
Academics live in a world where it is (mostly) no longer considered acceptable to assign tasks and the compensation for their performance purely on the basis of gender. However, nagging evidence of the continuation of past practice in this regard is abundant, if indeed troubling to neoclassical economists whose intellectual paradigm suggests that competitive market forces will correct for inefficiencies in labor allocation decisions based on discrimination alone. Thus, explaining the often radical differences in men’s and women’s work patterns in the past, let alone their persistence into the present, has become an important agenda item for economic history. The resulting explanations are, moreover, forced to shoulder a heavy burden of social and political freight, as gender inequality in the control of economic resources is a hot topic both in the popular press and among policy makers. In such a climate it behooves us to get the explanations right.

Sheilagh Ogilvie’s much expanded and revised thesis, published as A Bitter Living: Women, Markets, and Social Capital in Early Modern Germany, makes an important contribution to this task of getting it right. She begins this case study of Germany’s Württemberg Black Forest region by outlining the three questions she understands to be fundamental to the larger enterprise of understanding why “women have less access to resources” than men and why this has been “true of every society in history?” (pp. 1–2). First, what are the facts about women’s and men’s work in the past? Second, why are the facts they way they are? And third, what are the more general implications of women’s economic position? (p. 2). She next reminds her readers that despite her sweeping claim about inequality everywhere and everywhen, the detailed answers to the seemingly straightforward first question are not as well understood as we might expect. Scholars have been no more successful in documenting a linear progression towards greater gender equity across time than they have been in locating a “golden age of women” in some distant and long ago place. Rather, the search for data to answer the “what” question has yielded a remarkable diversity of experiences, which often prove hard, if not impossible, to rank order clearly.

It is precisely this diversity of past experience that should make us suspicious of easy answers to the “why” question as well. Yet the public arena of ideas demands easy answers (that is ones which are moncausal and, even better, linear). And there is no shortage of candidates. The three most important according to Ogilvie are the “technological,” which “regards women’s work as being determined by their physical endowments” (p. 7); the “cultural,” which looks to customs and mentalities, such as an ideology or creed of patriarchy (p. 10); and the “institutional,” which widens “the focus to the organization of the society in which women have to produce and reproduce” (p. 11). Ogilvie rejects all of these explanations, and their many subvariants, in favor of what she identifies as a “time allocation” approach. In this view, both men and women make “time allocation decisions within a framework of constraints” which include, among other things, technology, cultural norms, and institutions (p. 13). The pervasive but diverse range of economic inequities faced by women then comes out of this framework of constraints, the particulars of which can and do vary tremendously across time and space.
After such a broad and compelling introduction, the substantive chapters of the book get down to the historical nitty-gritty of making her case. A data-intensive documentation of men’s and women’s actual work experiences in early modern southwest Germany is used to debunk (sometimes repetitively) the three main monocausal explanations. The ingenious use of church-court records, in particular the Sabbath breaking cases, allows Ogilvie to infer the time individuals allocated to various kinds of tasks, as well as to identify the location of those tasks, either in the home or out of it. Because this evidence and analysis are central, the reader will probably continue to ask how robust the findings are, despite Ogilvie’s care in addressing that issue.

Only a few of the numerous and nuanced results can be highlighted here. Despite the clear disabilities associated with child bearing, primary responsibility for housework, dependent status within the household, and legal exclusions from community decision-making and guild participation, women of all ages and marital status could be found regularly engaging in market work, which often was heavy or took them far from home. Indeed, women were most conspicuously absent from the lighter, domestically located industrial work of proto-industrial textile manufacture for the export market. Women may have proved to be highly flexible in their work time allocations, but they were also highly exploitable as a result (p. 337).

Only in the closing pages of the book does Ogilvie seriously address the last of her initial questions, the “so what” most important to policy-makers. Not surprisingly, Ogilvie documents every aspect of what she calls “this perverse regime” as negative (p. 348). The exploitation of female labor, coupled with the legal and status disabilities suffered by wives, daughters, and independent women, was clearly bad for the women themselves. It was also bad for their children, whose health and welfare were compromised by the pressures on the mother’s time and consumption possibilities. The total productive capacity of the wider economy was also curtailed by the cultural and institutional constraints on a woman’s free allocation of her own time to what would have otherwise been presumably its most productive use. If this were not enough to spur the policymakers of the modern world to action, Ogilvie speculates at the very close of the book that “it is surely no coincidence that those European societies in which the industrious revolution began earliest and most intensely were . . . [those] where the institutional obstacles facing women who sought to change their time-allocation patterns were weakest” (p. 350). Functioning markets and access to capital are all well and good as far they go, but for Ogilvie the long road to development is paved with gender equity.

Anne E. C. McCants, Massachusetts Institute of Technology


Richard Unger’s study of beer is more than its title implies. The first chapters review brewing beer from the earliest documented instances in Mesopotamia and Egypt, giving ample attention to the technical problems of fermentation and preservation of potable beverages as presented in scientific literature. They also review the anthropological literature on brewing over time in diverse environments, providing a global setting for the subsequent, more-detailed findings on beer brewing from the twelfth through the early seventeenth century in Europe. An epilogue on the industrialization of brewing in the nineteenth century completes the framework that marks Medieval and Renaissance brewing as the transformational moment in a long and complex history.
The longitudinal approach in fact raises the possibility that brewing rather than the more commonly featured textile, grain, salt, or spice trades may best exemplify the changes brought by an emerging market economy and then by full-fledged capitalist development in Western Europe. By Unger’s count, beer underwent at least six distinct phases in somewhat over three centuries, the earlier and faster-paced in the Middle Ages followed by slower changes in the Renaissance centuries (pp. 231–32).

In the first period, twelfth to thirteenth century, urbanization led to specialization and the triumph of brewers in towns over home brewers. The introduction of hops, which replaced gruit (an herb mixture) as a flavoring additive and promoted this transformation, was followed by a spread of the technology that made hopped beer preferable in many different venues. Preservation of the product was a key to success, while taste may or may not have contributed to the triumph of the new hopped product. New markets for hopped beer took considerable time to establish themselves north of the beer-wine boundary line in Europe, yet even that boundary line proved elastic as hopped beer consumption spread south and east. A fourth phase, adjusting existing techniques to the production of this drink, and adjusting beer to the tastes of consumers, followed. Acclimatization to local conditions was followed by a fifth period when the consistency of the product increased and quantities began to satisfy demand. Bremen and Hamburg spear-headed these changes, which spread to Holland, Brabant, Flanders, England, Prussia, Scandinavia, and finally south to Bavaria by the late fifteenth and sixteenth centuries. With maturity in the trade, innovation switched from the product itself to ways of producing beer that made the most of investment and scale of production. A sixth phase of process innovation created the industry of the seventeenth century, marked by relatively fewer breweries of about equal size in terms of workers employed.

Throughout, taxing policies and urban, state, and royal laws influenced the industry’s course of development. Unger’s decision to consider brewing guilds in a separate chapter (pp. 207–30) has the effect of downplaying their overall influence on the beer trade’s development while emphasizing the consequences of the regulatory and taxing power of various concerned political entities, local to royal.

Along the way Unger relates details of regional differences in adapting to trade in hopped beer. Based upon his earlier study, *The History of Brewing in Holland* (Leiden: Brill, 2001) he notes that the importation of beer from Bremen and Hamburg disturbed the Count of Holland sufficiently in the fourteenth century that he actually banned hopped beer for a brief time (p. 77). The English resisted the adoption of imported hopped beer, perhaps out of taste preference for their native ale, but in time they too succumbed, and an important brewing industry was established in London. Wherever north German beer traveled it faced taxation.

The figures for consumption that Unger supplies are prodigious. For example, at Stockholm Castle in 1558 the daily rate of consumption per person was 4.5 liters. When rules were revised in 1577 aristocrats received 5.2 liters whereas tradesmen and workers got a mere 3.9 liters (p. 129). Keep in mind, however, that an aristocrat would be obligated to supply drink to his own body servants, who might in turn employ their own servants. Here, as on board ships, beer may have been of low quality and alcoholic content, “small” beer serving as a way to preserve a potable water supply in difficult circumstances. Nonetheless, the amount of beer consumed in Europe appears to have been impressive.

Brewing in noble households, a significant development by the sixteenth century, raises another interesting question. Whereas the Low Countries protected urban brewers from rural competition, England did not (p. 203), and England’s noble households brewed in significant quantities. In 1552 one English country house put
out over 83,000 liters of beer in a year (p. 112). In Eastern Europe such an assault on urban beer production would likely be chalked up to returning the economy to pre-urban modes of production, yet sixteenth century England is one of the early examples of full-fledged capitalist endeavor in the literature of economic history. Did aristocratic brewing in the rural countryside fail to cut into urban brewers’ markets in England? If so, why? Here, as elsewhere in this complex and detailed treatment ranging over five centuries of development, beer brewing may provide a more useful case study of economic change than other industries and trades. This study of the fortunes of improved production and technology and the emergence of elaborate trade networks that adapted to local conditions, then readapted again many times over, is a very welcome addition to the economic history literature.

SUSAN MOSHER STUARD, Haverford College

MODERN EUROPE


This book contributes to the debate over the ability of family firms to succeed in the second industrial revolution. Doreen Arnoldus has chosen six successful Dutch family firms in the branded, packaged products industry to analyze how the strategies these firms adopted contributed to their success. Her research highlights two important issues. First, these firms undertook the three-pronged investment strategy that Alfred Chandler promotes as necessary for success in the second industrial revolution, namely investment in large production facilities, distribution networks, and management talent (Scale and Scope: the Dynamics of Industrial Capitalism. Cambridge: Belknap Press, 1990, p. 31). Second, the firms were able to use the social networks within which they operated to sustain a comparative advantage over other competitors and these connections were enhanced by the family nature of the firms.

The book comprises nine chapters plus a final summary in Dutch, broken into five sections. Chapters 1 and 2 outline the purpose, sources, and firms used for the study. The second section, chapters 3 and 4, describes the internal or succession strategies of each firm. The third section, chapters 5 through 7, sets out the external strategies of the firms in the capital, labor, and raw materials markets and in the marketing of products. The fourth section, chapter 8, describes the social networks within which the firms operated. Chapter 9 concludes.

The six firms were among the largest 100 firms in the Netherlands between 1913 and 1930 (p. 23). Three originated in the early industrialized windmill region just north of Amsterdam, the Zaanstreek. The families that started these firms had been successful at consolidating windmill ownership over the previous centuries, their firms were predominantly engaged in crushing seed for oil, and they were from either Mennonite or Dutch Reformed religious backgrounds in a predominantly protestant region. The other three firms originated in or near Oss in the North Brabant region in the south. The families that started them were part of the Jewish minority in a predominantly Catholic region and were relatively recent immigrants to the area. They focused on producing meats and related products for the growing urban markets in the United Kingdom. The two groups of firms allow for the comparison of strategies between firms and across two different social networks to help understand the influence of environment on a firm’s strategic choices.
Internal succession strategies are choices that firms make to pass ownership and control from one generation to the next. Arnoldus’s review of succession strategies highlights two important factors about the recruitment of professional management. First, these firms went to great lengths to train top level management from within their own families, providing formal education and internal and external internships for their sons. This apparent nepotism was one approach to the problem of finding high-quality, loyal top management, not the only one. Thus, second, when outsiders were necessary to provide management capabilities not available from within, they were brought in, even at top levels.

The review of external strategies of the firms in input markets and marketing reveals three important features that determined the relative success of the firms. First, for all of these firms, the capital intensity of the products they produced increased gradually over the period, which facilitated financing through reinvestment of profits rather than requiring a large capital outlay at the beginning of the period. Second, the relative success of the firms was positively correlated with the extent to which the firms invested not only in new and expanded production facilities but also in distribution networks and raw materials supplies to ensure the throughput that Chandler highlights as important (Scale and Scope, p. 24). Finally, four of the six firms eventually needed larger capital outlays than the family could provide and drew on public capital (p. 217). The two that did not instead chose to sell the family firm at the point where large capital investments became necessary (pp. 96, 194, 215).

Social networks proved essential in positioning the firms for success. The difference between the two groups of firms is a particularly enlightening contribution of the book. The Zaanstreek network was based on centuries of location in the same area in the same business. The long history of accumulated capital and connections minimized the need for outside financing and provided raw-materials and distribution networks to which outsiders did not have equal access. The Jewish firms in North Brabant, on the other hand, relied on the international dispersion of their families to develop superior marketing networks for their products that allowed them to increase the size of their operations and cut costs. Either way, the firms were able to use family connections to reach a larger market and capitalize on economies of scale and scope in ways not available to firms without the same connections.

