of the Franklin Institute* 33 [May 1842]: 307–16 and [June 1842] 370–80, quotation on 380, italics in original.) In the post–Civil War years engineers investigated the optimum durability of rails, the economics of steel versus cast iron wheels, and whether track stiffness was most efficiently purchased with more ties or larger rails, and they continued to do so throughout the twentieth century. In 1913 the chief engineer of the Missouri Pacific captured the economic approach to engineering, explaining that the company did not guard all bridges because “the saving to the railway would not have equaled the increased interest charge” (“Report of Committee 7—On Wooden Bridges and Trestles,” American Railway Engineering Association *Proceedings* 14 [1913]: 1136–43, quotation on 1138).

Despite such difficulties, the book contains valuable insights on the sources and nature of technological change. The discussion of patent pools to control innovation is richly documented and addresses some earlier criticisms of his work (Jeremy Atack “Comment,” in Naomi Lamoreaux et al., *Learning by Doing in Markets, Firms, and Countries* [Chicago: University of Chicago, 1999], pp. 91–101). Usselman also enriches the story of Atack and Brueckner (“Steel Rails and American Railroads, 1867–1880,” *Explorations in Economic History* 19 [1982]: 333–59) and others on the decision to adopt steel rails in the 1860s and 1870s. This is one of the many places where his archival research pays off. By delving into corporate records he presents a complex tale of “bounded rationality.” Even sophisticated carriers such as the Burlington and Pennsylvania never investigated the value of steel system-wide; rather, they introduced steel as the solution to intense wear on main lines; yet

company choices sometimes reflected their desire to build traffic in steel and raw material shipments.

For me the most interesting parts of the book were two chapters where the author focuses on the development of a network of institutions to manage technological change and their role in shaping the evolution of steel rails. As economists have recently stressed, technological change is a network phenomenon but few have emphasized the role of public institutions in development and diffusion. In railroading, institutional innovation occurred within firms as companies created test departments and managers to oversee technology, and industry-wide with the founding of technical societies and journals. The author fails to note that these institutional innovations had no counterpart in Europe; they probably reflected the size of the American market. The case study of how these networks evaluated and helped improve and diffuse new rail technology around the turn of the twentieth century is nicely done and emphasizes the importance of incremental technological improvements in productivity growth.

MARK ALDRICH, *Smith College*

Making Men, Making Class: The YMCA and Workingmen, 1877–1920. By Thomas Winter. Chicago: The University of Chicago Press, 2002. Pp. vii, 208. \$40.00, cloth; \$17.00, paper.

To comprehend how republican Victorians in the Gilded Age became liberal moderns in the Progressive Era we must grasp the tensions between gender and class in shaping identity. Thomas Winter in *Making Men, Making Class* aids in our understanding of this fundamental shift by providing a study of the middle-class men who ran the Young Men's Christian Association (YMCA). YMCA secretaries, Winter argues, attempted "to transcend class lines and unite men on the basis of manhood [which] ultimately led them to articulate new definitions of manhood structured by class difference" (p. 7). *Making Men* is the story of YMCA leaders' desire to quell working-class radicalism by promoting an idea of manhood rooted in hard work, loyalty to employers, and Christian fellowship.

The managerial revolution that accompanied the rise of corporate capitalism composes the backdrop for *Making Men*. Winter reminds readers that this economic transformation not only created a permanent wage working class, but a new middle class of men who realized they would never live the republican dream of becoming independent entrepreneurs. Although managing workers gave them some social status, their positions placed them in between the two visions of manliness the public held: the hardworking manual laborer and the financially independent capitalist.

Winter found that YMCA secretaries redefined the notions of character and personality in order to deal with this identity crisis. Character "became a quality required of workmen to fulfill their role as effective and efficient producers of commodities and services while deferring to their social betters" (p. 12). Although character represented something workingmen should have, personality "encapsulated all the qualities required of a YMCA secretary, qualifying him to guide workingmen on the path toward character through example, advice, and council" (p. 12). Thus Winter posits, "personality came to signify power, manliness, and dominance over others, whereas character came to symbolize faithful, productive work" (p. 145). The question becomes: how did YMCA secretaries use their personality to install character in workingmen? The answer came from the Cleveland YMCA in 1872 when Reverend W. H. Goodrich of the First Presbyterian Church created activities for railroad workers. From this beginning, YMCA secretaries created first a railroad division

then, in 1902, an industrial department. Industrial departments provided bible study sessions, English language classes, billiard rooms, and libraries to motivate character development. Skilled railroad workers, who held beliefs similar to those of railroad secretaries, responded positively to the YMCA's programs, but the majority of laborers maintained their own notions of manly identity.

Workers' distrust of YMCA secretaries, according to Winter, was central to their rejection of the organization's notion of character. Funding for the industrial departments came from Charles L. Colby, John D. Rockefeller, and other industrialists. YMCA secretaries could not overcome the paradox of needing to gain worker confidence and employers' money. At first, YMCA leaders attempted to stay neutral in owner-worker conflicts. Eventually, secretaries supported employers' positions and attempted to make their organizations surrogates for unions. In defining subservience to owners as the crux of character, and functioning as a welfare capitalist program, the YMCA failed to gain workers' trust. When elites recognized that the YMCA could not capture laborers "hearts and minds," they stopped funding the industrial departments. This irony, Winter suggests, defeated this new language of middle-class manhood.

The strength of *Making Men* is Winter's ability to capture this irony. He conveys the turmoil these hardworking YMCA secretaries experienced as they negotiated the stresses of their jobs and the challenges to finding their manhood. Throughout *Making Men* Winter tells readers that YMCA secretaries were not simply corporate cheerleaders, but sincere reformers, devout Christians, and principled men.

Thus, it is hard to believe they stopped trying to find their manhood when the industrial departments dissolved. The lack of any suggestion or analysis of what happened after the industrial departments faded raises a number of questions. Did these men look for new outlets to preach their interpretations of character and personality, or did they seek new definitions? If they understood themselves as failures, did that mean they felt emasculated or feminized? Did other middle-class men adopt these concepts of character and personality in their struggles for masculine identity? Despite wondering what happened to the YMCA secretaries' search for identity, Winter is right to inform his reader that even though they failed, their story is important. *Making Men* sheds new light on how middle-class men grappled with gender and class.

JOHN ENYEART, *Stanford University*

The Telecommunications Industry. By Susan E. McMaster. Westport, CT: Greenwood Press, 2002. Pp. xiii, 191. \$39.95.

Susan McMaster has provided a short and accessible overview of the history of the telecommunications (telephone, until very recently) industry from the beginning (1875) through the year 2000. Those looking for an introduction to the development of the industry will find the book quite readable. Those, however, looking for a review of the extant business-history and economic-history literature on the industry will not find it here, nor will they find a good roadmap on where to look for more information.

This history deals almost exclusively with the United States. The telecommunications industry is global, and because Americans (e.g., Bell, Edison) were responsible for the foundational inventions, there must have been at least one-way interaction between the U.S. industry and the rest of the world. Apparently any impact in the other direction was so small that the U.S. story can be told without reference to the rest of the world, and the U.S. story is the only one covered.

Approximately the last 30 percent of the text is concerned with the period after about 1984—the breakup of the Bell System. This emphasis on recent events is understandable given the upheaval in the industry that occurred during this period, and makes this book also useful to anyone wanting to understand the current structure of the industry. The book avoids normative judgments on current policy and sticks with a largely descriptive approach to the various legislative and judicial decisions that have been constantly reshaping the industry for the past decade and a half. A bit more economics in this part of the book would have helped make sense of this complex period.

The telephone industry is an interesting subject for study. It provides one of the longest case studies of an industry buffeted by disruptive technological change. Its economic characteristics make (or have made) the market allocation of its services problematic, and this has made the industry a perennial object for public-policy attention. It has undergone remarkable changes in its basic institutional structure. Alexander Graham Bell's original patents gave his company (AT&T and associated companies) essentially a monopoly during its first eight years (1877–1895). After that, large numbers of “independent” (that is, non-Bell) telephone companies went into operation, sometimes in competition with Bell and each other and sometimes providing service in new areas. This period saw an enormous increase in the availability of telephones. The return of Theodore Vail to the presidency of AT&T in 1907 began a period in which the company was able to reestablish its monopoly position roughly by the 1920s, partly with the aid of federal and state governments. Rate regulation of the type originally developed for railroads was extended state-by-state to telephones and made it difficult for new firms to enter into competition with existing providers of local service. The “Kingsbury Commitment,” with the U.S. Attorney General in 1913 and the Willis-Graham Act in 1921 prevented AT&T from acquiring all local telephone service, but also reduced the opportunities for competition and aligned the interests of AT&T and existing telephone companies. Unfortunately McMaster draws very little from business and economic historians (such as David Gabel, Louis Galambos, Richard Levin, Joan Nix, and David Weiman) whose work has advanced our understanding of this period.

The period between the 1920s and the 1970s was one of structural stability for the industry despite constant technological change. AT&T was able to maintain its position during this period in part by successfully maintaining control over the technology through research and technological leadership. In this way a single firm, AT&T, maintained a national monopoly over long-distance service and near monopolies over both local service and the manufacture of telephone equipment. With the active connivance of government, the prices charged by AT&T for myriad services departed substantially from the marginal cost of providing those services. When combined with control over overall profits, this meant that competition over its most lucrative services threatened the AT&T's ability to provide other services.

By the 1970s various competitors, both firms and technologies, had laid siege to the industry's structure. AT&T had lost its control over the technology and it also faced a changed political climate in which competition was seen as a better protector of public interest than regulated prices. Government regulation of the pricing of both trucking and airlines had already ended. Both of those industries, however, already had competition. The emergence of competitors to AT&T must have seemed as much opportunity as challenge. AT&T's attempted to change its pricing in the face of these threats. The resistance of regulators to permit that probably made fundamental restructuring of the industry inevitable. I think elementary price theory is essential to analyzing this situation, but it is absent from this book.

Unlike the cases of trucking and airlines, deregulation of telecommunications probably led to more, not less, government involvement in the industry. Instead of determining

prices, government agencies, including courts, were determining the range of activities that could be performed by individual firms—which transactions could occur intrafirm and which would have to be done in the market. A little more economics could have helped make more sense out of the complex positions and actions of firms and government in this recent period.

The Telecommunications Industry is a solid and accessible introduction to the history of this interesting American industry. It is as much compliment as criticism that I finished the book wishing it were more.

JOHN L. NEUFELD, *University of North Carolina at Greensboro*

Public Lands and Political Meaning: Ranchers, the Government, and the Property between Them. By Karen R. Merrill. Berkeley: University of California Press, 2002. Pp. xix, 274. \$50.00.

The objective of this book by Karen Merrill is to use the history of public land policy to better understand the political history of the American West since 1870 and the recurring tensions between government, ranchers, and environmentalists. Although the book is aimed primarily at political historians, it is a useful reference for economic historians interested in property rights and land-use regulations.

Merrill criticizes as overly simplified some generalizations by economists, such as that land regulators are “rent-seekers” or “captured” by those they regulate. Nevertheless, from the descriptive detail provided, at times at least, these labels would seem fairly appropriate.

Although previous studies have identified some relevant long-term changes in government land policy, such as that from priority in privatizing public land to setting it aside for resource conservation purposes, Merrill argues that more attention needs to be given to other structural, political, legal, administrative, and environmental changes. Especially important were the actions of the Forest Service after 1905, and the Department of the Interior after 1934, to restrict private access to public land and limit it largely to those ranchers owning private land and water adjacent to the public land. These measures undermined the distinction between rights to public property and private property, raised the question of who should be the owners of public land, and had the effect of raising the value of adjacent private land. As such, the proper regulatory policy on public land became subject to different interpretations by government officials and ranchers. The former deemed it their responsibility to prevent ranchers from putting up fences or any other investments on the public land. The ranchers, on the other hand, viewed limitations on their access to contiguous public lands as infringing on their private property rights.

The monograph consists of a preface, an introduction, six chapters, an epilogue relating the historical discussion to the more contemporary challenge of land policy by conservationists and the “sagebrush” revolution of the 1980s, over 40 pages of notes, a bibliographic essay, and an index.

Chapter 1 provides a general overview of the cattle industry in the semi-arid American West, its organization and government policy from the 1870s to about 1890. It also explains the biases of politicians and the general public in favor of “noble” homesteaders and against ranchers (“cattle kings”) that distorted public land policy over much of the period. The 1890s witnessed new laws setting aside grazing and other public land as forest reserves.

Chapter 2 identifies certain structural and technological changes in animal husbandry and other environmental changes between 1900 and World War I. It was in this period that Theodore Roosevelt gave priority to public land policy, appointing Gifford Pinchot to head

a newly established Forestry Service within the Department of Agriculture to regulate the forest reserves.

Chapter 3 describes the changing market conditions between World War I and the late 1920s and government responses to these changes. Government turned from support for homesteading toward greater regulation of public land. As regulation increased, so too did rivalry between the Departments of Agriculture (the Forestry Service) and Interior for regulatory authority over the public domain (government land outside the forest reserves). From its inception the Forestry Service worked with the livestock associations and granted grazing permits of relatively long duration to the better established and more efficient ranchers, so as to maximize productivity. But when it tried to increase the grazing fees, Forestry earned stiff opposition from the stockmen backed by influential Western senators.

