

Book reviews

GREAT BRITAIN AND IRELAND

Anthony D. M. Phillips and Colin B. Phillips, eds., *An historical atlas of Staffordshire* (Manchester and New York: Manchester University Press, 2011. Pp. x + 190. 23 figs. 14 tabs. ISBN 9780719077067 Hbk. £35/\$62.50)

Including the editors, a team of 29 scholars have contributed some 80 short essays to this impressive volume, their discussions aided by no fewer than 309 specially prepared maps and diagrams reproduced in full colour. In most cases, four county-wide maps are provided to illustrate each essay, with a double-page layout being adopted. Most are concerned with depicting change over time, though some make comparisons at points in time. They are standardized by using a base map that divides the county into its constituent parishes and townships. It was compiled from the first-edition 25-inch Ordnance Survey maps of the county, which were surveyed and published during the last quarter of the nineteenth century. Tables, graphs, and contemporary illustrations are also included in the texts.

An introductory section covers the geology and natural resources of the county, along with developments in its administrative areas, both secular and religious, and, from the sixteenth century, the ways in which it was mapped, with extracts from early small-scale maps being included. Sections follow in chronological order dealing with the county's history in pre-conquest times and at Domesday, going back to Roman times; during the later medieval period, reaching into the sixteenth century; and in the modern era, though not usually tending to extend into very recent times. Reflecting the amount of source material available, emphasis is on the modern period, discussion of which occupies about two-thirds of the book. Within each section, various common themes are covered. These are demography, agriculture, industry, urbanization, communications, distribution of wealth, political organization, and religion. In the modern period especially, the themes are explored using several double-page spreads, enabling discussion to be undertaken in some depth. Moreover, revisiting the themes in each of the sections brings advantage in enabling readers to obtain long-term perspectives on matters in which they have particular interests. Even so, the lengthy time periods covered in the sections mean that some elements of change can only be dealt with in summary fashion and without consideration of how they came about, nor their impact.

In a study of this type, the identification and analysis of variations within the county inevitably comprise the major component of the book, with evidence being derived from a range of key primary sources. As might be anticipated, the patterns arising reveal marked variations between localities, serving as a reminder of the dangers and difficulties that arise in seeking to generalize about past developments. What emerges is a wide-ranging set of studies that provide original insights into matters of major historical concern and that demonstrate the types of historical analysis that can be meaningfully undertaken through locally based comparisons. They are presented in a highly accessible form—a matter of great importance in promoting and sustaining interest in social and economic history—that should appeal to a wide readership. The editors point out that, given the restricted length available for each essay, the analysis offered cannot be exhaustive. Accordingly, they aim to provoke readers into making their own enquiries and reaching their own conclusions, helped in so doing by a reference and bibliographical section that draws together the primary and secondary sources used in preparing the essays. In places, a little more in the way of critical comment on these sources, and of the broader historiographical

underpinning to which the essay themes relate, might have helped if these ambitions are to be realized. Yet the essays will greatly assist the endeavours of those seeking to deepen understanding of Staffordshire history, as well as of those undertaking studies based on the same themes relating to other counties. Indeed, they will provide a valuable contribution in facilitating comparative analysis at local, county, or regional level.

Among those who could profit most from consulting the book are undergraduate history students. The popularity among them of locally based studies for the project and dissertation work in which they are required to engage remains high. Not only do the essays in the book suggest appropriate starting points for such studies, giving guidance on what to investigate and how to proceed, but they also help to provide the context in which these studies need to be set, a matter that often brings anxiety for students and that can prove to be a serious stumbling block for them. Added value arises because the studies can involve them in applying basic quantitative techniques in their investigations. In these days of having to demonstrate the impact of academic publications, and of addressing the employability agenda, such considerations should not go unrecognized.

University of Central Lancashire

GEOFF TIMMINS

Jose Bosworth, Pat Hudson, Maureen Johnson, and Denise Shillitoe, eds., *The Middleton papers: the financial problems of a Yorkshire recusant family in the sixteenth and seventeenth centuries* (Woodbridge and Rochester, NY: Boydell and Brewer, 2010. Pp. xxv + 194. 3 illus. 3 tabs. ISBN 9781903564318 Hbk. £50/\$90)

The politics of religion in the Tudor and Stuart period have attracted considerable attention. The obstinacy of northern Catholic landed families, manifest by their widespread defiance of Protestantism, is almost defining. One can only wonder at the stupidity, or marvel at the conviction, of those gentry who suffered repeatedly the ravages of recusancy fines and sequestration as their political campaigns failed successively to achieve their objective. One such family provides the basis of this book: the Middletons of Stockeld. The book comprises a short introduction that provides valuable biographical and contextual material for the focus of the book: William Middleton (1551–1614) and his grandson William Middleton (1615–58). The bulk of the text comprises a transcript of household and estate accounts, 1578–82 and 1653–5, with a variety of associated and supporting documents in each case.

William Middleton senior inherited the estate as a minor and after much political wrangling became a ward of the Earl of Northumberland. This arrangement was financially costly for Middleton and during his minority the estate was neglected. He was implicated in the 1569 Rebellion along with his master, although unlike Northumberland he escaped with temporary incarceration at Hexham rather than execution. Middleton subsequently became a ward of the Crown for the remainder of his minority, to the financial benefit of the Crown. Middleton's financial difficulties were compounded by familial encumbrances: annuities were paid to two uncles, a brother, and two dowagers, and there was an informal arrangement for the support of his younger sister. He was married by 1578 and by October of that year he left Stockeld, initially for London, and then to France and Italy. Following eight months in prison in Naples in 1581 on suspicion of being a government spy, he returned to Yorkshire to resume the normal life of the gentry. He was imprisoned again in York for recusancy in 1600 and the estate became encumbered with fines of two-thirds of the estate's profits.

Such context is necessary for an understanding of the accounts, which form the main focus of the first half of the book. These were kept by his appointed 'overman' and four attorneys during his absence in the period 1578–82. These accounts are not for the faint-hearted and they require a close reading and some interpretation. Unfortunately, the

editors have not provided a detailed commentary on the accounts themselves, much less a detailed analysis. They do suggest that the income of the estate increased by ‘over 100 per cent’ (p. xv) in the four years Middleton was absent, asserting that this was due to more efficient livestock and cereal production. This claim requires further investigation and some justification, ideally with reference to wider economic conditions and agricultural prices. Unless the estate was being managed extremely badly before Middleton left, a doubling of income due to efficiencies in agricultural productivities is highly unlikely. Further doubts are cast by the fact that the Middletons were more dependent on rents and entry fines than they were on direct farming activities. Indeed, in some years, a quarter of their income came in entry fines alone. One cannot but observe that, despite the insights provided by these accounts into estate management during a period of severe financial and political difficulties, four years is simply not long enough to get a sense of changes in estate policy and practice, nor to measure their impact on agricultural productivity.

The accounts are supported by a range of selected source material: draft accounts, household accounts, an estate rental, an inventory of goods at Stockeld, and various pieces of correspondence between Middleton and those he entrusted with the management of his estate written from England, France, and Italy. These provide clear insights into the way in which Middleton maintained his strong influence over Stockeld and the logistical difficulties of getting money to him when he was abroad. Middleton got permission to travel abroad from Elizabeth I and his travel licence is reproduced. These sources combine to provide a rounded view of an individual, the difficulties he faced, and the demands he made of those on whom he depended.

The second half of the book focuses on William Middleton’s grandson, also William, in the first half of the 1650s. Middleton junior had served in the Royalist army since 1643, and the estate was sequestered in 1644. Debts accrued due to the loss of income and from 1653 Middleton set about purchasing his estate back, at a price of £10,000. A series of agreements was drawn up throughout the 1650s and 1660s, with repayments becoming progressively cheaper as the over-ambitious initial payments proved impossible to maintain. Even by 1670 there were still outstanding debts of £5,000. The accounts themselves suffer the same problem as the earlier accounts: they cover a short period and were kept under exceptional circumstances. There is simply no way of knowing just how the straitened circumstances affected patterns of behaviour, as there are no accounts of ‘normal’ times. However, unlike the earlier accounts, these are the household accounts of a gentry family at home: entertaining, eating, drinking, and generally supplying the household’s needs. The additional material again adds tremendous context and research potential: numerous documents regarding the legal arrangements for the repurchase of the estate, a series of correspondence between Middleton junior and his brother Matthew regarding the raising of the finance, schedules of debts, cattle sales, an inventory of household goods, and a shorter account hinting at the rather more carefree life lived by Middleton junior’s youngest brother. The editors have also provided a concise and extremely useful glossary of the regional terms used in the documents and brief biographical notes on dozens of individuals who appear in the text—the latter, frustratingly, being unreferenced, however.

This book is a welcome addition to the literature on the politics and socio-economy of the northern gentry in the early modern period. The editors have done the scholarly community a great service in bringing these documents together and they will no doubt contribute to many studies in years to come. These are not easy documents to use, however, and they require detailed and careful analysis before tentative conclusions could be drawn. Undoubtedly, these documents would repay the effort, and the editors have provided this opportunity. It is slightly bewildering that they have not taken advantage of this opportunity themselves and presented such an analysis by way of introducing the documents.

University of Central Lancashire

ANDY GRITT

Joan Briggs, Rita McGhee, John Smith, Jennifer Tindall, Ann Tumman, and Xenia Webster, eds., *Sunderland wills and inventories, 1601–1650* (Woodbridge and Rochester, NY: Boydell and Brewer for Surtees Society, 2010. Pp. xlv + 304. ISBN 9780854440696 Hbk. £50/\$90)

The Durham probate collection, held at Durham University Library, is now more accessible than ever. A Heritage Lottery-supported project, which finished last year, produced a new database that is now searchable online, and images of the documents should be available online in the near future <<http://familyrecords.dur.ac.uk>>. With the probate records so easily available, the Surtees Society's latest collection of wills and inventories might seem surplus to requirements, but this would do a disservice to such a well-produced edition.

The quality of the transcription and editing is undoubtedly high: the finer details include the nature of each of the signatures or marks given by the testators and witnesses. The editors' policy of leaving original spelling intact but standardizing the use of capitals and silently expanding contractions makes the volume easy on the eye and the documents a pleasure to read. The glossary is comprehensive, including general terms that will be familiar to historians across the country and others that are more esoteric or dialectical. The volume also includes an extensive index of people and places, including all the recipients named in wills and individual debtors and creditors. A subject index, including inventory contents, would have made this more appealing to economic and social historians, although it would have meant considerably more work. The introduction compensates partially for the lack of an index: it works systematically through a number of different subjects, filleting the wills and inventories for evidence and flagging up those that are of particular interest. The themes are well chosen, tailored to a number of important historical debates including agricultural change, room use, occupations, and family and kinship. The selection of landowners to illustrate the reach of credit networks is astute: George Fell of Ryhope (no. 2), for example, had at least 39 debtors in 14 other localities, and George Shadforth (no. 28) died with a Durham lawyer and a Newcastle coal owner among those in his debt. However, the sheer breadth of topics covered means that none is dealt with in particularly great depth, and the analytical material is taken from only a few secondary sources. An opportunity missed, perhaps, but certainly an enticement to further work on these documents.

It is unfortunate that the date span fails really to capture the economic transformation of Sunderland. A small fishing village in the sixteenth century—'a creek of Newcastle, little haunted by merchants and merchandise' (p. xxxix)—the town's coal shipments had expanded greatly by 1650. However, coal is under-represented in these records, with the exception of Ralph Wells's eye-wateringly detailed inventory of 1632 (no. 49). Among an impressive variety of other trades, he had also sold coal to some other merchants, and owed payment to a number of keelmen for 'tides' (loading trips out to the waiting colliers). The editors estimate that an absolute minimum of 36 keelmen were working between 1601 and 1625, but none of these were probated—perhaps unsurprising, given that under a fifth of eligible deaths in Bishopwearmouth were, and keelmen were among the poorest and most unsettled. As the editors freely admit, these early documents are 'more a record of the mortality of an older Sunderland' (p. xlii) than the growth of the new Sunderland; but the introduction also contains a tantalizing hint at the second half of the century, when 'it is evident from the wills and inventories . . . that Sunderland's [growth] was portentously rapid' (p. xlii). These later documents have already been transcribed for the broader Victoria County History project; dare we hope for another edition, to take in this rapid growth?

A subsequent volume would add to the economic narrative, but this one alone will certainly be very useful. Perhaps more unexpectedly, it is also an entertaining read. What could have been a dryly factual introduction is animated by the editors' nose for the lively and often macabre personal stories revealed by the documents, and an understated wit in their delivery. In 1604, an epidemic year, Anthony Gefferson was 'lying in a hollow in the

field, sick' when 'the curate approached him, and, no doubt from a safe distance', took his will (p. xvi). Alice Hall was accused in 1603 that 'upon Whitsunday last [she] did . . . receive to her house in time of divine service above forty persons that came not to church'. Given the likely small size of her house, the editors speculate that 'she was perhaps hosting an outdoor event' (p. xxxi). Even in the age of increasingly abundant internet source material, there is still great value in an edition such as this.

University of Durham

ANDREW BURN

Celia Cordle, *Out of the hay and into the hops: hop cultivation in Wealden, Kent and hop marketing in Southwark, 1744–2000* (Hatfield: University of Hertfordshire Press, 2011). Pp. xvi + 183. 3 figs. 3 tabs. ISBN 9781907396038 Hbk. £35/\$79.97; ISBN 9781907396045 Pbk. £18.99/\$30)

Out of the hay and into the hops is the ninth volume in Hertfordshire Press's studies in regional and local history series. It is an attractively packaged and accessible account which creatively interweaves the histories of hop growing and trading in two separate but linked communities: Wealden Kent and the borough of Southwark, London. Both areas, it is argued, had 'outsider' status based upon their physical, political, and economic histories. They were connected through the sometimes unequal and fractious (but always mutually dependent) relationships between hop growers, factors, traders, and brewers. This book examines the history of the people involved in the trade over a 300-year period.

Hops, while not central, were an important part of the agrarian economy of some areas of south-east England. Introduced into Kent by Flemish refugees in the mid-sixteenth century, they were cultivated on a small but increasing scale, reaching a peak in the latter half of the nineteenth century. Before the First World War Wealden acreages represented about a fifth of the English total. Cordle argues in chapter 2 that the optimism associated with the crop was akin to early gold prospecting, and an excellent examination of the disparate records of a number of farms that took the plunge into hops between the mid-eighteenth and nineteenth century reveals that the instability of yields and prices meant the result was not always an unequivocal success story. This chapter and the next (which turns to the 1890s accounts of Ernest Wickham) are fascinating in the level of detail they provide on methods of production, on the introduction of new innovations such as fertilizers, new pest control washes, and wire work for hop growing, and on the variety of labour practices involved in hop cultivation, harvesting, and drying over the course of the annual calendar.

These varied processes introduced new occupations for the local labouring population, and relied on the seasonal input of migrant workforces, both often working in family groups. The focus of the book is not on the hop-pickers, whose story has been told elsewhere. Instead they emerge as an essential—albeit sometimes disruptive—element in a complex, interdependent system. As the twentieth century progressed, hop acreages fell and cultivation became more concentrated in certain districts. The impact of tractors, the increased use of wire work, new drying methods, hop-picking machinery, and scientific research on hop disease and variety are detailed in chapter 4. It is argued that change and decline in Wealden hop cultivation was not uniform and again this chapter utilizes the histories of individual farms and farmers effectively to track these postwar change. The book then traces the links between the hop gardens of the Kentish Weald and the trading markets of Southwark, a district the author depicts poetically as one 'steeped in memories of the hop trade and a way of life now gone forever' (p. 125). Again, a number of sources are handled with a deft hand, really bringing the relationship between the growers and factors to life. The records of a mid-scale trading firm, Arthur Morris & Co., and the oral history of hop factor Ben Wright are used to great effect to outline how the whole process worked in the twentieth century. The

interdependence of each part of the production and marketing chain is revealed, and the centrality of family, community, and place to both locations emerges.

This book is innovative in terms of its scale and content, and in drawing the connections between the two districts of Kent and London it is a fascinating and valuable addition to the literature on hop culture. Its methodological approach is also very impressive and the use of a range of both quantitative and qualitative source material acts as a model for what a local and regional study can achieve. There are some quibbles. In particular the final substantive chapter, which covers the 1836 Tithes Commutation Act and the 1862 Repeal of Hop Duty, while adding detail on the wider legislative environment in which growers operated, did feel rather tagged on at the end, and the weaving of this story into the earlier chapters might have been more effective. In the end, though, this is a thorough and thoroughly pleasing study of one of England's 'alternative' agricultures.

Sheffield Hallam University

NICOLA VERDON

Jan Luiten van Zanden, *The long road to the industrial revolution: the European economy in a global perspective, 1000–1800* (Leiden: Brill, 2010). Pp. xiii + 342. 40 figs. 1 map. 12 tabs. ISBN 9789004175174 Hbk. £88.20/€105/\$144

Like many commentators, van Zanden sees institutions as the key to economic success. The central idea of the book is that the successful institutions that introduced modern economic growth developed out of medieval European experience. The experience was Europe-wide. To see the Dark Ages in Europe with its weak states and fragmentation as the font of modern growth is initially surprising. However, van Zanden argues that these conditions promoted a bottom-up process of institution building. These had plural, almost proto-democratic, characteristics that made them both resilient and efficient. The ultimate success of the North Sea economies of the Netherlands and England was to preserve and extend these institutions.

The church inserted itself into the power competition of early medieval Europe. It promoted a 'rule of law' that transcended the power of the state. It also established the proposition that the written word carried the 'truth' and that written documents verified property rights (often, of course, of religious houses). Within this world of weak and divided state power, bottom-up institutions came into being. In particular, various corporate bodies (*universitates*) developed as fora for collective action. These corporate bodies in turn developed institutional rules. During the high middle ages these institutions coalesced into city states based on concepts of citizenship.

Van Zanden emphasizes the role of the European marriage pattern. Again the medieval church played a key role, promulgating the doctrine that valid marriage depended on the consent not only of the families but also both the individuals involved. This reduced patriarchal power, led to later age of marriage, and encouraged individualism. With labour scarcity in the aftermath of the Black Death, opportunities for wage labour expanded, which relatively independent unmarried youths grasped. As a consequence, market allocation of labour became common in Europe, unlike most of the rest of the world where labour allocation remained within the extended family.

