
REVIEWS OF BOOKS

L. BALESTRIERI, *Venezia presente e passato: per una interpretazione ideologica della storia*. Venice, Universitaria editrice, 1978. pp. 174.

This book is as much about the future of Venice as it is about her past, the underlying assumption being that a study of the latter can provide insights that are useful to the former. The author begins with an analysis of the depressed conditions of the Venetian economy today, and notably the ongoing exodus of many of her residents toward the mainland, the painful sight of thousands of run-down and often vacant dwellings, the growing reliance on tourism as the main, but inadequate, source of income for her residents. These conditions he then traces back to the close of the eighteenth century, when Venice ceased to be the capital of an independent state and he argues that, from that time on, the city's economy suffered from a seemingly irreversible haemorrhage of investment funds as savings produced in Venice have not, by and large, been reinvested in the city itself, but have trickled out toward the mainland, and notably (in the last half-century or so) toward the sprawling petrochemical complex located on the fringe of the lagoon. As for ways to reverse this outflow of both people and capital and thus restore the city's prosperity, the author proposes the development of industrial activity in the city itself, a goal which, in his view, could be attained by the creation of a state-owned financial institution which, much like the Gosbank in the Soviet Union, would ensure that local savings are made available where they are needed most — in this case to local industrial firms.

The discussion and evaluation of such a proposal is best left to professional economists. Not being one of them, I would only venture that Balestrieri's argument would have been more persuasive had he indicated what specific industries he thinks would be viable in the peculiar, indeed unique, setting of a city like Venice. One cannot help but feel that industrial enterprise has long deserted the city of St. Mark precisely because no modern manufacturing firm can perform efficiently in the maze of narrow streets and canals and within the

obvious constraints of space that characterize the city: unless, of course, its very layout were radically altered at the cost of extensive demolition and rebuilding — a prospect Balestrieri most certainly does not advocate. He would also have made a more convincing case for his proposed public bank had he more fully articulated the latter's actual functioning and planning authority. To suggest the Soviet Gosbank as a model will not do, for the latter is a central institution that operates on a national scale in the context of a centrally planned economy, whereas his proposed bank would have a merely local scope and would have to function in a mixed economy.

Leaving the intriguing question of the economic future of Venice for economists to sort out, historians will be more interested in Balestrieri's analysis of her past. In it he identifies a turning point around 1600 when the old Republic made some crucial choices that were going to determine the city's future over the next two centuries. One such choice was the refusal, in the 1580's, of a Spanish offer for Venice to serve as sole distribution centre in the now Spanish-controlled spice trade; a second one was the refusal to open its harbour to foreign (and notably Dutch and English) ships and merchants; and finally there was the creation in 1619 of the "Banco Giro", a financial institution for the administration of the national debt. Balestrieri views the first two choices as fateful mistakes in that they irreparably undermined Venice's standing as a great centre of international trade, while he praises the third as the key factor that ensured the survival and continuing prosperity of the city until the fall of the Republic by avoiding crippling tax increases and by monetizing the national debt, thus providing the liquidity needed to lubricate the city's economy. His analysis is only partially convincing. As regards the spice trade, one ought to keep in mind that, as Frederic C. Lane argued over ten years ago in his *Venice, A Maritime Republic* (a book surprisingly missing from Balestrieri's rich bibliography), the Spanish offer had too many strings attached to be attractive and acceptable. The refusal to turn Venice into a "free" international harbour is less defensible, but its long-term impact can be easily exaggerated: later in the century, as Balestrieri himself points out (p. 78), seaborne trade came to be handled increasingly by non-Venetian ships, presumably as a result of a relaxation of the old restrictions on foreign shipping. The author stands on firmer grounds when he stresses, as other scholars have not done, the importance of the Banco Giro as an agency that both helped meet the Venetian Republic's growing financial needs and ensured an adequate money supply. There are, however, further questions that come to mind at this point: was the city of Venice the primary beneficiary of it all and, if so, what components of the urban economy did the bank stimulate? or could it be that the major beneficiary was the economy of the Venetian mainland both in the sense that, as the major source of tax revenue, it was spared crushing tax increases and in the sense that it benefited from fresh investments by residents of Venice? On the first question the author does not supply enough clear evidence. His reference to the transformation of the Venetian ruling elite from a class of merchants to a landed oligarchy suggests,

however, that a great deal of available investment funds found their way to the mainland and that the enduring prosperity of the city reflected primarily the high level of conspicuous consumption made possible by the tide of rent income accruing to landowners who lived in the city. As to the second question, Balestrieri does mention (p. 91) the growth of manufactures on the mainland as a partial explanation of the recovery by Venice's harbour during the eighteenth century.

Whatever the answer to these questions may be, the fact remains that the economic decline of Venice as an urban economy did not really get under way, as Balestrieri conclusively shows, until after Venice ceased to be the capital of an independent state. This would seem to indicate that, in the end, her economic well-being had come to depend on her political role and, specifically, on the large number of individuals employed in or somehow living off the public sector. If this was indeed the case, then one has few reasons for being optimistic about the chances of reversing the downward trend that has plagued the city's economy since the fall of the Republic. A lack of optimism, however, need not mean that a search for alternative solutions should be abandoned and Lucio Balestrieri has done all of us a real service by putting the problem of alternatives squarely on the table and by providing food for reflection on a most challenging topic.