Chapter 4 deals with the late 1920s and the somewhat different tack taken by the Hoover administration: a more cooperative, decentralized, multiparty approach in which the states would play a much larger role.

Chapter 5 carries the theme through the Franklin Roosevelt administration and the passage in 1934 of the landmark Taylor Grazing Act. Merrill views the importance of the Taylor Act not for effectively closing the era of homesteading, but rather for achieving long-sought agreement on policy for the public domain. That agreement, however, was achieved at the expense of embedding the act with subtle but important ambiguities that have subjected the relevant parties to recurring crises and dysfunctional conflicts ever since. These ambiguities explain how some analysts interpret the Taylor Act as a victory for organized ranchers whereas others see it as a victory for federal regulators. Yet, in the battle for regulatory authority over the public domain, the winner was clearly the Interior Department under Harold Ickes over Agriculture. Just as Pinchot and most of his followers had done at the Forestry Service, Ickes arranged for a popular western lawyer, Farrington Carpenter, to head up the Grazing Division of Interior and form the grazing districts that constituted a key institutional innovation. The seeming permanence of these districts gave the ranchers a greater sense of security in their access to grazing land than the permits of the Forest Service. At the same time, the law was quite explicit that grazing permits were under no circumstances to be seen as rights.

In Chapter 6 Merrill describes how Ickes came to fire Carpenter, why Interior was blamed for the further deterioration in environmental conditions on the public domain and why the ranching interests became increasingly antagonistic to Interior. Merrill argues that the failure to resolve unsettled issues in or subsequent to the Taylor Act has left enduring ambiguities as to the proper federal regulatory role, the meaning of public property, pricing the grazing permits, and the relationship between private and public property. The antagonism has ebbed and flowed with repeated attempts by Interior to tighten regulations, withdraw lands from grazing and increase fees to more like equivalent private rental fees.

As should be clear from the foregoing, Merrill's book contains neither clear-cut new hypotheses nor empirical tests. Yet, its long-term perspective and carefully assembled details of the contending arguments of the various players provide some fascinating insights.

JEFFREY B. NUGENT, *University of Southern California*

Organizing America: Wealth, Power, and the Origins of Corporate Capitalism. By Charles Perrow. Princeton, NJ: Princeton University Press, 2002. Pp. ix, 259. \$34.95.

Charles Perrow is interested in big organizations and how they shape communities, the distribution of wealth, power and income, and working lives. Today, organizations with

over 500 employees employ more than half the working population in the United States. There were no such organizations in 1800. Referring to William Roy (*Socializing Capital: The Rise of Large Industrial Corporations in America*. Princeton, NJ: Princeton University Press, 1997) and Naomi Lamoreaux (*The Great Merger Movement in American Business, 1895–1904*. New York: Cambridge University Press, 1985) Perrow argues that corporate capitalism was entrenched in five short years (1898–1903) during which more than half the book value of all manufacturing capital was incorporated. The firms were made giant by consolidating the assets of several firms in the same industry.

Bureaucratic organizations produce more than goods and services. Drawing on Marx and Weber, Perrow's book begins with a grand claim:

Bureaucratic organizations are the most effective means of unobtrusive control human society has produced, and once large bureaucracies are loosed upon the world, much of what we think of as causal in shaping our society—class politics, religion, socialization and self-conceptions, technology, entrepreneurship—becomes to some degree, and to an increasing degree, and a largely unappreciated degree, shaped by organizations (p. 3).

He asks how was a society committed to individual freedom, family firms, and decentralized power transformed into a “society of organizations”? His answer? Organizational agency. The large economic organizations that came to dominate are not a simple consequence of technological imperatives, Chandlerian organizational innovations, or economic efficiency but of the pursuit of wealth and power by a business elite composed of corporate top managers, major shareholders, and investment bankers (p. 206). It was an exercise in the creation of organizational power, which was itself the culmination of a long process punctuated by Supreme Court decisions and legislative acts that disfranchised the public in the realm of fundamental economic choices.

History counts. The country was organizationally unformed when the industrial revolution came along. Organizations faced fewer impediments in America because there was not a crown or church or nobles to “be jealous of the rise of private economic power” (p. 219). Whereas in England chartered companies had public representatives on the boards, the Supreme Court in 1819 broke with common law from England and limited public representation on the boards of private corporations. Two other Supreme Court decisions in the same year limited the liability of officers of chartered organizations and largely removed the private corporation from local regulation.

Supreme Court decisions combined with a weak state allowed emerging corporate elites free reign to create big organizations to accumulate wealth and power. Governments were called upon, especially before the development of capital markets, as a source of investment funds. In the case of the railroads, governments turned over nearly 10 percent of the nation's land (see L. Mercer. *Railroads and Land Grant Policy*. New York: Academic Press, 1982), often without public representation on the boards, in return for the promise of economic development.

Perrow's history, like Alfred Chandler's, focuses on the railroads as pioneers in organization building. For Chandler the burst in acquisition and merger activity across industry at the turn of the century is explained in terms of the rapid diffusion of organizational innovations first applied in the railroads and made possible by the creation of a national market by the railroads. For Perrow, “The key role of the railroads in organizing America was possible because they passed, quite quickly, into private hands with no significant regulation in the public interest. We were the only nation to allow the privatization of this immense public good” (p. 224).

Railroads did more than privatize a public good. The railroad elites engineered judicial rulings that established an organizational imprint on industries to follow by declaring

corporations to be “persons” with the attendant privileges, including the right to buy other firms and thereby consolidate market control. As others have argued (See G. Berk. *Alternative Tracks: The Constitution of American Industrial Order*. Baltimore: Johns Hopkins Press, 1994), in the pursuit of “nationalizing” strategies, railroad executives had the power to decide the fate of regions without market discipline or political accountability. Hubris and greed play a role. The reader can simply substitute the telecommunications industry of the 1990s for the railroad industry of the 1890s to draw links between overbuilding and corruption culminating in financial scandals.

An international comparison of the “strategy and structure” of railroads illustrates organizational forms that were discarded. Most importantly to Perrow, it highlights different path-dependent outcomes in the tension between nationalizing interests and regionalizing interests. To this day, regional high-income levels and noncorporate forms of business organization co-exist in parts of Europe. Not so America. Perrow summarizes:

The railroads’ twisting historical path—with its path dependencies of ownership (public, then joint, then private) and path dependencies of regulation (from close to almost none to some)—and with its scope (local to regional, then decisively national)—prompts an organizational view of history. This twisting path made the modern multidivisional multiproduct corporation possible, and once the national versus regional issue was settled, it did it virtually overnight (p. 183).

This book ends in 1910. In future works Perrow plans to show how the organizational revolution that the railroads effected spread to government, schools, and religious and voluntary organizations. Perhaps it will be for someone else to build on *Organizing America* to account for the wave of re-regulation beginning in the Great Depression and ending in the 1970s followed by yet another period of de-regulation. Perrow would not be surprised that inequality was held in check and that America enjoyed high growth in the period of regulation followed by increasing inequality, slower growth, and financial scandal following the return to a period of deregulation.

Organizing America is a provocative and passionate account of the nineteenth century origins of modern American corporate governance and its far-reaching effects. It is highly appropriate for our times.

MICHAEL H. BEST, *University of Massachusetts, Lowell*

Investing For Middle America: John Elliot Tappan And The Origins Of American Express Financial Advisors. By Kenneth Lipartito and Carol Heher Peters. New York: Palgrave, 2001. Pp. x, 268. \$27.95.

In this enjoyable work, Kenneth Lipartito and Carol Peters share with us the story of John Elliot Tappan, a Minneapolis lawyer who brought financial innovation to the American heartland. In an era before mutual funds, money market accounts, and in many locations safe diversified savings banks, Tappan saw the need for a safe, small denomination, financial instrument for middle-class savers. The result was Investors Syndicate and its “face-value” certificate, a combination of zero-coupon bond and term life insurance that savers of modest means could purchase in small installments. By providing the small investor with a safe means of saving a small amount each month, Investors Syndicate (later IDS and American Express Financial Advisors) would grow into one of the nations financial behemoths. Along the way, Tappan and his company would overcome financial panic, depression, war, epidemic, and corrupt postal inspectors (the federal regulators of the day).

This is neither a biography nor a company history. Instead, Lipartito and Peters center the story on Tappan's years at Investors Syndicate with illuminating glances at Tappan's early years in which he developed the morals and world view that would be the driving force behind Investors Syndicate. The authors spend considerable time placing the story of Tappan and his company within the context of late-nineteenth-century financial and political upheaval. To this end, Lipartito and Peters chronicle Tappan's upbringing in the nineteenth-century west. Although he was raised in the heartland and sympathetic to populist complaints, Tappan was no radical. He voted republican and responded to the regional inequities in interest rates not with protests, but with innovation. In short, Tappan was a businessman with the vision to recognize the need for financial intermediation on the frontier.

As Ken Snowden ("Mortgage Lending and American Capital Market Development in the Late Nineteenth Century," this JOURNAL 47, no. 3 [1987]: 671–91) and others have documented, the late-nineteenth-century mortgage markets were segmented along geographic lines. Eastern borrowers enjoyed low rates of interest while their western counterparts faced high rates of interest or no funds at all. At the same time western savers had few safe outlets for their savings. Into this void stepped John Tappan and Investors Syndicate. Tappan created the face-value certificate. This was a contract that paid a lump sum upon maturity and contained provisions for early withdraw and an insurance payment in the event of death. Savers could purchase this certificate by maintaining a schedule of small payments until maturity. Investors Syndicate collected money from sales of its face-value certificate and invested these funds in the cash starved western mortgage market. By acting as an intermediary between western savers and borrowers, Investors Syndicate was able to offer the common man a high yielding yet relatively safe outlet for his savings while simultaneously mobilizing funds for western mortgages.

The book is well researched. The authors draw on interviews with a family member (Carol Heher Peters is the great-granddaughter of John Tappan) and the nearly 20,000 letters written by Tappan while at Investors Syndicate. The result is a well-documented history of both John Tappan and his company. Lipartito and Peters explain Tappan's Victorian morals, his honesty, and his desire to help the working man better himself through savings. At times the authors exert too much effort to link Tappan's ideas of "manliness," populism, or environmentalism to Investors Syndicate's products and conduct. This is a minor complaint, however, in an otherwise glowing endorsement.

In short, *Investing for Middle America* is an excellent study of a financial innovator who successfully created and marketed a much needed product to middle-class investors. At a time when the small investor was virtually ignored by Wall Street, Investors Syndicate and its face-value certificate democratized the savings process for the betterment of all.

BENJAMIN CHABOT, *University of Michigan and NBER*

Looking for Work, Searching for Workers: American Labor Markets During Industrialization. By Joshua L. Rosenbloom. New York: Cambridge University Press, 2002. Pp. xvi, 208. \$20.00, paper.

Joshua Rosenbloom provides a superb study of the operations of the U.S. labor market between the Civil War and World War I. The book weaves fascinating descriptions of the various ways in which employers and workers established connections together with clear summaries of an extensive amount of background quantitative work. Although the analysis is firmly grounded on a series of more technical statistical studies, most of the book does

not emphasize econometrics. Instead, the findings are effectively summarized using graphs, simple means and telling anecdotes that illustrate the experiences of many workers. The book is beautifully written and can be used by economists, historians, and both graduate and undergraduate students to obtain a clearer understanding of how markets work.

Rosenbloom reiterates how important word-of-mouth networks were to the migration process, while offering ample documentation that many of these word-of-mouth and kinship migration flows would have never been established without the active stimulus of employer recruitment efforts. Employment agencies and labor agents played a relatively limited role, as they served primarily to supplement word-of-mouth networks for those who had little access to them. He also provides evidence on the use of strikebreaking and the extent of its importance to migration flows. Finally, the nature of the labor markets for skilled and unskilled worker influence how firms organized their use of skilled and unskilled workers.

Although many of the labor markets became highly integrated, market integration did not advance uniformly. Several large cities became prime locations where workers could find the emissaries of employers looking for work outside the word-of-mouth networks, and path dependence tended to reinforce the importance of these cities over time. The North was a tightly integrated regional market, closely linked to northern European labor markets. The labor market within the South was also tightly integrated. Yet there was persistent isolation between northern and southern labor markets. Only when World War I substantially interrupted the supply of immigrant labor upon which northern employers had become dependent did northern employers begin to forge connections with sources of supply in the relatively more labor-abundant south.

The book is extremely valuable, although I would adjust some concluding interpretations. Rosenbloom tells a story of path dependence in migration. We all can agree that history matters in that many people travel the well-trod path and fewer blaze new trails. However, I am troubled by statements such as the following: the connections established by chain migration and employer recruitment “linked particular sending and receiving regions *while excluding others*. Once established, certain patterns of migration tended to persist, *while others were simply never explored*” (my italics) (p. 37). Eliminate the italicized phrases, which seem to imply that migrants were locked into these paths, and I agree. The absence of much movement along a potential path does not imply lack of opportunity or that the path is barred. Although we know many people followed similar paths, we do not know all of the paths explored by migrants. In fact, histories of individual migrants show that quite a few followed unusual paths, which in turn led to new migration chains when the migrant met with success in the new location.