One of van Zanden's accomplishments is to provide proxies of institutional developments. He examines estimates of book production, in various parts of Europe and in comparison with the rest of the world, to indicate the growth of a knowledge economy. Book production expanded impressively throughout Europe but not elsewhere. He quantifies the knowledge economy further through an examination of the premium skilled labour received over unskilled labour, thereby indicating the extent that human capital was accumulating. European skill premiums fell sharply in the fourteenth century to levels that were very low by international standards.

Medieval Europe grew rapidly but did not diverge from the rest of the world. We now have considerable amounts of data on real wages in much of the world for long periods. These data show two things. First, after the effects of the Black Death real wages in Europe declined and by the sixteenth century approached near subsistence levels and did not differ greatly from advanced parts of Asia. More strikingly, however, wages in the Netherlands and England did not decline to the extent they did elsewhere and rose in the seventeenth century. This van Zanden dubs the 'Little Divergence'. This 'Little Divergence' is the key to Europe's success. Van Zanden argues that the North Sea economies extended the institutional preciosity of medieval Europe while much was lost elsewhere as empire and patriarchy prevailed. The North Sea economies developed successful fiscal-military states, modelled after medieval city states based on a concept of citizenship rather than power. The Netherlands was, after all, a confederation of cities that emerged from medieval Burgundy. England, perhaps, needs more attention. By the eighteenth century it certainly was a fiscal-military state whose actions reflected mercantile interests. Why it became so is less obvious, although the Civil War and the Glorious Revolution played important roles. However, the Tudor monarchy of Henry VIII, supposedly about the time the Little Divergence began, seems not to fit the model. Perhaps Elizabeth I is a key here. There were other important features. First, property rights were established in such a way that they were guaranteed to those without political power. Power had been constitutionalized. Furthermore, means existed by which institutions could change in a transparent manner if needed.

Finally, van Zanden provides a brief comparison with Asia. Here the most obvious differences are, first, the fact that the institutions lack the bottom-up character that Europe developed in medieval times; and, second, it is striking that the labour market is very sparsely developed, presumably because the European marriage pattern is not present. Not often are there books that cover such a range of history with such an interesting collection of hypotheses and evidence. The ideas are well developed but invite both development and challenge. The book should spark a lively literature. It is hard to think of greater praise.

University of Oxford

C. KNICK HARLEY

Eric L. Jones, *Locating the industrial revolution: inducement and response* (Singapore: World Scientific, 2010. Pp. vii + 272. ISBN 9789814295253 Hbk. £45/\$68)

Jones is no newcomer to the history of the British industrial revolution. In *European miracle: environment, economies and geopolitics in the history of Europe and Asia* (1981) and *Growth recurring: economic change in world history* (1988), he argued that despite some episodes of significant economic growth outside Europe before Britain's industrial revolution, Europe enjoyed unique and superior political systems and cultural norms long before the onset of industrialization. Europeans were 'peculiarly inventive', prudent in matters of family formation, and particularly blessed with site, location, and resource endowment. These qualities, he argued, laid the necessary early foundations for the subsequent great economic and material advances associated with industrialization. Jones's books were influential and controversial in equal measure. His suggestion that Europe's early industrialization owed much to that continent's cultural superiority undoubtedly resonated among many students of industrialization. Others, however, dubbed his interpretation 'Eurocentric', even 'racist', and unpicking elements of his interpretation has become a minor industry of its own in some quarters of the historical profession in the past two decades.

In some respects, *Locating the industrial revolution* shifts away from the ground covered in Jones's earlier work. Whereas *European miracle* and *Growth recurring* both had a very wide geographical focus, contrasting patterns of industrial development not simply across Europe but between Europe and Asia, this latest book is confined not simply to England,

but to just the southern half of the country. Interpretively, however, this book bears strong similarities to Jones's earlier work. In *Locating the industrial revolution* (and in contrast to his earlier work) Jones downplays the significance of natural resources in launching industrialization in Britain, but continues to place considerable emphasis upon the nation's political institutions and cultural values.

The idea behind his latest book is interesting. Jones argues that we might better understand the forces at work during the British industrial revolution by turning attention away from the much-studied rapidly developing north of England and by considering the parallel process of deindustrialization in the south of the country. The execution of this idea, however, is rather less convincing. A study of economic change in the south of England would certainly enrich our understanding of industrialization, but, with the exception of chapter 6, almost no fresh evidence is presented here in support of the thesis. (Chapter 6 is based on the family history of the author's own family. There are certainly some interesting stories told here, but the evidence is largely anecdotal and not really sufficient to add much to the very dense and complex literature on industrialization that already exists.) Elsewhere, *Locating the industrial revolution* simply depends on the existing literature. Yet as Jones himself admits, much of the historical work in print is 'quite minor' and is seldom written 'in ways that economists would approve'; whereas contributions from economists are 'rare and not particularly helpful' (p. 259). This leaves him with very little to develop the argument that he wishes to pursue.

The shift in emphasis away from the role played by natural resources, above all coal, rests upon a rather partial reading of the existing literature. Within England, Scotland, and Wales, industries clustered around the coalfields in the most remarkable and consistent fashion. During the eighteenth and nineteenth centuries, industries and population flooded into the coal regions, many of them largely previously undeveloped, in a pattern that has been documented thoroughly and convincingly in this journal (and elsewhere) by Gerald Turnbull (for example, 'Canals, coal, and regional growth during the industrial revolution', *Economic History Review*, XL (1987), pp. 537–60), Tony Wrigley ('English county populations in the later eighteenth century', *Economic History Review*, 60 (2007), pp. 35–69), and others. Jones's attempts to use a study of England, of all places, to suggest that coal deposits were incidental to the timing, pace, and location of early industrial development fails to convince this reviewer.

The ideas behind *Locating the industrial revolution* are promising and the book provides a useful general introduction to the economic history of southern England during the eighteenth and early nineteenth centuries which may be of interest to the general reader. Readers of this journal, however, are likely to find that this book adds little either to their knowledge of the economic history of southern England, or to their understanding of the process of industrialization more broadly.

University of East Anglia

EMMA GRIFFIN

Eileen Wallace, *Children of the labouring poor: the working lives of children in nineteenth-century Hertfordshire* (Hatfield: University of Hertfordshire Press, 2010. Pp. xv + 168. 35 illus. ISBN 9781905313495 Pbk. £14.99/\$33.95)

The contribution of children's labour to British industrial expansion in the eighteenth and nineteenth centuries is currently the subject of vigorous scholarly debate. Despite the accumulation of evidence that appears to place the importance of youthful work beyond dispute, traditional preoccupation with children's victim status which ignores their constructive role continues to impede a revised interpretation. In this short, empirically rich study of children's work in nineteenth-century Hertfordshire, Wallace implicitly adds to

this debate, while surprisingly neglecting recent literature. Her subjects are children, whose families are poor, but not pauperized, and whose working lives illustrate a range of experiences.

In this south-eastern English county situated to the north of London, a range of agricultural and manufacturing pursuits provided gainful employment for adults and children of both sexes through the long period of industrialization. Wallace devotes a chapter to each of the key activities, identifying not only the harsh circumstances of much child labour, but also its value to the household and to the wider economy. In Hertfordshire as elsewhere, farming offered multiple occupations for children, including the ubiquitous bird scaring for the very young, before many of them, especially girls, moved on to employment in other trades. Mid-nineteenth-century Hertfordshire contained such regional specialisms as the manufacture of straw plait and silk, both of which depended for several decades upon the labour of mainly female children. Paper production also enjoyed prosperity in the county with the help of children's work, while brick-making provided seasonal employment for all the family, and children played a full part in even the heaviest tasks. More gendered but less regionally specific was the work involved in domestic service and chimney sweeping. All of these trades, as well as children's employment in them, declined from the 1850s as a result of both economic and legislative forces.

While Wallace highlights the heterogeneous nature of children's work in these sectors, common themes emerge. The concern of the state to protect the well-being of young people, for example, was reflected in investigations and legislation of each activity; and despite inadequate implementation, the environment of children's labour improved somewhat. The regular tension between education and employment, which became increasingly pronounced from mid-century as family need for children's wages regularly clashed with the law, is exemplified expertly by Wallace, whose own background in education and close consideration of school records pays dividends here. Additional discussion of pay and conditions, of diet, and of wider matters of family and living circumstances is included in each chapter. Although the author makes effective use of census material, and the argument for widespread children's employment is clearly made, she appears more comfortable when exploring discursive and qualitative sources. Contemporary views, mainly those of government and poor law officials, are cited at length but not always appraised critically. The voices of children themselves are heard intermittently, though typically these are mediated through the agency of factory inspectors.

Wallace's findings support the prevailing interpretative preference for raising the profile of children's work. Using the case of Hertfordshire, she demonstrates what historians are finding elsewhere, that not only is it hard to imagine that many labouring families would have survived without their children's wages, but given the prevalence of children's work it is difficult to see how much business activity would have been sustained without this labour. Furthermore Wallace's evidence confirms recently developed arguments that father's occupation was a key determinant of the extent of children's employment, and that despite the vagaries of working children's lives, their stoicism and sense of pride in contributing to their family's income is palpable. Although Wallace identifies some key elements of gender differences in Hertfordshire employment, she is less obviously concerned with the longer-term implications of gender distinctions in children's work. It would be misleading to claim that Wallace's research would not have had greater impact had it been subjected to more robust analytical and historiographical framing, but her study does sit neatly within the growing body of scholarly work on the history of children, childhood, and child labour. As such, it comprises a significant contribution to the project of retrieving the neglected history of working children.

University of Leeds

KATRINA HONEYMAN

Derek R. Peterson, ed., *Abolitionism and imperialism in Britain, Africa, and the Atlantic* (Cambridge, OH: Ohio University Press, 2010. Pp. ix + 235. 9 illus. ISBN 9780821419014 Hbk. £57.95/\$64.95; ISBN 9780821419021 Pbk. £25.95/\$28.95)

Viewed in a long-term perspective, the abolition campaign was as unlikely as it was successful. Beginning in the period when the transatlantic slave trade was at its height, abolitionists managed within about a century to suppress the movement of enslaved Africans across the Atlantic and to make major progress towards ending the use of slave labour globally. The factors that shaped involvement in this campaign have been hotly debated since the 1940s, and particularly for Britain, with discussion often moving beyond the confines of academic institutions. Indeed, the gulf between various perspectives on abolitionism was clearly illustrated in 2008, the year of the bicentenary of the Act for the Abolition of the Slave Trade, which the British government celebrated with much fanfare. It was against this self-congratulatory backdrop that the Cambridge's Centre of African Studies hosted a series of lectures 'meant to focus critical scholarly attention on British abolitionism, to illuminate the broader geographic and temporal terrain in which abolitionism took place, and to claim the study of abolitionism as a field for Africa's historians' (p. ix), with the volume under review resulting from that conference.

Collectively, the introduction and six additional chapters span some five centuries and cover developments in West and East Africa, the Caribbean, and Britain. In a rich introduction, Peterson weaves together these wide-ranging contributions skilfully to develop a nuanced framework for understanding abolition in both African and transatlantic contexts. Underscoring a recent trend that highlights the ways in which non-European peoples engaged with the abolition movement, Peterson emphasizes that abolition was not merely imposed by Britain on the rest of the world. In his view, the Atlantic functioned as 'an echo chamber in which abolitionist symbols, ideas, and evidence were generated from a variety of vantage points' (p. 27). Such a framework allows space for global, regional, and local actors to shape the abolition campaign across time and space—a perspective that scholars often struggle to achieve in more narrowly focused monographs.

The volume is well balanced in terms of theoretical and geographic coverage. Four chapters deal with the relationship between abolitionism and imperialism from a loosely British perspective. Christopher Brown links Britain's largely neglected pre-nineteenth-century commercial projects in West Africa with its post-abolition imperial plans. While Europe's commercial schemes in Africa before the late nineteenth century have generally been characterized as commercial failures, Brown argues that they allowed abolitionists to imagine a future in which Africa featured as a centre of agricultural rather than human exports. Seymour Drescher's contribution situates the abolition campaign in a broader temporal and geographic perspective and contrasts it briefly with the various other forms of coerced migration that replaced enslaved labour. Robin Law explores the British campaign to suppress the transatlantic slave trade and the various ways that it encroached on the sovereignty of several African states. Boyd Hilton's chapter traces the history and representation of abolitionism in Britain itself and reflects on a number of key historiographical questions that have occupied scholars of abolition for over a century.

The remaining four essays move outside of Europe, with the American side of the Atlantic less well represented than its African counterpart. Philip Morgan provides the only treatment of the Americas in an essay that underscores the complex interplay between domestic and international forces in shaping abolitionist discourse and actions in the Caribbean and Atlantic worlds. John Thornton's essay examines written correspondence from a handful of African leaders who left documentary evidence about their attitudes toward the slave trade. He concludes that these leaders did not see slavery itself as morally reprehensible and that their letters reflect an interest in 'establishing a proper order of enslavement and an orderly slave trade' (p. 39). Peterson and Jonathon Glassman bring the

story into twentieth-century East Africa. Both of their contributions assess the political uses to which Africans put abolitionist discourse while also tackling the complex issue of how memory constructed in the abolition era informs contemporary social and political developments in colonial and postcolonial Uganda and Zanzibar.

One of the few major issues not addressed in this volume is how Africans confronted the tensions and concerns that the emergence of the transition from slave trading to imperialism raised. For example, although several scholars provide new insight into Sierra Leone's early growth from a European or transatlantic perspective, it would have been interesting to consider what this colonial project meant to Africans living on its outskirts. Or, to put it more basically, what did abolitionism mean to Africans during the time when it was first being imposed? This is not a criticism so much as an acknowledgement of one of the many new questions that this collection of essays raises.

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PHILIP MISEVICH

Denis P. O'Brien and John Creedy, *Darwin's clever neighbour: George Warde Norman and his circle* (Cheltenham and Northampton, MA: Edward Elgar, 2010. Pp. lvi + 444. ISBN 9780848445574 Hbk. £95/\$165)

This misleadingly titled book does not provide an analytical account of the intellectual and social milieu of George Warde Norman, an important writer on monetary policy and an influential and long-serving director of the Bank of England (1821–72), whose grandson Montagu Norman (1871–1950) was to prove a controversial interwar governor of the Bank. Instead it is a useful edition of Norman's autobiography, written c.1857–8, which survives in manuscript form, fragile and fire-damaged, deposited in the Centre of Kentish Studies, Maidstone. The editors provide extremely informative footnotes throughout the text, revealing much genealogical research, although not always successfully teasing out the identity of Norman's characters (for example, the Wilkinson at Shortlands was Ricardo's brother-in-law William A. Wilkinson).

This work is in many ways a supplement to the fine edition of *The correspondence of Lord Overstone* edited by O'Brien in three volumes and published in 1971, in which Norman featured strongly as a 'unifying thread'. It seems also to have been long in the making (for example, making reference only to the original rather than the 2004 *Oxford Dictionary of National Biography*). It makes no reference to the use of Norman's autobiography by later scholars, nor does it attempt to situate this unusual autobiography within the rare genre of self-writing by mid-Victorian businessmen. However, it has also to be said that Norman's account of his life, although full of much fascinating detail, does not provide much by way of illumination of his thinking on monetary policy, although this is fully discussed in the editors' valuable introduction. We do, however, see an elite career in the making—schooling at Eton, from whose evil effects he still claimed to suffer in his later life; a business life in the timber trade in the Baltic, which gave him an unusual expertise in things Norwegian, greater he believed than that of any contemporaries; the importance of patronage and family connection which led to an early place in the Court of the Bank of England; extensive travel throughout Europe; Norman's voracious reading; his interest in geology, his love of sport (especially cricket but also fishing), his long and disabling periods of nervous disorder and illness, his marriage and dutiful attention to kith and kin but agonizing over the rights and wrongs of primogeniture and entail. His religion remains obscure and curious—Unitarian by family background but in other respects, including attendance and church-building, an Anglican.

Of greater significance to his career were Norman's early intellectual interests through his membership of the fledgling Political Economy Club and through the philosophic

radicals, especially George Grote, his closest friend, before they fell out spectacularly in 1832 over an apparent if inadvertent and indirect slur on Mrs Grote's character. Norman also provides a short account of his political activities, especially in West Kent, for he remained a firm Liberal, an admirer of Richard Cobden's anti-corn law career but with less respect for Sir Robert Peel, whose monetary goals he shared and supported through his advice within the Bank. Unusually too for his background he appears as a critic of the Crimean War, which was to claim his eldest son. Finally we see Norman as a local citizen, a minor benefactor of Bromley, whence the social connection with Darwin, resident at Down House nearby, but to whom no reference is made by Norman. It therefore seems disadvantageous to a proper assessment of Norman's career to epitomize it by a vicarious reference to Darwin rather than to Norman's own achievements in financial policy and his institutional legacy. How important his role within the Bank of England was, however, will remain somewhat of a mystery, but this autobiography amply reveals the self-doubt which led Norman to decline to take on responsibility for the Bank as its governor, and confirms his friend Overstone's view that 'As regards the Bank Directors, Norman is the most experienced and I have little doubt the wisest of the body in council—when we come to action, and require prompt & firm decisions, the case may be different' (TNA, PRO, 3048/52, Overstone to Edward Cardwell, 7 January 1858).

University of East Anglia

ANTHONY HOWE

Minoru Yasumoto, *The rise of a Victorian ironopolis: Middlesbrough and regional industrialization* (Woodbridge and Rochester, NY: Boydell and Brewer, 2011. Pp. xvii + 230. 119 figs. 5 maps. 5 plates. ISBN 9781843836339 Hbk. £60/\$99)

The town of Middlesbrough, despite its modest size and brief impact on Britain's economy, has drawn the attention of a range of historians for two interrelated reasons: as the fastest growing industrial town of the nineteenth century, and as the centre of the world's pig iron production in the third quarter of the nineteenth century. Yasumoto's history of the town in the two middle quarters of the nineteenth century—from its foundation as a coal port in 1830 to its economic eclipse after 1880 as steel replaced iron—focuses on explaining and assessing the town's economic and demographic revolutions and some of their social consequences. To accomplish this task he has utilized both traditional sources such as the census, trade directories, and local press, and some new primary material, including evidence from the recently opened British Steel Archive. The book is divided into three broad sections: the formation of the town and its development as the 'economic hub' for the Cleveland iron district; the extraordinary demographic changes experienced between 1851 and 1871; and the response of the urban elite to social dislocation through the provision of hospital care.