Overall, the book presents an excellent picture of the forces that contributed to the success of these family firms. Although they faced unique challenges because of the need to provide for the immediate family, none of them can be considered shortsighted or focused on current income at the expense of long-term viability. The ultimate success of each firm was tied to the extent to which it engaged in the three-pronged investment strategy that Chandler proposes. The eventual transformation from family firm to managerial capital firm was tied to the inability to provide enough capital or management internally at a crucial point. It leaves us in a bit of a quandary. Do the results indicate that family firms can be every bit as successful as managerial capital firms or do they indicate that there is a limit to the successful family firm, beyond which managerial capitalism possesses compelling advantages regardless of issues of environment? Finally, were these firms able to maintain their family character longer because they were in the branded-packaged products industry, which Chandler finds had lower penalties for operating below minimum efficient scale (Scale and Scope, p. 27)?

JILL DUPREE, College of the Holy Cross

This book, originally published in Japanese in 1983, is still a masterpiece on the Japanese economy. It is based on Okumura’s original survey and interpretation and provides insight not only into the Japanese economic system but also into Japanese society. The author makes a case for the uniqueness of the Japanese shareholding system and seeks to develop a new view of Japanese capitalism.

The book consists of four parts. In Part 1, “What Is Corporate Capitalism?,” Okumura portrays Japanese society as highly company oriented. In this context, the status of the human being is judged by the status of the company to which he belongs. To understand this economic system, the author insists, one must examine a firm’s relations with its shareholders, its managers, and other firms in addition to its employees. He then introduces the concept of “juridical person.” Following the 1950 recommendations of the Shoup Mission, which advised the Allied Occupation on tax reform, a Japanese corporation has been treated under the law as a “juridical person.”

A corporation is regarded as “an assemblage of individuals got together to pursue a given undertaking,” and “it is appropriate to levy taxes on the profits that accrue to the company at the stage when they are received by the shareholders in the form of dividends” (p. 28). This means that dividends received by companies are nontaxable.

In Part 2, “The Japanese Company,” Okumura explains the Japanese shareholding system. In the mid-1960s the proportion of all shares owned by private individuals in Japan fell from more than 60 percent to around 20 percent while the proportion owned by corporations surged. The form in which corporations own stocks, which is called the “mutual share ownership” (sôgô mochiai) system, is peculiar to Japan. Under this system, companies within a group hold each other’s stocks. The purpose of the system is not to acquire dividends or increase share prices, and the length of time companies hold shares is much longer than in the United States.

Part 3, “Managers,” is the core of the book. Okumura discusses the zaikai, the businessmen’s group that controls the entire Japanese economy and coordinates conflicts of interest among companies. In 1982, 2,195 profit-making corporations—only 0.13 percent of all Japanese companies, excluding those in finance and insurance—shared 41.4 percent by value of the total assets of all Japanese companies. They were the leading representatives of Japanese industry and had 29,609 officers. The character of postwar Japanese big business has changed from private capitalism to corporate capitalism. Compared with their U.S. counterparts, Japanese managers are not eager to achieve short-term gains in their companies’ share prices; rather they seek long-term profit for investment purposes.

Okumura also mentions the inflexibility of Japanese corporations. Japanese companies promote managers from within, not from the outside. Japanese unions are “companistic” and function as sections of their corporations. The corporation’s status as a fictitious legal person aggravates the situation. The view that all employees represent the company means that no employee takes responsibility because no single person represents the company. As a result, no one accuses the company of mistakes.

In Part 4, “Relations Between Firms,” Okumura illustrates the mechanism of the Japanese interfirm relationship or keiretsu. Generally, heavy and chemical industrialization intensifies the tendency towards transactions between firms, and in the case of Japan the lack of diversity in business accelerates this propensity. As a result, Japanese companies form horizontal integrations with a number of
subcontractors. At the same time, firms integrated vertically specialize in one area. Therefore Japanese company groups are caught in the web of *keiretsu* and trade within it. This characteristic is strengthened by mutual shareholding and by the sending of officials to other companies as directors or the appointment of one person to the office of director of two or more companies.

Although this book splendidly uncovers one aspect of the Japanese economic system, it is rather hazardous to evaluate the relationship between that system and Japanese society only from this point of view. The aspect that Okumura discusses in the book explains well Japan’s urban economic situation. But its rural areas, which survive on public construction works, might operate according to a different logic. Another question is whether the comparison the author makes with the U.S. and British shareholding systems is suitable. Just as the Japanese system has a number of problems, so does the American system, in which actors pursue short-term profits and have caused a series of stock market scandals and sharp fluctuations of stock prices.

Despite such questions, this book sheds light on the Japanese shareholding system and its influence on the Japanese economy, though one wonders how Okumura would explain recent economic changes in Japan based on his theory. Preceding Okumura’s analysis is a lengthy introduction on “Banking and Industry in Japan” by Michio Morishima.

**HIROSHI ONITSUKA, Iida City Institute of Historical Research**

**UNITED STATES**


From a country founded by settlers and slaves, the United States has grown into a nation of immigrants. The volume *Not Just Black and White* argues that the arrival of successive immigrant waves—from the not-quite-white ethnics from Southern and Eastern Europe at the turn of the twentieth century, to dark-skinned newcomers from the West Indies and Africa today—has resulted in a repeated redrawing of the color line. At the same time, these immigrants forge new group identities in the United States in reaction to the prevailing black-white divide, leading to, in the words of the volume’s editors, “a dialectical interplay between social constructions and interethnic or racial relations” (p. 9).

In exploring this dynamic, half of the essays in this volume take a “then and now” approach, comparing the experiences of various European immigrants at the beginning of the twentieth century to those of Asians and Latinos/Latinas at the century’s end. These empirical studies focus on points of contact between groups—from casual networks within immigrant neighborhoods (Richard Alba and Nancy Denton, pp. 237–61) and ethnic businesses (Steven J. Gold, pp. 315–40), to the more intimate connections of family and marriage (Joel Perlmann and Mary C. Waters, pp. 262–77). As with most quantitative history, these essays are interested in documenting the social consequences of existing racial and ethnic categories, not with explaining the shifting salience of the racial categories themselves. The other half of the volume, which is perhaps less familiar to economic historians, explores the historical processes by which these racial and ethnic groupings emerged and changed over time.
The starting point for these contributions, laid out most clearly in a programmatic essay by Stephen Cornell and Douglas Hartmann (pp. 23–41), is that, whereas race and ethnicity are often taken as natural categories—based on the immutable realities of genetics or genealogy—these classifications in fact arise in particular historical contexts. Group identities develop through a combination of self-definition (“who we are”) and external assignment (“who they are, and how they are fundamentally different from us”), and thus change along with the surrounding population (p. 28). Cornell and Hartmann further differentiate ethnicity from race, noting that ethnic identities often develop within a group, whereas racial labels tend to be imposed from the outside. The category “African-American,” then, can be understood as an attempt to superimpose a layer of ethnicity—based on appeals to a common African heritage and a shared contemporary culture—over the “one drop” racial rule historically enforced by the white majority through social convention and Jim Crow legislation.

If racial-ethnic groupings are products of social interactions, how exactly do they grow and change over time? The volume contains little theorizing about the individual- or group-level incentives that encourage group formation, but instead offers a series of case studies. One effective example is Jose Itzigsohn’s account of a “situational” pan-Latino/Latina identity that arises within shared neighborhoods and churches and around mutual political aims, but exists alongside still-salient national allegiances. Another is Neil Foley’s fascinating history of the landmark court precedents clarifying the racial position of Mexican-Americans, first considered part of the “white race,” later redefined as a “special class” of whites in order to invalidate systematic exclusion from jury service, and finally classified an “identifiable [nonwhite] ethnic-minority group” to prevent school districts from achieving desegregation goals by transferring Mexican-Americans to black schools (pp. 341–57).

Although there is much to be learned from these particular cases, economic historians could add to this conversation by supplementing this historical detail with the comparative tools of economic theory. What are the costs to an individual—in the abstract—of identifying with an ethnic minority, as opposed to “passing” as member of the dominant group, and what are the benefits? What does a group gain in terms of voice or numbers by aligning with other, similar groups, and what does it lose in the specificity of its message? Attention within economics to identity and group formation has expanded greatly in the recent years, with group boundaries variously explained as an internal attempt to ferret out free-riders (Laurence Iannaccone, “Sacrifice and Stigma: Reducing Free-Riding in Cults, Communes, and Other Collectives” *Journal of Political Economy* 100, no. 2 [1992]: 271–92), or an external product of entrepreneurs sowing group discord (Edward L. Glaeser, “The Political Economy of Hatred.” *Quarterly Journal of Economics* 120, no. 1 [2005]: 45–86). These approaches, or others like them, could be profitably applied to understanding the processes by which formerly racialized immigrants from Southern and Eastern Europe became “white,” the development of pan-ethnic identities at the turn of the century and today, and the durability of the black-white divide.

In the end, the volume raises the implicit question of what implication the theoretical realization that racial and ethnic categories are not reflections of an enduring reality, but instead are social products with histories of their own, should have for empirical historical work. The pragmatic answer may be: none at all. Even if ethnic-racial groups do not reflect actual “genetic or kinship links among persons . . . what matters is that people think there are such links, and furthermore, that they think those links are important” (Cornell and Hartmann, p. 28). And, as the empirical contributions to the volume substantiate, these groups, no matter how arbitrary, have real implications for individuals' socio-economic outcomes and life chances.
Nevertheless, as Kenneth Prewitt’s essay on the history of racial and ethnic categories in the U.S. Census (pp. 145–64) makes clear, markers of group membership in historical data should be used with caveats and caution. Historians must grapple both with the fact that group definitions in the census change over time—for example, Latinos were first identified by their distinctive Spanish surnames and later through self-reported “Hispanic origin”—and, perhaps more seriously, with the time lag between the emergence of a socially salient group and its codification in the census and other survey instruments. For example, the flexibility to identify with multiple racial groups was first allowed only with the 2000 census, despite the long-standing existence of multiracial individuals. Bureaucracies only collect information on a group when its activities begin to interest the government, or when the group itself grows large enough in size or stature to lobby for separate inclusion, as in the case of the multiple race option. In this context, efforts to supplement census data with “local” sources (e.g., the records of emigrant aid societies, the ethnic press) become more important in our understanding of group fortunes.

Prewitt’s essay on the racial and ethnic categories in the census would make a good addition to economic history courses, introducing students to the challenges of using the census to study a stable phenomenon over time, as well as to the inherently political nature of its content. And, readers already interested in, say, Latino/Latina integration or immigration law may find the case studies, which are uniformly well written, a valuable resource. As an integrated volume, though, Not Just Black and White is less successful. The conceptual issues it raises—how do racial-ethnic groups form? How do the boundaries between these groups change over time? And how does this fluidity bear on our interpretation of historical data?—are interesting and important ones for economic historians to consider. But, the volume touches on these larger questions only indirectly, thus failing to stake out a theoretical position on the process of identity formation or to suggest new approaches to empirical work in a world of shifting group identities.

LEAH PLATT BOUSTAN, Harvard University


This is a narrative social history that describes the culture that grew up in the anthracite coalfields of Pennsylvania. Harold Aurand has chosen a time period that stretches from the opening of significant underground mining of anthracite coal until its replacement by open pit mining in the 1930s. His central thesis is that hard coal miners developed a culture defined by their jobs and conditions in their industry. Their values included on the one hand a deep mistrust of others and on the other hand a support network based on reciprocity within their communities.

This thesis is similar to, but somewhat different from David Alan Corbin’s (Life Work and Rebellion in the Coal Fields: The Southern West Virginia Miners, 1880–1922. Chicago: University of Illinois Press, 1981). Corbin and other labor historians who have chronicled the development of working class life during the period of American industrialization have found in one form or another that a distinct culture arose that was related to their experiences with industry. There has not been a broad agreement on what this entails, Coburn stressed rebellion and conflict, whereas Aurand stresses an adaptive value system. Like that of Coburn, and other labor
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historians, Aurand’s work has valuable and detailed insights into the workings of the industry and of the labor market that go beyond the central thesis of his work.

Aurand’s description of the technology and mining of hard coal, the division of labor that was part of that technology and the selling of that coal is a valuable primer on how the industry actually worked. These topics range from the size and value of various lumps of anthracite; a description of technological innovations in the industry between 1835 and 1935 (such as steam to electricity); pay systems; and the social pyramid within the mines. This is not an exhaustive list and the level of detail enables a reader to quickly get a grasp of essential features of this industry.