Rosenbloom also emphasizes the persistent “isolation” of the southern labor market while talking about close links to northern European labor markets. There was southern isolation in the sense that prior to World War I relatively few southerners moved north. Rosenbloom (p. 177) argues that the absence of much South-North migration in the face of an increase in the North-South wage gap was due to the lack of long-term chain-migration information networks. I believe he should give more emphasis to the size of the wage gaps and less to the trends in the wage gaps. The materials in figures 5.3 through 5.8 and table 5.1 show that the wage gap between the North and the South was substantially smaller than the wage gaps between European countries and the North throughout the period, even in 1910. Based on the wage gaps, we normally might consider Europe and the North to be less integrated than the North and South, but there were such large migration flows from Europe that it is clear that there was a connection between Europe and the North. Let me suggest an alternative story that fits the facts while relying less on barriers to movement from South to North. Southerners largely were disinterested in moving North because, as

the book notes on p. 176, they could do nearly as well by migrating westward within the South. Northern employers had problems attracting southern workers because southern wages were not low enough prior to 1910 to make moving North appealing. Northern employers had much more success attracting people from Europe where wages were substantially lower than they were in the South. When northern wages increased relative to southern wages and European immigration was cut off, southern workers and northern employers found each other relatively quickly, and a substantial number of southerners moved North. Where Rosenbloom's conclusion emphasizes opportunities denied by the absence of information flows over a long period, my story emphasizes opportunities in the North ignored by southerners until they became relatively more lucrative. These stories are not mutually exclusive, and in the final analysis, some combination of both of these stories probably captures the situation.

I should emphasize that my criticisms are directed at a small number of summary sentences. If he changed only about 10 to 15 sentences in the book, I would be in full agreement. This is an excellent book with a tremendous wealth of valuable material. I strongly recommend it.

PRICE V. FISHBACK, *University of Arizona*

Rethinking the Great Depression: A New View of its Causes and Consequences. By Gene Smiley. Chicago: Ivan R. Dee, 2002. Pp. xii, 179. \$24.95.

Gene Smiley provides here a narrative overview of the causes and consequences of the Great Depression. The book is intended for a general audience, and evidences a solid command of the range of literature devoted to this remarkable period. The text lacks footnotes or references, but concludes with a comprehensive annotated bibliography covering the major sources from which he has drawn. Although Smiley's approach is relatively nontechnical, readers may nevertheless not be able fully to appreciate its nuances without at least an elementary course in microeconomics and an intermediate level course in macroeconomics. Thus it may be most suitable as supplementary reading in economic history courses.

The book has five chapters. The first sets the stage, providing a largely factual account of the progress of the economy in the 1920s and its descent into Depression through 1933. The second chapter is more analytical, focusing on the causes of the initial downturn. Here Smiley provides a clear exposition of what has now become the conventional view: the downturn was due principally to a shrinkage in the money supply aggravated by Fed policy in the face of bank failures and within the context of U.S. adherence to the gold standard. If shrinkage in the nominal money stock was the ultimate cause of the Depression, its proximate determinant for Smiley was the rise in real wages resulting from initial nominal wage rigidity in the face of output price deflation. There is relatively less attention to the precipitous drop in autonomous consumption spending after 1929 or the collapse of real investment spending by 1933 to about a quarter of its 1929 value (within a monetary framework, these are shocks to nominal income resulting from velocity changes, not the nominal quantity of money).

Chapter 3 covers slow recovery from 1933 to 1935. With the devaluation of the U.S. dollar, the Fed was freed from its golden fetters. The explanation of the severity of the downturn now shifts to the role of the NRA in cartelizing the economy and most specifically in raising real wages. Indeed, a recurring theme in the book is an emphasis on real wages that were too high, either because labor successfully fought for them, or because government leaders and legislation and in some cases corporate leaders encouraged or facilitated them.

Chapter 4 covers poor performance over the 1935–1939 period. The downturn in 1937/38 is attributed conventionally to misguided monetary stringency and somewhat less conventionally to the “regime uncertainty” hypothesis developed by Robert Higgs. The hypothesis is that Roosevelt’s turn to the left, in the context of legislation giving a green light to CIO organizing drives, made capitalists unwilling to invest. The final chapter considers the institutional legacy of the Great Depression and how it has influenced our thinking about managing the macroeconomy.

This is a careful, systematic review of literature on the Great Depression, not a once over treatment in search of evidence supporting predetermined positions. Smiley does however have a point of view, one reflecting some skepticism about the benefits that government and those who occupy it bring to the economy through their “management” of it. Most of the time the point of view remains in the background, impinging only lightly on the exposition. Nevertheless, some of the author’s judgments are colored by his perspective, leading to interpretations that other economic historians may question.

Once one has it in for government, and is ready to credit governmental or quasi governmental agencies such as the Fed with sins of both commission and omission, it is easy to make Washington bear more blame than it deserves. For example, I remain skeptical about the regime uncertainty hypothesis. The most significant obstacle to full recovery in the latter years of the Depression remained construction, and I simply do not believe that the reasons real estate developers did not do more was because they were afraid Roosevelt was going to confiscate their houses before they could be sold. Many of the larger corporations in this period continued to make substantial bets on the future. Rising spending for R&D throughout the Depression years, particularly toward its end, would appear to be anomalous within the context of the hypothesis.

Another example: “Although delayed two years by the NRA, the recovery from the Great Depression of 1929–33 was finally underway” (p. 104). The only evidence we have for this conclusion is that a government program with a lot of warts existed during a period when the economy did not recover much. On the other hand, in its absence, it is possible the economy would have declined even further. The NRA did play a role in arresting deflation, which may have been beneficial for a time. I do not know whether it delayed recovery by two years, and I do not think Smiley really does either, because the conclusion is only asserted. The point here is not to defend the NRA, but to caution against jumping too easily to conclusions about its impact.

Finally, some of the interpretations of tax and spending policy have a bit of a political spin: “Increased inheritance and gift taxes aimed at the rich would have ‘tended to reduce spending and slow the recovery’” (p. 108). This seems as likely as it is that George W. Bush’s attack on the “death tax” will provide a boost to aggregate demand in 2003. Similarly, Smiley questions the stimulative effect of the \$1.4 billion of spending to pay veteran’s bonuses because of the putative reduction of private investment spending required because the Treasury had to borrow funds in private capital markets to finance this. The appeal to crowding out is problematic, given the large amounts of slack labor and capital then available. There were no obstacles (in terms of dangers of igniting inflation) to monetizing any resulting deficits. Thus responsibility and blame for high interest rates should rest entirely with the monetary authority.

The book is well written, strives for comprehensiveness and balance, and has few factual errors. An exception: U.S. Purchases of overseas assets are referred to as capital imports, whereas they should be described as capital exports (p. 159). Overall I can recommend this work to those looking for up-to-date supplementary material for courses in U.S. economy history in the twentieth century.

The American Dole: Unemployment Relief and the Welfare State in the Great Depression.
By Jeff Singleton. Westport, CT: Greenwood Press, 2000. Pp. viii, 243. \$72.95.

In this book, Jeff Singleton provides a detailed history of relief programs prior to and during the Great Depression. He also assesses the obstacles to welfare reform since the 1930s, and he generally argues for more reliance on social insurance and public employment as alternatives to means-tested welfare programs. The book will be of interest to scholars seeking to understand the details and evolution of relief institutions before and during the New Deal, as well as to those interested in the historical origins of modern policy.

Overall, I found the historical accounts informative and interesting. After the introductory chapter, Singleton turns his attention in chapter 2 to the “rising tide of relief,” describing changes in the nature of relief spending during the decades prior to the Great Depression. He explains how Progressive ideas (including mothers’ pensions) not only increased the level of relief spending, but fostered the professionalization of social work, and how the increase in relief spending relied on both public and private funding. A particularly interesting topic considered in this chapter is the pre-Depression increase in concern about the effects of largely structural unemployment on the need for relief. In chapter 3, Singleton describes the “myth of voluntarism” and explains how pre-Depression institutions shaped the crisis in relief policy at the onset of the Depression. He also documents the great variation among pre-New Deal relief programs throughout the country and the Hoover-era steps toward increased reliance on federal funding. Chapters 4 and 5 focus on the New Deal’s general relief and work relief programs. Singleton provides a wealth of institutional details, describes the evolution of the programs, discusses the great controversies over the appropriate type and quantity of relief, and analyzes the links between the New Deal and pre-New Deal programs. Chapter 6 focuses on the reform efforts made during the middle years of the New Deal, explaining how New Dealers tried unsuccessfully to end “the dole” and replace it with social insurance (most notably through the Social Security Act) and public employment (most notably through the WPA). In the concluding chapter, Singleton summarizes and relates his historical analysis to contemporary issues in welfare reform.

Although the book’s historical analysis is interesting, most economists would find the book more valuable if its assessment of the obstacles to welfare reform focused more closely on the key issues of scholarly debate. First, the book lacks sufficient clarity with respect to identifying the believers and promoters of some of the welfare “myths” debunked in the book. Some of these myths (such as inaccurate beliefs about the cost of welfare and workfare programs) contradict what one can read in a public finance text and, thus, might be more accurately labeled as rhetoric used by politicians and other partisans. Second, the book would make a greater contribution if it paid some attention to the relevant economics literature on the rise of social spending. For example, when Singleton (p. 14) mentions “all efforts” previous scholars have made to explain the growth of social spending, he does not include the mainstream economics literature. Third, when Singleton discusses the country’s “inability” to reform welfare, he could explain the political dilemmas more precisely if he added a concise discussion of how the economics of work incentives creates inevitable trade-offs when designing formulas for allocating money to heterogeneous welfare recipients. Quite simply, there is no obvious way to design (in the real world or in graphs with indifference curves and budget constraints) a simple formula-based program with all of the following: generous benefits for those who earn little, strong work incentives (that is, a high opportunity cost of leisure) for those who can work, and benefits concentrated among the most needy. This fundamental dilemma plays a key role in the debate over welfare reform, but receives relatively little attention in the book. Although these shortcomings

make the book less than ideal as policy analysis, they pertain principally to its relevance to modern policy debate, not to its discussion of history. Singleton presents an interesting and detailed account of Depression-era relief programs in the United States, and he provides new insight into the manner in which those programs evolved from the pre-Depression relief system.

ROBERT K. FLECK, *Montana State University*

Moose Pastures and Mergers: The Ontario Securities Commission and the Regulation of Share Markets in Canada, 1940–1980. By Christopher Armstrong. Toronto: University of Toronto Press, 2001. Pp. x, 424. \$60.00.

Christopher Armstrong's book is concerned with a topic of remarkable timeliness following the wave of financial scandals—Enron et al.—which shook the American economy in 2002. The timing of the book could not have been better as it deals with crucial issues: information asymmetries, investor protection against manipulations of information, scope and type of regulation needed to insure “full, true, plain disclosure,” and the behavior and ethics of brokers, promoters, and other insiders. The fact that it covers Ontario in the four decades since World War II shows that the issues are perennial and never likely to be outmoded.

Ontario is a particularly interesting case because a large proportion of the securities issued and traded were in the mining sector, a rather reckless business where information is a particularly tricky issue and where it seems rather easy to make people dream and believe that they will become rich fast and easy (here we can perhaps see a parallel with the dot com sector of the 1990s). Armstrong described Ontario regulatory institutions (Ontario Securities Commission; Toronto Stock Exchange) as having a reputation of laxity relative to a more rigorous U.S. Securities and Exchange Commission. A picture indeed quite different from the conventional one of cautious and conservative Canadians versus wilder capitalist Americans.

Also of great interest is the issue of extraterritoriality, referred to in the book as “The Canadian Problem” where we can see how difficult it is to regulate behavior across borders.

The author is a careful, rigorous scholar and a specialist of the history of regulation in Canada and particularly in Ontario. *Moose Pastures* is the follow-up to *Blue Skies and Boiler Rooms: Buying and Selling Securities in Canada, 1870–1940* published in 1997. In the 1980s, he also wrote a very useful study with H. V. Nelles on the regulation of utilities (*Monopoly's Moment: The Organization and Regulation of Canadian Utilities, 1830–1930*. Philadelphia, 1986). Armstrong's methodology and style are always the same: he supplies very detailed accounts on every key event, scandal, personality, committee or commission and this, chronologically. *Moose Pastures* is set up in two sections: Part 1 covering the period from 1940 to the mid-1960s focusing on the mining industry and the problems of fraud and extraterritoriality and Part 2 covering the rest of the period to 1980 with a wider scope on other fields (insurance, industrial) and problems (foreign ownership; mergers). Each section has six chapters and each chapter typically covers a few years.

Moose Pastures is, undoubtedly, a very rich and dense book, but the other side of the coin is that it requires of its readers a great deal of patience. The author recognizes in the preface that his colleagues and editor who read the first version “tactfully suggested that it was so long that readers were unlikely to have the time or energy to make their way all through it.” He adds that a considerable amount of detail was chopped out as a result. I would say that more chopping could have been done without losing the essence of the

arguments. Many readers would be frustrated to have to figure out by themselves the main turning points and features from that maze of details.