The first chapter rehearses much of the established narrative of the formation of the town, though it does deploy new evidence on the role of the chief investors in the early development of the town and the process by which it progressed rapidly from a rural farmstead to a self-governing borough. This is a useful contribution to the literature on urban governance as the creation of municipal and organizational infrastructures is rarely addressed in studies concerned with small or rapidly growing communities in this period. He then considers the rapid development of the iron industry in the town, utilizing the idea of the 'economic hub' to show how growth was based on the convergence of a number of interrelated beneficial factors including the presence of coal and iron deposits, rapidly developing transport infrastructure, and the emergence of systems of market and technological intelligence and the founding of closely linked ancillary trades. The main body of the book is concerned with the demography of the new town and the flows of its

population. Deploying nominal record linkages from the 1851, 1861, and 1871 censuses, the study demonstrates who came to the town, where they came from, their sex, age, and occupations. Yasumoto demonstrates the extremely high levels of mobility in the population, his findings suggesting a population turnover of around 80 per cent in both decades, significantly higher than similar towns in Europe and America. The age and sex ratio of the population—predominantly male and aged between 15 and 30—is probably less of a surprise, but it does show that skilled workers were more likely to be older and to come with their families, though not markedly more likely to stay for more than 10 years. Yasumoto then turns his attention to the structure of the labour market as revealed by the census and by the available information on the educational infrastructure in the town. From the latter he concludes that employers relied heavily on high wages to attract migrants rather than developing the means to train and retain workers from within the local population. This, he suggests, proved damaging when steel emerged as a competitor to pig iron and Middlesbrough proved unable to compete, in part due to the lack of a skilled workforce.

The final substantive chapter is concerned with the provision of hospital care in the town as a response to the considerable range of health hazards created by the rapidly developing industrial environment. Yasumoto's demographic analysis suggests that, contrary to the situation after 1900, the death rate in the town remained at the lower end of the scale for the main industrial centres up until 1880. These findings notwithstanding, it is apparent that iron and steel making was a dangerous trade in which accidents were commonplace. As a result, two hospitals developed in the town focused largely on treating accidents at work, one largely funded by the workers' own contribution and the other, Yasumoto claims, led by the local elite as a more traditional charitable institution. Whether or not the division was this clear-cut it does show that while the employers were reluctant to pay for training they did support a good infrastructure for healthcare through the workplace which may have helped them to attract and retain labour. Overall this is a useful contribution to the urban, demographic, and medical history of mid-nineteenth-century Britain. The chapters are fairly self-contained and the book relies very heavily on tables (over 100 in the body of the text plus appendices) and statistics to tell its story. As a result there is no strong narrative thread running through the book; rather it provides a series of snapshots which illuminate wider debates about nineteenth-century economic and demographic change through the microcosm of England's 'Infant Hercules'.

University of Huddersfield

BARRY M. DOYLE

Michael Heller, *London clerical workers, 1880–1914* (London: Pickering & Chatto, 2011. Pp. xi + 262. 16 tabs. ISBN 9781848930544 Hbk. £60/\$79.20)

According to conventional wisdom, clerical work in the mid-eighteenth century was high status and high paying. A clerk was a trusted servant who was but one step removed from the owners or senior managers of a bank, commercial enterprise, insurance company, law office, or government body. In the late nineteenth and early twentieth century the clerical sector began a rapid expansion, the number of clerks grew substantially, the clerical market became overstocked, and the pay and status of the clerk began to decline.

It is this hypothesis of decline that Heller's excellent book seeks to refute. The main thesis of this monograph is that the growth in the number of clerks largely reflected increased demand for clerical labour, and rather than declining, the social and economic position of the male clerk was transformed largely to the better. Heller argues that the prevailing view of a slow decline in wages and loss of status was created later by clerical

unions in order to advance their agenda, and has gone largely unquestioned by subsequent historians. However, the foundations for this view are shaky and are undermined by a thorough examination of the evidence.

The book examines a range of clerical labour market conditions and changes to these conditions over the period 1870 or 1880 to 1914. Its focus is solely on London, the centre of the British clerical labour market. The main conclusions can be summarized as follows. The status of the clerical elite (clerks in banking, law, insurance, and Civil Service) remained high, and openings received many applications. Salaries were stable or increasing during the period. Internal labour markets, characterized by lengthy careers with the same employer, well-defined promotion paths, and standardized pay scales, were increasingly the norm. Employers established sports and social clubs for their employees, and a large proportion of clerks took advantage of these offerings. There were improvements in formal education, with the establishment of vocational curricula at schools. Virtually all juniors entering a clerical position possessed a secondary education by the end of the period. New technologies such as the typewriter and adding machine changed the nature and pace of work, but were fundamentally not deskilling. The growth of female employment in the sector did not undercut the salaries or status of male clerks, as women were largely restricted to lower status and routine positions, leaving the better clerical positions for the men.

Heller is to be applauded for the forcefulness of his arguments and the thoroughness of his research. He is almost certainly correct in arguing that previous commentators have exaggerated the evidence pointing towards decline in the years before the First World War. However, he is guilty of the analogous shortcoming of ignoring or playing down any evidence of decline. For example, Heller notes that salary scales at the London and County Bank and London County Council remained unchanged between 1870 and 1909 and 1890 and 1914, respectively, but then fails to point out that, because the price level rose by 16 per cent between 1890 and 1914, that real salaries must have declined. Similarly, table 4.11 shows that tenure-adjusted real salaries at London County and Westminster Bank branches declined quite substantially between 1890 and 1913, but this fact is underplayed in the analysis thereafter (which focuses on rising tenure). Of particular concern is the fact that when examining trends in salaries, Heller always bases his comparisons on an 1870 or 1880 starting date. However, a comparison between 1870 (or 1880) and 1914 masks considerable real wage increases between 1870 and 1890, followed by declines after 1890. A clerk working in 1914 would likely have looked back over the previous couple of decades when assessing changes to their standard of living, and would have taken little consolation from the fact that the wages of an earlier generation of clerks had been increasing. The choice of starting dates clouds other analyses in the book. For example, Heller argues that female employment did not reduce male wages because male wages did not decrease before the First World War (p. 113). However, this argument neglects the fact that between 1870 and 1890, when male wages were increasing, female employment remained very low (increasing from 1.0 to 5.4 per cent of the British clerical labour force according to census data). The main increase in the female clerical labour force over the period occurred between 1890 and 1911 (from 5.4 per cent to 22.2 per cent), precisely the time when male wages were decreasing.

While I am somewhat sceptical about the main conclusion of the book, I nevertheless recommend it strongly to scholars in business or labour history. The book provides a thorough, nuanced picture of clerical work and the London labour market around the turn of the last century. It brings a welcome breadth of analysis to the clerical labour market, combining various qualitative and quantitative sources and covering many aspects of the clerks' careers. It also raises the analytical bar for scholars seeking to preserve the traditional narrative that clerks experienced a decline in their standard of living in the decades before the First World War.

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ANDREW J. SELTZER

Tiziano Raffaelli, Giacomo Becattini, Katia Caldari, and Marco Dardi, eds., *The impact of Alfred Marshall's ideas: the global diffusion of his work* (Cheltenham and Northampton, MA: Edward Elgar, 2010. Pp. xvii + 251. 1 fig. 1 map. ISBN 9781847205124 Hbk. £69.95/\$115)

The word 'Marshallian' is recorded by the *Oxford English Dictionary* as first appearing in the *Economic Journal* (vol. 4 (1894), p. 756), where it was defined as 'a reconciliation of the "Walrasian" and "Marshallian" methods' and was invoked in concluding a *précis* of a paper in a then recent number of an Italian journal (*Giornale degli Economisti*). This is not mentioned in the work under review but is introduced here for two reasons: to illustrate an early example of engagement with Marshall beyond Cambridge and Britain, which is the principal concern of this collection of essays; and as indicating the date of arrival of the adjectival form of the author's name around which would cluster ideas (rightly or wrongly attributed) and the possibility of a 'school', as Frank Taussig was to ask in his 1924 Marshall obituary, and as next cited in the *OED*, 'Will there be a Marshallian School?'. In large part underlying the essays here is the retrospective question, 'Was there ever anywhere a truly Marshallian School (and if not, why not)?'.

The Marshallian intellectual seed fell sometimes on fertile ground, and sometimes it did not. In all, 30 contributors in 25 chapters organized into six parts (I. 'Conventional' and "new" views of Alfred Marshall'; II. 'The home reception'; III. 'Marshall in English-speaking countries'; IV. 'Marshall in Continental Europe'; V. 'Marshall in Asian countries'; and VI. 'Marshall in the views of some "itinerant" economists') report the multiple voices heard in the 'reception' of Marshall. Some contributors, one feels, have taken more readily than others to the approach suggested by the editors (pp. ix–x) but all, necessarily, have had to write of the state of the profession (almost) globally in Marshall's day and after. Such a contemporary survey is of value in itself, but there is more here than this.

The conditions under which Marshall in particular and 'Cambridge' more generally could be received varied: from one extreme, where at the University of New Zealand examination papers in the period 1894–1924 were set by *inter alia* H. S. Foxwell, J. N. and J. M. Keynes, and J. S. Nicholson (Anthony M. Endres, pp. 86–7), to the other in Russia where, after the unpublished c.1917 translation of the *Principles of economics* by V. Y. Zheleznov, no translation appeared until 1983 (Irina Eliseeva, pp. 122, 126), by which time almost everywhere Marshall's ideas had passed into the 'history of economic thought' category. Apart from the language barrier and the necessity of translation—including the case of the first Spanish translation in 1922 of the *Principles* having been made from a French translation (Lluís Argemí, p. 163)—Marshallian thought had to contend with rival schools, such as the German Historical to which prior allegiance was most often claimed, as well as the rival schools centred on, for example, Léon Walras and Vilfredo Pareto; and matters were to be far from helped by Paul Samuelson's scathing dismissals of Marshall (Roger Backhouse et al., p. 75).

Two general informative essays on Marshall's place in the history of economic thought (Anthony Brewer) and the state of Marshallian scholarship (Peter Groenewegen) are followed in part II by two essays dealing with the reception of Marshall and his fate on the 'home front' (Carlo Cristiano, Cambridge; Keith Tribe, Britain). Here, uniquely, were all the elements for a positive and lasting outcome: no language barrier, immediate access to his works (including, but not greatly mentioned, reports of his testimony before public commissions), and his students increasingly in influential teaching positions. Things *did* get off to a good start with the appearance of 'Marshallian' textbooks in the years just prior to the fifth (1907) and just after the sixth (1910) editions of his *Principles* (Tribe, p. 45); yet it was not to last. It was from Cambridge itself that the Marshallian vision began to unravel, through the efforts of his successor in the chair (A. C. Pigou), Piero Sraffa, and others. Perhaps Marshall had left his best too late: 'Had all these [later] books come into being,

and had they been published at an earlier point in Marshall's career, the Professor would very likely have had a much more profound influence upon his pupils' (Cristiano, p. 18).

Space does not permit the detailed discussion that this wide-ranging collection deserves; but a few general remarks can be made. On the negative side we note the lack of any discussion of the fate of Marshall's thought in South Africa and South America—even 'nothing to be said' is in itself worth reporting. It would also have been of some interest to hear just how sound the translations of Marshall were—the substitution of *economia politica* for 'economics' in the title of an early Spanish translation of the *Principles* (Argemí, p. 163) hints at what could yet be uncovered. A persistent irritation was in seeing, not in all cases but too often (even in the index, p. 242), Marshall's 1892 *Elements of economics of industry* with the definite article preceding *Elements*, especially where one is sure that the writer knows better (Groenewegen, pp. 83, 85). On the positive side, as well as (as noted) providing us with well-researched summaries of national developments, the quantitative analysis provided by Backhouse et al. in dealing with the US invites fruitful imitation; and the concluding essays on the reactions to Marshall of selected economists (Pareto, Schumpeter, and Georgescu-Roegen) add, in addition to what is said of them elsewhere, to our appreciation of these seminal figures.

This collection of essays, edited by economists from the universities of Pisa, Florence, and Padua, is but the latest evidence in book form of the continued interest in Marshall in Italy; as the editors tell us, it 'developed as a nearly spontaneous sequel' to the *Elgar companion to Alfred Marshall*, edited in 2006 by Raffaelli, Becattini, and Dardi (p. ix). It is a worthy sequel and will prove to be a valuable complement in Marshall studies.

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GEOFFREY FISHBURN

Robert W. H. Miller, *Priest in deep water: Charles Plomer Hopkins and the 1911 seamen's strike* (Cambridge: Lutterworth Press, 2010. Pp. 226. 10 illus. 4 tabs. ISBN 9780718892326 Pbk. £25/\$52.50)

Charles Plomer Hopkins (1861–1922) was an Anglo-Catholic priest who became deeply involved in the plight of British seamen during the late nineteenth and early twentieth centuries. This engagement culminated in a key role on the trade union side in the 1911 seamen's strikes, led by Havelock Wilson; part of the broader strike movement of 1910–14 which George Dangerfield made a central drama in *The strange death of Liberal England* (1935). Hopkins's career began on the Indian sub-continent, first in Rangoon where he graduated from church organist to priest in charge of the Seaman's Mission. Later, in Calcutta, he moved beyond social welfare and spiritual ministry to seamen and emerged as a passionate advocate for their employment rights against strong opposition from the ship-owners. Against the background of legislation promoted by Plimsoll and others, he came into contact with labour organizations. When his bishop forbade active engagement with trade unions, he formed his own Seamen's Guild, a quasi-union, modelled on Medievalist lines consistent with his brand of faith.

The other side of Hopkins's life was a strong ritualistic Catholic yet Anglican religiosity. This saw him create a small branch of the obscure Order of St Paul and then establish his own priories, including Alton Abbey, Hampshire. His ideal was a brotherhood that lived together to serve the benighted and ill-treated seamen; a vision that the author sees as pioneering further Anglican and Catholic missions—alongside similar work by Methodists and Baptists. Where Hopkins stands out is in the depth, longevity, and expertise of his commitment to seamen. His detailed knowledge of the industry and its practices and laws led to him becoming a central trade union advocate and expert witness in the later years of his life. The nature of this difficult-to-organize industry, with its hostile Ship Owners

Federation and unstable workforce, engaged in dangerous and irregular work, created the opportunity for an 'outsider' like Hopkins. The better-known outsiders in the organization of the unskilled were socialists like the ubiquitous Tom Mann, so it is good to be reminded that they were only part of the labour story. Hopkins and Wilson were political Liberals during the 1911 strike, but no less committed to the men's cause. These strikes—linked to wider transport and other disputes—marked the beginning of a turning point in British industrial relations. Largely successful, they improved wages and safety and saw the first grudging recognition of the Seamen's Union; though full conciliation awaited the end of the First World War. Dangerfield's celebrated account (which Miller surprisingly ignores) saw this strike wave as marking the advent of twentieth-century class politics. Hugh Clegg, more prosaically, saw these as transitional disputes to establish a national system of collective bargaining, and this is largely how Hopkins regarded the matter.

Why does Hopkins matter and what does this life add to our knowledge of this period? I think two aspects stand out. First, labour history of the twentieth century tends to neglect religion or at least to suggest that the only religion that matters is socialism. Indeed, this period is often seen as some sort of global prelude to the Russian revolution and the emergence of the 'modern' marxist labour movement. Thus most historical focus has fallen upon the Syndicalists. In this view, religion is something 'archaic' and belonging to the Victorian era. Yet, as Miller shows, this religious man was at the centre of the seamen's movement, while his distinctive branch of faith continued advancing through his life. Second and related, where labour historians have looked at religion, they have tended to obsess on either nonconformity—most notably Labour Churches or Primitive Methodism—or on immigrant Irish Roman Catholicism, rather than the variegated strands of the Church of England. Hopkins's life demonstrates one of the many ways that Christianity could relate to the working classes: all across the board from rational Unitarianism to the 'smells and bells' of Hopkins's eccentric and largely unofficial Anglo-Catholic brotherhood.

This is a well-researched study, using trade union sources such as Warwick Modern Records Centre. The author is a partisan of high church religion, but does not allow this to cloud his historical assessment of the evidence about Hopkins and his life. The two main religious and labour institutions of Hopkins's life yield most of the evidence, as is often the case with such well-recorded institutions. However, the lack of personal and family record leaves Hopkins, the man, as something of an enigma. There are interesting questions about his faith, politics, and sexuality that simply remain unanswered. He is a high churchman with Christian Socialist friends, but we cannot quite reach the personal, moral, and intellectual faith that binds these elements together. We glimpse enough, however, to know that the 'hard' economic history of trade unions, strikes, and collective bargaining cannot be fully understood without knowing the 'soft' faith and ideals of those who engage in it.

University of Loughborough

PETER ACKERS

Barbara Walsh, *When the shopping was good: Woolworths and the Irish Main Street* (Dublin: Irish Academic Press, 2011. Pp xvii + 286. 74 figs. 9 tabs. ISBN 9870716530534 Hbk. £45/\$69.95)

When Woolworths UK closed its doors in November 2008, it appeared that the opportunity for any in-depth analysis of its pioneering role in British mass retailing had also vanished. Yet its demise has acted as a catalyst for historical appraisal, including Paul Seaton's *Sixpenny romance* (2009), which covers the UK-based Woolworths as a whole, and now Walsh's more substantial history of its operations in Ireland (both north and south of the border).

Woolworths opened their first Irish store at Grafton Street, Dublin, in 1914, five years after the foundation of their British subsidiary. There still were only 10 Woolworth branches on each side of the border in 1940, though their economic and social impact was, perhaps, greater even than in mainland Britain. Woolworths introduced a fixed-priced, leisure-based retail format that was more novel to Ireland than to Britain, encompassing a huge variety of cheap necessities and inexpensive luxuries. This proved enormously popular in Ireland; a trip to Woolworths representing the high point of a journey to town for many rural people. Woolworths also provided valuable job opportunities at relatively high wages for young women, especially in areas where women's paid employment was traditionally restricted mainly to farming or home-based work.