Perhaps the most valuable part of this work lies in the description of the cartel, called the “combine,” that dominated the anthracite industry for much of the period covered. Indeed one is reminded of OPEC when one reads, “the general sales agents would meet monthly to set prices and determine a tonnage level that would support those prices (p. 20).” And that tonnage “would be allocated among carriers according to a fixed formula based upon their respective mining capacities (p. 20).” This led to individual mines not producing coal for a certain number of days, thus Aurand writes, “The combine relied upon a labor surplus to maintain low wages” (p. 82).

Throughout his work Aurand is a bit unclear at relating all of the elements of monopolistic behavior to one another, as when he states, “the mining subsidiaries could not deal with the industry’s crucial problem—excess capacity” (p. 21). Excess capacity is a consequence of a cartel restricting supply, and it is a problem that could be solved by breaking up the cartel. Instead Aurand describes the problems of the industry as something like capitalist anarchy; that “the industry suffered from overcapitalization . . . that forced it to maintain low labor costs while artificially maintaining high prices” (p. 7). This, of course, is a pretty good description of how a monopolist would function, and this is where Aurand is somewhat off point. Effective cartels restrict production, maintain high prices, hire less labor, and consequently enjoy long-run profits. In other words excess capacity and underemployment of resources, including labor, are characteristic of this form of market organization.

Indeed, once one understands this point, then this work could be read as a virtual case study of a cartel’s actions in terms of both some of management’s strategic decisions and their impacts on employees. One wishes that so well-informed an author could have organized his work based on a more thorough understanding of the effects of a cartel; this would have probably enlarged this work into a definitive history of the hard coal industry.

In terms of the basic thesis relating to the evolution of a distinct culture among these miners I found the evidence and discussion somewhat less compelling. On the one hand I am not sure that values such as toughness and independence were that different from mainstream culture. On the other hand, the evidence for this seemed weaker than other parts of this work. This should not, however, distract one from the genuine value that can be found in this excellent book.

LAWRENCE W. BOYD, University of Hawaii West Oahu


One need not be much of a historian to marvel at United States history, and the history of our frontier provides many of its most exciting tidbits. Nearly everyone
knows that brigands waylaid stagecoaches, trains, even riverboats, posing a threat to anything that moved. Raiders, both white and red, assaulted towns, farms, and ranches, taking anything that was not securely tied down. Gold dust was seized from miners as they brought it to town. Upon returning dejectedly home, the miner might find his claim had been jumped. Rustlers stole cattle and horses, though if vigilantes pursued them promptly the rustlers—or those suspected of being the rustlers—were likely to be lynched from the nearest tree. Cattlemen fought virtual wars among themselves, and with sheepherders and farmers, three groups struggling to make mutually inconsistent uses of the land. People fought over water in arid regions. Wild animals were pursued to the verge of extinction. The cavalry hounded Indians relentlessly, ultimately driving tribes from their traditional haunts. Occasionally, of course, the harried warriors managed to ambush the troopers and the wayfarers they tried to defend, resulting in horrific, bloody, mutilations. Upon moving west, and thus deprived of the civilizing influence of formal law, even people who had been the most upstanding citizens back east might settle disputes with a six-shooter.

Together, individually, and with several other coauthors, Terry Anderson and P. J. Hill have devoted more than three decades providing much needed texture to that informal cultural memory. The high points of all their books and articles dealing with the moving frontier are now encapsulated in *The Not So Wild, Wild West*. This newest offering is no mere recapitulation of their earlier work, however, but is at once a synthesis and an extension. Old ideas and findings have been condensed and consolidated, but at the same time revised in light of Anderson and Hill’s evolving understanding of property rights economics. New ideas and findings have been added to the mix. Similarities and differences among all that material, both new and old, have been meticulously elucidated.

Anderson and Hill do not argue that our nation’s casual cultural memories have been fabricated. Those memories persist because in large measure they recount events that actually happened. Instead, Anderson and Hill insist that a preponderance of casually remembered events is drawn from a statistical distribution’s tails. On reflection, every sports fan will understand the claim—teams are remembered in later years because they performed magnificently (right-hand tail), or occasionally because they were comically inept (left-hand tail). The vast majority of teams, those that finished the season mired in mid-league, might as well never have taken the field to judge by what the next generation will remember of them. The same is true for more substantive matters. All the unremarkable occurrences that form the vast bulk of life’s events are, well, unremarkable and thus in serious danger of being soon forgotten, or at best relegated to dusty volumes moldering in obscure archives.

Anderson and Hill have avidly sought out those moldering, obscure archives, finding a much less frenetic world than our off-centered cultural memories have preserved. The lynchpin of the Anderson and Hill argument is that violence and similar efforts aimed merely to transfer entitlements comprise negative sum games. Any resources that are devoted to extracting transfers cannot be used for production, and people trying to eke an existence from the harsh and unyielding environment they found on the frontier could ill afford to squander potentially productive resources so lavishly. Even when a society is well removed from the precipice of survival, entrepreneurs eagerly seek improved ways to govern any process that generates waste, providing the entrepreneur expects to capture a part of what might be saved. With or without formal law, entrepreneurs have strong incentives to design and profit from mechanisms that conserve resources. Indeed, before an effective innovation has been discovered, the search may be aided by the flexibility that is provided by the absence of formal law. As the burgeoning literature of public choice informs us, on occasion
formal law does not further entrepreneurial effort at all, but becomes still another route by which entitlements can be transferred. The target is often exactly those returns entrepreneurs expected from their effort to find a way to reduce waste.

Having constructed their theoretical scaffolding through the book’s early chapters, Anderson and Hill then employ it in later chapters to hang the empirical findings they have ferreted from the archives. Among other matters, their well-targeted variant of property rights economics elucidates trade and warfare between European immigrants and the tribes they found in America. It elucidates the evolution, successes, and failures of bottom-up institutions for the governance of mineral extraction and the harvesting of wild animals. The advent of formal law, commonly assumed to provide the very foundation of real property, often destroyed economically effective property rights in land that had formed spontaneously much earlier. As a rule, wagon trains were models of decorum despite being leagues and months removed from the nearest courthouse. Ranchers, drovers, and cowboys gradually evolved intricate interrelationships that can be understood only in light of the peculiar features of their industry. People innovated revolutionary new techniques to economize on water use in an environment where that resource was much more valuable than it had been back east (the Army Corps of Engineers can only dream of similar successes).

In brief, while facing a multitude of potentially deadly threats, frontier entrepreneurs did mighty well on average, both for themselves and for the society in which they lived. The result was a time and place that, though undeniably wilder than the one back along the seaboard, was not nearly as chaotic as our cultural memory would have it. The Not So Wild, Wild West concludes with a chapter speculating about the next frontiers mankind will face, frontiers that (if we play our cards right) will be much tamer than we now fear.

As Nobel Laureate Paul Nurse remarked during a televised interview, science is not a storehouse of truths but a compilation of logical but tentatively held hypotheses. The positions taken by Anderson and Hill will certainly be modified and elaborated by subsequent scholars. Indeed, the law of averages implies that with time a few of their claims may well be found wanting altogether. But whether one adopts or resists the conclusions that Anderson and Hill have drawn from the information they have so laboriously collected, no serious scholar can claim expertise in the economic history of the United States frontier without being thoroughly familiar with this book. The Not So Wild, Wild West is the Anderson and Hill capstone, the essential Anderson and Hill. If you are unfamiliar with their work, start here.

David D. Haddock, Northwestern University


It is widely recognized that the 60,000-page U.S. tax code is an unwieldy mess. In January 2005, President George W. Bush appointed a bipartisan commission to make recommendations for tax reform and simplification. If this book is a guide, it is unlikely that any fundamental changes in the tax code will result, even though there are enormous federal budget deficits looming. According to W. Elliot Brownlee, such a change occurs only in response to a real crisis and as part of a broader transformation of government.
The first edition of *Federal Taxation in America* was published in 1996, along with a companion volume, *Funding the Modern American State, 1941–1995: The Rise and Fall of Easy Finance*, a series of essays edited by Brownlee that is still available from Cambridge University Press. In this second edition of *Federal Taxation* the story is brought up to the eve of the 2004 election. Brownlee’s basic analysis and views have not changed in the interim. The introduction and conclusions have changed slightly to account for the shifts in contemporary tax policy and tax reform proposals, but the core of the argument remains that “tax regimes” shift only in response to a national crisis or emergency. According to Brownlee we are still operating under the tax regime, with its heavy reliance on the income tax, crafted in response to World War II. There have been reforms of the income tax, and setbacks to reform, but there has not been a crisis since the war serious enough to shake the system to its core. Crises in the past, including the original constitutional crisis, the Civil War, World War I, the great depression, and World War II, all resulted in creation of what Brownlee calls a fundamentally new tax regime with its “own characteristic tax base, rate structure, administrative apparatus, and social purpose” (p. 2). Thus, Brownlee sees five distinct tax regimes in American history, not all of which seem to me to be equally distinct. The historical arguments of the book are made in chapters that are only slightly altered from the first edition, with the contemporary material added. (See Mark H. Leff’s review of the first edition in this JOURNAL 58, no. 2 [1998]: 597–99.)

Brownlee takes what he calls a “democratic institutionalist” approach to the history of taxation. This means that he recognizes that democratic forces outside the institution of government itself helped to shape tax policy, especially the extent to which the tax system was explicitly redistributionist. On the other hand, basic government institutions—the President, Congress, courts, and the bureaucracy with its technical experts—clearly shaped and implemented tax policies. It is hard to argue with such an inclusive approach, although it lacks theoretical precision. This is basically a stages theory of the history of federal taxation, although there really is only one radical shift that I can detect, from the taxation of consumption, which lasted from the beginning of the nation until World War I, to income, which has lasted from World War I to the present. Although very important, economically meaningful changes in what articles of consumption have been taxed, exactly how expansively income has been defined, and how progressive income tax rates have been, these changes do not seem to me to be as fundamental as the issue of consumption versus income taxation, an important aspect of the contemporary discussion.

On the whole, this is a very useful book, although I think it has some flaws. I was quite disappointed that absolutely no data were presented. Some really good graphics, or thoughtful tables, would have helped place in context the relative importance of the various components of federal tax revenues over time. The rise and fall of the importance of excise taxes on alcohol and tobacco, for example, is a story demanding visual, quantitative illustration, especially in the period leading up to prohibition. Taxes are inherently quantitative and some basic measures should have been easy to provide. On the positive side, the descriptions of why particular taxes were proposed by people or presidents and then passed by Congress at different times are excellent, and especially helpful for crafting classroom lectures and discussions. I could easily see building an economic history seminar around this book, aided by the excellent historiography and bibliography at the end of the book. In terms of scholarship, as Brownlee shockingly notes, “much remains to be done” (p. 276).

WILLIAM J. HAUSMAN, College of William and Mary
The antebellum federal censuses did not record the rate of illiteracy among slaves, but if they had, it is likely the rate would have been 90 percent or higher (ages ten and older). In the aftermath of the Civil War, African-American illiteracy declined sharply, mainly because successive generations of children learned to read and write. In 1870, 84 percent of Southern-born African-Americans were illiterate. By 1900, a generation later, the rate had fallen to 51 percent; by 1920, slightly more than a half-century after Emancipation, illiteracy among southern-born blacks stood at 26 percent.

I say “mainly” because the census evidence suggests that adult slaves who were illiterate generally remained illiterate once freed. However, some did manage to learn to read and write before the Civil War, and others did while the War was progressing, or shortly thereafter. Heather Andrea Williams’s excellent new book, *Self-Taught*, tells their story, emphasizing the role that literate African-Americans played in “jump-starting” the extraordinary reduction in illiteracy after the War. As such, the book offers a highly useful corrective to the conventional wisdom that most of the initial decline can be attributed to the courageous efforts of sympathetic whites, such as the famed “soldiers of light and love”—Northern white female teachers who ventured South after the Civil War to teach former slaves to read and write.

Following a brief introduction, *Self-Taught* is arranged into nine substantive chapters, an epilogue, and a legal appendix documenting various antebellum statutes pertaining to the instruction of slaves and free blacks. Chapter 1, “In Secret Places,” discusses the great difficulties that slaves faced in acquiring literacy. Fearing insurrection, southern state legislatures made it a crime for persons to instruct slaves or free blacks to read and write, among other related prohibitions (for example, employing a slave to run a printing press, in the case of Georgia). This did not stop some owners from attempting to reap private rewards associated with slave literacy—Williams notes several examples—but most slave owners, it seems, believed that literate slaves were more likely to foment rebellion or run away. Williams also discusses how slaves compensated for the lack of reading material by developing superior listening and memory skills and the ability to communicate news through word of mouth.