Throughout the book, we can see a very complex interplay among the actors: governments (mostly Ontario's but also the federal, the American, and the other provincial and state governments), interest groups from the financial sector (the Broker Dealers Association, the Investment Dealers Association, the Toronto Stock Exchange) and the industrial (Canadian Manufacturers Association, promoters groups), and even investors (through financial newspapers). It would have been very helpful to position them in an analytical framework. One that comes immediately to mind is the political economy model of regulation *à la* George Stigler ("The Theory of Economic Regulation," *Bell Journal of Economics and Management Science* [Spring 1971]: 3–21). Another very valuable input from the economist's perspective could have been the lemons model of G. A. Akerlof ("The Market for Lemons: Quality Uncertainty and the Market Mechanism," *Quarterly Journal of Economics* [August 1970]: 488–500), as the fraudulent behavior of some individuals had a detrimental effect on the reputation of the Ontario financial institutions and community. We see them juggling through the years (and the book) between self-regulation and governmental intervention to solve the problem.

It would also have been useful to put Ontario's case in a comparative perspective. There are many hints that Ontario's government and regulators were much more lenient than their American counterparts. What about the other provinces, especially the ones with a significant mining industry such as British Columbia and Quebec?

Armstrong's book provides a good historical perspective on perennial and complex issues in the capital markets. The author's last sentence (p. 345) is that the complexities of actual financial products (derivatives and the like) are such that "there will be plenty for security regulators to do for the foreseeable future." We may add that the events of 2002 show that there is much room for improvement.

RUTH DUPRÉ, *HEC Montréal*

American Agriculture in the Twentieth Century. By Bruce L. Gardner. Cambridge MA and London: Harvard University Press, 2002. Pp. ix, 388. \$49.95.

With meticulous detail, Bruce Gardner provides insight into the evolution of U.S. agriculture over the 100-year period between 1900 and 2000. In doing so, Gardner reflects on several facets of agriculture, such as innovation, on-farm productivity, the declining number of farms, the income status of U.S. farmers, and the role of government agricultural policy. Much of Gardner's analysis relies on census data, which are fraught with shortcomings. Definitions that seem straightforward, such as "farm" or "farm operator," are actually quite complicated, so even a seemingly simple task such as tracking the number of farms over time is not trivial. Gardner skillfully addresses the challenges presented by the deficiencies of census data.

In the second chapter, Gardner examines technological innovation, which simultaneously increased productivity and reduced farmer self-sufficiency. Mechanization proceeded so slowly that the number of farm animals continued to exceed the number of tractors until 1945. Farmer dependence on the market increased as tractor use required purchases of gasoline and repair services. New crops—for example hybrid corn—were developed through genetic improvements. Chemical fertilizers and pesticides were developed. Gardner devotes a large part of the chapter to discussing the difficulties inherent in productivity measures, and ends the chapter with the conclusion that productivity increased rapidly after the end of World War II.

The next two chapters are devoted to farms and farm communities. Tracing the change in the number of farms over the century is greatly complicated by the changing census definition of a farm: Gardner notes that the definition was changed in 1910, 1945, 1959, and again in 1974. During the century, the number of farms increased between 1900 and the mid-1930s, at which point farm numbers began declining. The decline accelerated in the 1950s, and in fact, half of U.S. farms were lost between 1950 and 1970. Surprisingly, farm size and numbers followed a different pattern in the last decade of the twentieth century, and the number of farms increased slightly and the average farm size decreased from 460 to 434 acres. Although the number of farms declined over the century, the acreage devoted to production increased 13 percent between 1900 and 2000.

Farms grew larger during the century and more specialized. The average farm in 1900 produced 5.1 commodities, had slightly fewer than 150 acres, and had net real income of \$10,000 (in 1992 dollars). The real value of farmland was about \$350 per acre (in 1992 dollars) and 35 percent of farms were operated by tenants (13 percent cash tenants and 22 percent share). In 1929, 6.3 percent of farm operators worked 200 or more days on off-farm work. In contrast, the average farm at the end of the twentieth century produced 1.8 commodities (1992), and was slightly larger than 450 acres. In 2000, real net income per farm totaled \$19,300 (in 1992 dollars), and the real value of farmland was \$900 (also in 1992 dollars). Tenancy declined: in 1997, 10 percent of farms rented. In 1997, 85 percent of the average farm's household income came from off-farm sources. Gardner discusses a surprising result—that income inequality among farm households declined during the twentieth century, and that farm households had a lower incidence of poverty than nonfarm households.

The fifth chapter discusses markets. One of the most striking findings—consistent with those found by others—is that real prices received by farmers declined throughout the twentieth century, with the exception of a few years prior to 1920, when farm prices reached an all-time high. Gardner identifies two additional periods of high prices: World War II and the mid-1970s. Following each of the three periods of high prices was a price collapse. The overall decline in farm prices (farm prices fell by half) was not accompanied by an equivalent decline in consumer prices (consumer prices fell by 25 percent). The farmer's declining share of the retail food dollar may be due to added marketing services, an increase in purchases of highly processed foods, technological change at the farm and processing levels, or increased retailer and processor market power.

Gardner addresses government policy in the sixth and seventh chapters, while pointing out that many economists have criticized agricultural policy, such as the commodity programs, as being inefficient. Despite the criticisms, the vast amount of farm-related legislation (he lists 96 laws, selected from a larger set, passed between 1902 and 2000) indicates that farmers have clout in the political arena. The last few chapters delve into farm household income and economic growth in the agricultural sector at regional, state, and county levels.

Throughout the book, Gardner describes the evolution of American agriculture in the twentieth century, demonstrating that farm incomes have increased, farmer income inequality has declined, on-farm productivity has increased, and consumer food costs have declined. I am left questioning whether this picture of American agriculture is too rosy, as the costs of agricultural production, specifically environmental degradation, are not fully discussed. Despite my uneasiness about the lack of attention to the costs of modern agriculture, I found Gardner's discussion of American agriculture thoughtful, thorough, and absorbing.

Redefining Efficiency: Pollution Concerns, Regulatory Mechanisms, and Technological Change in the U.S. Petroleum Industry. By Hugh S. Gorman. Series on Technology and the Environment. Series Editors, Jeffrey Stine, and Joel Tarr. Akron, OH: University of Akron Press, 2001. Pp. xv, 451. \$49.95, cloth; \$39.95, paper.

In this well-written, documented, and technically complete book, Hugh Gorman describes the response of the American petroleum industry to pollution over the course of the twentieth century. The industry, which grew and matured during this period as an integral part of modern industrialization, faced serious, and often dramatic pollution problems. They were inherent in production from common oil pools that encouraged haste, waste, and excessive surface storage; in transportation through pipelines and tanker trucks and ships; and in refining and storing complex hydrocarbons that easily escaped into the air, soil, or aquifers. Reaction to pollution brought new technologies, organizational forms, firm collaboration, and regulation—all of which are described and documented from primary and secondary sources throughout this volume. Gorman partitions efforts to address pollution into two “ethics”—an efficiency ethic that characterized industry action through the 1960s and an environmental ethic that came into being in the 1970s. The efficiency ethic describes antipollution efforts to reduce the costly wastes associated with extraction and shipment, including saving lost oil from “gusher” wells and leaky tanks and pipelines, as well as capturing natural gas and water voided in production that could be re-injected and used to propel oil to the surface. Efficiency also required greater productivity and less waste in refining through reducing vapor and hydrocarbon discharges and recycling acids and other chemicals. The firms could capture the benefits of internalizing the externalities associated with these pollutants. In tables 2.1 and 4.3 Gorman lists some of the pollution and waste-related problems encountered in oil production, shipment, and refining that were addressed effectively by firms without much government intervention. He describes the role of the major trade association, the American Petroleum Institute, in generating information for oil firms to reduce externalities.

In Part 3 of the book, Gorman examines pollution problems that were less easily internalized by firms and more subject to government regulation. He rightly claims that efficiency incentives were not sufficient to motivate firms to take action. With enactment of the Water Quality Acts of 1965 and 1972 and the Clean Air Acts of 1967 and 1970, and creation of the Environmental Protection Agency in 1970 the federal government had the mandate and the administrative structure to more aggressively regulate pollution in the industry. Spectacular oil spills, such as the 1967 *Torrey Canyon* spill and 1969 Santa Barbara oil leak, galvanized public opinion for more regulation. At the same time, the environmental movement was gaining strength in American politics. Standards were adopted for control of emissions and fluids from production, transport, and refining of petroleum. Tankers were required to have new designs to reduce oil spills. Gorman provides an interesting account of how the oil industry responded to these new regulatory requirements. He argues that a new industrial ecology emerged with an environmental ethic that displaced the old efficiency ethic, and this underscores the volume’s title, *Redefining Efficiency*.

Although the emphasis on two ethics, one for efficiency and one for the environment, provides Gorman with a way of partitioning the petroleum industry’s response to pollution into two parts, I do not find the use of these two “ethics” particularly compelling. Rather, I interpret early actions by the oil industry to reduce waste as sensible efficiency-enhancing activities and later compliance with regulation, as an industry response to highly visible and difficult externalities. Absent regulation, the industry most likely would not have borne the costs of addressing them. I do not see a shift in ethics, if these are viewed as sets of morals

or beliefs. Nevertheless, this is not a major shortcoming. Gorman has provided a readable, valuable description of how an important industry responded to pollution problems, as production technology and regulatory constraints changed during a period of major economic growth.

GARY D. LIBECAP, *University of Arizona*

Birthquake: The Baby Boom and Its Aftershocks. By Diane J. Macunovich. Chicago: The University of Chicago Press, 2002. Pp xiii, 314. \$37.50.

Before I had even finished reading the first chapter of Diane Macunovich's new book, three things were crystal clear:

People matter: a society's demographics need to be considered explicitly when trying to understand or to forecast its economic behavior.

Einstein's conclusions about relativity apply to economies: changes in the relative size and age composition of a population can lead to major changes in its social and economic behavior.

Economic demographers rule! From now on, users of long-term forecasting models will need to include information on changes in age structure and cohort size if they wish to forecast events more than a few years ahead.

Diane Macunovich's new book is an important contribution to the demographic literature and definitely worth reading. *Birthquake* should be read by a wide variety of people, economists and sociologists and political scientists and policy makers, because her arguments and her evidence will make us all think productively about many of the models we take for granted. Her immediate focus is the U.S. Baby Boom and its effect on the economy, marriage and fertility choices, and social mores during the Baby Boomers' passage from childhood to the brink of retirement. She uses Easterlin's relative cohort hypothesis (*Birth and Fortune*, R.A. Easterlin, 1987) to investigate how changes in age structure lead to changes in marital and fertility behavior over generations, and how these changes can in their turn lead to major changes in social behavior and ideation. She carefully and clearly lays out the first-, second- and third-order effect of changes in relative cohort size. Most of the book is sufficiently nontechnical that any well-educated person can read the first chapter, skip the technical material in the rest of Part 1, and enjoy Parts 2 through 4. Using the economic history of the Baby Boomers, Macunovich investigates the effect of male relative cohort size on income, marriage patterns, fertility choices, women's educational choices, and labor market decisions. In Part 4, she continues the analysis into the effects of relative cohort size on macroeconomic variables. This is an ambitious work and Macunovich accomplishes her goals neatly.

One of her oft-repeated questions is "why then?" when looking at the timing of major social and ideation changes. Why was feminism so hotly debated in the late 1960s and 1970s, when women could vote and were already classed as independent, legal persons and not earlier, and why has the debate quieted in recent years? For her answer, see chapter 7. The "why then?" questions set up the discussion of how ideas and beliefs can adapt to changes in behavior, rather than behavior changing to fit new ideas and beliefs. This is a concept brought up in undergraduate social psychology texts (as I recall from long ago), but not usually considered in economics texts. This is another good reason for reading this book—and an even better reason for recommending it to noneconomists.

One aspect that will excite a certain amount of disagreement among readers is Macunovich's use of *male* relative cohort size and *male* relative income as the deciding

variables in her analysis. Why are young women, in this advanced age, reacting to the expected and actual earnings of men and not to their own potential earnings? Why does Macunovich suggest that young women plan their futures based on male expectations, and act as though they themselves were only marginal workers and actors? Do young women really still aspire to the profession of full-time wife and mother rather than doctor or lawyer or college professor? A revelation on this issue will be found in appendix A, "Expectations in the Williams Class of 1999."

After a thorough-going discussion of the experiences of the Baby Boomers, Macunovich explores the possibility of using the Easterlin hypothesis and the General Fertility Rate to forecast the path of the economy and its turning points into the future. One of her findings is that by using relative cohort size and its effect on relative income, one can predict turning points in an economy's future. Her model uses male relative income and male relative cohort size as the major independent variables, with the fraction of men in the military (proxied by military expenditures) and net imports as supporting variables. The model fits the experiences of the Baby Boomers quite well; simulations using the earlier period of the data forecast the remainder of the period accurately, generating cycles of income and cohort size through 2050. Finally, Macunovich explores the use of similar models as tools that can forecast turning points in the economy and hints at their use to explain pre-twentieth-century behavior as well. Because relative cohort size at age of entry into the labor force can be estimated from the numbers of births 20 years earlier, and the effect on male incomes predicted from relative cohort size, it may be possible to forecast economies accurately for at least ten years ahead and perhaps far longer. If so, *Birthquake* will be a very important work indeed.

JUDITH WOERNER MILLS, *Southern Connecticut State University*

Going Shopping: Consumer Choices and Community Consequences. By Ann Satterthwaite. New Haven, CT: Yale University Press, 2001. Pp. 1, 386.