Woolworths had an overtly paternalistic management system, and was popular with its predominantly female shop-floor staff in terms of both wages and conditions. It was also characterized by particularly strong internal labour markets, executives almost universally starting on the shop floor and working their way up. Trainee managers (almost exclusively male until recent decades) faced a rigorous system of assessment and rapidly changing postings, with pay closely linked to results. Yet even at Woolworths meritocracy had its limits; in 1948 the manager of their exceptionally profitable Dublin superstore earned a bonus commission which rivalled that of its senior executives. Board-level consternation ensured that his commission rate was immediately cut!

Management maintained a carefully neutral stance to the various socio-political controversies that emerged in the Republic and Province, while maintaining good relations with local political elites. Yet despite policies favouring Irish-produced goods, avoiding controversy often proved difficult. For example, the growing dominance of the Catholic Church in the political life of the new Republic led to severe censorship of publications considered 'indecent or obscene', which was interpreted sufficiently broadly by the censorship board to cover many of the novels store managers received in bulk. Despite checking deliveries against an ever-growing list of prohibited titles (ironically, even the romantic love story *The girl from Woolworths* was banned in Ireland) some books inevitably slipped through the net and prosecutions followed.

While Woolworths continued to flourish in both Britain and Ireland during the early postwar years, difficulties had emerged by the 1970s. A policy decision during the early 1970s to transform the company from a 'buyer-orientated' to a 'market orientated' retail group translated, in practice, to the use of marketing as a substitute for the good-quality, competitively priced goods which had hitherto been the cornerstone of their competitive success. Woolworths struggled to adapt to new market conditions, while the US parent's own growing financial difficulties led to the sale of its controlling interest in British Woolworths (which also included all their stores in Ireland) in 1981. The new owners—a London consortium of venture capitalists and merchant bankers, Paternoster Stores plc—rapidly removed all but one of its previous directors, who were replaced by people with no previous retail experience.

In 1984 it was decided to close all operations in Ireland, despite good results for several stores on both sides of the border. However, political pressure from the Northern Ireland Office led to a reprieve in the Province, where Woolworths enjoyed popular support in both loyalist and nationalist areas (though this had not spared them from frequent bomb attacks). Stores in the Irish Republic were both less politically sensitive and had higher VAT and wage rates. Their closure marked only one phase of a continuing process of decline, restructuring, and closures, which was to culminate in the demise of the entire Woolworths group 14 years later.

Walsh has produced an excellent history of Woolworths, which is a thoroughly enjoyable read and will be of considerable value to those interested in Woolworths' operations in Ireland and in the wider British Isles. Drawing on both personal testimonies and a wide variety of published and archival sources, this book is a considerable achievement in

providing a comprehensive history of an area of Woolworths' operations which did not enjoy managerial independence from Woolworths UK. Those interested in the social history of retailing will also find a great deal of interest here. My only criticism is that the book lacks a conclusion. This would have been valuable in drawing together the various arguments regarding how a once dynamic and hugely popular retailing group managed to lose its way so badly during its last decades—a period when Woolworths' traditional retail format re-emerged in the shape of various 'pound shop' chains, that, ironically, were to occupy many former Woolworths branches.

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PETER SCOTT

Michael Wadsworth and John Bynner, eds., *A companion to life course studies: the social and historical context of the British birth cohort studies* (London: Routledge, 2011. Pp. xvii + 237. 14 figs. 33 tabs. ISBN 9780415495400 Hbk. £90/\$150)

This book aims to explore social change in Britain since 1945 with regard to the five main life course studies: the 1946 National Survey of Health and Development (NSHD); the 1958 National Child Development Study (NCDS); the 1970 British National Birth Cohort Study; the 1991–2 Avon Longitudinal Study of Parents and Children; and the 2000–1 Millennium Cohort Study. Noting that their historical context has received little attention, the book sets out to describe the fundamental changes that affected life chances in each generation, along with government policy. The hope is that this will provide a resource for interpretation of the findings from the cohort studies, and perhaps stimulate new cross-cohort comparisons. In that sense the book is a significant extension of Wadsworth's *The imprint of time* (1991).

In their introduction, Wadsworth and Bynner (both of whom directed the 1946, 1958, and 1970 studies) introduce the five studies, arguing correctly that they map on to many of the major policy questions and progress of the study of individual development and health in the postwar period. Noting the strengths of the design, they claim that the studies make it possible to answer a unique range of questions in the social and health sciences, along with evaluating the effects of policy programmes. They argue that the period saw unprecedented change, increasing government intervention, and the rise of evidence.

The authors of the succeeding chapters provide competent, sometimes authoritative, surveys of their respective areas. Lindsay Paterson traces changes in politics, citizenship, and social capital, while Lynda Clarke and Ceridwen Roberts survey family structure and family policy and practice, noting for instance the way public policy interest in families grew from the 1990s. Gary McCulloch provides a masterly account of the changing educational context in Britain since 1945, both in terms of individual experiences and the effects of education policy. Hugh Pemberton looks at economic policy and practice, while David Ashton and Bynner trace the labour market, employment, and skills. Wadsworth covers health policy and health care, and Ken Roberts deals with leisure.

Wadsworth does draw upon the 1946, 1958, and 1970 studies in his survey of health policy and health care. However, most of the other contributors know very little about the birth cohort studies, and say little about them. Paterson is forced to concede that questions about social participation, social attitudes, and attitudes towards politics were not asked in the early waves. McCulloch's only point about the birth cohort studies is in terms of how they have contributed to education policy, while Pemberton merely suggests that economic researchers might make more use of them. Ashton and Bynner only consider the birth cohort studies in their conclusion, and much of the chapter by Ken Roberts on leisure is very familiar. In many of these chapters, the cohort studies are only considered at the end, in terms of their potential for future research.

Bynner and Wadsworth's conclusion is more interesting, in trying to summarize the transformation of Britain since 1945, and in identifying cross-cutting themes. They look at the birth cohort studies in terms of trajectories; transitions; turning points; and intergenerational transfers, and the influences on them—their location in historical time and space; linked lives; human agency; and the timing of events and transitions. Bynner and Wadsworth argue that the historical context supplies the basis of cycles of interacting influences on the lives of cohort members. Disentangling these must be one of the next major steps in longitudinal and life course research.

However, it is arguable that it is a rather different set of questions around the birth cohort studies (especially the NSHD and the NCDS) that is of most interest to the historian. Why were the studies (which only subsequently became follow-up surveys) established, and what were the reasons for their changing focus? Which organizations (including the Eugenics Society and the National Children's Bureau) were involved in them, and what was the significance of institutional location? Who took the lead roles (including James Douglas, David Glass, Mia Kellmer Pringle, and Neville Butler)? What were their disciplines, and what tensions were there between them? Why were the research councils (with the notable exception of the Medical Research Council) so hesitant about committing funding, and what ideological entanglements came with funding from government departments? Finally, what can the birth cohort surveys tell us about the broader intellectual history of postwar Britain? In focusing on history simply as the context for the interpretation of survey data, this book does not take us very far in answering these important questions.

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JOHN WELSHMAN

Forrest Capie, *The Bank of England, 1950s to 1979* (Cambridge: Cambridge University Press, 2010). Pp. xxviii + 890. 29 figs. 23 plates. 28 tabs. ISBN 9780521192928 Hbk. £90/\$150)

Shortly after Capie was commissioned to write this history, a former senior Bank official remarked to me that he was 'extremely thankful that we have someone writing our history who knows something about monetary policy'. The history of the Bank's struggle to make sense of monetary policy in the 30 years after the Committee on the Working of the Monetary System (the Radcliffe Committee) published their report in 1959 is a convoluted one and it needs a qualified monetary historian to unpick this story. Capie fits the bill perfectly.

Although Capie begins this history with the Bank's constitution, structure, personnel, and objectives and functions in the 1950s, for this reviewer the text becomes more alive from chapter 3 with a discussion on the Radcliffe Committee. As Capie notes, the Committee 'either ignored much of what there was in monetary theory and analysis before Keynes or explicitly rejected it' (p. 135). This proved to be a great mistake. During the 1960s, 'there was considerable confusion as to what monetary policy was, what it could achieve, and how it might be implemented' (p. 309). Several problems arose in the 1960s including the management of long-term debt and what to do about the excess liquidity in the banking system. During the 1960s, little progress was made in reducing the liquid assets ratio of the banking system because of the expansion of holdings of commercial and other bills and of money at call with the discount market. Throughout, the Bank was reluctant to use interest rates for domestic reasons. It was only after a Bank/Treasury seminar held by the IMF in the late 1960s that progress was made on educating officials in monetary thought. The revival of the quantity theory was a slow process in the Bank, and arguably was never accepted by most officials during the period covered by Capie.

The biggest problem for the Bank in the 1960s came from the external side. The Bank valiantly shored up the foreign exchange reserves during a troubled period for sterling after

1964, caused by the schizophrenic policies of the Wilson government. The ability of the Governor, Rowley Cromer, to secure \$3 billion of credit from other central banks during the sterling crisis in November 1964 within 36 hours is a testament to his skill as a central banker. Equally important is the other international assistance for sterling which the Bank was able to secure throughout the remainder of the 1960s. Capie carefully traces all these operations and discusses sterling's travails in several chapters through to the end of the 1970s.

Monetary policy in the 1970s has yet to receive a thorough examination through archival sources. Capie tells the Bank's side of the story, and it is not a happy one. Despite the presence of Charles Goodhart—arguably the most capable monetary expert the Bank ever employed—Capie shows how a number of senior Bank staff were confused about monetary policy and in particular how to control inflation. Christopher Dow, recruited to the Bank in March 1973, seemed at sea with the money supply numbers. Instead, with inflation running at more than 16 per cent in 1974, 'what dominated the discussion in the Bank and the Treasury . . . was what monetary policy was, how it should be described, how it could be presented, and ultimately, how it could be implemented' (p. 648).

As Capie's book illustrates, in the 1970s, the Bank appeared happier to conduct monetary policy with the mindset that they were still operating in a pegged exchange rate regime, where it was hoped that incomes policies could contain inflation and when fiscal policy was seen as more powerful than monetary policy. With the election of the Conservatives in 1979, Margaret Thatcher was determined to populate the Treasury with believing monetarists and hoped to rid the Bank of believing Keynesians. This proved more difficult to achieve than she had hoped. Capie's remit allows him to touch on some of the debates that came to dominate the realm of monetary policy in the early 1980s, but there is a lot more to be said on monetary base control; the Bank's role (alongside side that of HM Treasury) in the 1980 Green Paper, *Monetary Control*, and the 'which monetary aggregate?' debate which persisted until the mid-1980s.

If Capie's frustrations with how the Bank managed monetary policy after 1959 occasionally show through, he is more circumspect about some of the difficulties he has encountered in trying to write a history without access to all the papers. The destruction of many of the Bank's papers on the secondary banking crisis is troubling. Before 1980, there are no records of the 'Books'—the morning meeting of the Governor, where business matters and tactics were discussed. Unlike the welter of Treasury records which detail at length that institution's internal discussion on sterling's travails after 1967, there have been few, if any, discussions found in the Bank's files when Capie was writing this history.

Despite the focus of this review, there is more to this fascinating volume than a history of domestic and external monetary policy down to 1979. Capie's volume also covers the institutional history of the Bank and the growth of regulation, London as a financial centre, and financial innovation. Monetary historians have been presented with a first-class history. It is highly recommended.

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MICHAEL J. OLIVER

GENERAL

Paolo Malanima, *Pre-modern European economy: one thousand years (10th–19th centuries)* (Leiden and Boston, MA: Brill, 2010. Pp. xvii + 422. 103 figs. ISBN 9789004178229 Hbk. £111.70/€133/\$182)

It is possibly a symptom of impending relative economic decline that the past few years have witnessed a small flood of books recounting the growth of the European (and more broadly western) economy since the early middle ages, as if one had to reassure oneself that it really happened. Although such books exhibit less triumphalism than 30 years ago, the

core of the argument remains the same: between the late seventeenth and early nineteenth century western economies succeeded in breaking out of the 'Malthusian' trap through a series of innovations that relieved the energy constraint on agricultural and industrial production. Prior to that epochal transformation of the production function, the 'spirit of enterprise' was gradually unleashed by the weakening of legal and other institutional constraints on capital and labour mobility and property rights in land, so that by the time the critical innovations came on line, the institutional forms to exploit them were in place. The innovations, and in certain measure the more flexible market institutions, reflected a more 'rational' and empirical attitude towards natural and social phenomena that emerged in the early decades of the seventeenth century. These are the stylized facts. Except for minor details, they have served as stylized facts of western economic history for more than a century.

Malanima's survey is an honourable member of this large class of works, which rationalize the comparatively recent (and almost certainly temporary) ascendance of the west in the world economy as a consequence of more perfect markets and more innovation. The analytical structure is based on the propositions of classical economics that privilege the role of diminishing returns to labour on the one hand, and exogenous shifts in technology and institutions on the other, as explanations of pre-modern economic change. Despite the logical attractiveness of the assumption of diminishing returns, its applicability to an economy-wide level of aggregation has not been demonstrated empirically. As is well known, price and wage evidence commonly adduced to prove the proposition turns out on close econometric inspection to be inconclusive. In much the same way, the claim that the 'freeing up' of markets in the late medieval and early modern period accelerated economic growth is entirely plausible, and is supported not only by authority but also by repetition; but its quantitative significance has not been established empirically and at the level of a national economy it is doubtful it ever will be. We do not possess the relevant test statistics (or even adequate theoretical concepts) for assessing the dimensions of market 'perfection' relevant to economic growth. The ante-bellum slave market was as perfect a labour market as one can imagine but no one argues seriously that it promoted long-run economic growth in the regions where slavery was dominant.

These caveats aside, Malanima has produced a good textbook for undergraduate and graduate courses in European economic history. The book's architecture is classical and sound. The quantitative estimates bearing on population growth and energy supplies are novel and gratifying. The bibliography is first-rate; in addition to the standard works in English and French, it contains many works in German and Italian that deserve reading. It is long past the time when economic historians could rely on English (or French) alone for insights into the nature of European economic history. Some sections nevertheless rest on a badly dated literature. The discussion of the medieval manor as an implicit contract between lords and peasants is in particular need of revision, and the material on agricultural productivity in my opinion underestimates the elasticity of agricultural supply before the nineteenth century. Perhaps the most disappointing feature of this otherwise fine work is the omission of any discussion of income distribution and class structure, which within living memory constituted the core of courses in economic and social history. The economists have pretty much written these still vital topics out of the curriculum.

The flaws in this otherwise admirable production thus lie not so much in execution, but in the genre. With notable exceptions, economic history written by economists has not lived up to the expectations of a half-century ago. Over time, it has come to contain less history and more economics. If that rebalancing of the intellectual portfolio had earned higher rates of return in insight, there would be much reason to rejoice. Alas, as so often happens, the promise of a new dawn has not materialized. Certain indefensible claims by the older school of economic history have been effectively removed from the discourse, and particular corners have been tidied up by quantitative research; but on the whole the

overall narrative of European economic development since the early middle ages is much as it was in the middle of the last century, minus the social and political content that gave it meaning. In the end, such books are not so much histories as they are well-worn interpretations informed by conventional growth theory.

McGill University

GEORGE GRANTHAM

May Lindemann, *Medicine and society in early modern Europe* (Cambridge: Cambridge University Press, 2nd edn, 2010. Pp. xii + 300. 17 figs. 7 tabs. ISBN 9780521425926 Hbk. £50/\$85; ISBN 9780521732567 Pbk. £18.99/\$32.99)

Can the second edition of a book, published 11 years after the first, still deliver on its claim to provide 'new approaches to European history'? In the case of the book under review, the answer is a resounding yes. It is no mere revision, of the kind we so often see, consisting of the addition of a handful of recent bibliographical references or an epilogue, which leaves the old text untouched. While the basic structure of Lindemann's 'concise introduction of health and healing in Europe from 1500 to 1800' (to quote from the back cover) remains essentially the same, as do its aims—to examine medicine 'from a social and cultural perspective, rather than a narrowly scientific one'—just about everything else has been judiciously reconsidered, rewritten, or brought up to date by the author. The social history of medicine has moved on, and so has this survey.

Fortunately, this has not meant jettisoning some of the more useful elements of the first edition. What struck me so favourably back then, when I reviewed it for the *Review*, were the historiographical discussions Lindemann placed at the beginning of each chapter; the complementary considerations of modern and period understandings of disease causation and cure; and the good balance between English and Continental examples. These features remain true of this new edition; indeed, the historiographical surveys have been expanded to account for more recent developments. If the book remains resolutely social and cultural in its historical orientation—which I personally regard as a strength but which will no doubt disappoint a few post-modernists—other limitations have been rectified. Thus the present edition casts a much broader net in its discussion of sources of cure, with a more extensive analysis of both magical healing and religious elements; there is an even greater attention to patients and to non-canonical practitioners than in the first edition; the chapter dedicated to hospitals has been substantially revised; and more attention is paid to southern Europe and to the sixteenth century than was the case in the first edition. Lindemann's book remains a most welcome survey and teaching tool. Its clarity of presentation and readability mean that, despite being some 50 pages longer than the first edition, it is eminently suited to student use.