Chapters 2 through 4 discuss the acquisition of literacy while the Civil War was progressing and during the heady early days after the War. Williams emphasizes the initiative and “self-help” activities of former slaves in their quest for literacy. Chapter 3, perhaps the most interesting in the book, examines the acquisition of literacy by black soldiers while serving in the Union Army. Chapter 4 analyzes the advocacy role played by various state-wide conventions held by African-Americans in the South shortly after the War.

Chapters 5 through 7 focus on the process by which schools came into being, on those who taught in them, and on the scarcity of materials to assist teachers (e.g., textbooks). As Williams discusses in chapter 5, self-reliance was almost a necessity in the establishment of schools for the newly freed slaves; few southern whites were willing to help, and northern whites were scarce on the ground. Self-reliance, too, played an important role in the establishment of the teaching force. It may surprise readers who are accustomed to the conventional wisdom that northern teachers were chiefly responsible for the initial decline in illiteracy to learn that according to a federal report in 1868, black teachers accounted for slightly more than half of those teaching ex-slaves. And as Williams demonstrates, almost certainly the vast majority
of the black teachers came from the South. Although a few were well-schooled by the standards of the time, the majority were probably barely literate themselves.

Chapter 8 delves further into the classroom environment, as well as the factors circumscribing school attendance. African-American parents, Williams claims, had an intense desire to have their children educated but faced numerous practical difficulties. Sheer poverty, lack of adequate clothing, impassable roads, and white opposition conspired to limit regular school attendance. Chapter 9 concludes by examining the difficult and contentious process by which the various state legislatures of the ex-Confederacy passed laws establishing common schools, albeit racially segregated ones.

Williams makes highly effective use of traditional literary sources but, save one table and the occasional passage, only limited use of quantitative evidence, even when such evidence could have bolstered her case or raised new questions for analysis. For example, her legal appendix, as previously noted, documents the various antebellum laws that prohibited the instruction of slaves. These laws, generally, also prohibited the teaching of free blacks, but were they effective in doing so? This question could have been addressed using the 1850 and 1860 integrated public use micro-data samples of the federal census because in both years, the literacy status of free blacks was reported. According to the census data, school attendance rates of southern black children rose dramatically between 1870 and 1880; presumably the passage of the common school laws played an important role in generating this increase, but how important is not known, as far as I am aware. These are, however, minor caveats. Beautifully written and cogently argued, Self-Taught deserves the attention of all scholars interested in early history of African-American schools.

ROBERT A. MARGO, Boston University and NBER


Andrew W. Cohen argues in The Racketeer’s Progress that historians severely misunderstand Progressivism and the New Deal. Historians claim that by 1900 or so a modern economy had come into being, an economy characterized by integrated and cooperative commerce and manufacturing produced by class-bound industrial workers guided by a separate and impermeable class of managers, stockholders, and professionals, themselves overseen by big business leaders. Although prosperous for all, this economy triggered economic, social and political problems, which were not finally solved until managers et al. finally extended Progressivism into the New Deal sufficiently to control such a complex and interrelated society-economy. Thus, the New Deal originated from Progressive ideas largely generated by modern business leaders or associated intellectuals. New Deal in place, all were happy. Historians further believe that unionized labor resisted efforts by the state to regulate or change working or social conditions. However, reading Chicago history as indicative of national history, Cohen says the opposite is true: that modernization was violent and contested, that social classes were permeable, that unions and government cooperated (indeed, that they were in large degree coterminous [p.7]), and that urban workers and small businessmen invented and applied New Deal cartelization remedies to restrain excessive competition locally long before they were reapplied nationally in the 1930s. The New Deal, thus, was not the triumph of Progressive bureaucratic social
management, but the result of the percolation up of long-accepted local urban arrangements. Finally, Cohen claims that the pattern of union extortion and racketeering to protect work territories and prices from new ways or providers so common in 1920s Chicago was validated by similar New Deal regulations; thus, Chicago unions’ thuggery was not really criminal. In sum, Cohen argues that the New Deal was no more than federal compulsion that local small business remedies to local competitive disruptions (common in the 1920s) be applied nationally, especially by large corporations.

I do not think *The Racketeer’s Progress* anywhere proves its case. But in its telling, it presents much colorful and personal early-twentieth-century history of Chicago (a place always fun to visit), and some useful insights and reminders. Its evidence is selective quotation from Chicago court cases, newspaper reports, and union and business association archives. The book’s argument appears to be that any example inconsistent with the overstated historical consensus disproves the consensus, and that correlation is causation. For instance, that Chicago business leaders did not universally embrace the cooperation with public, workers, and competitors championed by luminaries of the National Civic Federation proves that these ideas did not influence reform then or later (pp. 34–35). That the New Deal cartelized employers and employees just as Chicago craftsmen and small employers had tried to do in the 1920s (pp. 86–87) shows that “New Deal industrial policies originated in the . . . storefronts, saloons, and union halls of cities like Chicago” (p. 265). Of course, this is inadequate reasoning. Human history is chaotic (because the records are so jumbled, inadequate, diverse, and self-servingly misleading), so one seldom finds any monolithic body of acts and opinions. Furthermore, Adam Smith told us long ago that “people of the same trade seldom meet together . . . but the conversation ends in . . . some contrivance to raise prices,” and the ways to raise prices—restrain production and allot exclusive territories—have remained relatively constant. Thus, that 1920s Chicago small shopkeepers and labor leaders tried to cartelize trade and production as did the New Deal does not mean that Chicago shopkeepers and labor leaders authored the New Deal. I could go on, but the long and the short of it is that Cohen’s claim that the New Deal percolated up from the bottom rather than descended from Progressive thought to be imposed from the federal top down is unproved.

*The Racketeer’s Progress* does not do what it claims to do, but it does several nice things. Its many case studies of Chicago craftsmen and small employers attempting to exclude this or that worker—blacks from nearly everything, women from men’s work—exemplifies that (generally) somebody must be unemployed at a job if its wage is to be raised above the competitive equilibrium wage. Its documentation of the entanglement of the building trades with local government confirms for Chicago a general truth: growing cities generate many contracts, so the building trades stay close to government and trade for favors. Its documentation of unions’ use of illegal tactics (such as demanding bribes not to bring the workforce out on strike or in protest of “unsafe” working conditions [p. 104]) at smaller and more isolated work sites is consistent with economists’ notion that illegal activities will be used more where they cost less, and such illegal activities are less costly for unions with clout at city hall and with public sympathy (unions are deprived because they are deprived). Its documentation that larger and more capital-intensive chain movie theaters were more unionized than other capital-intensive employers is consistent with (although it certainly does not prove) the argument that producers of publicly consumed services are greatly vulnerable to threats of violence or intimidation (such as a stink bomb dropped in the dark) argued by Roger Faith and me. Its documentation that unions and employers cooperated to cartelize and protect industries finds for Chicago what we predicted (“An Agency

Cohen’s most difficult argument for me to summarize and evaluate is in the title. Cohen contends that labeling unions engaging in price fixing or bombing or maiming or extorting as “racketeering” had profound consequences for unionism (pp. 261–63). At first, racketeers engineered conspiracies as well as violence. But the New Deal *required* price fixing, and implicitly equated racketeers only with violent criminals. So unions, defined as racketeers, became suspected criminals. Unions’ legal status then depended upon the goodwill of criminal justice officials, Cohen argues. This forced unions to abandon efforts to justify their actions (conspiracies against consumers and other producers), which Cohen feels are justifiable, in favor of toady ing-up to politicians, who by not defining them as racketeers would permit unions to enjoy the benefits of the National Labor Relations Act. But toady ing-up unions stopped justifying themselves to the public and so made themselves vulnerable to changes in politicians’ perceptions, as unions now had no independent source of power. Having lavished all their attentions upon politicians, unions had no remaining channels to influence the public. If I have it right, this is an argument with great potential. However, without a theory of unions or a convincing understanding of why crime and unions are so interrelated, Cohen is unable to develop it. All in all, *The Racketeer’s Progress* is an unrewarding read.

JOSEPH D. REID, George Mason University


The aim of the book is to provide a comprehensive theoretical and empirical analysis of the economic geography of Hollywood. For Scott, “Hollywood, in its current incarnation, is one of the most remarkable examples of a successful industrial agglomeration anywhere in the world” (p. 58). Although the author claims that one of his objectives is to show how Hollywood developed over the twentieth century and came to dominate global markets in entertainment, the book is not a work of history, but rather one that describes and analyzes the various product, production, and labor-supply characteristics of contemporary Hollywood. A historical backdrop is provided, but this is necessarily schematic, although useful information is provided in the form of maps about the spatial organization of Hollywood, circa 1915 and 1930.

Scott’s thesis is familiar. Hollywood is an industrial district, consisting of a dense cluster of production and servicing units that are strongly interdependent, and that generate positive externalities. To a large extent, the competitive advantage that this agglomeration brings to the incumbents has counter-balanced any propensity for firms to disperse to lower-cost locations. These externalities give form to a tripartite industrial structure, enabling small “independent” firms to coexist alongside the “Majors” and their subsidiaries and closely associated firms. Evidence for this has been the rapid growth of the independent sector since 1980, measured both in terms of employment and firm formation. During this period, the “Majors,” while retaining their vertical reach, have become part of larger multinational media and entertainment conglomerates. As during the “Classical” period, circa 1920–1950, the “Majors” dominate Hollywood, but they now operate as part financiers, as well as producers and distributors, through a set of multiple profit centers by means of a complex web of arm’s length contracts. The “Majors” no longer take all of the risks
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associated with production, which they did in the “Classical” phase of their development.

For the researcher, untangling this contractual network is problematic, given the “paucity of data” (p. 9). Scott is hampered in his endeavor by a lack of contractual information describing the fine detail of the relations between firms that would have allowed him to map interfirm networks. His method is to collect disparate empirical evidence that “admittedly falls short of finality” (p. 9). Thus, he draws widely on official publications for industrial classification and employment data in the Hollywood area. Perhaps the most novel data that Scott brings to his subject are residential location patterns of different categories of film industry labor—craft, writer, agents, and managers—provided by their respective trade unions/guilds. For the most part, the author is satisfied to analyze the descriptive statistics that he presents in a set of useful tables, but occasionally is tempted into the realm of modeling as he seeks to measure the strength of explanatory variables governing the spatial spread of production facilities and those factors affecting the number of films made in-house, and with independent participation by the “Majors.”

The book is well written, with detailed chapters on television, studios, soundstages and sets, digital visual effects industry, local labor markets, and Hollywood in America and in the world. Scott has a good grasp of the debate about the nature of Hollywood and brings to it the tools of a social scientist. His analysis is measured and evidence-based, and his conjectures about the likely duration of Hollywood’s domination of world markets are balanced and drawn from a wide literature, which he discusses knowledgeably. However, as an economic historian, I would have welcomed a fuller exploration of the breakdown of the old studio system, and the change in firm formation and contractual relations in the face of the rapid decline in the market for films on theatrical release that occurred during the period 1950 to 1980. I was also disappointed that the author did not examine the media-platforms revolution that has taken place with regard to sources of revenue, where today only approximately 30 percent of film revenues come from worldwide theatrical release. Further, the absence of any references to the work of economic historians, such as the recent excellent paper by Stephen Broadberry and Andrew Marrison (“External Economies of Scale in the Lancashire Cotton Industry, 1900–1950.” *Economic History Review* 55, no. 1 [2002]: 51–77), leaves Hollywood, and Scott’s conjectures about it, contextually unlocated in relation to our knowledge of the salient characteristics of other industrial districts.

Nevertheless, Scott has produced a refreshing book on Hollywood. His perspective is one of a geographer, and his method that of a social scientist. He produces plentiful evidence to support his thesis that contemporary Hollywood should be conceived of as a tripartite industrial structure, and the book represents a major contribution to our understanding of it.

JOHN SEDGWICK, London Metropolitan University

GENERAL AND MISCELLANEOUS


Unlike historians whose principle aim is to construct plausible narratives of social evolution, economists seem drawn as if by some powerful pheromone to the policy
implications of their historical investigations. The present 17 essays offered in celebration of the late Charles Feinstein’s retirement reflect that fatal attraction; each draws a moral for the future from an examination of the past. The papers are organized in three groups. The first analyzes the sources of economic growth; the second considers the way political choices affect economic outcomes; the third surveys the methods by which economic outcomes are valued. Taken together, the essays provide an excellent representation of a certain kind of historical scholarship.