Adding to the growing list of retrospective studies of shopping and consumption is this engaged survey of the impact of American retail trade on community culture and social interaction. Although the author (a city planner in Washington, DC) is broadly within the tradition of Jane Jacobs and other critics of the commercialization of urban space, hers is a contemporary, well-informed, and nuanced judgment of the impact of malls, remote retail, big box stores, and other expressions of contemporary shopping. Even though this book may not meet the expectations of the professional historian, it does attempt to put very present-minded concerns about the social impact of contemporary retailing trends into a historical context.

Ann Satterthwaite surveys the evolution of retail trade from Ur and medieval fairs to modern department stores and suburban malls. All this is merely background, however, to her main concern—the decline of community-based retailing in both downtown commercial districts and country stores. The department stores with owners committed to their cities and the rural general stores that provided opportunities for social exchanges gradually gave way to externally managed Malls of America and new retail nexuses such as Tyson's Corners in suburban Washington that are cut off from political, social, and cultural centers. Community is lost to the highly individualistic goal of spending.

The author makes the familiar argument that shopping has become a substitute for socializing and citizenship. Department stores not only introduced variety at fixed prices, but stimulated desire and emulation, especially in women. The "I shop therefore I am"

mentality (p. 146) emerged from a loose set of causes: advertising, credit, two-income households, suburbanization, counterculture, cocoon living, and the new individualism of a mobile workforce. Although Satterthwaite acknowledges new critiques of consumerism (such as simplicity movements and ecological shopping), she sees the consumer as much as the retailer as a problem in the “restoration” of the citizen-shopper.

A long, less critical chapter on retail trends follows: She sees that time pressures and declining interest in the pleasure of shopping are leading to more “fast-and-easy” retailing expressed in remote shopping via the internet and phone as well as in small, more accessible strip shopping centers replacing mall mini-cities and Super Wal-Marts. Although she remains skeptical, “entertainment shopping” with theaters, themed restaurants, and amusement rides may still be on the upswing. But she is cheered by a recent trend toward more personalized specialized stores and farmers’ markets. None of this is especially surprising, but more problematic is that her predictions, without systematic analysis or data, become expositions on the obvious and wishful thinking. In the end, she really cannot seem to decide which path we will take: Big Box bargain shopping or “Face to face, communal shopping” for “authenticity, quality, and diversity of goods” (p. 239).

The balance of the book is a sketch of the history of planned communities, including trends in zoning and changing ideas about the placement of shops (edge or center of residential developments). Especially interesting to her is the “New Traditionalism,” recent efforts to recreate village-like residential developments and small-town centers. She looks to European and Japanese models of resistance to mega-malls, cites local American attempts to regulate mall and suburban sprawls, and offers guarded support for public-private urban renewal efforts. But, as is so often the case in this book, she offers insufficient analytical structure or detail to do more than whet the readers’ appetite.

For the historian (economic or urban), this book describes more than it explains. The author frequently jumps from the past to the present, combines scholarly data with general impressions (often based on her local roots in Washington), and often is repetitious. The book’s lack of an analytical framework leads to a scattering of fact and opinion. Better editing from Yale might have improved this book. But, underlying these organizational weaknesses is really the dilemma of contemporary social reformers. In a tone of hope against hope, the author insists that “shopping is a public concern” (p. 345). But in the present economic and political climate, there are few real costs to ignoring community in retailing and probably even fewer benefits for embracing it. Community values were incidental to the market throughout the history of retailing and, when “community” is so very hard to find outside the market, it is even more difficult to find it within the market.

GARY CROSS, *Pennsylvania State University*

Lake Michigan Passenger Steamers. By George W. Hilton. Stanford, CA: Stanford University Press, 2002. Pp. xii, 364, \$75.00.

The author, a retired UCLA economist, has written a number of highly specialized transportation studies. In his *Lake Michigan Passenger Steamers* much as in his *Great Lakes Car Ferries* and *American Narrow Gauge Railroads*, George W. Hinton acknowledges that “the principle purpose is to provide antiquarian scholarship” (p. xi). Here we learn about the wooden and steel, sailing and steam ships that operated on Lake Michigan from the early nineteenth century until well into the twentieth century. Although some attention is devoted to the interlake trade, the passenger lines that draw most of the author’s attention are those that served Lake Michigan points exclusively.

Historians and economists will find thorough descriptions of the ships, the routes, the commodities carried by the Lake Michigan lines. The book begins with a fascinating description of the hazards of navigation, as wind, sand bars, and five different types of ice often brought ships to their ruin. With western settlement and the completion of the Erie Canal, the numbers of ships plying the Great Lakes, especially the route from Buffalo westward, soared. Interlake trade, however, had a relatively short existence as the trans-Appalachian railroads lured away passengers and then freight. But trade on Lake Michigan prospered throughout the nineteenth century, peaking in the early twentieth century. Here ships provided passenger services, especially excursions from the major cities, Chicago and Milwaukee, to points along the eastern and western shores of Lake Michigan. One of the most profitable commodity trades was the carriage of fruit from the eastern shore of Lake Michigan to urban markets. With the development of trucks, the fruit trade would abandon the steamers, but passenger services flourished until the Great Depression. The last passenger vessel, the *Milwaukee Clipper*, did not stop its regular service until 1970, although most of the Lake Michigan passenger trade had dried up by the 1930s.

The author provides a succinct description of the development of different types of ships and an analysis of how they fared against competing forms of transport. Beginning with wooden schooners, the passenger trade shifted to paddlewheel steamers and by the second half of the nineteenth century to propeller steamers. Along the way, they faced competition from railroads, interurbans, trucks, and buses. Sometimes the lines tried to join the competing forms as they served railroads, interurbans, and trucks at various points. They also tried to dampen the competitive fires among the passenger lines themselves, but several attempts at cartels reaffirmed the competitive nature of the business.

The heart of this handsome coffee-table volume is the intimate picture one gets of the ships, the passengers, and the routes. The author provides ample illustrations of the amenities of the ships as well as the advances in engines and design. Particular attention is given to a few of the major passenger liners, notably the ill-fated *Eastland*, the *Christopher Columbus*, and the *Theodore Roosevelt*.

The second half of the book provides corporate histories of the ten major Lake Michigan lines. An appendix provides a ship registry for these ten Lake Michigan lines. Economic historians who study lake transportation will find this a treasure trove of information. The author has mined the secondary literature on the lake trade and supplemented it with a careful reading of the Lake Michigan area newspapers, especially the *Milwaukee Sentinel*.

DIANE LINDSTROM, *University of Wisconsin-Madison*

GENERAL AND MISCELLANEOUS

Altruistically Inclined? The Behavioral Sciences, Evolutionary Theory, and the Origins of Reciprocity. By Alexander J. Field. Ann Arbor: University of Michigan Press, 2001. Pp. xvi, 373. \$ 65.00.

Alex Field is an eminent economic historian who has made important contributions to the field. In this book he shows the ambition and the erudition to venture into a wider area, and criticize the entire practice of economics and the social sciences in our time. The paradox he raises in this book has been widely discussed in recent years. It is that the standard model of economics starts off with the assumption that the individual is rational and utility maximizing, and thus will behave in certain predictable ways, among them that they will play "defect" in strategic games that have the nature of one-shot Prisoner's Di-

lemma. Yet in the real world people are nicer and less selfish than the grim neoclassical model predicts. Altruism—acting against one’s direct interest—is an important part of economic behavior. The interesting phenomenon, says Field, is not only that we drop anonymously gold coins in a Salvation Army box or serve without pay on boring university committees. The important things are acts of *omission*: members of human society do not normally commit acts of aggression and treason even when we have a chance to do so and they are demonstrably to our advantage.

The challenge Field sets for himself is to explain not only how humanity by and large maintains an altruistic equilibrium, but also how it got this way in the first place. After all, evolutionary theory suggests that individuals who selflessly sacrifice resources that could help them survive or reproduce will be selected against and disappear. On the surface, playing Nash against nonkin is always a dominant strategy. This implies that altruism is essentially impossible because altruists will always be outcompeted by selfish players even if the resulting equilibrium is Pareto dominated by cooperation.

To resolve this problem, Field relies on two recent advances in evolutionary thinking. One view comes from evolutionary psychology—most closely associated with the work of John Tooby and Leda Cosmides—and argues that the human mind consists of interactive *modules* that evolved separately. Field submits that a “cooperative module” evolved in the human mind that dictates that we do not always play Nash in one-shot Prisoners Dilemma games (or similar set-ups). This module survived the evolutionary pressures against it because of a second mechanism, namely *group selection*. Evolutionary biologists have long debated whether selection could occur at a level higher than the individual, with no clear consensus emerging except that the process is possible in principle but of uncertain importance. Field firmly embraces the view—developed by Elliott Sober and others—that group selection is not only possible but important, and that human groups with a propensity to cooperate were selected for as a whole even if *within* that group individualists would outcompete cooperators. If group size and the proportion of cooperators were sufficiently correlated, cooperative brain modules would be selected for in the population at large. In adopting this approach, Field differs in his position from Paul Rubin’s recent *Darwinian Politics* (New Brunswick, NJ: Rutgers University Press, 2002) who points to alternative mechanisms (such as mutualism) that could sustain altruism.

In this way Field explains the emergence of human institutions that otherwise would not be explicable, those that require cooperation and trust between parties. Soldiers throw themselves on hand grenades to save their buddies even if they are not related. Had humanity consisted exclusively of egoist individualists who invariably “play Nash,” reasons Field, the world as we know it could not exist. Absent a Prisoners Dilemma “solution module” [the part of the mind that makes individuals behave in an altruistic way], says Field (p. 80), organized war would be impossible. So, too, for that matter would peace.

The vast bulk of the examples chosen by Field to illustrate altruism come from recent experimental work. In this respect this reviewer felt a pang of regret: one would have hoped that as an economic historian he could have complemented the experimental results with examples from economic history, which, after all, reflect real human experience rather than the artificial environment of experiments. Almost all economic transactions involve an element of strategy; it is always possible to cheat and swindle customers, employers, investors, landlords, bankers, and tax collectors. History, of course, provides plenty of examples of “rational” dishonest behavior, yet Field’s work rightly stresses that if we are all single-minded egoists, there should have been much more of this behavior to the point of making sophisticated markets impossible.

It is historical fact that markets emerged in many corners of the earth, and that nobody has seriously questioned Adam Smith’s famous statement about the “certain propensity in human

nature to truck, barter, and exchange one thing for another.” Yet this propensity stands in direct contradiction to the axioms of selfish utility maximization, which suggests in its extreme form that such markets would collapse into the bleak world of Nash solutions. Without the cooperative module, modern markets in which trade occurs anonymously between perfect strangers with no expectation of reciprocity might have been impossible. Smith’s observation that the trading instinct “is common to all men, and to be found in no other race of animals” interestingly presages Field’s contention that the origins of this property is evolutionary.

The orthodox cliometric tradition in modern economic history has accepted almost without question the standard assumptions of rational, utility-maximizing individuals that economists normally employ. Economic agents at all times are supposed to have unitary behavior, consistent preferences, and not to take knowingly actions against their best interests. Others, who have emphasized the role of culture and cultural beliefs in economic institutions, have emphasized the diversity and contingency of outcomes. Is the assumption of strict rationality anchored in cognitive science? Field notes that the human mind is the outcome of millions of years of evolutionary pressures in small groups. Some of the activities in that environment clearly favored rational calculations and cost minimization (e.g., in foraging). But recent research in evolutionary psychology does not suggest that the kind of mind *homo economicus* is supposed to possess was at all “optimal” and made evolutionary sense. Abandoning the widely held assumption of narrow selfish rationality may resolve many puzzles that economists, including economic historians, have struggled with for a long time, including the question of altruism.

There is much in this book that gives rise to admiration. Field has set new standards for interdisciplinary erudition: he moves with ease from game theory and experimental economics to evolutionary theory and psychology. His reading is immense and the thesis advanced is presented as persuasively as possible. Whether he will actually help change the mind of true believers to abandon their strong rationality assumptions remains to be seen. The book is overly long: at times Field sounds a bit repetitive and a long chapter criticizing the work of Robert Frank is more of a digression than a help. More seriously, the evidence for a “cooperative module” is only circumstantial. There may be some inherited “modules” in the brain—such as the one that hardwires newborns to learn languages—but the existence of some hardwiring does not necessarily prove the existence of other modules that explain every puzzle in human behavior. The other foundation of Field’s theory, group selection, is also still rather controversial. Although it has been shown that it *can* happen, the relative importance of group- as opposed to individual selection remains unknown.

All the same, Field has done us a great service by writing this book. Research in economic history has justly rejected tales of “dumb peasants” and shown how economic actors displayed good sense, learned quickly about their environment, and were responsive to relative prices and technological opportunities. Yet between the two extremes of the “dumb peasant” and the hyper-rational selfish utility maximizer there is a lot of space for nuance and subtlety. Outside economics, in psychology, anthropology, and biology there are scholars who have much to teach us about how economic agents behave. It is to be hoped that some of Field’s hypotheses and insights will inspire innovative research in economic history.

JOEL MOKYR, *Northwestern University*

The Spirit of Capitalism: Nationalism and Economic Growth. By Liah Greenfeld. Cambridge, MA: Harvard University Press, 2001. Pp. xi, 541. \$45.00.