University of Leicester

DAVID GENTILCORE

Jon Stobart and Ilja van Damme, eds., *Modernity and the second-hand trade: European consumption cultures and practices, 1700–1900* (Basingstoke: Palgrave Macmillan, 2010. Pp. xii + 281. 10 figs. 14 tabs. ISBN 9780230229464 Hbk. £55/\$85)

The second-hand trade was not simply a transient feature of imperfect early modern markets, nor was it a marginal facet of retailing, claim editors Stobart and van Damme. They make a compelling case for the complexity and variability of second-hand markets, the range of used goods disbursed, and the customers served. Further, they challenge the assumption that modern production and retailing spelled the demise of this sector: on the

contrary. This volume adds to a growing literature on the characteristics of a trade that persisted through the early modern and into the post-modern era. The hunt for second-hand goods, from furniture to clothing, remains a perennial source of entertainment plus an essential resource for customers of charity shops, yard sales, and eBay in the developed world; while in the developing world the second-hand trade displays equal complexity in retail venues, commodities sold, and the social ends achieved through the purchase of used goods. The dozen chapters in this volume offer an engaging analysis of second-hand practices spanning early modern England, through the Low Countries and Scandinavia, and ranging beyond Europe to the eighteenth-century Cape Colony and nineteenth-century India. Topics are equally diverse, from old clothes, to fine antiques, rare books, and church 'jumble'. The editors' introduction offers a cogent critique of the teleologies of retail history, wherein the rising efficiencies of production and distribution quash second-hand markets. As they note: 'Modernity was fragmented and contingent, and so too was the market for second-hand goods' (p. 5).

Valuable correctives are provided in this volume, modifying generalities in the history of consumerism, a field shaped by research on north-west Europe in general and England in particular. There is a danger in generalizing from English-based findings; the range of case studies, from a host of geographic locales, provides important evidence of the variability of markets and customers. The Scandinavian experience, presented in two chapters, illustrates the different legal and material practices of that region. In one instance, Martin Wottle notes that the concept of 'new' commodities in early nineteenth-century Stockholm was not at odds with the purchase of second-hand goods. As he observes, newness represented qualities along a continuum. Sofia Murhem, Göran Ulväng, and Kristina Lilja assess auctions in Sweden over the eighteenth and nineteenth centuries, comparing urban and rural practices, including the social and gender patterns of purchase, the variable meanings of these goods, and the participants in these events, as furnishings circulated horizontally and vertically through society. This volume captures the persistent, variable, and evolving patterns of this sector, whether in the newly developing 'antiquarian culture' and its associated antiques trade identified by van Damme, or in the sale of old books, or Georgian country house artefacts. Each facet of the second-hand trade reveals the composite of financial and aspirational motivations behind the sale and purchase of second-hand goods, within evolving material environments.

Important perspectives are offered by Tracey Randle on the eighteenth-century Dutch Cape Colony and Robin Jones on nineteenth-century British India. Both authors introduce issues of race, gender, and material culture, set within the powerful administrative structures imposed by colonial authorities. Randle dissects the circulation of goods through Cape auctions, charting the varying activities of men and women as they negotiated the acquisition of goods, sometimes skirting the regulations of the Dutch East India Company and acquiring desired commodities through informal means. Most intriguing was the example of Anna de Koning, the daughter of a freed slave, whose rising wealth over a lifetime is revealed in the final auction of her moveable property. Those attending this sale manoeuvred to secure items essential to their presentation within this community, markers of their social mobility. Men and women grappled with the question of how best to materialize goods suited to occupation, locale, and ambition. Jones points to the vital importance of second-hand furnishings for British administrators in nineteenth-century India. Jones identifies the unique ways in which 'the notion of prestige, status and tastes as expressed through one's furnishings was problematized in British India' (p. 120). In contrast to middle-class Victorians in Britain, those resident in India crafted a particular relationship with second-hand markets distinct to this imperial project. Divestment of furniture was a recurring ritual of middle-ranked and elite British residents occasioned by transfers, return to Britain, or the all too frequent deaths that marked Britons' experience of the sub-continent. Vivienne Richmond's study of the common jumble sale closes this

volume. These ubiquitous sales signalled changing ecclesiastical demographics and problematic financial demands that remain a feature of the established church to this day. The merits of this volume are many, not least as a welcome addition to the history of retailing and consumption.

University of Alberta

BEVERLY LEMIRE

Richard E. Spear and Philip Sohm, eds., *Painting for profit: the economic lives of seventeenth-century Italian painters* (New Haven and London: Yale University Press, 2010). Pp. xii + 384. 34 tabs. ISBN 9780300154566 Hbk. £45/\$85)

In *Painting for profit*, Spear and Sohm have gathered essays by a team of seven distinguished scholars who address the role of money in the personal and professional lives of seventeenth-century Italian painters. With consistently vivid writing (including Sohm's stylish translations of the Italian contributors' essays), this volume reconstructs and analyses long-forgotten protocols, practices, and conditions that belonged to the financial world these artists knew. The product of a genuine intellectual dialogue nurtured at various points by the hot-house environments of the Sterling and Francine Clark Institute and the Getty Research Institute, this beautifully produced volume makes accessible the latest thinking and much new data on one of the most intriguing research frontiers in seventeenth-century Italian art.

The structure of the collection addresses effectively the historical elephant in the room—that is, the inconvenient circumstance that 'Italy' did not exist as a unified political-economic entity in the seventeenth century. Instead of presenting just one economic history of 'Italian' painting, the main body of the volume consists of essays individually examining the major, politically independent urban artistic centres of the Italian peninsula: Spear deals with Rome; Christopher Marshall with Naples; Raffaella Morselli with Bologna; Elena Fumagalli with Florence; and Sohm with Venice. Despite the unique character of each, these major centres of artistic activity nonetheless have much to compare, and the editors do not miss the opportunity to consider valid generalities and draw broader conclusions. In his introduction, Sohm maps the project's broad themes including the 'ethics and psychology of money' (p. 2), market practices, and modes of pricing and payment in a pan-Italian perspective; and near the end of the book, social historian Renata Ago compares data gathered from all five of the city centres covered in the volume, noting continuities, similarities, and differences in local economic conditions, costs of living, social stratification, the economic functions of professional associations (such as guilds), the roles of the *pater familias* and of patrimony in the socio-economic structure, the urban phenomenon of luxury consumption, and the motivating forces of prestige and social climbing. Finally, Richard Goldthwaite places these conclusions in an even broader perspective, drawing upon his vast knowledge of the economic history of fifteenth- and sixteenth-century art to bring the major trends into vivid relief.

It would be difficult to label the genre of history to which this collaborative effort belongs. While the authors place a new emphasis on the analysis of quantitative data, the results are often embedded in socio-historical narratives dealing with the artists' personal dreams and aspirations, their struggles for status, prestige, or in some cases mere survival, the predicament of women painters (Morselli's essay on Bologna makes major contributions here), financial arrangements and quarrels with patrons, and anecdotal accounts of business practices. The project does not represent a complete break with Francis Haskell's archetypal social history of art, *Patrons and painters* (1963), but rather a significant development of it, placing emphasis on the artist's side of the artist-patron relationship, and also encompassing the entire scope of the art market including the production of works on

speculation and the re-sale of what one might call ‘used’ art. For this reader, the appeal of the volume lies in its acknowledgement of the human dimension of money matters, especially in the stories of those forgotten ‘minor’ painters whose documentary traces afford a glimpse of the often harsh financial realities of the seventeenth-century artist’s life. In sum, *Painting for profit* seems to propose a mixed methodology combining elements of social and economic art history with something resembling the Ginzburgian *microstoria*. For his part, Goldthwaite sums up the importance of the volume by suggesting that the study of art markets and market values may provide an important measure of *cultural values*—a measure lacking in Haskell’s purely socio-historical methodology.

The alluringly alliterative title of this volume recalls that of a competing collaborative project entitled *The patron’s payoff: conspicuous commissions in Italian Renaissance art* (2009), edited by art historian Jonathan Nelson and economic historian Richard J. Zeckhauser (reviewed in this journal, 62 (2009), pp. 756–8). The editors of the latter volume, systematically employing principles of information economics and working through the implications of Zeckhauser’s principles of ‘signaling’, ‘signposting’, and ‘stretching’, manage to achieve a rather different but equally coherent socio-economic analysis of art with an emphasis on the role of patronage. Together the two volumes might announce a new wave of interest in the economic history of early modern art. It remains to be seen whether the next generation of scholars will take up the challenges of this field of inquiry.

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ANTHONY COLANTUONO

Yolanda Blasco and Carles Sudrià, *El banco de Barcelona (1844–1874), historia de un banco emisor* (Madrid: LID Editorial Empresarial, 2010. Pp. 419. 58 figs. 19 illus. 67 tabs. ISBN 9788483561102 Pbk. €24)

Joseph Schumpeter, Alexander Gerschenkron, and Rondo Cameron all argued that to understand economic growth and industrialization we must understand banking. Economic historians have contributed decisively to an understanding of the relationship between financial development and economic growth. Although this is not their explicit objective, in writing a history of the Banco de Barcelona (BB) Blasco and Sudrià provide an assessment of its role in the Catalan economy of the mid-nineteenth century.

The BB, one of the most prominent and venerable Spanish financial institutions, was founded in 1844 and, after surviving all kind of circumstances, failed in 1920. Thanks to the banker and historian Francesco Cabana we know a good deal of the BB from its inception to its liquidation. However, the discovery by Blasco of the archive of the bank, now in the Arxiu Nacional de Catalunya, has opened a promising window with which to re-examine its history. The research relies heavily on board minutes, memoirs, correspondence, and accounts. This rich store of material underpins this deeply researched study, which is richly detailed and insightful, although it focuses only on the 30-year period during which the bank was a note issue institution (in 1874 the Bank of Spain was granted the monopoly of issue for the entire country). The first part covers the initial decade, and the second the remaining 20 years through to 1874.

The founders of the BB were rich merchants from the Barcelona bourgeoisie. The bank began with a large nominal capital of 20 million reales (the Spanish currency before the peseta was introduced in 1868). The bank was initially successful, but soon faced serious difficulties that checked its promising start. There was first the crisis of 1848, but thereafter the various attempts by the Ministry of Finance to deprive it of its privilege of issue. The BB weathered the storms but its subsequent evolution was less than its ambitious founders had expected. However, the average return on equity of these years was relatively high; the share prices were always listed over the par and shareholders received handsome dividends.

The Catalan economy also benefited from an institution that offered new financial services and contributed towards diversifying the money supply of the thriving city of Barcelona.

In 1856 a legislative change liberalized the establishment of new banks. Sixty credit companies and issue institutions were founded, of which a dozen opened their doors in Catalonia. Competition became fierce and the BB resented the reversal of the regulatory environment. Although the BB's total assets and banknotes in circulation increased, it lost its regional pre-eminence. Paradoxically, it was the 1866 financial crisis, well examined by the authors, that gave the BB the opportunity to regain its financial hegemony in Catalonia. The bank, however, did not survive the crisis unscathed. In the following years its performance was disappointing; its initial impetus vanished; its role as the major issuer of the region waned; and the management of the bank became excessively conservative.

Blasco and Sudrià close the book with a brief epilogue that focuses on the reaction of the BB to the royal decree that granted the monopoly of issue to the Banco de España. To the directors and shareholders of the bank the government's decision was unacceptable. They put up strong opposition, but it was to no avail. As with the rest of the provincial issue banks, the BB was condemned either to merge with the Banco de España or to change its by-law and be transformed into a simple, non-issue financial institution. The BB chose the latter. Cabana argued that to continue as a commercial bank was the best decision, while other writers have maintained that shareholders would have fared better joining the mighty Banco de España. Rather disappointingly, Blasco and Sudrià prefer not to take sides in this debate.

Throughout the authors use the *duro* as their chosen monetary unit, equivalent to the historically famous *peso fuerte*. It is a strange choice, since the Spanish money unit of account was the *reale* until 1868 and thereafter the *peseta* until the adoption of the euro. The authors' choice, which makes comparisons difficult, should have been explained. The two chapters surveying financial performance are informative, but the same data are then repeated in the chronological chapters. Moreover, the BB is studied in isolation from the rest of the Spanish financial system. It would have been of particular interest to offer some comparisons with other banks of issues. Had the authors done so they could have told us that by 1874 the BB was the second largest financial institution of the country, surpassed only by the Banco de España. The BB was much larger than either the Banco de Santander or the Banco de Bilbao, the two current Spanish financial giants. Although some conclusions are given at the end of each chapter, the volume lacks an overall assessment of the institution.

Notwithstanding these minor criticisms, Blasco and Sudrià have written a fascinating history of the Banco de Barcelona and in the process made an important contribution to our knowledge of the Catalan financial system; in short, a net addition to the Spanish economic literature. Hopefully, the authors will continue their research to provide a full account of the remaining 50 years of the Banco de Barcelona.

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PABLO MARTIN-ACEÑA

Haim Gerber, *State and society in the Ottoman Empire* (Farnham: Ashgate, 2010. Pp. xvi + 296. ISBN 9780754669852 Hbk. £70/\$139.95)

This volume brings together 15 studies (referenced hereafter by their chapter number in upper case Roman numerals) by a pioneering historian of Ottoman economy, society, and law written over a period of 30 years from 1978 to 2007. Eight articles focus on early modern Ottoman history, examining issues related to endowments, commercial partnerships, women, family structure, Muslim/non-Muslim relations, civil society, and law; four discuss changes in nineteenth-century Palestine and Syria in conjunction with the impact

of Ottoman reforms; and the last three examine the historical origins of modern territorial and/or ethnic national sensibilities in the Islamic Middle East.

Gerber's style of choosing a research topic and building an argument provides the volume with some consistency. He identifies certain generalizations in western scholarship and popular perceptions of Islam, the Middle East, and the Ottoman Empire. He then scrutinizes these generalizations in light of his (and other recent) empirical research based on primary sources of Ottoman history (particularly court records and manuscripts). For instance, his article on endowments in fifteenth-century Edirne (essay I) engages with the view that the Ottoman-Muslim *waqf* (endowment) was 'a typically pernicious institution designed mainly to allow the usurpation of state resources (by political grandees)' (p. x; cf. VIII, p. 75). He shows that the *waqfs* rather served the public benefit in Edirne and that recent works on *waqfs* in other cities reach a similar conclusion.

His articles on business partnerships in seventeenth-century Bursa (II) and on the views of the Ottoman-era jurists (exemplified by Khayr al-Din al-Ramli (d. 1671) in particular) (VIII) challenge the claim that classical Islamic law had 'ossified' and ceased to have practical relevance by the thirteenth or fourteenth century (p. xi; cf. II, p. 109; VIII, pp. 166–7). His Ottoman monetary system paper (III) is critical of the opinion that the Ottomans had 'no idea' (p. x; cf. III, p. 319) how to tackle the persistent problems of coinage; rather, he suggests their monetary policies (including currency debasement) were quite typical of the early modern period and that we should avoid conclusions that are not based on systematic, in-depth, and comparative analyses of such policies and their implications.

Gerber's article on the social and economic place of women in seventeenth-century Bursa (IV) offers a picture that is the 'complete opposite' (p. x) of the dominant belief that Ottoman-Muslim women had 'an extremely depressed' position in society and 'were barred from participating in public life' (IV, p. 231). In another article (V), Gerber observes that the Ottoman urban family in Anatolia was 'basically nuclear' (p. xi) and did not conform to the extended and patriarchal types as it is supposed. His article on Muslim and non-Muslim relations in Ottoman urban centres in the seventeenth and eighteenth centuries (VI) observes that these relations were 'much more symbiotic and friendly than usually assumed' (p. xi), especially in the economic sphere. Similarly, in an article on civil society in the Ottoman Empire (VII), Gerber challenges the view that the Ottoman regime 'was the worst despotism the world has ever known' and shows that certain autonomous civic institutions such as guilds and endowments, as well as certain legal practices current among the population, set limits to 'the mighty-looking Ottoman state' (p. xi; cf. VIII, p. 80).

His articles on Ottoman Palestine and Syria in the era of reforms (IX–XII) contest the argument that the reforms 'remained a dead letter' with the Ottoman government 'as rapacious as ever' (p. xii). In the process they deploy useful information on population, administrative changes, taxation, and economic development. His articles on the origins of modern nationalism in the Middle East (XIII–XV) challenge the view that modern national identities are, and had to be, modern constructs because Islam allows no other meaningful basis of solidarity than religion. In the process, they provide interesting information about pre-modern identifications with specific lands—such as *Filistin* (now Palestine), *Misr* (Egypt), *Sham* (Syria), and *Rum* (Turkey)—that have become objects of patriotic sentiments in the modern era. He also points to the impact of the existence of a literary tradition and/or imperial language on the formation of modern 'ethnic'-linguistic identities.

Most of the suppositions or ideas that Gerber challenges appear implausibly simplistic. Gerber's (perhaps heuristic but inelegant) choice of emphatic words in describing the positions he criticizes is partly responsible for this impression. However, the predominantly linear, Eurocentric, and neatly dualistic analytical frameworks and/or anachronistic and

narrow nationalist perceptions of history that inform the same positions are the main problem. The broadening horizons of the discipline of history expose the inadequacies of these once dominant paradigms in explaining complex historical phenomena. Thanks to this development, along with the scholarship of critically minded as well as empirically grounded historians such as Gerber, the historiography of the Middle East in general, and the Ottoman Empire in particular, has reached a level of refinement that renders quarrelling with spent positions superfluous. It is time to become engaged in building constructive arguments.

Brown University

ENGIN DENİZ AKARLI

V. Necla Geyikdağı, *Foreign investment in the Ottoman Empire: international trade and relations 1854–1914* (London and New York: I. B. Tauris Publishers, 2011. Pp. xiv + 218. 2 maps. 7 plates. 10 tabs. ISBN 9781848854611 Hbk. £54.50/\$92)

At a time when Turkey is trying to come to terms with the 1980 military coup, which was highly instrumental in providing the groundwork for the liberalization of the Turkish economy in line with the Washington consensus, Geyikdağı focuses on an earlier period of a 'great transformation' for the Turkish economy. Geyikdağı often draws parallels between these two periods and illustrates how, in both cases, the transformation processes have been shaped by the priorities of foreign powers.