Jan de Vries opens the first set by extending and documenting his hypothesis that the growth in material welfare in northern Europe between 1650 and 1820 was mainly due to increased participation by household members in market-oriented work. He attributes the change to a shift in preferences towards market-supplied goods. His moral is that economists should take tastes into account when considering changes in factor supply. In the following essay, Jane Humphries constructs a microeconomic argument that the English apprenticeship system was exceptionally well suited to induce investment in marketable skills. Her moral is that institutional design matters, though her specific instance is impossible to assess without testing it against the default of Continental apprenticeship. Stephen Broadberry presents his findings on the growth and productivity of human capital in Britain, Germany, and the United States. The most interesting one is that the significant widening of the U.S./U.K. productivity gap occurred after 1973, which suggests it was due to the expansion of postsecondary education. As to whether apprenticeship is superior to other forms of training, no moral can be drawn. The next essay by Paul David and Gavin Wright examines technology by comparing the dynamo and the integrated circuit as general purpose technologies. This account of the timing and consequences of the electrification of manufacturing deserves a place on any syllabus in twentieth-century economic history. Its moral is that complementarities matter and that technologically driven productivity surges are unpredictable. Nick von Tunzelman directs attention to the form of institutional research. He concludes that the British system modeled on the university laboratory probably encouraged greater originality than the American system, which being modeled on the hierarchical business corporation was more tightly focused on practical results. Each system has its merits and its flaws.

The second group of essays engages politics. Nicholas Crafts shows that the East Asian escape from backwardness was achieved more by raising the quantity of factor inputs than by creating conditions for sustained improvement in their productivity. The moral here is that institutions created to solve one problem may block solutions to another. Christopher Davis and James Foreman-Peck consider the post-Soviet reforms in Russia by comparing them with British conversion to a peace-time economy after the two world wars. Not surprisingly, they find that the British did a better job, though one wonders whether the comparison of Britain with an economy that had abolished private property for more than seven decades is appropriate. Any moral may be misleading. Carol Leonard’s comparison of the failed de-collectivization of Soviet agriculture with the mitigated success that attended the post-Emancipation privatization of the peasant commune is more plausible. In both cases privatization was held back by the perceived risks of going it alone. In the recent episode, that risk was greatly amplified by the total absence of private farms from which the needed information could be obtained. The feedback between the number of peasants choosing to go private and the level of risk perceived by those on the margin of choosing to do so imposes local path-dependence. The geographical patterns suggest that the extremely strong incentives provided by physical proximity to major urban markets have been necessary to tip the balance. The moral here is that history really does matter.
The Soviet essays illustrate the heavy hand of the past on the economic present. That hand is clearly visible in Francis Wilson’s beautiful essay on of South Africa’s post-Apartheid transformation. He shows how the system of temporary permits by which employers organized a supply of migrant labor for the mines and plantations—which pre-dated Apartheid by several decades—depressed the economic development of the sending districts by regularly removing a significant proportion of able-bodied males. His story of how labor markets have affected the geographical patterns of social investment has certain analogies with the economic history of the American South. The next two essays are about money. In their essay on the integration of the Italian monetary system Leandro Conte, Gianni Toniolo and Giovanni Vecchi show that the regional differences in short-term interest rates on commercial paper survived formal integration by several decades. The essay echoes recent doubts about the degree of monetary integration in the United States after the Civil War. Finally, Barry Eichengreen and Peter Temin document the inertial force of the “faith-based economics” that informed bankers and politicians in the 1920s and 1930s.

The last section looks at results in historical perspective. Avner Offer’s survey of social indicators suggests that the resurgence of GDP as the main measure economic performance at the expense of broader indicators showing slower growth in subjective welfare reflects a general conservative trend towards valuing factor productivity over social and economic equality. Anthropometric measures of well-being are reviewed by Roderick Floud and by Timothy Leunig and Hans-Joachim Voth. Floud shows that the major rise in British height and body mass occurred in the cohort born between 1915 and 1940. Leunig and Voth observe that the height metric loses its usefulness as average European height approaches the physiological maximum, it ceases to be a measure of changing living standards. Anne Digby and Sheila Ryan Johannson take up the question of the potential contributions of traditional and scientific medicine. They suggest that although science has provided powerful means of curing disease, folk wisdom may hold greater potential for preventing it. The final papers take up social insurance. Peter Solar and Richard Smith compare English and Continental systems of poor relief in search of a model appropriate to the coming European common market in labor. Mark Thomas and Paul Johnson review the social security “crisis,” and not surprisingly find that the privatized systems prevailing before the advent of tax-financed schemes cannot be pasted onto a twenty-first-century economy.

These are all good essays, some of them spectacularly so. Charles Feinstein has been duly honored by them.

George Grantham, McGill University


History matters in many ways, some more significant than others. One school of thought with an ancient pedigree holds that the suite of events unrolls according to an immanent plan that necessarily realizes the best, or in one version the plenitude, of possible worlds; the chief alternative holds to the contrary that history is contingent upon events that possess no intrinsic logic other than the empirical fact of their having happened. That philosophical divide is the backdrop to this fine series of essays honoring Paul David’s lifelong advocacy of the proposition that contingency matters for the history of economies.
The editors have organized the 17 essays in this collection into four classes. The first takes up the logical foundations for contingent history. In a characteristically elegant argument, Kenneth Arrow demonstrates that the necessary condition for path-dependence is the irreversibility of investment, what in a simpler age we used to call fixed cost. As one expects, the importance of that irreversibility depends on the durability of the investment relative to the frequency and durability of complementary investments. The temporal disjunction establishes the coordination problem that defeats global optimality. Douglas Puffert takes a more discursive approach to this question by analyzing the several types of network externality. Just as Arrow shows how the panoply of positive externalities can be reduced to a single principle, Puffert illustrates the enduring worth of taxonomy in distinguishing cases. In understanding concrete cases, the details matter as much as the principle. Perhaps the most interesting piece in this set is Melvin Reder’s meditation on the tension between historical and teleological explanations in economic history. He notes that the debate between neoclassical economists and partisans of path-dependence fundamentally turns on who should bear the burden of proof, which depends on whether the case is heard by historians or economists. This is a fine essay that deserves a place in seminars on the methodology of economic history.

The second section considers specific examples of path-dependence. In a review of postwar monetary institutions Charles Calomiris argues that the financial rules and institutions designed to socialize financial risk were a response to the spike in that risk that attended the Great Depression. The socialization of financial risk inevitably created new forms of moral hazard that in the fullness of time provoked renewed excess in risk-taking and financial instability. Whether this is an instance of path-dependence, or simply an event in a Hegelian developmental process that inexorably runs to crisis can be debated. The Gold Standard was terminated by the First World War. The current regime may well be destroyed by American fiscal and military policy. Were these random or predictable events? Phillip Lim’s essay applies the path-dependent paradigm to the history of Korean developmental policy by arguing that the emergence of powerful business interest groups in response to the policy shifts of the early 1960s made it difficult for the Korean government to change course in the 1980s and early 1990s, when private debt-equity ratios mounted to unsustainable levels. This seems to stretch the paradigm beyond the point of usefulness. Peter Temin’s review of the legal and economic arguments over how the common cost of long-distance communications infrastructure should be allocated between the original provider of that infrastructure and potential competitors who require to be connected to it observes that the “path” of regulation seems to have been set by a consistent hostility to the incumbent firm by the FCC, deriving its role as a protector of competition. Although the narrative certainly defines a path, the story could be strengthened by identifying the branching point at which that path was taken. The most useful essay in this set is the Paul Rhode’s account of how the Pacific Coast coped with the aftermath of World War II. He shows that the flush of wartime investment probably tipped the balance to making the Far West a major economic region several decades earlier than might otherwise have happened. One thinks of the Canadian province of British Columbia, which remains mired in resource extraction, as the null hypothesis. This is an excellent piece that deserves a place on any reading list in twentieth-century American economic history.

The last two sections are more heterogeneous. In the section entitled “Context Matters,” Timothy Besley, Stephen Coate, and Timothy Guinnane propose that the English Work House test was a policy response induced by the rising cost of distinguishing the deserving poor from slackers and malingers. This is a “just-so”
story, which though plausible as economics seems at odds with the notion of path-
dependence. Susan Carter, Roger Ransom, and Richard Sutch also strain the paradigm
in their excellent study of the life-cycle transition in antebellum America. This is a
first-rate survey of the literature on the antebellum fertility decline, and gives new
evidence favoring Paul David and William Sundstrom’s child-default theory of
deleing fertility over Richard Easterlin’s target bequest model. By contrast, David
Weiman’s account of the early development of Bell’s regional and long-distance
telephone networks confirms the grasp of the long arm of the past. He shows that the
sequence and pattern of long-distance connectivity mapped the pattems created earlier
by rail and telegraph. That hierarchical ordering of urban centers provided an ideal
mold for connectivity supported by the technology of telephonic communication in the
first half of the twentieth century. Serendipity is also a form of path-dependence. This
is an excellent essay. The remaining essays deal with other parts of David’s opus.
Peter Mancall, Joshua Rosenbloom, and Thomas Weiss report conjectural estimates of
eighteenth-century economic growth in the Lower South; Mark Thomas and the late
Charles Feinstein perform a critical analysis of the index-number problems in the
value-added approach to measuring economic growth and plump for Fisher’s index.
Warren Sanderson, Thomas Mroz, and David Weir take up demographic matters.
These last essays are essentially methodological and intended for specialists rather
than the general economic historian.

The collection testifies to Paul David’s impact on the practice of economic history
in the last third of the twentieth century. It does not exhaust that impact. One wishes
for an essay on the historiography of the American tariff, and a deeper account of the
sequence and institutional foundations of scientific and technological innovation. But
this is to quibble. The organizers of this volume have met the obligations of the genre:
to honor the man, and to advance his work.

GEORGE GRANTHAM, McGill University

Origins of the Modern Career. Edited by David Mitch, John Brown, and Marco H. D.

This book contains 15 essays that explore career patterns over the last 150 years in
Europe and the Americas. Initial versions of most of the papers were presented at the
2001 Luxembourg Conference on Employment Security and Career Mobility in
Historical Perspective and at the 2002 International Economic History Association
Congress. The essays cover seven countries—in order of appearance, England,
Canada, the United States, Argentina, Sweden, Germany, and Italy—with emphasis on
the late nineteenth and twentieth centuries. The book begins with an excellent
introduction by the editors that sets out the primary questions: what is a career in a
modern historical sense and what are the key historical influences on the modern
career. The authors focus attention on industrialization, the emergence of bureaucratic
management structures, internal labor markets, unionization, education, migration, and
discrimination by race, sex, and religion. Also by way of introduction, Laura Owen’s
overview of skill formation and job attachment models from the labor literature and
Ineke Maas’s summary of event history analysis from the sociology literature provide
clear theoretical frameworks for the analysis that follows.

The remaining 12 essays tackle the primary task of the volume; they apply detailed,
individual-level longitudinal data to the historical analysis of the career, defined as a
life history of education, training, jobs and job mobility, and nonmarket work. Five
essays examine the creation of the formal and informal structures that exerted a strong influence on the development of careers after 1840. The authors emphasize unionization and the rise of bureaucratic management structure and well-defined internal job ladders. Particularly noteworthy is the study by Andrew Miles and Mike Savage of careers at three large British employers from 1840 through 1940. They demonstrate the clear usefulness of longitudinal data by using observations on individuals over time to measure the effect of formal career structures on job mobility. They observe decreasing upward mobility and conclude that the evidence favors a model of social closure and strengthening class barriers over a model of human capital formation. This illustrates an important characteristic of the book as a whole: its heavy reliance on the theory and methods of sociology. This book is at least as much a work of sociology as of economics. Also noteworthy is Mary MacKinnon’s analysis of unions and labor organization in the mechanical shops of the Canadian Pacific Railway early in the twentieth century. MacKinnon offers an effective blend of historical narrative and micro-level empirical analysis. She uses information on 9,000 CPR employees to illuminate important elements of the large 1908 railway strike, for example that popular reports of weak strike discipline were factually incorrect. If there is a weakness to this approach, it is only that MacKinnon does not make full use of it. As her attention turns to subsequent periods of unrest and a second great strike, she moves away from using the data to inform the narrative.