The title of this book attests to the fact that the author “communed” with Max Weber’s famous thesis whilst doing her own research. She is absolutely at one with Weber—and

Keynes! —in insisting that there is a spirit of capitalism, an irrationality underlying rational calculation whereby one continues to work even when one's needs have been met. Spirit of this sort is held to be a cultural idiosyncrasy rather than an a universal norm. However, this spirit has nothing to do with religion, not least for the central reason given by Tawney—namely that Protestant reformers sought to control the economy quite as much as did their predecessors. The author offers us a bold and sustained alternative view: economic growth resulted from one thing and one thing only—the presence of nationalism.

Much of the argument rests on Greenfeld's earlier *Nationalism: Five Roads to Modernity* (Cambridge, MA: Harvard University Press, 1992). Thus the opening chapter takes as given the notion that England developed nationalism first, adding to it here a consideration of various texts, above all those of Defoe, in which economic activity is praised because of the services it provides for the nation. All this allowed England to take an entirely different economic path from the continent, with the key social portfolio being in place, it is claimed, as early as 1600. A second chapter—going beyond the earlier book—argues that the failure of the Dutch to consolidate their early economic breakthrough has everything to do with an absence of national unity: bondholders were perfectly rational in sitting on their assets whilst economic activity was anyway not highly valued given the religious basis of society—two arguments, it should be noted, that do not fit easily together. These chapters are followed by consideration of the main type of nationalism at the core of Greenfeld's earlier work, that is, nationalisms based on resentment. France, Germany, and Japan are considered in turn, the latter being treated at very great length. A final part of the book considers the United States. Nationalism came so early here, in Greenfeld's view, that economic growth was assured. Hence her purpose in considering this case goes beyond the main thesis so far described. In a sense the book returns at the end to its beginning in seeking to explain why our culture has come to believe that, just beneath the surface, everyone is a potential utility maximizer. The explanation offered concentrates on the power of the economics profession, to which perhaps obvious notion is added the sort of idiosyncratic touch characteristic of Greenfeld's powerful and opinionated mind. American academics at the end of the nineteenth century are held, following the work of Richard Hofstadter, to have suffered from status deprivation and resentment at the newly rich. The assertiveness of economics is seen most of all as a claim to power. Crucially, Greenfeld resists many of the notions of mainstream economics. For one thing, rational choice theory is held to be nonsense: the great heroes of the Gilded Age were manifestly irrational, and nothing but their crude, uneducated passions could have built the power of America's economy. For another, the economics profession is held to favor regulation far too much. One detects here a touch of American populism: get the state and the intellectuals off the back of the wealth creators!

In the division of intellectual labor, there is much to be said for sociologists producing bold theses which set the cat amongst the pigeons. This book certainly does that, not least because it is ingenious and powerfully sustained, and it accordingly deserves widespread attention. It contains marvelous set pieces, most notably perhaps the analysis of German romanticism. Further, Greenfeld is very often right, not least when offering cautionary notes at the end about both the Euro and the hype surrounding the notion of globalization. But doubts do arise. For one thing, Greenfeld takes for granted another Weberian assumption, that of "the European dynamic." This is not necessarily easily accepted now, and it is a pity that Greenfeld has not kept up to date with recent debates. One reason why this matters is that no alternative explanations are considered: there is no discussion, for example, of the importance of regimes of useful knowledge in breakthroughs of the West. More importantly, an essential vagueness pervades the argument. No definitive specification is given as to why nationalism matters for economic growth. It is important not to be misun-

derstood here: I have no doubt that there can be a connection, as in Estonia after 1989 where national determination to survive allowed it to bear the rigors of structural adjustment without complaint. Greenfeld is probably right to say that national unity helps the economy in allowing high levels of participation; equally there may be something to the notion that nationalism raised the prestige of economic activity. There may be less to be said, however, for an additional view, implied rather than clearly stated, namely that capitalists in a new nation care more about their country than their profits. It is to be hoped that Greenfeld returns to this area, for her final thoughts on the matter would assure the impact of a very interesting book.

JOHN A. HALL, *McGill University*

Jewish Immigrant Entrepreneurship in New York and London, 1880–1914: Enterprise and Culture. By Andrew Godley. Basingstoke, England, and New York: Palgrave, 2001. Pp. xii, 187. \$60.00.

In this imaginative and readable book, Andrew Godley argues that culture matters in economics, and that some cultural traits encourage entrepreneurship, and therefore material prosperity, more than others. More specifically, he joins debates among British historians over the causes for Britain's relative economic decline around the turn of the twentieth century. He argues that British culture was in fact anti-entrepreneurial and concludes that this was likely to have had a negative impact on the country's economic fortunes. This is, therefore, really a book about Britain and its economic culture. Though it certainly has interesting insights into Jewish history as well; the author uses the Jews primarily as a "control group" in his historical "experiment."

Godley argues that Eastern European Jews emigrating to both the United States and Britain were more rapidly and thoroughly socially mobile than any other group in either society, and that this mobility came primarily through business. But American Jews improved their lot at a much faster rate than did British Jews and displayed higher rates of entrepreneurship. The question is why this was so. Although a Jewish preference for business can explain Jewish economic performance compared to other immigrant populations, it cannot explain the differences between the cohorts that wound up in different countries. After all, Godley contends, Jewish immigrants to both countries came from identical regional, social, and cultural backgrounds, left their homelands for identical reasons, and arrived with comparably small amounts of capital.

Godley dismisses the idea that structural differences between the two countries' economies could account for the difference. He notes that standard economic theory holds that the supply of entrepreneurship should follow profits. The higher the available profit, the more people will go into business for themselves rather than work for wages. But in this case, it turns out that profits in the garment industry, the principle Jewish industry in both places, were higher in Britain than in the United States—exactly the opposite of what one might expect.

So, why was there more entrepreneurship in the United States? Noting that immigrants to both countries enthusiastically embraced their adopted homelands, Godley concludes that while immigrant Jews in the United States absorbed the dominant pro-enterprise American ideology, Jews in Britain adopted anti-business British craft traditions. For the most part, this is argument by the process of elimination, but Godley does bring some positive evidence to support his conclusion. Making innovative use of British Jewish marriage records, he finds that many grooms listed themselves as "journeymen," even

though this was an inaccurate description of their status given the degraded craft conditions in the Jewish sector of the garment industry. East European Jews had no craft tradition, Godley argues, and Yiddish did not have a word for “journeyman.” Moreover, those grooms who described themselves as journeymen also showed more affinity than other blue-collar grooms for other indicators of “Englishness.” Godley concludes, therefore, that the journeymen grooms identified strongly with British craft traditions and saw skilled manual work as a preferable alternative to business.

Godley is rigorous in his methodology, but one might question a few of the book’s assumptions. First, there was indeed a craft tradition in Jewish Eastern Europe, though perhaps not as strong as that in England, and there certainly is a Yiddish word for “journeyman” (*gezel*) as opposed to apprentice or master. Second, Godley’s contention that, having come from a society with an elaborate bureaucracy and multitude of rules and regulations, Russian Jews had a predisposition to cooperate with state record-keepers seems off the mark. It ignores the widespread corruption and inefficiency of the tsarist regime, as well as the well-developed Jewish culture of evasion of the burdens imposed by a government to which they felt no allegiance. Finally, Godley overstates the degree to which New York remained a “one-sector economy,” underestimating the importance of such alternatives as storekeeping in non-Jewish, as well as Jewish, neighborhoods.

It is not clear how these assumptions affect the outcome of Godley’s economic-historical experiment. It may be that researchers will have to go even further than Godley does in integrating culture into economics—a difficult task because, as the author notes, culture is not easily measured. Still, cultural assimilation *is* observable, if not precisely quantifiable, in the writings and other artifacts left by the immigrants themselves. The next step may be to integrate their perspective into the story.

DANIEL SOYER, *Fordham University*

Monetary Standards in the Periphery: Paper, Silver and Gold, 1854–1933. Edited by Pablo Martin Acena and Jaime Reis. London: Macmillan, 2000. Pp. 264.

This conference volume brings together a collection of papers dealing with the monetary arrangements of “peripheral” countries in the later part of the nineteenth century, with occasional excursions in the interwar years. It comprises an introduction (written by the two editors plus Agustin Llonza Rodriguez) and six chapters. These chapters cover respectively the experience of six Latin countries: three are European and three are Latin American. The three European experiences are dealt with by Giuseppe Tattara (Italy), Jaime Reis (Portugal), and Pablo Martin Acena (Spain). The chapters dealing with Latin American countries are written by Winston Fritsch and Gustavo H. B. Franco (Brazil), Agustin Llonza Rodriguez (Chile), and Jose Antonio Ocampo (Colombia).

The resulting book has several merits. One is the fair amount of internal consistency it displays, which is much higher than that normally reached in this sort of volumes. Another one is that it succeeds, by bringing together leading specialists of the respective countries, in providing the general reader with a starter in these matters while also raising a number of issues which are currently being discussed on the research frontier. This makes it a valuable source for a more specialized public as well. The references provided for each chapter combine both general sources in the English language and more specific indications, including national sources. In addition, there is a wealth of statistical tables documenting exchange rates, balance of payments, and fiscal or monetary performance. Although these do not have a systematic character, they nonetheless provide information

about what is available for research purposes. The introduction, finally, is a genuine (if a bit heroic) attempt at providing a unified framework or roadmap, which the reader may use to handle the rich supply of detail contained in the volume. A detailed and through index, finally, allows the more hurried student to go directly to more specific items.

Another thing this volume does very well is (by focusing on countries whose experience with fixed-exchange-rate regimes and convertibility was less than smooth) to force us to reconsider a traditional question in monetary history. Because the literature has more often dealt with the experience of “core” countries, which faithfully adhered to gold from the mid 1870s onwards, it has tried explain why these nations stayed on fixed exchange rates for so long. But as the Introduction of this volume explains, for the countries under study, the number of years on a flexible exchange rate vastly surpasses the number of years of convertibility. The question is thus reversed: why did these nations stay on flexible rates for so long?

The key unifying answer, which is developed in a more systematic fashion in the Introduction and can also be found in various forms in several chapters, has to do with real shocks and economic structures. Following a variant of the Ricardian model of the role of relative price effects in the adjustment mechanism, the Introduction puts forward a “Scandinavian model” which focuses on the dynamics of the prices of traded vs nontraded goods. This model implies that the external adjustment will be more painful if exports are poorly diversified and the volatility of the price of exports is large. When this is the case, pressure on the exchange rate is larger and schemes to peg the exchange rate are, other things being equal, more fragile and short lived.

To a large extent, however, these ideas are not new. Leafing through the volume, however, the reader will discover lessons that are more numerous and varied. Moreover, they challenge conventional thinking on the costs and benefits of the gold standard. Convertibility, for one thing, was not associated in these countries with “sound” macroeconomic practices, as illustrated by the Portuguese case. As Jaime Reis argues, wearing the gold-standard badge of honor “did not necessarily mean behaving decently.” Moreover, everybody understood that, as illustrated by the fact that “Portugal was never able to obtain good terms for its external loans, just as the price at which they usually stood in international markets implied a high risk premium.” (p. 103). The “choice” of going on gold or floating, to the extent there was a choice, seems instead to have been driven by political considerations. Alternative arrangements advantaged one group or another and the prevalence of given systems often reflected the balance of power. Some chapters do raise some intriguing issues, such as the experience of the Bank of Spain, which after 1903 resisted the government attempts to give it more independence, displaying a strong preference towards providing government finance (Martin Acena). Clearly, these chapters show that study of the political economy of nineteenth-century macroeconomic institutions deserves a closer attention than they have received so far.

Quite importantly, several chapters also show that the choice of the exchange-rate regime was heavily influenced by financial markets. Capital flight when a large portion of the debt was denominated in a foreign currency created the risk that an exchange crisis would become of a debt crisis (this is discussed in detail by Tattara and more briefly mentioned about Brazil in the 1890s by Fritsch and Franco). Similarly, an influx of capital when the currency floated threatened competitiveness through real appreciation. This was, according to Fritsch and Franco, the main reason for Brazil’s adoption of the gold standard after 1906. Finally, a fixed-exchange-rate system could in some cases conflict with domestic financial stability. The Chilean experience, told by Llona Rodriguez, suggests that fixed-exchange-rate arrangements on the other hand conflicted with domestic financial fragility by preventing the action of a Lender of Last Resort. This would have, accordingly, explained Chile’s collapse from convertibility both in 1878 and 1898.

The hypochondriac reader may complain that this financial interpretation of the experience with gold convertibility in the periphery, in effect partly acknowledged in the Introduction, does conflict with the relative prices model discussed previously, and ask for a bit of clarification. In a sense the emphasis on financial phenomena would suggest that peripheral countries, which were more volatile and thus more vulnerable to capital flight, would have had more motives to go all the way to a strict gold standard. So why did they float so often? This reviewer will merely congratulate the editors for putting together a volume that should help the community to identify the relevant questions for future research.

MARC FLANDREAU, *Institut d'Etudes Politiques de Paris*

Early Globalization and the Economic Development of the United States and Brazil. By John DeWitt. Westport, CT: Praeger, 2002. Pp. 178.