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A. BRIGGS, *Wine for Sale. Victoria Wine and the Liquor Trade, 1860-1984*. Chicago: The University of Chicago Press, 1985. Pp. ix + 199. Tables, graphs, illustrations.

If Lord Briggs' *Wine for Sale* is an official history of the Victoria Wine Company it is a strange one. First he devotes only slightly more than half his pages to the company in his laudable aim to present its evolution within a general context. Secondly the history of the company is not more than an outline, to use the author's own term. Such brevity has resulted, as he explained, both from the paucity of documents for its earlier history, a result of the German bombing of the London dock area during World War II where the archives resided; also from an overabundance of documents dispersed among the archives of other companies, a result of Allied Breweries' absorption of many once independent commercial institutions. It is this evolution from an independent family business to absorption into a vast "empire," a term Briggs used in reference to the Allied Group, that provides the guiding thread and the dynamics of the book. Integral parts of this evolution were aspects of British life that markedly influenced the commerce of wine: tariff rates and domestic taxes, various moral outlooks on alcoholic beverages, popular notions about wine, beer and spirits, and the changing structure, procedures and scale of the wine trade.

The book really begins with the Gladstonian struggle to reduce tariffs on the lighter table wines of France produced mainly in the Southwest and Bordeaux. Since there was a strong teetotal element in the English Liberal party, the argument ran that wine with moderate alcohol was a sobering force, its use with meals discouraging the drinking of stronger beverages such as gin, liquors, port and sherry. The bill, as finally carried, included not only drastically lower tariffs but easier licensing, with all shopkeepers allowed to retail wine. The number of licenses rose dramatically as the tariff rates fell. Unfortunately the figures given on page 37 do not accord with the graph, and the figures must be in error.

Five years after the Anglo-French treaty, an experienced wine merchant, William Winch Hughes, founded the Victoria Wine Company, a very opportune time, Briggs notes, as consumption of French claret, in particular, went up steadily. In this chapter Briggs seems to be writing an official history. Hughes's business expanded as did wine drinking and his retail outlets grew in number. Like most retailers he sold for cash and his customers brought their own bottles or jugs to his chain stores where the clerks filled them directly from the barrel. The cheapest wines were of Spanish or Portuguese origins, including Tarragone, a heavy red beverage "often called port." There was also a large demand for cheap clarets during the 1860s. Now it is evident from these observations that Hughes knew his customers' taste, and it was not for light wines, but rather for "strong alcoholized drinks," that would "keep." Alcohol was a preservative and widely in demand, so Hughes blended his wines to enhance their body and added enough alcohol to bring them to 38 degrees while in bond. He also offered wine which he bottled himself, and guaranteed as unadulterated. I suppose that adding alcohol does not adulterate, whereas adding water does. Such highly spirited drink hardly made for the sobriety that the defenders of the 1860 trade treaty had in mind. Hughes, like practically all wine merchants, was a blender who adapted lighter wines, such as those from Bordeaux, to the English taste for fuller, more potent drinks. He died in 1885 a wealthy business man.

His success, however, did not signify that Britain had become a nation of wine drinkers. Like most chain stores, Victoria Wine sold beer, hard liquor and mineral water, a diversification that safeguarded profits from sudden changes in taste; and a wise policy because the French wine boom did not continue beyond the 1860s. Lord Briggs seems to be of two minds about the failure of the British to consume wine on a significant scale: low tariffs, in 1860, led to lower prices and higher consumption, but only for a decade. Drinking habits, he argues, were formed more by general economic and social factors. Rising living standards encouraged expenditure on other items: cigarettes, travel, and so on, and yet he does not dismiss tariffs and taxes. In the 1960s and after, Sir Stafford Cripps' lower import duties led to reduced prices and greater consumption, this time of light French wine, which at last attracted a later generation of Britons who consumed less beer than their prewar parents. Fashion in taste changed like fashion in clothing.

Lord Briggs provides some interesting details about stores that were man-

aged chiefly by women, many of whom spent their working lives with the company. They were replaced by men, not when the company first went public in the 1920s, but after World War II, when it was taken over by Allied Breweries, the largest "drinks group" in Europe. The wine company retained its name, functioned with some autonomy and amalgamated other wine companies taken over by Allied so that in the 1970s it controlled nearly a thousand retail outlets spread over Britain, and now sold bottled wine, along with other hard and soft drinks, all at reasonable prices. Lord Briggs feels that it played an important role in the democratizing of wine consumption. At last, it would appear, the dream of Gladstone was becoming a reality. Fashion in drink had moved toward the lighter style.

This is a book I can heartily recommend as a study of business evolving from individualistic to corporate organization, of taste from traditional to adventurous, of life-style from Victorian to swinging.