Two studies analyze the influence of gender on careers. Alison Kay presents a promising first look at self-employment as an alternate career strategy for middle-class women in nineteenth-century London. Insurance company and census manuscript data may indicate that a wider range of nondomestic occupational and career choice existed for middle-class English women than has often been thought. Small-business ownership offered particular flexibility. Unfortunately, the sample size is so small that representativeness of the data is a real concern. Pat Thane uses rich data that track female graduates of Cambridge University throughout the twentieth century to tell a fascinating and complex story of career patterns for highly educated women. It is an often frustrating story of good but decidedly second-best opportunities and of a labor market in which employment offerings lagged behind the educational offerings of the universities.

Finally, five essays examine the effect of industrialization and economic modernization on career patterns. The study by Maas and Marco H. D. van Leeuwen provides perhaps the book’s clearest demonstration of the indispensability of micro-level panel data in studying certain aspects of historical career development. They observe career events at five-year intervals for an entire population of men in a rapidly modernizing agricultural province of Sweden. They show that whereas modernization increased job mobility, it was almost entirely of a lateral nature as agricultural workers took unskilled positions in the sawmills. This pattern is not surprising, though the absence of greater chances for upward mobility after industrialization is less predictable a priori. John Brown and Gerhard Neumeier complement this picture with their careful empirical and econometric analysis of the key role that job shopping played in the career formation of migrants to Munich around the turn of the century. David Mitch uses meticulously constructed data on agricultural workers in Norfolk to show that work in even this traditional sector demonstrated some career aspects, such as strong recruitment from within and increasing employment stability and job tenure over time.

Scholars interested in labor mobility and the historical development of labor markets since the nineteenth century will find much of interest in this book. Sociologists should be as interested as economists. The book contains virtually no
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econometrics, which while perhaps welcoming a broad audience also fails to take full advantage of the many rich sources employed. One of the book’s weaknesses that stands in contrast to its obvious strengths is that nearly all of the essays, whether by choice or due to data limitations, do not take full advantage of the theoretical or statistical richness of the models presented in the introductory section by Owen and Maas. Also, the focus of each essay taken individually is quite narrow (for example, workers at a few companies in one place). Although a narrow focus provides detailed information, it sacrifices breadth, which the authors duly acknowledge.

Overall, the studies contained in this volume represent an important and useful effort to apply micro-level longitudinal data to the study of career formation and job mobility across a wide range of times and places. They demonstrate an important methodological advance, deliver new empirical results, and suggest many fruitful directions for continuing work.

JASON LONG, Colby College


Choose any two of these three topics: race, liberalism, and economics. Now trace the connections between the two that you have chosen over the course of the nineteenth and twentieth centuries, incorporating both fundamental philosophical concepts and policy implications. Doing this could easily produce a large, multi-volume study. This book attempts to take on all three of these topics in this very ambitious way. The book succeeds in being surprising, enlightening, and provocative, though it suffers from some minor shortcomings typical of collective volumes of this type.

The genesis of the book was a conference held at Middlebury College, and the 11 main chapters in the book are revised versions of papers presented at the conference. The conference itself was “provoked” (the editors’ term) by David Levy’s work, both in sole-authored papers and in papers co-authored with Sandra Peart, on the place of race (and racism) in the history of economic thought. In a nutshell, that work argues that the “classical liberal” economists of the 1800s were fundamentally antiracist in that they emphasized human homogeneity and trusted in the market to allow all humans of all backgrounds to pursue their desires. Moreover, Levy and Peart argue that the twentieth-century Chicago-style revival of classical liberalism is also innately antiracist. “Modern liberal” or paternalist thinkers of the nineteenth and twentieth centuries are seen to be fundamentally racist in that their promotion of paternalistic market regulation is consistent with a belief in the basic inferiority of some groups.

The 11 chapters are organized into three sections: one on “Classical Economics and Early Approaches to Race,” one on “Neoclassical and Modern Approaches to Racism,” and one on “Policy Issues.”

The first section is the most coherent and most closely tied to the central themes of the conference. It includes a survey of views of race in the natural sciences between 1700 and 1900 by Brendan O’Flaherty and Jill S. Shapiro, a paper by Levy and Peart on race in classical liberal and modern liberal economic thought, and responses to Levy and Peart by Susan Zlotnick and by Falguni A. Sheth. The O’Flaherty and Shapiro chapter provides valuable context for the rest of the book. Among other things, it examines two questions at the center of the book’s themes: Is “race” an innate, biological category, and does our understanding of race necessarily improve
over time through better evidence and competition in the “marketplace of ideas”? The authors answer “No” to both questions.

Levy and Peart and their respondents then examine how various views of race correspond to different views of the functioning of markets. Levy and Peart trumpet John Stuart Mill as their primary champion of a classically liberal view of the economy and also of a homogeneous, nonracial view of humanity. They argue that other thinkers of the age, including noneconomists such as Charles Dickens and Thomas Carlyle, were fundamentally racist because their support for “paternalist” economic reforms presumed that there were “victims” of the operation of free markets who needed help, and that those victims must have been inherently inferior. Zlotnick and Sheth each argue that these thinkers had much more complex views both of markets and of race than Levy and Peart allow. Just as natural scientists have debated the genetic basis for racial categories, these authors are debating whether there is something “genetically” antiracist about classical liberalism and related economic thought and something “genetically” racist about paternalist, modern liberalism, or whether these connections vary across thinkers and over time. Levy and Peart clearly want to argue for an innate connection, but Zlotnick’s and Sheth’s criticisms are persuasive.

The second section focuses on twentieth-century economic writing on race, both theoretical and empirical. It includes a second paper by Peart and Levy, here examining the influence of eugenics on early neoclassical economists, as well as a review of economic theories of discrimination by Robert E. Prasch, a review of empirical studies of racial inequality by William A. Darity Jr. and Patrick L. Mason, an essay on racial inequality and justice by Marcellus Andrews, and Glenn C. Loury’s response to some of the critics of his book *The Anatomy of Racial Inequality* (Cambridge, MA: Harvard University Press, 2002). These chapters do not cohere as well as those in the first section. In various ways, though, most of these essays touch on the “vicious circle” concept of racial inequality most closely identified with Gunnar Myrdal’s work *An American Dilemma: The Negro Problem and Modern Democracy* (New York: Pantheon, [1944] 1962). In this view, preconceptions about the inferiority of blacks produce a limitation on the employment and educational opportunities made available to them, and this limitation then produces poor outcomes for blacks, which are taken as evidence of their innate inferiority. Loury’s chapter, although on its face somewhat tangential to the core of the book, expresses the power of this idea most clearly. The concept also turns up (too briefly) in Prasch’s chapter and in Darity and Mason’s. Levy and Peart’s arguments never fully engage with this notion that markets may not be good tools for eradicating discrimination when different initial market outcomes produce subsequent “pre-market” discrimination and inequality in skill acquisition. This phenomenon—in which differences that result from market processes are misinterpreted as innate—connects nicely to the views of race presented in the O’Flaherty and Shapiro chapter and to the themes of the first section. However, these connections are not emphasized and expanded as much as they ought to be, probably due to the nature of the book as a collection of conference papers.

The final section includes two papers, one by David Colander and one by Vanita Gowda and Brendan O’Flaherty, that attempt to examine the policy implications of the ideas discussed in the prior chapters. Colander argues that modern liberals should be less suspicious of the racial views of economists who write in the Chicago tradition. He thinks that the failure of modern liberals to recognize the pragmatic power of policies that emphasize market incentives arises from the failure of the proponents of such policies (Chicago-style economists) to recognize the moral nature of questions about racial inequality. Colander supports, among other things, the use of policies that
are racially blind but that harness market forces to aid blacks and other disadvantaged
groups in indirect ways. Gowda and O’Flaherty then present an unusual example of
such a policy. They argue that the government should take a more neutral, less
restrictive stance toward the development and use of better, healthier “recreational”
drugs. The effect would be to reduce the pernicious health effects and violent
externalities of the current illicit drug trade. As these problems (especially the latter)
are particularly present in black communities, such a policy would have
disproportionate benefit to blacks.

In sum, this book is somewhat sprawling in terms of the issues it tries to address,
and some of the common themes that are present tend to fall into the cracks between
chapters. It is, however, a very provocative and engaging collection of papers, all of
which are exceptionally well written and accessible to nonspecialists. It achieves the
goal of the editors: to make us think deeply about race, liberalism, economics, and the
connections between them.

THOMAS N. MALONEY, University of Utah

Understanding the Process of Economic Change. By Douglass C. North. Princeton,

It will come as no surprise that Doug North’s latest book is about institutions.
Perhaps more surprising is that it is not about economic growth per se, but rather
economic “change.” Change is a given but, in the history of the world, growth is far
rarer, a phenomenon that requires many initial conditions, “adaptive efficiency,” and
much good luck.

Understanding consists of 13 chapters, divided into Part 1 (“The Issues Involved in
Understanding Economic Change”) and Part 2 (“The Road Ahead”). Following an
introductory chapter that sets the stage, chapter 2 introduces two fundamental
concepts, “uncertainty” and “ergodic.” “Uncertainty” refers to situations in which
humans cannot reliably assign probabilities and, therefore, insurance is not possible.
North contends that uncertainty is ubiquitous and that much human activity throughout
history can be categorized as attempting to reduce uncertainty. Rich counties have
succeeded in taming much natural uncertainty but at the cost of greatly complicated
social interactions. Conventional economic theory proceeds as if the economy were
“ergodic” that is, predictable, in an expected value sense, but North contends that the
world is “non-ergodic”—unpredictable.

Chapters 3 and 4 dip into cognitive science and philosophy. In North’s view, we
must have a theory about belief formation before we can have a theory about
institutional formation. How do humans come to have beliefs about the world? North
sketches a current debate in cognitive science—is the brain better modeled as a
computer or does human learning follow a “connectionist” model? He concludes that,
unlike the “rational actor” model of economics, most problem solving is accomplished
through “pattern recognition.” Chapter 4 is about “consciousness” and its role in
human and social development. North’s account is not about the origins of
consciousness but rather two implications—the development of supernatural or
religious beliefs and the conflict that arises when these beliefs are challenged by new
knowledge about the physical world

Chapters 5 and 6 are the core of the book. Institutions are the “rules of the game,”
formal and informal, and economic performance is ruled by institutional performance.
Institutions fundamentally derive from beliefs about the world, accumulated over time
and preserved in culture. Most changes in the institutional structure are incremental because the stimuli are marginal and because the longer an institutional structure is in place, the greater the difficulty of altering that structure (“path dependence”). Every so often, however, there are cataclysmic events, natural and man-made that result in revolutionary institutional change, such as in the formation of the Soviet Union, discussed later in the book.

Part 2 begins with chapter 7. This chapter is about the “evolving human environment” and, unlike the other chapters in the book, contains data and graphs. After chiding economic historians for spending too much time studying the Industrial Revolution, North reviews some of the basic evidence documenting just how much social and economic change has been compressed into the past few hundred years. He also emphasizes a key idea in economic growth, specialization. Specialization, in response to widening markets, leads to gains from trade. But this is trade among anonymous individuals and will not occur unless institutions develop that facilitate such trade.

Chapter 8 is about “order” and “disorder.” In North’s view, the great difficulty in getting a country on the path to modern economic growth is to establish order over disorder. “Order” does not mean regulation by the state, but rather an environment in which institutions constrain behavior in ways that are growth enhancing. Successful countries are those that establish institutions that allow order to be re-established quickly when an event—such as a civil war—interrupts the growth process, as it did in the United States. Chapter 9, “Getting it Right and Getting it Wrong” discusses the characteristics of “good” institutional frameworks versus bad institutional frameworks. Countries “get it right” when belief systems and institutions adapt to new information; they get it wrong when belief systems and organizations are rigid and resistant to change.

Chapters 10 and 11 are related in that they attempt to put into practice some of the theoretical ideas developed in preceding chapters. Chapter 10 is about the “rise of the Western World” and largely recapitulates the analysis of North’s earlier (and famous) work. Chapter 11, the more novel of the two, applies the institutional paradigm to the meteoric rise and fall of the Soviet Union. The Soviet system was born out of chaos. The belief system of the major protagonists—Lenin, Trotsky, and so on—derived from their observation of pre-Soviet Russia, their reading of major texts (Marx), and their own theorizing. These beliefs were implemented in ways that later proved tragic—for example, the collectivization of agriculture. Very quickly, the state became enormously powerful, with power itself an end as well as a means. When faced with failure, the Soviet state engaged in “reform”; but these reform efforts, in North’s view, were a pale imitation of the adaptive efficiency of western capitalistic institutions. According to North, the fall of the Soviet Union was not so much a case of a system disintegrating but a conscious choice on the part of party officials to dismantle the state, in order to take control of assets that were perceived (often correctly) to have economic value.