The author of this ambitious volume tackles the question: why is the United States rich and Brazil poor? Given the importance of the question and the promising comparative approach, readers of this JOURNAL will be tempted to look into John DeWitt's book. Unfortunately, they are likely to be disappointed. The author claims that internal and external factors combined to generate growth in the United States and to breed underdevelopment in Brazil. The external factors hinge on the purported unfairness of the international system. Brazil, according to DeWitt, was "a weak state that could be treated like a palooka and pummeled with impunity" (p. 113). The internal factors of growth or backwardness adduced in the book are based on a series of case studies of regions or industries, such as coastal towns or whaling, along with generalizations about plantation economics. Although there are many useful insights sprinkled throughout the book, the methodology and bibliography are confused and outdated: there are no time series or statistical tests employed in the text; quantitative data are few and almost entirely descriptive; and no mention is made of recent publications emphasizing institutions and factor endowments as sources of economic divergence between the United States and Latin America. In particular, it is troubling that no mention is made in the text or bibliography to Stephen Haber's edited volume, *How Latin America Fell Behind* (Stanford, CT: Stanford University Press, 1997). This omission prevents DeWitt from addressing the standard text in the literature and severely detracts from the volume's credibility.

To be fair to the author, he does make some relevant points about the way plantations and slavery tend not to be conducive to industrial development, at least in the zones where they hold sway, and how Brazil's fragmented geography hindered the development of domestic trade and industry. But these facts have been well established for a long time—and it is far from clear that plantation agriculture is detrimental to industrial development economy-wide, after all, coffee led to industry in São Paulo. Likewise, there is promise in his comparative method. His comparison of Salem, Massachusetts and Paraty, a port town near Rio de Janeiro, is apt and well drawn. Yet the reader is left to draw her own broader conclusions. Was the failure of Paraty to industrialize a sign of some deeper problem in Brazil's coastal towns? Was Salem characteristic of other New England towns? The text leaves such questions largely unanswered.

DeWitt makes much of alleged similarities between the U.S. South and Brazil in general. Plantation slavery and rule by large landowners, present in both regions, is taken as a basic source of backwardness. Certainly, slave owners were politically retrograde: they limited political participation and invested but little in improving their stock of human capital—resulting, arguably, in bad political and economic institutions. For this line of reasoning to

work, however, we would have to be convinced that, absent slavery, Brazil would have looked more like the northern United States. There are many reasons to believe that Brazil's counterfactual line of development would have resembled Argentina's instead—with a continued predominance of patrimonialism, inequality, and limited political participation. In other words, slavery was not the only source of institutional backwardness in Brazil.

Finally, DeWitt seems unaware of the existence of a variety of estimates of GDP per head that show Brazil ahead of the United States through 1700 and the United States South on par with the North through the first half of the 1800s. If this is so, it makes little sense to blame Brazilians and Southerners in the United States for pursuing an economic strategy that placed them near the top of world wealth tables circa 1800. In hindsight, the political economy of New England proved better suited to rapid growth through commerce and industry in the nineteenth century; it, along with a few fortunate places around the globe, was the exception. Perhaps the fundamental problem with this book, and many like it, is that the comparison of the United States and other economies is often invidious and distorting. Other comparisons may prove more illuminating.

ZEPHYR FRANK, *Stanford University*

International Trade and Political Conflict: Commerce, Coalitions, and Mobility. By Michael J Hiscox. Princeton, NJ, and Oxford: Princeton University Press, 2002. Pp xiv, 209. \$49.50, cloth; \$18.95, paper.

This short book has a novel thesis, which is that the degree of factor mobility at the national level influenced the politics of foreign-trade policies. When factor mobility was high, tariff legislation was class legislation. When mobility was low, tariffs were decided by interest-group competition. Michael Hiscox brings data on mobility to bear on the history of foreign trade policies of the six countries—the United States, Britain, France, Sweden, Canada, and Australia—over the last one or two hundred years or so, devoting a chapter to each. He then tests his ideas quantitatively on U.S. congressional voting between 1924 and 1994, finding that, when the indicators of factor mobility were low, an “interest group theory” better explains U.S. tariff politics than does a “class legislation theory” (and the reverse when mobility was high).

Accepted by Hiscox is the claim that foreign-trade policy depended on the kind of political lobbies, coalitions, or political parties that formed around the issue. Also accepted is the idea that lobbies, coalitions, or parties formed in reaction to or anticipation of the effects of tariffs on the incomes of factors of production. What is novel is the simple and powerful idea that the nature of the political struggle over tariffs was determined by the degree of factor mobility between industries.

If factors of production were quite *immobile* between industries, then a tariff on imports of iron manufactures, say, would increase the demand for the domestic factors used in iron products. Ironworkers and iron masters would have a common interest in obtaining import protection of their products. The story can be extended to include factors used to produce intermediate inputs. So, when factors of production are immobile between industries, tariff lobbies form at the industry level. Elected representatives will tend to vote on each tariff item according to what industries are located in their electoral districts.

In contrast, if factors of production are very *mobile* between industries, then an import duty protecting one industry or subset of industries will change the *national* prices of factors of production. The effects of an iron tariff on factor incomes will not be confined

to ironworkers and iron capitalists. In fact, a system of import protection will tend to divide the interests of the various factors of production, rather than unite them. Then, said Hiscox, the tariff takes on the nature of *class* legislation.

Here, I expected more discussion of Mancur Olson Jr.'s argument in *The Logic of Collective Action* (Cambridge, MA: Cambridge University Press, 1965), that narrowly based interest groups can better overcome their "free rider" problems than can broadly based interests.

For his national indices of factor mobility, Hiscox assembled data on the interindustry dispersion of wages and profit rates across the manufacturing sector over long periods. These historical data are most interesting in themselves, as they mostly show U-shaped patterns: a long fall, followed by a rise in the twentieth century. Their main purpose, however, is to act as lenses through which the histories of six tariffs may be examined. For example, factor mobility has been low in France, so in French politics sectional interests cut sharply across class politics. An implication is that an antiprotectionist policy stance was facilitated in Sweden by deliberate policies to keep labor and capital mobile (as Hiscox relates).

Reading these chapters, it was sometimes unclear to me when the main focus was on variations in the general level of protection, and when on the nature of the political conflict. Also, for Australia, Hiscox's story ended too soon, with the newly elected Labor Party in 1983 taking a united stand against tariff cuts (p. 126); however, in power over the next decade, Labor switched micro-economic policy regimes, and slashed tariff protection.

Mono-causal explanations can change the way other scholars think about an issue. This book changed mine. It is well written, and convincing in its main argument, which should be taken up by students of the histories of tariffs and other regulations.

The book is based on a work that won the American Political Science Association prize in 1997 for the best dissertation in International Relations.

JONATHAN PINCUS, *Productivity Commission, Melbourne*

The Record of Global Economic Development. By Eric Jones. Northampton, MA: Edward Elgar, 2000. Pp. xviii, 226.

For those taken in by the title of this book (as I was, in agreeing to review it) this volume will be a disappointment. It turns out to be a collection of lectures which, as the author tells us, "reflects the development of my interests in recent years, involving themes such as the rise of East Asia, Protectionism, and cultural, institutional, and structural changes . . . all informed by my original professional perspective as an economic historian" (p. xvi).

Part 1 (chapters 1–4) is largely a restatement of the theses developed in *The European Miracle* (New York: Cambridge University Press, 1981, 1987) and *Growth Recurring* (New York: Oxford University Press, 1988), and a response to critics of these volumes. The subject of the two chapters of Part 2 is "Protectionism." The first rebuts on the basis of the historical record the view that present day English farmers deserve reimbursement for safeguarding the environment. The second, in a stunning disciplinary transposition of the concept of protectionism, refutes the view ascribed to linguists that "each and every language on the planet" should be preserved, pointing out that there are opportunity costs. The three chapters of Part 3 on the East Asian Miracle and recent crisis place particular emphasis on rejecting "cultural" explanations of rapid growth there, and stress the establishment of Smithian-type free-market institutions, albeit imperfect—hence the crisis. The first chapter of Part 4 offers some prescriptions for Australian businessmen for coping with what they perceive as the threat of

“globalization.” The concluding chapter of *The Record of Global Economic Development* is a history of Australia’s retail grocery industry in the twentieth century.

As always, Eric Jones is anything but dull. He happily takes on big questions, and writes in a knowledgeable, assured, and lively style. He is appropriately dismissive of the provincialism of economic history, stressing the need for a long worldwide space and time perspective of the type he employs.

An overall assessment of this disparate collection is hardly possible. Instead, let me confine myself to a general observation on Jones’s methodology. Repeatedly in the book he is critical of a “school of thought” he terms “the quantifiers,” a term he comes close to using as a pejorative. He tells us that “typically” the quantifiers dismiss earlier periods [before the 1820s] as of little interest . . . [that] they show no interest in regions other than the West . . . [and that their] professional bias . . . is . . . away from the documentary research required to investigate periods lacking in predigested statistics” (p. 4). If he truly believes in this caricature, what prevents him from breaking new ground? For how, without quantitative evidence, are we to believe Jones’s own arguments? For example, he tells us in this volume, as he did in *Growth Recurring*, that Song China from the tenth to thirteenth centuries AD was a case of “intensive growth,” that is, rapidly rising real GDP per capita. His stated criteria include “reports of *large* investments in infrastructure, especially communications; *noteworthy and well-diffused* improvements in productive technology . . . ; *marked* urbanization; and *structural change* via the withdrawal of labour, or labour time, from farming into cottage industry . . .” (pp. 13–14, italics added). What is the meaning of adjectives like “large,” “noteworthy,” and “marked” in this context? Do they mean as large, noteworthy, and marked as the change per unit time observed in similar magnitudes in the experience of economies documented as experiencing intensive growth? Without numbers to back up these adjectives, how are we to know? If Robert Fogel’s painstaking work (dare one say “documentary research”) a half century ago could provide plausible estimates of the cost of a virtual canal system in the early-nineteenth-century United States, might not comparable ingenuity be applied to Song China?

Lacking this, we must either accept Jones’s assurances that “the consensus of sober professional opinion” among general historians makes his case, and that this opinion “has the *more* validity because so *few* general historians are seriously interested in the economy” (p. 13, italics added), or we can wish, as I do, that the recent development of the interests of this talented economic historian had led him into pioneering efforts at quantification.

RICHARD A. EASTERLIN, *University of Southern California*

Painting outside the Lines: Patterns of Creativity in Modern Art. By David W. Galenson. Cambridge, MA and London: Harvard University Press, 2001. Pp. xvi, 251. \$29.95.

In this engaging book, David Galenson formulates a complex question about modern art that he tries to answer with statistics, analysis, and exposition, enlivened with a rich sprinkling of well-chosen quotes. Posing an initial question “At what stage of their lives have modern painters normally done their best work?,” he finds that this age varies widely from artist to artist. This leads to the central problem: “is it by chance that some have made their greatest contributions early in their careers, and others late in theirs, or is there some general explanation that accounts for the variation?” (p. 4).

As the reader is made aware from the start, Galenson claims indeed to have discovered a general explanation. “General,” at least, for the two groups of artists he studies: “fifty painters born from 1796 through 1900 who lived and worked in France, and seventy-five

American painters born from 1870 through 1940 These painters were chosen to include all the artists whom art historians consider to have been the most important figures in two key periods in the history of modern art” (p. 5). Galenson compiled charts on his two groups, with the “peak age” of each artist measured by the prices of their paintings at 260,000 auctions held between 1970 and 1997 (pp. 16–17).

As a control measure of significance, Galenson compared the judgment of the market with that of art historians. For the French artists, art-historical ranking is derived from the illustrations in “thirty-three books that have been published in English since 1968 and that provided illustrated surveys of at least the full history of modern painting” (p. 24). Qualitative judgment of the Americans is equated with the choice from their works included in retrospective exhibitions. Although the age-price highpoint does not coincide perfectly with art-historical judgment, it comes close enough for Galenson to conclude that “an artist’s most valuable work is usually also that which experts consider his most important” (p. 31).

In both groups, Galenson found—and this is the crux of his book—“there was a tendency over time for artists to produce their most valuable work . . . at progressively younger ages” (p. 33). The key examples for the French are Paul Cézanne (b. 1839), who created his most highly valued work at the age of 67, as against Pablo Picasso (b. 1881), who hit his personal ceiling at 26. A parallel phenomenon in the American group is provided by Mark Rothko (b. 1903), who topped out at 54, compared to Jasper Johns (b. 1930), who has never improved on his work of the age of 27. These pairs typify the experience of their respective generations.

The rest of *Painting outside the Lines* is devoted to accounting for this effect. Galenson provides two main kinds of explanation that I find unconvincing in themselves and not altogether compatible with each other. One concerns the artistic temperament, the other the behavior of markets.

Innovative artists come in two varieties, he says, the experimentalists and the conceptualists. Experimentalists never stop worrying about how to make their art better, and conceptualists never start—they pre-plan, produce, and then move on to something new. The former group ripens slowly and reaches maturity late, the latter makes a quick splash on the scene which they subsequently are unable to repeat. These labels and their application, although backed up by intriguing anecdotes and analyses, retain a considerable measure of arbitrariness. Vincent van Gogh, whom I would have ranked among the experimenters, is a conceptualist in Galenson’s book. Moreover, Galenson does not acknowledge that the differences behind these two work attitudes are deeply personal. He assigns them to whole generations at a time, relating them not to individual psychology but to career choices. The Impressionists and Abstract Expressionists are experimentalists, he writes, whereas the Post-Impressionists and Cubists and Pop Artists were conceptualists.