The first chapter lays out the building blocks of the traditional political and economic order in the Ottoman Empire, one which remained fairly intact until the late sixteenth century. Developments in arms technology then created the need for a modern central army. This not only brought about the dissolution of the decentralized military structure, but also necessitated taxation collected at the centre in order to finance the new army. Accordingly, the Ottoman government introduced a series of reforms, including tax-farm auctioning and subcontracting. As the financial problems of the state mounted, the government responded by increasing the length of tax-farming contracts and introduced new methods of tax collection. However, none of these attempts provided a long-term solution to Ottoman budget deficits, with governments frequently resorting to currency debasement. These fiscal-political devices contributed to the difficulties of the Porte by restricting its ability to pursue a protectionist development strategy, thereby depriving the state of its control over customs duties, a considerable source of revenue for its European counterparts. Moreover, the tax exemptions granted to foreign citizens and protégés of the European powers, who dominated the most profitable economic activities in the period, imposed serious restrictions on the development of state revenue. As the government could not respond to the increasing budget deficits through institutional reforms, more frequent debasements, or domestic loans, it turned to foreign capital markets. In this chapter, Geyikdağı also emphasizes that the economic principles prevailing in the Ottoman state were in sharp contrast with the mercantilism widely adopted in Europe. At a time when Europeans adopted protective measures on imports and aimed to stimulate exports, Ottomans held on to a policy regime that granted financial privileges to foreigners in order to enhance imports while prohibiting the export of grains and other necessities from time to time, this based on the traditional concern of the Porte to provide food and other necessities to prevent scarcity and political unrest in the capital and other large cities.

In the second chapter, Geyikdağı analyses Ottoman foreign borrowing between 1854 and 1914. During the Crimean War there was a desperate need for funds to cover military expenditures and the Porte resorted to foreign loans for the first time, raising 14 in the next 20 years. Geyikdağı points out that alongside economic factors (high interest rates), political motives (furthering the lender countries' influence on the Empire) played a

significant role in the massive flow of European funds into the Empire. Nevertheless, unproductive use of these external resources (such as the import of military equipment and luxurious consumption of the state bureaucracy) resulted in a growing debt burden. In 1875, when the Empire became bankrupt, more than half of its revenue was committed to debt service. In 1881, under foreign pressure and while seeking to regain access to foreign capital, the Porte compromised its financial autonomy and ratified the Decree of *Muharrem*, which led to the establishment of the Ottoman Public Debt Administration (OPDA). By the decree, the Porte transferred its most liquid revenue sources to the control of the OPDA, an administration mainly controlled by the representatives of European lenders. As the Ottoman government continued to borrow from abroad, the control of the administration over the resources of Ottoman economy extended far beyond the assignments of this decree.

The third chapter analyses the historical background and country composition of foreign direct investment (FDI) in the Ottoman Empire. FDI mainly consisted of infrastructural investments to enhance foreign trade capacity, accelerating significantly after the establishment of the OPDA as this institution reduced economic and political risks for foreign investors. As in the case of foreign loans, Geyikdağı notes that political motives played a significant role in investment decisions; in particular there was a fierce rivalry among major European powers in the case of some railway projects. Chapter 4 lays out the sectoral breakdown of this FDI in great detail. Railway projects dominated, being 33 per cent of total FDI. Since the time of Sultan Abdülmeçid (1839–61), Ottoman statesmen had shown great interest in the construction of railways that would facilitate internal trade as well as increasing the political power of the central authority. However, the Empire lacked domestic capital and expertise, resulting in projects being handed over to foreigners who were granted the monopoly of operating the lines for the concession period in return for construction of the railway. Moreover, the government also made financial guarantees in case revenue fell short of estimates, resulting in railways being a very secure as well as profitable FDI. Once again, the OPDA played a major role as the guarantor of these concessions. European capitalists who sought profits amidst the disorder that prevailed in the country often asked for the protection or the cooperation of the OPDA. The involvement of the administration in railway projects secured their investments to a large degree, minimized their risks, and enabled them to enjoy handsome profits.

Geyikdağı's work is a welcome contribution to the literature on the late Ottoman economy. It is a valuable source, not only for the students of Ottoman economic history, but also for the general reader who aims to have a better grasp of the problems of the modern Turkish economy from an historical perspective. The book surveys a substantial literature on the topic alongside newspapers and journals of the period. The language of the book is easily accessible to a large audience lacking an economics background or prior knowledge of the Ottoman economy.

Istanbul University

MURAT BIRDAL

Bettina Gramlich-Oka and Gregory J. Smits, eds., *Economic thought in early modern Japan* (Leiden: Brill, 2010. Pp. xxii + 298. 4 tabs. 3 maps. ISBN 9789004183834 Hbk. £88/€108/\$154)

This is the first volume of a series 'Monies, markets and finance in East Asia, 1600–1900'. Based on workshops in America and Germany, 10 contributors, mainly from the US and Japan, have produced this informative, quality volume on the emergence of political economy—*keizai*—in early modern Japan. Since the translation of a number of important works, more sources have gradually become available in English on the market economy of

the Tokugawa period which led to the comparable, yet distinctive, industrialization path of the Japanese economy (for example, in 2004 Akira Hayami, Osamu Saito, and Ronald P. Toby, eds., *Emergence of economic society in Japan, 1600–1859*). However, relatively little has been published in English on Japanese economic thought, with a few notable exceptions such as Tetsuo Najita's influential *Visions of virtue in Tokugawa Japan: the Kaitokudō Merchant Academy of Osaka* (1987). The present volume provides us with a new research framework to analyse the growth of the market economy as understood by the early modern Japanese.

After the informative introduction by Mark Metzler and Smits, the following chapters are ordered chronologically from the medieval period through to the beginning of the Meiji era. Despite the wide range of subjects discussed in each chapter, there are two major themes across the contributions. First, the volume presents a new approach to the emergence of the mercantilist practice described as *kokueki*, that is, 'benefiting the nation' or 'national interest' in the eighteenth and early nineteenth centuries (p. 5). This builds on Luke Roberts's *Mercantilism in a Japanese domain: the merchant origins of economic nationalism in 18th-century Tosa* (1998) which opened up the debate on the ambiguous concepts of 'nation' in the Tokugawa period, a concept ranging from large domains to Japan as a whole. Chapters in this volume illustrate the eighteenth-century concept of *kokueki* as the well-being of society as a whole rather than that of a dominant lord. Ochiai Kō's chapter on import substitution of sugar links the interest of domain, shogunate, and people. Mark Ravina's essay on Confucian trust banking illustrates the shifts in the benefit of the domain revenue and the benefit of society. Jan Sykora's chapter provides a new case study of Saga domain. A methodologically notable contribution is made by Gramlich-Oka, who traces the formation of the *kokueki* discourse in economic policies in the shogunal court circle. Reformulating the concept of the public sphere, she reveals convincingly the process by which knowledge and ideas held by urban commoners developed into mercantilist policy. The emergence of the *kokueki* ideology, or more generally political economy, as a distinct intellectual field reflected the eighteenth-century economic reality. As Hiroshi Kawaguchi argues, eighteenth-century Japanese people, the governing classes and commoners alike, became more fully respectful of the autonomy of the marketplace. Such a socio-economic context for the market economy discourse in early modern Japan provides a useful case for a comparative study with other countries such as early modern China and Europe.

Another major theme also features a transnational perspective. The anachronistic view of 'isolated' Tokugawa Japan has been old-fashioned for decades; in fact, a growing number of works have revealed that east Asian foreign relations centred especially on China played an important role in commercial, political, and intellectual shifts in Japan throughout the early modern era. Ethan Segal's essay in this volume traces the monetarization of the Tokugawa era back to the wide circulation of imported Chinese coins in the medieval period. However, China was the source not only of money and goods, but also of the ideas and vocabulary of *keizai* thought. A fascinating essay by Ravina illustrates the adaptation of the proposals for poor relief granaries in Song China into financial institutions in Tokugawa Japan. In Smits's contribution, a Ryukyuan intellectual official utilizes Confucian terminology to analyse market organization. These essays show how the Tokugawa thinkers adopted the neo-Confucian terminology and adapted it to describe new economic and social realities at that time. In fact, even beyond the Tokugawa period, Sumiyo Ishii's concise yet important essay tells us that the neo-Confucian education and epistemology of early Meiji provincial businessmen not only did not clash with the Meiji modernizing ethos, but also helped them to pursue the promotion of modernization in traditional industrial sectors.

For such a colourful book, the lack of a concluding chapter may seem a strange strategy, but the final chapter by Metzler locates the eighteenth and early nineteenth centuries in the

long-term polarities of economic policy regimes, upon which this book focuses. This book is recommendable highly to those who are interested in the long-term development of the Japanese economy and society. To that end there is a useful glossary of Japanese names and terms.

London School of Economics & Political Science

MINA ISHIZU

Koichi Hamada, Keijiro Otsuka, Gustav Ranis, and Ken Togo, eds., *Miraculous growth and stagnation in post-war Japan* (London and New York: Routledge, 2011. Pp. xxi + 217. 53 figs. 14 tabs. ISBN 9780415615181 Hbk. £95/\$152)

What is the value of history in setting national priorities for the future? This book employs economic history to fashion a template for pulling Japan out of its two-decade-long economic and political malaise. The assumption underpinning the analysis is that those very factors that worked so well in fuelling catch-up during the high-speed 'miracle growth' era (1955–75) have been dragging Japan down since the 1990s. The authors offer the following recipe for kick-starting the Japanese economy: do not jettison the political and economic skills that served well in the past, but do reorient them. Thus government played an important role in overcoming market failures during the miracle growth era through a combination of industrial policy and infrastructure creation, and it can do so once more; for example, by improving tertiary education, rendering it capable of generating major breakthroughs in the frontiers of science and technology. Hamada et al. make the case by documenting what worked in the past and why; and comparing systematically past with present performance.

On the first theme, a group of chapters derives fundamental principles from a select group of industry studies and in one case from the analysis of Japan's foreign policy. We are only able to highlight a couple of chapters. First, Katsuhito Iwai makes a case that the strength of the Japanese corporation lies in the fact that it is not merely a contractual association of individuals without any real personality, but rather is a real/legal entity shored up by crossholding of stock within *keiretsu*-style groups, a form that he believes can better manage the transition to a post-industrial world in which developing human capital takes precedent over squeezing output out of manufacturing plants. Second, Kōnosuke Odaka, relying on oral histories secured from seven prominent top-level managers, documents the thinking behind the promotion of quality control (QC) circles and just-in-time production especially in the automotive industry, and in particular in Toyota Motors. Out of this analysis Odaka plucks the idea of a matrix-organizational form in which horizontal divisions (for example, quality assurance, schedule management) interact with vertical divisions (such as car-body assembly, painting, and power train construction).

A second group of chapters takes up explicitly the dynamics of the post-1990 era, linking the analysis of the contemporary period to history. These make the point that what worked in the past can be reworked to fuel a brighter future. Again only two can be highlighted here. Thus Otsuka and Tetsushi Sonobe provide an intriguing, Schumpeterian-inspired discussion of cluster-based industrial development which leads to the policy conclusion that such clusters need to be created in cutting-edge scientific fields, while Yujiro Hayami and Yoshihisa Godo show that Japan and Korea closed the gap between themselves and the US in primary and secondary but not tertiary education (by contrast Taiwan closed the gap in all three educational arenas); this spawned generations of homogeneous middle-educated workers who could perform teamwork tasks efficiently in factories, but were not good at developing frontier enterprises in scientifically oriented fields. In short, tertiary education must be expanded and improved, and

the base for more creative thinking in schooling must be firmly established at the primary and secondary level (presumably by abandoning tests that emphasize memorization).

From this reader's perspective, this book has much to offer, particularly the chapters pitting the miracle growth and stagnation eras against each other. That said, the book ignores some key issues: rapid population aging and low rates of immigration; the impact of outsourcing of Japanese corporate production to China, potentially fomenting income inequality and deflation in Japan; the similarity between the stagnation of higher education in Japan and that of many European countries; the changing trade pattern of Japan, Taiwan, and Hong Kong, with intra-regional trade growing at the expense of trade with North America and Europe; the negative side of industrial policy in shoring up declining sectors; excessive Tokyo-centrism (making the Japanese economy highly vulnerable to earthquakes in the Tokyo region); and the high debt to GDP ratio in Japan. In short, while the book makes a valuable contribution it is too short, too selective, to address comprehensively the myriad of problems facing a stagnating Japan.

University of Victoria

CARL MOSK

Aldo Musacchio, *Experiments in financial democracy: corporate governance and financial development in Brazil, 1882–1950* (Cambridge: Cambridge University Press, 2009). Pp. xxv + 298. 17 figs. 47 tabs. ISBN 9780521518895 Hbk. £55/\$85)

Experiments in financial democracy is a stimulating and well-documented account of the evolution of Brazilian financial institutions and markets from the 1880s to the mid-twentieth century. The book challenges the view that legal origins or colonial institutions determined a particular path of financial development in former colonies, and that common law countries are intrinsically better equipped at promoting financial development than civil law countries. Relying on an impressive array of archival sources, including business records, government regulations, and judicial cases, the author shows that Brazil, a civil law country, had relatively well-defined financial institutions in the nineteenth and early twentieth century, only to suffer a reversal after the Great Depression. Up to the 1920s Brazilian laws mostly protected creditors' rights, including the right of secured creditors to recover collateral in case of default, and bondholders' status as privileged creditors. Disclosure laws, government-guaranteed dividends, and, most importantly, corporate by-laws gave investors the confidence needed and a voice in their company's fate, which, as the author shows, prompted a level of investment in bonds and stock markets unparalleled in Brazilian history. The author demonstrates that, given the minor role banks played as long-term creditors at the time, this financial awakening was critical for Brazilian industrialization. This institutional environment also contributed greatly to moderate levels of ownership concentration and control.

The reversal occurred in the aftermath of the Great Depression when scarcity of capital, macroeconomic instability, and a new political alignment gave Getulio Vargas the leverage to change government regulations according to his political aims. Thus, labour unions acquired a privileged status in the new bankruptcy law, gaining precedence over creditors' rights. Many other protections for creditors were eliminated accordingly, prompting a high degree of concentration of ownership and control. In sum, politics and the macroeconomic environment were key determinants of Brazil's financial trajectory. The book thus confirms research by Douglass North and Barry Weingast, Daron Acemoglu, Simon Johnson and James Robinson, Stephen Haber, Raghuram Rajan and Luigi Zingales, among many others, that suggests, contrary to the legal origins view, that political institutions are a key determinant of financial development. Because Brazil lacked well-defined political insti-

tutions and a legitimate and stable universal system of property rights Vargas was able arbitrarily and drastically to modify Brazilian political and economic institutions in favour of its political base and in detriment of creditors. Using a variety of financial market indices Musacchio shows that these institutional changes had lingering effects on financial markets, such that to this day they have not been restored to their healthier and stronger pre-First World War level.

A discussion in the same level of detail of the political environment before the Great Depression would have been welcome. Protection of investors and creditors are not guaranteed unless the credibility problem is successfully addressed. As William Summerhill suggests in a 2008 essay ('Credible commitment and sovereign default risk', in Stephen Haber, Douglass North, and Barry Weingast, *Political institutions and financial development*) that was not the case in Brazil in the early twentieth century. The commitment problem was successfully handled in the constitution of 1824 but reappeared with the military coup of 1889: 'the ensuing republican government severely weakened the legislative branch. Inflationary policies in the early 1890s destroyed the value of the domestic debt and undermined the government ability to raise new funds at home. In contrast to its admirable XIX century record, Brazil embarked on a path of serial default in the XX century' (p. 228). Thus, there remains a puzzle: how was it feasible for bond and stock markets to flourish in a period (the early republic) with weak political institutions and government credibility? It appears to be the case that the strong political voice of commercial associations and creditors helped ease the credibility problem, and that market stabilizing institutions—those that support macroeconomic stability—also played a positive role in the development of financial markets, that is, the adoption of the gold standard, but the book does not elaborate on this issue.

An additional point that would have been good to develop further is the enforcement of bankruptcy laws. In particular, much of the discussion of the enforcement of bankruptcy laws relies on cases, all of which belong to the appellate courts in Rio de Janeiro, but it is not clear which cases made it to these courts (presumably not all cases went beyond the lower level courts), and whether those cases are truly typical rather than the exception. The same could be said with respect to the role of judges, who Musacchio argues were biased in favour of creditors' protections before Vargas's reorganization of the judicial system. The author's observations are based on too few judges, and he gives too large a weight to their decision-making process, which would be more appropriate under the common law, and not under the more restrictive structure of civil law, where judges generally have less room for interpretation. However, all of these are minor points that should not distract us from acknowledging the author's exceptional contribution. In sum, *Experiments in financial democracy* is an excellent book, and a must-read not only for Brazilian and financial historians, but also for scholars interested in the interaction between institutions and growth.

University of Vermont

CATALINA VIZCARRA

David Eltis and David Richardson, *Atlas of the transatlantic slave trade*. Foreword by David Brion Davis; afterword by David W. Blight (New Haven and London: Yale University Press, 2010. Pp. xxvi. 41 illus. 189 maps. 61 tabs. ISBN 9780300124606 Hbk. £30/\$50)

Sometimes one can only applaud. The creation of the Transatlantic Slave Trade Database, a work of many authors, but in which Eltis and Richardson, the authors of this magnificent atlas under review, played a major role, is one of the great historical achievements of our time. The database is an unprecedented empirical enterprise that provides as accurate a set of numbers on the magnitude, directions, and characteristics of one of the most important

events in human history: the forced transportation over five centuries of millions of mostly West Africans to the Americas. Few events in history are more momentous and have involved more people from more parts of the globe than this economic activity. And few events parallel it for evil. Even if it was an event not driven by hatred (though there was a lot of that), it was driven by greed.

Richardson and Eltis have spent decades on this project. The results of their work are spectacular. We now know so much more about the transatlantic slave trade than we did just a couple of decades ago that is hard even to remember how we used to think of the slave trade. Richardson and Eltis continue to hedge their findings with recognitions of how much they do not yet know; we should recognize how much, thanks to them, we now know that we did not know before. Their work, and the work of others involved in projects on the transatlantic slave trade, has enriched our field immensely. Doing this work has not been cheap—millions have gone into the creation of the database and now this atlas, but I can think of few better uses of taxpayer money from several countries than the articulation of a mass of empirical data on this momentous historical process.