Chapter 12 offers some thoughts on what sort of policy lessons can be gleaned by studying the economic past. For North, these lessons comprise both the failures, much more numerous, as well as the success stories. In view of the previous chapters, not surprisingly, he recommends very careful study of where culture comes from, as well as the malleability of culture in the face of economic and other shocks. Cultural specificity also comes through loud and clear—what worked in the past in Western Europe or the United States would not necessarily translate to other parts of the world. Chapter 13 concludes with some appropriate Nobel-like musings on the economic future. In this regard, North is not nearly as optimistic about the world-wide diffusion of economic growth as is, say, Robert Lucas.
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North has written many books about institutions. If you had to recommend just one to your favorite graduate student, would *Understanding the Process of Economic Change* be it? In my case, the answer would be “no.” For this purpose, *Understanding* is too abstract, too sketchy, too little tied to hard evidence, too dependent on North’s previous writings. The book also suffers from a lack of connection to recent work in economic history; for example, Robert Allen’s recent book on the Soviet Union is not cited, much less discussed. For my money, the correct answer to the question just posed is North and Thomas’s *The Rise of the Western World*. For North’s many admirers, however, *Understanding* will no doubt be a favored addition to their bookshelves, a summing up of his life’s work on the role of institutions in economic development.

ROBERT A. MARGO, VANDERBILT UNIVERSITY AND NBER


Why another book on cotton textiles? The editors of this volume of collected articles argue that though there have been many books on national industries, there was a need for one with a global focus. The editors’ stated goals were first “to reveal diverse trajectories followed by different industries as well as their common patterns of development,” and second, “to gauge the varying contribution made by the industry around the world to social and economic development” (p. 16). The volume succeeds remarkably well in the first objective, though somewhat less well in the second. Given the ambitiousness of the overall project, that is not a bad record.

The volume stems from a 1997 conference organized in Manchester by Douglas Farnie and David Jeremy, and a session they organized at the 1998 Twelfth International Congress of Economic History in Madrid. Including the editors, there are 15 contributors, and 18 essays. Farnie and Jeremy are noted textile scholars who have made many contributions to what they themselves call the “ocean of literature” on the cotton textile industry. They gathered a panel of international experts: Takeshi Abe, Linda Grove, Tesuya Kuwahara, and Kaoru Sugihara from Japan; Stephan H. Linder from Germany; John C. Brown, Joshua Rosenbloom, Gary R. Saxenhouse, and Gavin Wright from the United States; John Singleton and Alfonso van der Krann from Australia; and Stuart Thompstone and J. K. J. Thomson from England.

Farnie and Jeremy set the overall tone of the volume. They chose Degas’s *Un bureau de cotton à Nouvelle-Orléans*, 1873, for the frontispiece. This signals the importance they attribute to marketing. In his chapter, “Merchants as Prime Movers,” Farnie argues that the merchant must be returned to his central place in the story of cotton textiles. He disputes the notion that the fate of a national industry is a necessary result of exogenously determined relative costs. Instead, success or failure follows from the occurrence or absence of creative merchandisers. One interesting section in this chapter contrasts the clever branding technique of the U.S. Knight brothers’ “Fruit of the Loom” cloth with the insistence by British distributors that manufacturers produce homogeneous products. British distributors ensured the maximum level of cross firm competition and the lowest prices (p. 50). But the U.S. firm is still functioning, whereas most of its British competitors are gone. Jeremy seconds the emphasis on individuals in his chapter “The International Diffusion of Cotton Manufacturing,” which describes the channels of diffusion of textile technology.
These included the often discussed immigration of skilled artisans, but Jeremy also gives credit to the numerous international exhibitions, state sponsorship, and foreign direct investment. His main theme is the role of individuals, whether an artisan selling his skills, a government agent, or a machine maker actively creating a market for his goods. Jeremy takes this theme up again in the chapter, “Organization and Management in the Global Cotton Industry.” He argues that the main job of management is to create links between producers, distributors and consumer markets. The section of this chapter on “culture” cites Gregory Clark’s influential paper on inherent differences in labor across countries (“Why Isn’t the Whole World Developed? Lessons from the Cotton Mills,” this JOURNAL 47, no. 1 [1987]: 141–73), but Jeremy’s focus is on the differing managerial cultures in the United States, Britain, and Japan and its effect on their organizations and marketing.

The emphasis on individual initiatives and their accomplishments is echoed in many of the other contributions. Van Der Krann attributes the creation of a modern Dutch cotton textile industry to the intervention of the King, William of Orange. Thompstone discusses in detail the role of Ludwig Knoop in bringing textile technology to Russia. Rosenbloom focuses on the innovations and political maneuvering of Francis Lowell at an early, crucial stage in the development of the New England textile industry. And Kuwahara discusses the role of Sanjū Mutō in the creation of a distinctive Japanese management system. Mutō was president of the Kanegafuchi Spinning Company. Kuwahara analyzed the 8,271 managerial circulars Mutō wrote between 1902 and 1930. These testify to Mutō’s efforts to standardize products. He was the first Japanese manager to gather and use voluminous statistical material, intra- and inter-firm. And he had put much of this managerial technology into place before 1914.

There are two other recurring themes I would like to mention. One is the linkage between choice of technology and national characteristics. Given the simple technology of textile manufacturing, one might assume all countries used the same technique. But that would be incorrect. Several articles touch on this theme. Among these, Thomson compares the diffusion of specific machines in Catalonia relative to Britain, the United States, and France. He explains the circumstances for particular periods of fast or slow diffusion. Even within this one small region, the process was not uniform. Saxonhouse and Wright exploit a large data set on production and sales of British spinning machines, 1878–1933. The data include the number of spindles per machine, machine speeds, and the yarn counts the machines were designed for. Britain was the world’s dominant supplier of this equipment. And, because staffing levels per machine, as opposed to per spindle, remained constant or fell across most countries in this period, this data set allows an international comparison of productivity. Although all countries had access to the same technology through British firms, countries differed in their choices in ways one might have expected given their particular histories. These data confirm that the mere availability of technology does not determine success.

A second recurring theme is the role of protection in the creation and continuance of national textile industries. There have been only three countries that were able to compete effectively in unfettered world markets: England to 1914, Japan between 1920 and 1970, and only since the 1960s, China. And the only country I am aware of that developed a prosperous industry without the benefit of early protection was pre-1914 India. Britain itself had erected barriers to Indian cotton goods in the eighteenth century. Singleton’s article shows that without the success of the Imperial Navy, Britain would not have had a successful textile industry. The navy insured that the industry would have free access to the Indian market. And because virtually every sovereign nation erected protective tariffs very quickly in the nineteenth century, if
there had been no India, what would have become of Lancashire? The role of
government policy, or its absence, is a major or minor theme in every article in
the volume, with the exception of those of Saxonhouse and Wright and Kuwahara.
The role of the individual, the particulars of technological choice, and the role of
government differed across countries, but were always important. In its illumination
of these three themes, the book offers many insights on contrasting trajectories of
growth, and thus succeeds in its first goal. There is, however, little discussion of the
overall place of the textile industry in each nation’s development. But that simply
suggests the usefulness of another volume.

SUSAN WOLCOTT, Binghamton University

The Changing Face of Central Banking: Evolutionary Trends Since World War II. By

Pierre Siklos indicates that “The aim [of this book] is to try and isolate how
monetary policies have been shaped by economic, political, and institutional forces.”
(p. xviii) This is an extraordinarily ambitious task. Normally, a statement about a book
being ambitious is followed by a statement that the goal was not reached or was
impossible anyway.

Such is not the case here. Pierre Siklos has largely succeeded in providing an
interesting and informative analysis of how monetary arrangements have changed
since World War II and an explanation of why, using an extraordinary blend of theory,
data, and institutional considerations. Siklos summarizes conclusions of much of the
literature on monetary policy since the 1950s. He also presents a valuable set of data
on institutional arrangements for 21 central banks, as well as evidence on how those
institutional arrangements affect inflation outcomes. Coverage includes Western
European central banks and those of the United States, Canada, Australia, New
Zealand, and Japan.

Siklos has kept the analysis at a fairly general level, with mathematical and
econometric details in the background. At least some of these details are available in
associated working papers. For a reader used to seeing them, it is sometimes
frustrating to be unsure how something was done, although it is not particularly likely
that the empirical results presented are sensitive to technical choices. The overall task
in this book is sufficiently broad that leaving the details in the background usefully
makes the book accessible to a wider readership.

The first part of the book analyzes central banks’ institutional arrangements and their
implications, including the importance of personal as opposed to institutional
reputation. The empirical analysis of the central 90 pages indicates that these
institutional factors are important for central banks’ behavior. The last part of the book
shows how experience with alternatives led to the conclusion that inflation targeting is
the best monetary policy for generating low inflation and why this is a correct
conclusion.

For postwar European central banks, there is an overarching question: Why was
there a big increase in inflation in the 1970s—sometimes called the “Great
Inflation”—and much lower inflation in the 1990s? Siklos concludes that the Great
Inflation was an episode of bad policy due to poor institutions. Central banks were
insufficiently accountable, with vague goals. A lesson was learned: central banks are
generating less inflation now because they are more accountable and are targeting
inflation instead of mistakenly attempting to stabilize economic activity.
This answer offers an interesting contrast with Thomas Sargent’s analysis of the same question in *The Conquest of American Inflation* (Princeton, NJ: Princeton University Press, 1999), which also relies on learning, but of a different sort. In Sargent’s analysis, the central bank learns about the economy instead of politicians and monetary economists learning the best way to organize and operate a central bank. Siklos specifically rejects a principal-agent analysis of central banks (p. 237). Instead, as is typical in much recent literature, he proceeds from an assumption that a central bank is attempting to maximize the welfare of the population of the country. Although this is a nice thought—at least to someone working at the Federal Reserve—there are reasons to think that it misses details, details that are likely to be important for understanding the implications of institutional arrangements. Analyzing the principal-agent problems between the voters, representative government, and the central bank would clarify the roles of goals as well as of disclosure and accountability in the operation of the central bank.

Siklos concludes that we have learned the correct lesson: An accountable central bank with sufficient disclosure and an inflation target will generate low inflation, a state of affairs that can continue for the foreseeable future.

I am not so optimistic. As Siklos points out, failure to achieve the inflation target has had no real consequences for the governor of the central bank, even in New Zealand, the country that probably has the closest connection between unwanted inflation and the governor’s position. A couple of alternatives, which are hard to distinguish given current data, are more plausible to me personally.

Sargent’s view has been examined in the most depth. In his analysis, central bankers learned in the 1970s that the Phillips curve was steeper than they thought and, therefore, that the gains from inflation were less than originally thought. As a result, inflation fell. As he sees it, the central bank learned something incorrect: while it appears that there is an exploitable Phillips curve tradeoff between inflation and unemployment, there really is not. For the future, Sargent’s analysis suggests that a period of low inflation will generate data consistent with a flatter, exploitable Phillips’ curve, and central banks will increase inflation again.

Alternative explanations for the Great Inflation can also be built on the ability of unexpected inflation to redistribute wealth across voters for a time.

Even given these alternative explanations, Siklos’s analysis of inflation targeting probably reflects the reigning orthodoxy. It may even be correct.

The book is reasonably well edited in general. It may seem churlish to say this after the author has put in so much work, but my understanding of the book would have benefited from more editing of the writing.

Overall, this book is a very valuable addition to the literature. It is worthwhile reading if you are interested in central banking generally or the history of central banking since World War II, or if you want a readable introduction to how central banks ended up targeting inflation.

**Gerald P. Dwyer Jr., Federal Reserve Bank of Atlanta**


The editors of this book aim to open up new perspectives on the history of welfare by defining it more broadly than as a system where the state and voluntary sector
provided for the poor. They argue that for many social groups family and kin played an important role in the provision of welfare, even when there was property. Indeed, the ten articles in the book (in addition to the editors’ Introduction) focus on the arrangements made for people with property, and on the influence that gender, the market, the state, and voluntary institutions had on those provisions.