Underlying the shift from slow to fast workers is a theory about the art market. At the moment when the older generation is accepted, their work skyrockets in value and is snapped up by the market. This leaves a gap to be filled by quick-thinking newcomers (p. 130). Here too Galenson loses me. It would seem more logical to assume that new arrivals at a scene like that described would be better off producing work in the style of the recently established classics rather than taking off in risky new directions. In such a situation, one would also expect the younger cohort to hit their stride faster than the older one, irrespective of eventual differences in style or working practice.

I have a more fundamental objection to *Painting outside the Lines*. To my mind, the entire book is based on a simple fallacy. Galenson measures what was important in French nineteenth-century art by 33 books published in English since 1968 and (the obviously linked) prices on the art market of the same period. He shows no awareness that this is a time- and place-bound judgment. Had he consulted French books, exhibitions, and auction

results of the period 1900–1935, a move that would have created a better parallel for his American cohort, who are judged in his book by fellow countrymen of the immediately following generations, his chart would have looked completely different. It would have had different names, different price ratios, and different peak ages for the masters involved. In other words, Galenson is insisting on a correlation between a moving variable, the shifting sands of taste, and a fixed value, the historically unique behavior of certain individuals in a given situation. His criteria, it must also be said, betray a dismayingly American bias when it comes to establishing hierarchies of artistic importance.

Despite the defects of its central argument, *Painting outside the Lines* is a stimulating read and a welcome attempt to apply quantitative research methods to modern art.

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The Origins of Nonliberal Capitalism: Germany and Japan in Comparison. Edited by Wolfgang Streeck and Kozo Yamamura. Ithaca, NY: Cornell University Press, 2001. Pp. xvii, 261.

This volume explores phenomena frequently noted (yet seldom analyzed) in the scholarly literature: the profound similarities in the industrialization processes and the contemporary political economies of Germany and Japan. These parallels—not just in the early stages of industrialization, but through the experiences of depression and war, and on to the rise of postwar “miracle” economies in both nations—are often casually ascribed to the late-developer effect, to the strategic imitation of German economic institutions in Japan, or to cultural factors, from lingering “feudal remnants” to enduring “traditional” social structures. Tagging the economic regimes which had evolved in Germany and Japan by the 1970s “nonliberal” capitalist systems, the essays in this collection seek to investigate systematically “the many similarities between the two capitalisms, the no less intriguing differences between them, and the differences between the two and Anglo-American ‘standard capitalism’” (p. xiii). More specifically, this volume examines “the origins of some of the social institutions that have constrained the spread of free markets within the capitalist economies of Germany and Japan while providing them with alternate mechanisms of economic governance” (p. 5). Throughout, the contributors argue for a more subtle, historically grounded, and systematic understanding of the distinctive practices and institutions of the German and Japanese “nationally embedded capitalisms.”

An introduction by Wolfgang Streeck provides a lucid overview of the concept of nonliberal capitalism, methodological assumptions, and the major arguments of the following chapters. Some of Streeck’s observations—his synthesis of parallels and differences in the evolutionary trajectories of German and Japanese capitalisms and, above all, his speculations on the future prospects of the nonliberal capitalist systems—would have been more appropriate in an afterword or concluding chapter (which this volume lacks entirely). The five empirical essays that make up the body of this work, while not providing a comprehensive treatment of the German and Japanese political economies, are impressively varied in terms of subject matter, approach, and conclusions. Gerhard Lehbruch, for example, concentrates on the discourses of embedded capitalism in Germany and Japan, arguing that the congruence of institutions and hegemonic belief systems ensured the path-dependent evolution of production regimes. Philip Manow’s essay on the origins of social welfare systems stresses the impact of industrialization in pre-democratic environments and shows how nonliberal welfare arrangements have “helped to underpin trusting, long-term coopera-

tion between economic agents on the shop floor" (p. 119). Gregory Jackson, on the other hand, rejects the late-developer thesis in his study of corporate governance, detailing the historically contingent "co-evolution" of long-term commitments of capital and labor to Japanese and German firms. In a similar vein, Sigurt Vitols argues persuasively that the distinctive banking systems of Germany and Japan "rather than being a heritage of a pre-industrial past [were] a product of modern political construction" (p. 18), and specifically, of the states' regulatory responses to interwar financial crises. Finally, Kathleen Thelen and Ikuo Kume's contribution, reprinted from the 1999 volume of the *Journal of Japanese Studies*, demonstrates how late development and disparate indigenous models of skill formation shaped the distinctive industrial training practices of Germany and Japan.

This collection does not focus specifically on the future prospects of the now-troubled German and Japanese economies, although a planned companion volume will concentrate on their transitions since the 1990s. Nevertheless, the authors here provide a strong historical foundation for considering the potential trajectories of nonliberal capitalist systems in the new millennium. As Streeck and others suggest, further economic liberalization seems inevitable in an age of global capital, U.S. economic ascendance, and declining "state capacity" in Germany and Japan. But while change in longstanding institutions, practices and discourses may be certain, how painful this process will be and how closely Germany and Japan will converge toward America's "normal" capitalism remain far less apparent.

This volume is not without its weaknesses. Notably, because only one contributor is a specialist in Japanese studies, almost no use is made of Japanese-language sources, and the authors' characterizations of Japanese culture and "traditional" institutions are often excessively stylized. On the whole, however, this is an unusually valuable work on an understudied topic; ambitious in its comparative, interdisciplinary focus, it achieves an admirable balance between historical detail and schematic simplification.

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Gender, Growth and Trade: The Miracle Economies of the Postwar Years. By David Kucera. London: Routledge, 2001. Pp. xi, 217.

The central theme of this book is the interplay of women's employment and the macro-economy in postwar Germany and Japan. David Kucera introduces the distinction between two possible types of flexibility in the labor market. The first occurs in the absence of interference with the market mechanism from laws or institutions. This flexibility may reduce unemployment and increase the response time to shocks, at the cost of low wages and insufficient training. In the second type of flexibility, a buffer group provides this flexibility, while core workers are protected from the vagaries of the market and receive training and high wages. Kucera argues that by using women as a buffer group to protect men, Japan is able at a macro-level to reap the benefits of both the "low-road" and "high-road" approaches. Bolstering this argument, and showing that Germany pursues instead the simpler "high-road" strategy, is the most important aim of the book.

In the literature review of the first chapter, Kucera concludes that no convincing link has been shown between interference in the labor market and higher unemployment or lower employment, an assessment with which I agree. This suggests to me that Japanese duality might be unnecessary, particularly as Kucera notes that adjustment of hours in Germany makes up to a large extent for the lack of flexibility in the number of workers. Nevertheless, because this literature is not conclusive, an appraisal of the dual system is certainly valu-

able, and Kucera's review of existing papers analyzing Japanese dualism shows that an extension and update are warranted.

In the second chapter, Kucera conducts regressions for OECD countries to demonstrate that the phenomenon of women's employment being more procyclical than men's is much more pronounced in Japan than elsewhere, and interestingly, that it is also more pronounced in the United States than in (West) Germany. In analysis by sector, he finds that women do act as a buffer in certain German industries. Finally, in a cross-section analysis of industries by country, he analyzes the determinants of the procyclicality of women's employment. A thought-provoking result of this analysis is that in Germany and the United States, women act as a buffer in industries where they are a large fraction of employment, whereas in Japan the opposite is true.

Kucera devotes the third chapter to an investigation of institutional reasons that might explain these results. He observes that unions are stronger in Germany, and claims that women have more influence in the union movement there. Japanese women work on temporary contracts, and leave the labor force when they are of child-bearing age or during recessions. This appears to be due to a combination of lack of child care and maternity leave, and the knowledge that for cultural reasons or as a way of job rationing they will be unlikely to find a job. Kucera also points to lack of child care in West Germany as a factor reducing female labor-force participation. In both the Japanese and German cases, this provokes the question of why the market does not supply child care: is there market failure due to imperfect information, or is there merely difficulty in moving from one equilibrium to another? The later discussion of eastern German women suggests that they have sustained an equilibrium with higher child care, employment, and unemployment. The combination of the regression and institutional analysis makes a convincing case that dualism exists in Japan, and has the benefits and costs claimed. A less convincing line of argument in this chapter is connected with explaining why dualism has become stronger in Japan since the 1970s.

In the fourth chapter, Kucera returns to original quantitative analysis by testing the hypothesis that trade with poorer countries has disproportionately hurt female employment in rich countries. This is an intriguing hypothesis, which Kucera finds to be true of Germany but not of Japan, but he admits that, more generally, industry shifts have been very favorable towards women. Furthermore, he uses the factor content of trade method, which is generally frowned on by trade economists. In this chapter he also probes the question of why the gender wage gap is falling in Germany and rising in Japan.

The last chapter focuses on how the picture for Germany has been altered in recent years by the reunification experience of the formerly communist part of the country. Kucera summarizes very perceptively the literature showing how the position of eastern women worsened with transition.

The comparison of Germany and Japan is very fruitful. If one has a certain familiarity with the labor markets of the two countries, one knows that in neither Japan nor West Germany has the position of women in the labor market been a very strong one. The contrasts are thus somewhat surprising. The book also counteracts a tendency in western accounts of the Japanese labor market to focus on (male) life-time employment as its defining characteristic.

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At Home and Abroad. By Francine D. Blau and Lawrence M. Kahn. New York: Russell Sage Foundation, 2002. Pp. xi, 314. \$34.95.

In the 1970s the United States had far higher wages than the rest of the developed world; 1979–1981 median weekly earnings of men in full-time employment were \$609 in 1998

prices, compared to an average of \$419 across six other developed countries. However, the United States also had a higher unemployment rate; in 1973 it was 4.8 percent compared to an average of 2.1 percent in 11 other countries. Fast forward two decades. Median real wages in the United States, although still higher than in the other countries, had fallen 5.5 percent whereas in the other countries they had increased an average of 22.6 percent. However, whereas the unemployment rate in the United States fell slightly over the period, it skyrocketed in most of the other countries, averaging 8.2 percent in the European Union in 1999. By the late 1990s the United States also differed from the rest of the developed world in a number of other labor-market outcomes. The U.S. had: a lower average duration of unemployment and less prevalent long-term unemployment, higher labor-force participation rates among both men and women, longer average hours of work over the course of the year, and greater earnings inequality.

The reasons behind this divergence in labor-market outcomes are the topic of this book. Francine Blau and Lawrence Kahn examine an enormous amount literature that has been written over the past two decades. There are two main schools of thought. First, differences in outcomes are due primarily to differences in the relative supply and demand of skilled and unskilled workers. Secondly, different sets of labor-market institutions led to different adjustment mechanisms in the face of economic shocks. Although the supply and demand hypothesis should be intuitively straightforward to readers of this *JOURNAL*, the institutional hypothesis, which is the main focus of the book, deserves further explanation. The United States has the most flexible labor market in the developed world. Relative to other developed countries, in the United States: union density and collective-bargaining coverage are much lower, the minimum wage is a lower proportion of average hourly earnings, it is easier and less costly for firms to dismiss workers, and unemployment benefits are less generous in terms of both the replacement ratio and the length of coverage. According to the institutional hypothesis or “unified theory,” this labor-market flexibility allowed American firms to adjust to adverse demand shocks by reducing wages. On the other hand, labor-market rigidities in other countries prevented wage reductions and firms could only respond to adverse shocks by reducing employment. The hypothesis is compelling and provides an explanation for a wide range of differences in labor-market outcomes, such as the observed differences in wages, the unemployment rate, the duration of unemployment, and the participation rates.

After outlining the evidence on differences in outcomes and institutions, Blau and Kahn turn to the critical issue of whether the evidence supports the institutional hypothesis. They examine an enormous number of both macroeconomic and microeconomic studies, much of it their own research. The macroeconomic evidence indicates that a wide range of institutions including the bargaining environment, active labor-market policies, and unemployment insurance have a statistically significant impact on cross-country differences in unemployment rates over the period. They regard this evidence to be supportive of the institutional hypothesis, but far from conclusive, as macro models can be very sensitive to research design. They then examine the microeconomic literature. This literature uses several very different research designs including within-country analysis where institutions have different relevance to different individual, within-country analysis where institution design changes over time, and cross-country micro-level comparisons. The evidence is less equivocal than with the macroeconomic studies, but on balance supports the view that institutions have far-reaching effects on labor-market outcomes at the individual level. One of the strongest conclusions consistently supported by the evidence is that, after controlling for personal characteristics, wage inequality is far greater in the United States than in other countries and that the difference in inequality can be directly attributed to labor-market institutions.

Many readers of this JOURNAL may find the time period covered by this book (roughly 1970–2000) to be too recent to be considered economic history. In my view this would be a shame. This book sets out to analyze an important issue from recent economic history and does a very commendable job. The authors' research is wide in scope and highly credible. Their approach is likely to shed further light on other historical episodes where national labor markets had divergent outcomes, for example the Great Depression. The book also offers a clear implication to policy makers, namely that there are no free lunches, and policies designed to increase wages or reduce inequality are likely to have employment costs.

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