This atlas is one monument to these multi-author and multi-disciplinary research efforts. It is a beautiful work. Indeed, that is the only fault I can find with this work. It is ideal as a book for the coffee table except that what it reveals in massive detail involves a primary example of man's ability to be brutal towards other men, women, and children. The atlas contains a multitude of maps, grouped into seven parts, all of which follow the logic of the Transatlantic Slave Trade Database. They are gorgeously produced, mostly in full colour, accompanied usually by extremely apt quotations either from contemporary historians or, more often, from primary sources. Together the effect is terrific: the presentation of the raw data in the form of maps and graphs is mediated by always appropriate reflections on what this meant for the people caught up in the traffic that made some people very rich and which immiserated so many others. In total there are 189 maps on such matters as where enslaved people came from, where they went, who shipped them, and how many times enslaved people rebelled on the voyages that they were sent on. The wealth of information is skilfully presented with few maps having so much information on them that they are hard to absorb. All in all, it is a magnificent achievement.

It also reflects some of the changes in historiography that have resulted from the labours of many, including these authors, on difficult sources in innumerable archives that cast light on the economic and cultural factors that shaped the transfer of millions across the Atlantic. The Atlantic slave trade used to be seen as a topic in American, especially North American, history. North America is hardly ignored in this atlas but much more attention is given to those parts of the Americas where most enslaved people went to, notably the Caribbean and Brazil. Even more attention is paid to Africa, a continent increasingly seen as being as diverse as early modern Europe and increasingly seen as not just a passive participant but an active facilitator of the trade in human flesh. As Davis notes in a sensitive and sensible introduction, the complicity of Africans in every part of the slave trade comes as an upsetting surprise to many uninformed readers. It also shows the complexity of historical processes. Uncovering the true dimensions of the transatlantic slave trade has been one of the great historical achievements of our time; working out what this mass of data means for early modern globalization and, even more, for the vital issues of morality, human agency, and human complicity with evil, will take up our attention for a very long time. Three cheers to the authors for their achievement in making this area of human activity better known and thus more understandable than it has ever been.

University of Melbourne

TREVOR BURNARD

Noel Maurer and Carlos Yu, *The big ditch: how America took, built, ran and ultimately gave away the Panama Canal* (Princeton and Oxford: Princeton University Press, 2011. Pp. xv + 420. 31 figs. 6 maps. 48 tabs. ISBN 9780691147383 Hbk. £24.95/\$35)

In anticipation of the 2014 opening of the newly renovated Panama Canal, a great deal of interest has been shown in the origins and history of the canal. Its history is both colourful and full of tragedy, commencing early in the nineteenth century. Much of the existing research focused on French exploits in Panama and the process through which the US obtained the rights to the Isthmus of Panama and eventually constructed the canal. Some accounts were historical in nature, recording the details of the creation of Panama as an independent country, the acquisition of the rights to the Isthmus of Panama, and ultimately the trials and tribulations of constructing the canal. However, few works have focused on a comprehensive calculation of the profitability of the canal for the US. Calculations of the canal's profitability have dealt with what is referred to as the 'social saving' of the Panama Canal or the net economic gain to the US that resulted from the construction of the canal.

Maurer and Yu provide not only a comprehensive history of how the canal came to be, but provide very careful calculations of the social saving of the canal. Although the magnitude of the social savings calculated by Maurer and Yu differ from those found in earlier research, there is no doubt that the social saving of the Panama Canal was positive and significant. The advent of the railroad may have had a greater impact on US national income than did the Panama Canal, but the latter clearly had a significant impact, especially in the first half of the twentieth century. According to the authors, the social rate of return, adjusting for a range of values for the elasticity of demand for shipping, exceeded the rate of return on US Treasury bonds for the entire interwar period. In 1925 dollars, the average annual social savings of the Panama Canal exceeded \$100 million per year between 1921 and 1937. The canal continued to be profitable for many years after the Second World War.

The authors, as the title suggests, devote much of the book to non-economic aspects of the Panama Canal's history. It is well documented that Theodore Roosevelt had a very 'strategic' role in the creation of Panama and the acquisition of the rights to the Isthmus for the construction of the canal. An important contribution of this book is the light shed upon the operation of the Panama Canal Company, which became a self-aggrandizing institution. The impact of discrimination against Panamanians in terms of businesses and employees that were not allowed to benefit from the canal is developed extensively. The imperialistic behaviour of the US in its dealings with Panama is laid out painstakingly for the reader. The authors describe how, even when the profitability of the canal was questionable, nationalistic pleas were made to block President Carter from going through with handing over control of the canal to the Panamanians.

Growth in trade with southern Asia and the Pacific-rim countries during the 1980s led to resurgence in the importance of the Panama Canal and, along with growth of trade with China since the early 1990s, has increased significantly the profitability of the canal since the Panamanians took over its operation in 1979. Increased profitability has allowed the Panama Canal Commission to invest heavily in improvements for the canal. The major enlargement project scheduled for completion in 2014 will substantially increase the capacity of the canal to accommodate shipping traffic using larger ships. Asian trade passes through the canal en route to Gulf and eastern ports and increasingly exports from the US to Asia are passing through the canal. The scheduled enlargement will allow for an expansion of this trade.

This book ties together successfully an assessment of the profitability to the US of building the canal along with the domestic and international political value of having the canal. It nicely balances the economic and strategic benefits to the US that result from building the canal with the accurate, but less than complimentary, assessment of the

humanitarian and geo-political aspects of its maintaining control over the canal. Only with Carter's transfer of the canal to the Panamanians in 1979 did the US begin to behave as a responsible world power that lives by the values it extols. Although one might argue that more time than necessary is spent on the imperialistic nature of the US's behaviour with regard to Panama, it is a topic that needed light shed upon it.

Vanderbilt University

WILLIAM K. HUTCHINSON

Mark W. Geiger, *Financial fraud and guerrilla violence in Missouri's Civil War, 1861–1865* (New Haven, CT and London: Yale University Press, 2011. Pp. ix + 306. 36 illus. ISBN 9780300151510 Hbk. £35/\$55)

In his search to discover the key to understanding Missouri's troublesome Civil War experience, Geiger found a smoking gun: a broad conspiracy by a group of the state's affluent pro-Confederate citizens to divert public and private funds to bolster the southern cause in Missouri. The result is this book, which not only tells the story, but links the economic issue to the later social collapse of the state. As a regional financial history, Geiger's innovative, compelling, and highly original work stands as a landmark of historical methodology; however, as a history of guerrilla violence, it is less convincing.

In 1861, as the secession crisis sputtered to a close and war drew closer, Missourians turned out in great numbers to join pro-Confederate militias; however, financing their enlistment and supplying them became a major issue. The answer to this problem was revealed by a small group of politicians and bankers who used their influence to redirect monies in banks and in state hands to support the southern cause. With this newfound wealth with which to fund the southern uprising in Missouri, the enrolments swelled and Confederate sentiment boomed. It would not last, though, and after the organized Confederate cause collapsed in Missouri, the state's financial institutions followed, resulting in the economic destruction of Unionists and Confederates alike.

Geiger's approach may be the most innovative to appear in Civil War history in the past decade. A former financial auditor, Geiger is well suited to this type of historical investigation. His linking finance to economy to instability to disloyalty, and ultimately, to outright guerrilla warfare, is powerful, but somewhat speculative. It can be seen as a case of 'where there's smoke, there's fire' history. What Geiger finds sometimes borders on the circumstantial, but his reasoning is solid and his evidence convincing, particularly to scholars whose works have oftentimes approached these incidental synergies. It would not be surprising to see the appearance of similar studies of other Border States in the coming years and this book becoming a catalyst which redefines the nature of these interregional examinations.

The story of financial manipulation is a compelling one. Geiger identifies the principals who committed the crimes and then hid them from view. Noting that the total amount of fraud exceeded \$3 million and was spread over dozens of institutions with the implicit knowledge of bank cashiers and directors, the author effectively dramatizes the size and scope of the conspiracy. After the war, bankers went back to work, this time trying to recover the sums lent in the fraud. He notes that one company had given money to 86 clients in three different counties on promissory notes and had to file suit on each in an attempt to recoup the losses.

Despite the power of his financial analysis and the logic of his historical process, Geiger attempts to establish the link to guerrilla warfare with less effect. Perhaps it is the nature of such a technically innovative work to leave a weakness, but there is a marked difference between the financial and guerrilla sections of the book. While the relationship between financial fraud and guerrilla warfare is possible, even likely in specific cases, it was not as certain as the author suggests. Geiger's guerrilla analysis would have been strengthened by

exploring a variety of motivations, including those not directly related to his thesis. Doing so would have provided the level of nuance that would have strengthened this, the final section of the book.

In addition to the financial and guerrilla elements of Geiger's argument, he also ties the Missouri experience into how the state remembers the war. Geiger describes how modern Missourians embrace a low-culture memory of the Confederacy and links it to the state's financial crisis and guerrilla heritage. Like Anne Marshall's recent work on Kentucky's Civil War memory, Geiger's work will likely foster further exploration of the field.

Despite my appreciation for Geiger's book, his writing style sometimes tends towards hyperbole. For example, of Missouri's guerrilla war, he writes, 'This was the worst such conflict ever to occur on American soil' (pp. 3, 100). While the statement is highly debatable and can never be conclusively confirmed, nor denied, it might be safer for a scholar to mitigate the language that often locks one into an unenviable defensive position. Geiger explains that guerrilla 'violence in Missouri was more extensive than other places' (p. 101), but so was the completeness of the Union's occupation of that state. He also notes that 'the violence continued at a high level long after there was any chance that Missouri could fight its way into the Confederacy' (p. 101). That happened throughout the border region even until the last days of the war. He suggests that Missouri guerrillas burned courthouses and records as a way to obscure their debt and he is correct (p. 111). However, Champ Ferguson destroyed casebooks in Fentress County, Tennessee, to hide a murder indictment and other guerrillas burned Kentucky, Virginia, and Tennessee courthouses to throw civil authority into turmoil, call into question land claims, destroy records of tax responsibilities, and obscure debt.

Ultimately, Geiger's work is financial history first, guerrilla history second, and memory third. While he lays out a plausible and convincing claim of the conspiracy to arm southern sympathizers, his analysis of the guerrilla war that followed it is less convincing. This criticism; however, must be tempered because of the innovative nature of his work. While imperfect, his approach should serve as a call to scholars of the Civil War and Reconstruction, particularly those working in nuanced fields such as guerrilla warfare and wartime memory.

University of Virginia's College at Wise

BRIAN D. MCKNIGHT

Perry Mehrling, *The new Lombard Street: how the Fed became the dealer of last resort* (Princeton, NJ: Princeton University Press, 2010. Pp. xii + 174. 2 figs. 14 tabs. ISBN 9780691143989 Hbk. £20.95/\$29.95)

Mehrling's latest book traces the evolution of monetary, credit, and financial arrangements from Bagehot to the recent crisis. This is a welcome effort that is presented in way that is accessible to general readers and undergraduates. The main message is that the Federal Reserve changed from lender of last resort to dealer of last resort during the 2007–9 crises. Along the way, Mehrling traces the shift away from the real bills or productive credit doctrine as part of the growing recognition that the central bank is responsible for money and credit and many lenders and borrowers. He does not point out, however, that the Federal Reserve has never clearly stated the extent of its responsibilities during panics and crises. Bagehot's main criticism of the Bank of England was its failure for many years to announce that it was the lender of last resort. Bagehot understood that, by following a pre-committed policy, the Bank reduced uncertainty and panic. Much of the author's analysis concerns what he calls 'shiftability' (pp. 38, 42), a term that is defined only by its context. It seems to mean the ability to sell (shift ownership) of a portfolio asset. The Federal Reserve provides assistance to banks when they cannot borrow in the market, but

several, specialized intermediaries (dealers) financed non-bank financial firms. After August 2007, the specialized dealers were unable to provide financing. The Federal Reserve assumed the role of ‘dealer of last resort’ (pp. 136–9) by offering loans to prevent failure of illiquid financial firms and by supplementing the mortgage market operations that brought Fannie Mae and Freddy Mac to public conservatorship.

Let me pass over with a small grumble the issue of whether the Fed’s crisis lending was appropriate action for a central bank. One reason Mehrling is far more favourable than I am is that he does not discuss how the \$1.5 trillion of excess reserves will be reduced to a more manageable range. I want to concentrate on his claim that the Fed’s expanded role should continue: ‘The central lesson of the crisis is that the American system requires the Fed’s support as dealer of last resort, not just in the money market . . . but also in the capital market and not just for Treasury securities . . . but also for private securities. The practical intertwining of money markets and capital markets is the defining institutional feature of the American system, and that feature requires a similarly integrated backstop by the central bank’ (p. 115). Why do all financial markets ‘require’ Fed support at times? To the limited extent that Mehrling answers this question, he relies on the claim by the late Hyman Minsky that the capitalist economic system is unstable. I regard this claim as an unsubstantiated conjecture, so I draw very different conclusions. We know that, after the Bank of England took Bagehot’s advice, individual banks failed but the system did not. Two reasons for this were much higher bank capital ratios and more certainty about the Bank of England’s lender of last resort policy. We have drifted, and more recently galloped, into a system without either adequate bank capital or a well-specified lender of last resort policy.

The primary social purpose of a lender of last resort is to protect the payments system. We now have adopted a policy of preventing failures by large financial firms and even automotive firms. Mehrling says that the ‘wholesale money market is much larger than the mere trillion or so that the Fed took on’ (p. 125). He ignores the \$400 billion of losses at Fannie and Freddy. What started with the bailout of First Pennsylvania Bank in the 1970s has not reached a ‘mere trillion or so’. Nothing limits future size, and moral hazard suggests larger failures are in our future. Mehrling does not ask why taxpayers should welcome a system that charges them for banks’ failed risk-taking. One of the major flaws in our current financial system is that risk models used by major firms and the Federal Reserve do not recognize so-called ‘fat tails’. These occur because there are large, discrete jumps in perceived risk, as for example in the end of the Soviet Union, the failure of Long-Term Capital, and the recent Japanese tsunami. Firms and central banks that do not predict or anticipate these jumps should be required to hold enough capital to put the cost onto the stockholders and not the taxpayers. Mehrling never considers Fed policy and housing policy as a source of financial instability. In the recent crisis, there were enough private and public mistakes to suggest that more public responsibility or more detailed regulation will not prevent a future repetition. Before concluding that taxpayers should pay for regulator and banker errors, let’s increase capital and put the stockholders to guard their firms.

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ALLAN H. MELTZER

Jonathan G. S. Koppell, ed., *Origins of shareholder advocacy* (New York: Palgrave Macmillan, 2011. Pp. xiv +292. 20 figs. 1 illus. ISBN 9780230107328 Hbk. £55/\$100)

I should declare a prior disappointment. Early in 2009 an intriguing call for papers invited contributions to a conference at the Millstein Centre for Corporate Governance at Yale. This was to mark the four hundredth anniversary of Isaac Le Maire’s letter of protest to the Dutch East India Company (VOC), which argued that the board were trampling on the rights of shareholders. My proposal for a paper, co-authored with Mark Freeman and

James Taylor, on the governance of early British stock companies was turned down by the organizers. This is the volume of that conference.

Personal feelings of rejection aside, I think that this is a remarkable collection of essays. It is the first such volume to explore principal-agent problems between owners, managers, and directors in different forms of business organization during the medieval and early modern periods, while attempting international comparisons within and between at least some of the chapters. It is remarkable also because of the eclectic mix of topics, and the fact that most authors endeavour, not always convincingly, to tie their findings to modern issues in corporate governance (perhaps under editorial injunction). Eight of the 12 chapters examine business organizations before 1800. Three focus on the early history of the VOC, founded in 1602. Oscar Gelderblom, Abe de Jong, and Joost Jonker provide an analysis of the governance structure provided by the VOC's first charter. The authors call it 'deviant' (p. 51), in the sense that it was no template for the modern corporation. The VOC had a permanent capital from the outset, and its investors enjoyed limited liability and freely transferable shares, but they had no voting rights and little access to financial information. Directors were appointed by outsiders and held their seats for life. The overwhelming influence on the company was its quasi-public character and its need to combine colonial warfare with trade, thus reasons of state overrode the interests of private investors. De Jong describes the pamphlet attacks on the VOC during the early 1620s, and makes the same point about the powerlessness of shareholders. Faulty corporate governance, however, did not preclude commercial success as the Dutch expanded their trading power in Asia and the dividends flowed in. In the third VOC chapter, Andrew von Nordenflycht argues that in the long run the company found it impossible to raise additional capital given the state's de facto expropriation of the original shareholders. By the eighteenth century the VOC had become an example of managerial entrenchment and the politically determined persistence of an inefficient economic institution.

Moving beyond the Netherlands, Gonzalez de Lara charts the transition in contractual form in medieval Mediterranean trade from the sea loan (a bond-like contract) to the risk-sharing *commenda* (an equity-like contract). Employing evidence from Venice, she explains this shift by pointing to the improved market discipline and investor protection brought about by enhanced state regulation and litigation. Thomas Hall examines the Virginia Company of London, where in 1619 a shareholder revolt put the pugnacious Sir Edwin Sandys in power. For reasons not made entirely clear, Hall blames the company's one-shareholder-one-vote franchise for enabling Sandys to manipulate shareholder support. Sandys' management proved disastrous and the company was dissolved five years later. Corrado Malberti uses a dataset of 100 Italian company charters from 1638 to 1808 to demonstrate that the greatest tensions arose in companies with the least dispersed ownership structures. Agency problems were mostly resolved through exit strategies, such as the limited duration of most companies, and the frequent distribution of profits to shareholders. Reza Dibadj also finds shareholder powerlessness to be the root cause of the problems at the French Company of the Indies in the final years of its existence, 1763–9. William Goetzmann and Sebastien Pouget examine three mill companies in fourteenth-century Toulouse, where a lawsuit, concerning who could contract a debt on behalf of shareholders, led to a new elected council of shareholder-directors (*conseillers*) assuming oversight of the managers (*bailies*). One interesting suggestion is that this reform derived from local municipal government. The influence of political models on the development of governance forms in business is a point surely worth further attention.

The remaining four chapters all postdate 1800. There is a fascinating account by Randall Morck and Fan Yang of the Rishengchang bank in China's Shanxi province during the nineteenth century, which had multiple classes of shares with carefully differentiated voting rights designed to align incentives. Managers, for example, made no money investment but were given non-transferrable 'expertise' shares that paid the same dividends as equity.