The Introduction gives an overview of the traditional approach to the history of welfare and goes on to lay out alternative ways of analyzing who needed welfare and when, who provided it, and what influenced those variables. Because the focus of the book is on welfare provided by families, households, and kin, the editors also examine the relationship between risk and strategy in the context of welfare, and how strategies were shaped by the life course (marriage, retirement, guardianship, divorce, inheritance).

The articles that follow cover a number of different historical periods—from the seventeenth to the twentieth century—and geographical areas, including France, Switzerland, Holland, Bohemia, Germany, Sweden, and the United States. What is striking in them is the enormous variety of strategies employed by people in these areas to ensure the long-term survival of the family, and how this involved different approaches and attitudes for men and women. That those differences are hardly a thing of the past is demonstrated in the article on the United States by Marsha Shapiro Rose that compares inheritance among the Rockefellers and Bingham in the twentieth century. Although the female heirs in both families received a share of the inheritance, they were excluded from managing or administrating it, even when it came to the family philanthropies. The family patriarchs did not see the women as capable of administration, and, of course, the women were never encouraged to acquire the necessary skills. Therefore, male hegemony perpetuated itself. Moreover, the women received a smaller share than their brothers and were more likely to inherit the home and family jewels rather than the family business.

Such gender inequalities could also be found in France during the ancien régime. As one would expect, according to Gérard Béaur, in areas of impartible inheritance sons were usually (but not always) given priority. Interestingly, though, in areas with partible inheritance, women were not necessarily much better off or treated more equally, as men tended to inherit the land whereas women received money. Sometimes this transmission of land took place upon death, and sometimes land was transferred inter vivos so that it would fall outside the inheritance. Many variations existed, but the upshot was that the inheritance strategies followed in impartible regions, and their outcomes, were not much different from those in partible ones, and did not necessarily result in excessive subdivision of land. Moreover, the author claims that a case can be made that some of the partible regions ended up being economically more efficient than the impartible ones because capitalist agriculture actually benefited from the subdivision and recombination of land. Because people did not inherit an adequate amount of land they viewed it as capital to help set up a household.

Rose Duroux discusses an interesting case study in France in which women did inherit the land. The inheritance system in the High Auvergne revolved around male migration to Spain, where men could earn enough money to ensure the continuation of the family enterprise when they returned and married. With men absent, women inherited the house and property, putting them in a good position to secure an advantageous marriage, often to a returning migrant. Such a marriage also could provide the cash needed for noninheriting siblings, who could then collect a dowry and marry. In this area of France, female inheritance linked with male migration operated together to secure economic stability and to prevent partition.
A theme that recurs in the articles on Germany and Sweden is the impact of the market economy and the commercialization of property on the definition of gender roles and on the financial welfare of women. Robert Beachy shows that, starting in the late seventeenth century, women in Saxony, acting as merchants, were held responsible for the debts they incurred: they could not hide behind a guardianship law that held them not legally competent to sign documents. This resulted from a change in interpretation of the law, recognizing the many women involved in family firms. The protective aspects of the law continued for women not involved in business, and their property could not be touched by the creditors of their husbands. However, as corporate capitalism grew in the nineteenth century, guardianship was used by insolvent merchants to hide their assets, and the law became a target for critics and reformers. It was abolished, and as a result married women’s property now fell under the administration of the husband, and could be seized by his creditors. Not necessarily progress for women!

Similar pressures of economic change and growing individualism affected guardianship laws in Sweden and led to their abolition in the early twentieth century, when the Marriage Act of 1921 established formal equal rights. As Kirsti Niskanen points out, however, the formal equality enshrined in the law did not automatically lead to real equality. In the case of divorce, a woman’s equal right to property had to be balanced with societal expectations of her role in the family, resulting in unequal, and traditional, property settlements: house to the woman, business to the man.

Finally, the Dutch case is instructive in that the very commercially oriented community in Amsterdam turned its back on complete individual responsibility and instead chose to protect the future of family and property by building and supporting communal orphanages.

Whether these articles will change historians’ approach to the study of welfare only time will tell, but in the meantime they challenge traditional interpretations and give interesting analyses of the subtleties and choices involved in strategizing family survival. The book should be of interest to both specialists and students.

JOYCE M. MASTBOOM, Cleveland State University


The 11 essays, all save one newly commissioned for this work, largely represent extensions of previous work by the authors. As one title notes, cartelization or collusion is “a practice without defenders.” The hostile attitude of the economics profession was established by Adam Smith’s observation (itself cited in several of the essays) that collusion is “... a conspiracy against the public.” In Common Law countries historically, and in European countries since the advent of the Treaty of Rome, this hostility has inspired legal sanctions against collusion, with significant implications for the scientific study of collusion.

The first consequence is that most collusive activity will be covert, resulting in a lack of reliable statistical data from which to draw valid inferences. A related problem is that only a subset of collusive activities will be detected. (This provides another answer to the question, “When is collusion successful?” which is a topic in several essays, i.e., “When it is undetected by authorities!”) The question then is how representative are the industries where attempted collusion has been detected?
Uncertainty about this limits the lessons that can be drawn from case studies of such identified colluding industries.

Another consequence of legal hostility towards collusion is that “exceptional circumstances” are often discovered to apply to specific industries. They frequently result in conscious exemption from, or at least, nonenforcement of antitrust strictures. Once again, although this may create a larger data “footprint,” it calls into question whether observed behaviors may be generalized. The point is particularly relevant here, because at least half of the papers examine such special cases. Except for the brief introduction and an initial paper reviewing prior empirical work, the balance of the studies consider periods prior to the development of modern legal doctrines regarding collusion.

The essays fit comfortably into the traditional Structure-Conduct-Performance tradition of Industrial Organization. This is largely a consequence of the data limitations discussed previously, which make it impossible to estimate models employing more modern structural specifications. However, the contribution of these papers may be greater than either the authors or the editor of this volume realize.

Start with the third element of the structuralist formula, Performance. Janice Rye Kinghorn and Randall Nielson’s “A Practice without Defenders: The Price Effects of Cartelization” examines what, according to economic theory, ought to be the chief effect of collusion, increased prices. Their method is to compare the relative price performance of cartelized German industries with that of their British counterparts. They first demonstrate that prior to the formation of cartels German coal and steel prices closely tracked their British equivalents. After the adoption of a cartel German prices became a fraction (and a decreasing fraction at that) of British prices. Although one may have reservations about the result—perhaps British firms colluded more effectivley covertly than German ones did openly or some tariff or nontariff barrier in international trade prevented equilibration—it is striking and thoroughly counter-intuitive for most economists.

In the same performance-related vein, Andrew Dick’s, “If Cartels Were Legal, When Would Firms Fix Prices?” notes that the Webb-Pomerene Act created a broad exemption in United States antitrust laws for exporters. Despite the exemption granted from 1919 to 1965, as Dick observes (p. 151), “…the most noteworthy observations is that relatively few industries ever made use of the antitrust exemption. In a typical year, only 30 to 35 cartels were active, and collectively these cartels accounted for between 5 and 10 percent of US exports.” When collusion promises such great rewards and the U.S. government has promised to look the other way, why were there so few cartels?

These puzzles are addressed in Dick’s and other contributions to the book. They largely involve questions of the sorts of market structures that facilitate the establishment of cartels. Dick’s essay uses sets of logit regressions matching 125 industries that established Webb-Pomerene cartels with 125 noncartelizing export industries. Many of the variables Dick identifies represent the “usual suspects” in investigations of collusion, i.e., high concentration, barriers to entry, and nondurable and undifferentiated goods. The notable extension is that regionally distributed industries are more likely to cartelize, and in this case the cartel will usually take the form of a joint sales agency. Dick speculates that the economies achievable through joint export marketing programs for regionalized industries were responsible for their cartelizeation.

This speculation finds support in Peter Grossman’s, “Why One Cartel Fails and Another Endures,” in which he examines the comparative cases of the Joint Executive Committee and Railway Express. The Joint Executive Committee was an attempt to
cartelize freight carriage on the important Chicago-New York rail routes. It failed, besieged by cheating and repeated entry. In contrast, the Railway Express cartel, performing a function similar to FedEx today, proved extremely durable and successful. Grossman explains the performance differences by the fact that, in addition to the obvious price rise achieved by the express cartel, membership also permitted member firms to realize substantial economies. Cheating both lowered revenues and raised costs, making firms reluctant to exit the cartel.

Two other contributions highlight additional reasons for cartelization beyond the traditional achievement of monopoly prices. The first is Christopher Gilbert’s, “International Commodity Agreements as Internationally Sanctioned Cartels.” This presents a plausible model of situations where governmental cooperation with a cartel may actually be welfare enhancing. William Sjostrom’s “The Stability of Ocean Shipping Cartels” suggests that, given integer problems in the supply of ocean shipping, there may be no sustainable competitive outcome in these markets. Both provide case study evidence supporting their models.

In summation, this is an interesting and useful volume (including other contributions not discussed here due to space constraints) that could have been even better with some additional editorial organization and commentary.

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This useful volume on the productivity missions to America after the Second World War is based on a 1997 conference in Caen, which acted as a preconference for the 1998 International Economic History Congress in Madrid. The missions were established by countries receiving Marshall Aid, and although there is a voluminous literature on the Anglo-American Council on Productivity (AACP), there is very little available in English on other European countries and Japan, so this volume plugs an important gap in the literature.

In the first section on the United States and its actions on the international scene, useful chapters by John Gillingham and Jacqueline McGlade make clear that the origins of the missions came from within Europe, overawed during the Second World War by the American productive effort. Indeed, American businessmen were not particularly keen to help European industry become more competitive and take markets away from them. However, American progressives were keen to promote state-sponsored modernization in Europe as they lost influence at home, and as the Cold War progressed more conservative elements in the United States were keen to raise European productivity as a way of freeing up resources for defense spending. None of this, however, should be allowed to detract from the great generosity of U.S. actions at this time. Productivity missions typically allowed managers and workers from a European industry to make a visit to the United States, where they attended presentations and were shown around factories. Very few requests to visit plants were turned down. To put this in perspective, imagine modern European manufacturers showing teams of South Korean or Taiwanese experts around their best factories with a view to helping them adopt best practice at home!

The first section ends with chapters on Norway, the Netherlands, Germany, and Italy, which leads into the second section on further national experiences: two chapters
on the United Kingdom, three on France, and three on Japan. One of the most striking features of this section is the very different assessments of the authors on the impact of the program. Whereas both chapters on the United Kingdom take it for granted that the AACP was a failure, the authors of the chapters on France are equally clear that the Association Française pour l’Amélioration de la Productivité (AFAP) was a tremendous success, and the Japanese chapters take an equally upbeat view of the productivity missions. Because the individual chapters use quite different methods, the reader can only speculate on the reasons for these very different evaluations.

The third section on productivity missions and their implementation sector by sector focuses on three particular industries: iron and steel, electrical goods, and rubber. The selection of industries is not explained, and I could not help wondering what impact a different choice would have had on the general tone of the volume. The productivity missions were heavily tied up with the issue of mass production methods, and although the industry studies here all tackle issues of volume production, they are not the obvious ones to illustrate the theme of Europe’s experience with mass production.

The fourth section, on the fall-out from the productivity missions, is potentially the most interesting for the nonspecialist reader. The unexplained decision to base it entirely on the French experience is therefore regrettable, particularly given the comparatively optimistic evaluation of the French authors. One obvious problem here is that most of the authors eschew quantification and appear equally skeptical towards economic analysis. Surely in attempting to assess the impact of the missions it would be helpful to have some basic data on starting levels of productivity and subsequent growth rates. I could not help thinking, for instance, that the reason for the negative evaluation of the British authors and the positive evaluation of the French authors may have had something to do with the relatively slow productivity growth achieved in postwar Britain, compared with the rapidly growing French economy. And yet this may have had rather more to do with the fact that France was further behind the United States than Britain at the end of the war than with the impact of the productivity missions. It is also important to make a distinction between the impact of the missions on the institutions and structures of industry and the impact of any such changes on productivity. For it is not difficult to make the case that British industry Americanized more than any other European industry, and the AACP may well have been successful in this respect. The problem is that the excessive Americanization of British industry was damaging, and the European countries that succeeded did so largely on the basis of a more skilled, labor-intensive, flexible production system, of which Germany was the most successful example. As François Crouzet notes, half a century after the productivity missions, it is clear that European industry has continued to follow a separate path, which he calls the Rhenish model.

However, it would be inappropriate to end on a critical note. This is a very valuable book, which is bound to become the standard reference work in an important area. The introduction by Barjot and the conclusion by Crouzet are very effective, providing between them a really excellent overview and guide to the individual chapters.

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