These were augmented by contracts that permitted the enslavement of their wives and children as hostages, which unsurprisingly helped to eliminate insider fraud—a salutary lesson for modern times perhaps? Richard Sylla and Robert Wright present early results from their huge dataset of US corporate charters in the 1790–1860 period. These contained several mechanisms that helped to mitigate principal-agent problems. Medium-sized companies, however, were most likely to fall between the two props of good governance: monitoring by shareholders or by securities markets. John Armour and Brian Cheffins examine pre-1950 precedents of ‘offensive shareholder activism’ in the US, defined as a hedge fund acquiring a stake in a company with a view to effecting governance change. Similar actions were relatively rare before 1950 for a variety of reasons; nevertheless, Armour and Cheffins uncover many instances of individual investors targeting public companies to gain control, so they conclude that in this period too underperforming companies faced some investor-driven discipline. The volume is rounded off with a discussion by Stephen Davis, director of the Millstein Centre, of the lessons for modern governance that history provides. Above all, even when minority investors enjoyed substantial rights, they often found themselves out-manoeuvred by more powerful interests.

As Koppell states in his introduction, this volume reveals the complexity of the principal-agent problem and the importance of context in the development of any governance regime. Of course, there is an obvious criticism of a collection like this, that it is no more than the sum of its parts. Nevertheless, the book does successfully demonstrate that agency problems have a history that long predates Adam Smith, let alone Bernie Madoff.

University of Hull

ROBIN PEARSON

Vaclav Smil, *Prime movers of globalization: the history and impact of diesel engines and gas turbines* (Cambridge, MA: MIT Press, 2010. Pp. 264. 88 illus. ISBN 9780262014434 Hbk. £22.95/\$29.95)

Prime movers is the history of two technologies—diesel engines and gas turbines—and how they helped create the modern global economy. While economists and economic historians have long appreciated the centrality of declining transport costs in facilitating trade, especially long-distance bulk trade, the argument here is that a lacuna exists in the globalization literature concerning the technologies central to that process. Smil’s method involves ‘studying history’s grand sweep as a continuous (but unevenly progressing) transition to new prime movers’ (p. 2), where the concept of the ‘prime mover’ is narrowly defined as technologies ‘used to energize fleets of ships and airplanes’ (p. 2). Engineering detail dominates key chapters, and a focus on two key personalities—Rudolf Diesel (1858–1913) and Frank Whittle (1907–96)—lend sections of the book a distinctly biographical feel. A well-sourced historical and engineering study, *Prime movers* is a natural addition to the libraries of those interested in the history of motive power and/or the technologies facilitating modern globalization.

The histories of diesel engines and gas turbines nicely illustrate many key themes from the innovation literature. Both prime movers had extremely long developmental trajectories emerging out of the technologies they replaced: ‘No technical inventions arise entirely and instantly *de novo* . . . their development shows often long periods of theoretical gestation or preliminary exploratory experiments or an interaction of both’ (p. 21). A patent incorporating scientific principles behind gas turbines was filed as early as 1791 (although contemporary materials technology meant commercializing the design was not practical), and Diesel was developing a theoretical basis for his engine by 1890. Diesel’s aspirations for his engine were to power automobiles and provide an economical source of stationary power to small-scale manufactures (he perceived the high fixed costs of steam as one

source of concentrated large-scale industrial concerns). Illustrating the pure uncertainty of innovation gasoline engines came to dominate the automobile market, electric dynamos, and small stationary installations (while stationary diesels grew to rival even the largest steam engines). Long-distance maritime trade, not a focus of Diesel's initial efforts, ultimately drove the engine's evolution. The need to power ever larger container ships over longer and more challenging trade routes led to the development of very large diesel engines generating in excess of 100,000 bhp, powering transports carrying more than 10,000 shipping containers (with specialized varieties of engines transiting the Suez and Panama canals). Continual economic pressure on fuel consumption produced engines of remarkable thermal efficiency. By contrast, the concerns of commercial passenger carriers have produced gas turbines that are among the most reliable engines ever produced (due to passenger safety concerns) and increasingly quiet and smooth (since noise and vibration negatively impact passenger comfort).

Smil argues that prime movers are the most significant elements of globalization's technological matrix: 'Without prime movers, all technical advances in designing and building better means of transportation and all organizational, managerial, and transactional improvements aimed at the intensification of international trade would remain just interesting unrealized concepts' (p. 156). Concerning diesels and turbfans specifically, 'there are no comparably reliable and affordable alternatives ready to displace them either on sea and roads or in the air' (p. 233). Taken together these arguments imply that without these specific prime movers 'globalization could not have scaled its current heights at anywhere near its reach, cost, and efficacy' (p. 156).

Prime movers convincingly establishes the centrality of diesel engines and gas turbines to the global economy. Both dominate their respective markets (large marine transports by diesels, air transport by gas turbines). More surprising perhaps is the bewildering range of applications to which the engines are applied: 'Nearly all of the heavy machinery that is used to build roads, bridges, tunnels, houses, commercial, and industrial enterprises is powered by diesel engines' (p. 147). Diesel engines are central to modern mechanized agriculture and are employed at every stage of the global trade network, measured in tons per kilometre: some 94 per cent of global trade is now diesel-powered, and that very high ratio is still growing.

Less convincing is the argument that these particular prime movers are irreplaceable. One is put in mind of the debate over railroads and US economic development. Establishing that no substitutes exist for diesel and/or gas turbines in a world of endogenously evolving technologies requires contemplating a counterfactual history where alternative prime movers had developed more fully. Rival technologies are mentioned—gasoline, nuclear, steam—but their possible development is not adequately considered. It is clear that, given the resources that were actually dedicated to improving diesels and turbfans over the last 200 years, they are vastly more efficient than their competitors, but how efficient could fully realized substitute prime movers become? How might the global trade system adapt to these alternative prime movers? On these questions no evidence is deployed.

Lewis and Clark College

CLIFFORD BEKAR

Youssef Cassis, *Crises and opportunities: the shaping of modern finance* (Oxford: Oxford University Press, 2011. Pp. xvii + 200. ISBN 9780199600861 Hbk. £25/\$45)

Financial historians owe an enormous debt of gratitude to the global financial crisis of 2007–8, not least that, in exposing the conclusions reached in some of the previous research on earlier crises, it has also created a demand for the work of those who can put current events into historical perspective. With the future still clouded with uncertainty,

some insight into the causes, course, and consequences of past financial crises provides a point of reference, at least in terms of what not to do if not a reliable guide for the action to be taken. To this end Cassis, a highly respected financial historian, has used his knowledge and experience to compare and contrast the events of 2007–8 with a series of earlier financial crises. He picks eight financial crises for his purpose beginning with Barings in 1890, through the US panic of 1907 and the disruption caused by the outbreak of the First World War, before dealing with the Wall Street crash and the global banking crisis of the 1930s as his first four. There is then a hiatus until the next four begin, starting with the period of global instability in the early 1970s, the international debt crisis of 1982, the Japanese banking crisis of 1997/8, and then concluding with the global financial crisis. Throughout the approach is to describe and analyse what took place, emphasizing the differences rather than similarities.

To Cassis each crisis is different, and his aim is to provide the evidence to support such a view. Banks, in particular, are vulnerable in a crisis because they borrow short and lend long, profiting from the interest rate differential. However, the normal practice of banks hardly explains major financial crises. In each case there was always an additional feature that helped explain the severity of the crisis. All of this is set out in the first two chapters that provide a chronological account of financial crises since the mid-nineteenth century, focusing on the particular examples chosen. Interesting as these are, it is the subsequent chapters that provide the substance of the book and make an important contribution to the literature on the current global financial crisis, which Cassis regards as one of the most severe in world history. These begin with a chapter on banks as these were the greatest victims of any crisis. Though generalization is difficult, what emerges is that it was the smaller banks that failed and that concentrated banking systems were more resilient than decentralized ones. The next chapter then moves on to the question of governance, especially the principal-agent problem where ownership and management were separated. This was increasingly the case as banks grew in size. In a world where individuals no longer owned the banks that they ran, bonuses were a means of providing rewards for their efforts. The problem was how to punish failure and that remains an ongoing conundrum. Regulation is next treated with the observation that financial crises create strong demands for government intervention but that does not prevent them recurring. Too often a particular group of individuals or institutions is blamed and so the fundamental causes of a crisis are ignored. There is a perpetual dilemma between regulation to prevent another crisis and that to improve the efficiency of the financial system, with the result that supervision alternates between being too lax and too restrictive. It is this that helps to make each crisis different as the controls introduced in the wake of the previous one stimulate evasion and innovation, which are then exploited when supervision is relaxed in the interests of competition.

Allied to national regulation is international cooperation and this is given a chapter of its own. Before the First World War what international cooperation there was concentrated on maintaining monetary stability under the gold standard. It was the financial instability that followed the war that led to increased cooperation but this proved ineffective given the severity of the problems. Renewed efforts followed the end of the Second World War and these did prove to be longer lasting and more productive, especially central bank cooperation. Again, however, international cooperation created its own complications as it removed authority from experienced bankers and placed it in the hands of officials, who placed a growing reliance on statistical measures. As is well known the great improvements in international cooperation failed to predict or prevent the current crisis though it did limit the damage inflicted. Finally, the last chapter assesses the impact of crises on the relative standing of financial centres and comes to the conclusion that they were not of significance.

The overall conclusions that emerge from this short book are that each financial crisis is different and that financial crises are unavoidable, being an integral part of the functioning

of financial markets. What is required is not measures to prevent financial crises but to ensure that they do not inflict long-term damage on financial systems or the real economy. For those conclusions alone the book is essential reading for those who look for the way forward from the current global financial crisis. More generally, this is a very well written account of financial crises from the late nineteenth century onwards by an author who is a master of his subject.

University of Durham

RANALD MICHIE

Agnar Sandmo, *Economics evolving: a history of economic thought* (Princeton, NJ and Oxford: Princeton University Press, 2011. Pp. viii + 489. 14 illus. 2 tabs. ISBN 9780691140636 Hbk. £62/\$90; ISBN 9780691148427 Pbk. £24.95/\$45)

The body of writing addressed to the manifold strategies with which humans confront the problem of their own subsistence and in so doing create institutions and arguments is very extensive, a literature already established and flourishing in antiquity. More prosaically, during the last century and arguably beginning in English with John Kells Ingram's *History of political economy* (1888), a literature of the history of economic thought has developed alongside the development of economics as an academic discipline. As with economic history, a course in the history of economics was until quite recently—the 1960s and 1970s—thought to be part of a conventional economic education, but it is now rare for undergraduates to be exposed to anything more than a very cursory treatment of the work of past economists. Nonetheless, economists of various stripes continue to write introductions to the history of economic thought, an observation that leads to the question: who are these books for? This in turn raises a further question: what kind of histories are these?

Sandmo's book is yet another history of economics that runs for the most part from Smith to the 1950s, with a very brief treatment of pre-Smithian work (two pages on Hume, one and a bit on Quesnay and Physiocracy) and a somewhat less attenuated review of developments in the second half of the twentieth century. Originally published in Norwegian, and hence clearly answering my first question, the text has been revised in translation to English, where the answer to this question is no longer so clear, an English readership being both more diverse and already served by a range of texts. Sandmo suggests that the work is directed to a reader who has little formal training in economics (p. viii), but the benchmark for such a readership was established by Robert Heilbroner's *Worldly philosophers*, first published in 1953 and still available in a Penguin edition. Heilbroner combines biography, context, and thought with a deceptively easy style, covering much the same period—from Smith to the postwar world. Sandmo does include more economists—writers such as Dupuit, Gossen, Wicksell, Hotelling, and Stackelberg are discussed—but the significance of these names will be relatively inaccessible to a reader without any economic training. Moreover, by definition, a reader with a modern economic training is unlikely to have any especial interest in reading about these past economists, let alone actually read their books.

Sandmo writes from the standpoint of an academic economist, using the terminology of a modern economist, assuming not only that this conceptual armoury is most appropriate to the investigation of past economic thought, but that it represents a standpoint most suited to the elementary exposition of economic thinking. Given that economists are by and large ignorant of the history of economics, and that the justification for this ignorance lies in an assumption that economics is a progressive science whose progress is marked by the supersession of earlier work, presenting the history of economics within the framework of modern economic thinking becomes a paradoxical exercise. For the application of a

modern instrumentarium merely reveals a history made up of half-formed ideas and wrong turnings whose merit lies in anticipating the precision of what comes later, but which has yet to be achieved.

Moreover, the story conveyed by such a history turns on what economics looks like today, something of which the target reader is assumed to be largely ignorant but which importantly is not the reason that such a reader would turn to such a text—this is not a textbook of elementary economic instruction, although around a century ago textbooks intended for this purpose often introduced their readers to economic thinking by reference to classical sources like Smith or Ricardo. An introductory history of economics has therefore to be motivated by something other than the structure of modern economic thinking, something Roger Backhouse's *Penguin history of economics* (2002) clearly recognizes, his opening remarks ending with a section entitled 'The story told here' (pp. 8–9). Furthermore, Backhouse begins with classical antiquity and arrives at Hume one-third of the way through the book, but still manages to include a chapter on twentieth-century econometrics and mathematical economics.

The assessments and judgements that Sandmo deals out along the way are, however, at least even-handed, if sometimes brusque and opaque. There is indeed no clear thread of argument, simply a summary of the work of various writers who happened to write about matters that, today, seem to be of historical relevance to the work of academic economists. As we know all too well, what interests the modern academic economist does not exhaust the realm of economic problems and issues that are of wider public interest.

University of Sussex

KEITH TRIBE

Mark Casson, in association with Peter J. Buckley, Ken Dark, Marina Della Giusta, Andrew Godley, Mohamed Azzim Gulamhussen, Teresa da Silva Lopes, and Nigel Wadeldon, *Entrepreneurship: theory, networks, history* (Cheltenham and Northampton, MA: Edward Elgar, 2010. Pp. viii + 400. 25 figs. 24 tabs. ISBN 9781849800396 Hbk. £79.95/\$140)

Casson's *The entrepreneur: an economic theory* (1982) is a seminal work in the development of modern theoretical scholarship on entrepreneurship. It was timely, published just as the current wave of scholarship and teaching on the subject was forming, but in several respects *The entrepreneur* and Casson's subsequent work stand apart from the vast majority of recent entrepreneurship research. First, Casson insists that we understand entrepreneurship as an economic function rather than as an organizational form, such as a start-up or small business. Second, while maintaining at core an orthodox economic perspective, Casson has readily engaged research and concepts across the social sciences in elaborating his views on entrepreneurship. This has been particularly true of his engagement with history and his collaboration with historians.

Entrepreneurship: theory, networks, history builds on nearly three decades of thought on the subject. The book consists of a collection of papers, most of which have been co-authored with collaborators from across a number of disciplines. At its heart lies Casson's basic theoretical contention, outlined in the introduction, that entrepreneurship should be conceptualized as an economic function that involves the exercise of judgement in the pursuit of uncertain economic opportunities. By relaxing neo-classical economic assumptions about perfect information and markets, Casson argues, we can begin to theorize the function of entrepreneurs as economic agents who use superior judgement in the face of this uncertainty to bring valuable goods, services, and organizational changes into markets. This basic theoretical stance allows Casson to range far and wide in chapters about the ways entrepreneurial judgement affects, among other things, the growth of firms, the operation of networks, and the development of markets and states.

Part III, 'History', will be of particular interest to readers of this journal. The section consists of a set of co-authored papers in which Casson and his collaborators move back and forth between historical evidence and Casson's theory of entrepreneurship to bring fresh, sometimes surprising, perspectives to a number of familiar subjects. Casson and Godley, for instance, show that the Singer Sewing Company's global expansion, first in the UK and then in other parts of the world, was driven by entrepreneurial decision-making rather than by more routine managerial calculations of transaction costs and vertical integration. Singer, the authors point out, was a pivotal case study for Alfred Chandler in his synthesis of the development of vertically integrated firms and the revisionist interpretation of its expansion raises doubts about the extent to which the development of large, modern firms can actually be explained without reference to entrepreneurship theory. Next, Casson and da Silva Lopes look at the development of global brands in consumer goods industries. They point out that while founders of brands may have played an important role in giving a brand its initial identity, the extension and expansion of these brands globally was often executed by large multi-brand firms that acquired the initial brand and had the entrepreneurial knowledge and insights to rejuvenate them for the international market. In the following chapter Casson once again collaborates with Godley, this time to bring a new perspective to the much debated issue of entrepreneurship in Victorian Britain. Rejecting the hypothesis of entrepreneurial decline in the period, they point out instead that the judgemental talents and resources of entrepreneurs in the UK were drawn away from textiles and other manufacturing and toward the development of large-scale projects, such as railroad building, both at home and throughout the Empire. In the final chapter of the section, Casson collaborates with Dark and Gulamhussen to apply entrepreneurship and multinational theory to the imperial state. Doing so, they argue, offers insights into 'why, where, and when empires develop' (p. 326) and they test this against a selection of cases.

One of the most notable characteristics of the book is the way in which Casson and his collaborators extend entrepreneurship research away from the start-up organizational contexts to which it is conventionally relegated. Indeed, there may not be a single technology-based start-up in this 400-page volume. Instead, entrepreneurship theory is applied to networks, large firms, non-profit organizations, cooperatives, and nation states. One can hope that Casson's efforts to broaden the institutional contexts for studying entrepreneurship spurs similar efforts among other researchers. Though well crafted, the historical chapters in the book only touch on a fraction of the theoretical propositions and contentions that Casson makes. Indeed, some of Casson's most interesting theoretical elaborations attempt to deal with the underlying character of entrepreneurial judgement, which he ascribes to a mix of superior personal information, memory, and interpretative abilities in the face of uncertain opportunities. Here the theory of the entrepreneur reaches into territory that traditional business and economic history has been slow to explore, but a frontier to which it must venture to gain deeper historical insights into the nature of the entrepreneurial process.

Describing his vision for how progress could be made in the study of entrepreneurship, Schumpeter once drew on a sailing metaphor. Research, he suggested, needed to tack back and forth between theory and history without losing sight of the other. Casson's book has done much to push forward entrepreneurship theory and provides full sails for any who wish to tack entrepreneurship research back into historical waters.

University of the Pacific

R. DANIEL WADHWANI