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G. CHAUSSINAND-NOGARET, *The French Nobility in the Eighteenth Century: From Feudalism to Enlightenment* (Cambridge: Cambridge University Press, 1985). Translation by William Doyle - 197pp.

In 1967 Sasha Weitmann, an American sociologist, completed his doctoral thesis based on the famous cahiers of 1789 in which he demonstrated quantitatively that the demands of the French nobility were not very different from those of the Third Estate. Guy Chaussinand-Nogaret has expanded and amplified Weitmann's thesis, reworking the same cahiers and writing a long essay on the French nobility in the eighteenth century. He concludes that by 1789 the nobility in its great majority had become anti-absolutist, liberal-constitutionalist, and even civil egalitarian in the Enlightenment mould. In short, he claims that the XVIIIth-century nobility has an undeserved reputation for political and ideological rigidity; they were in fact identical to the elite of the Third Estate in their political and social programme for the nation. First published in 1976 in French, this book has been superbly translated into English by William Doyle.

It has long been recognized that the French nobility was deeply fissured by wealth, status, function, and culture. It has also been accepted — even by orthodox Marxist historians — that a part of the *noblesse*, especially those at Court and in the larger towns, were infected by or acculturated to Enlightenment assumptions about social utility and individual worth. No historian can deny the existence of a liberal nobility — Lafayette is the classic example — who joined the French Revolution at least in its constitutional-monarchist phase. However, Caussinand-Nogaret goes much further. He maintains that the nobil-

ity as a whole (ca. 120,000 individuals by his estimate) was so infected. His 134 noble cahiers (5000 grievances) are decisive evidence of a liberal (Whig) political programme. Since about 1750 the *noblesse* had undergone a crisis of conscience; "they put the interests of the Nation before their own." (p. 139)

Now the noble cahiers are precious and interesting testimony. In many aspects they do reveal a liberal mindset, if that poor tired word can still serve us with sufficient specificity. There were high percentages of demands for freedom of speech and press, for the abolition of *lettres de cachet*, for thorough reform of the civil and criminal law, and even for equality of taxation (88.5%). There were equally numerous demands for a decentralized government; nobles wanted to establish (or re-establish) the provincial estates throughout the kingdom (81.3%) and give them tax powers. This emphasis on decentralization is significant for it points to a particular form of liberalism which associates despotism with bureaucratic centralization and localism with freedom — a Tocquevillian perspective. Moreover, the noble cahiers are much more ambiguous regarding seigneurial rights, honorific privileges, and 'vote by order' in both the national and provincial estates. Chaussinand argues ingeniously that the majority of nobles were willing to concede 'vote by head,' but not every reader will be convinced, especially in the light of the subsequent massive recalcitrance of the nobility in the Estates General on this crucial issue.

Interpreting the intent of the cahiers is not always easy. Weitmann's thesis had the advantage of comparing the cahiers of the Third Estate with those of the *noblesse*, which highlights the difference over voting procedures and other issues such as seigneurial courts and dues. Does the failure to defend seigneurial rights explicitly mean that they were negotiable or that they needed no defence, being fully protected by the inviolability of property (47.8%). True, even the Third Estate in the National Assembly hesitated on this issue.

There is no doubt that Chaussinand has made an important contribution to the history of the French Revolution and of elites. The nobles who drew up the cahiers in early 1789 appear to have been a reasonable cross-section of a very diverse group — rich and poor, urban and rural, robe and sword, educated and quasi-illiterate. Their cumulative demands do add up to a kind of English Whig liberalism with a very strong element of decentralization and minimum government. The nobles were hardly egalitarian, however, and their reluctance — in the cahiers and subsequently — to merge with the Third reveals a profound ambivalence toward civil equality, even with the other non-noble elites of the Nation. A cultural shift had indeed taken place in France. It was bound to affect the nobility, but not all of it in the same measure or manner. Chaussinand evokes an evolution from blood to merit, but the word "mérite" can be understood in many ways, not all of them incompatible with the prerogatives of ancestry.

There were surely bridges of political thought between nobles and non-nobles, but to say that the goals of the nobility and those of the elite of the Third Estate were identical in 1789 is to go too far. Chaussinand would avoid a great deal of difficulty (see the Afterword to the English Edition) if he would settle for

ambiguity (not "turnabout") and for the persistent force of separateness especially among the legions of country *gentilshommes* who were to gain fresh wind in 1815.

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J. DE VRIES, *European Urbanization, 1500-1800*. Cambridge, MA: Harvard University Press, 1984.

Rare is the book which a reviewer wishes he had written. This appeared at first glance to be just such a book, and I maintained that feeling throughout its reading. Its strength lies in the propitious combination of inductive and deductive methods DeVries employs. His is a truly empirical approach, by which I mean that his conclusions are based on sensible interpretation of the evidence; his results do not turn on the statistical significance of a coefficient in a regression equation. His conclusions are not based on fragile assumptions, and he analyzes the data without forcing them into a preconceived theoretical construct. He uses the computer with integrity: the machine does not expropriate the research agenda, rather, one remains convinced that DeVries's mind is continually guiding it. In short, he emerges as a temperate but convincing cliometrician.

DeVries assembles a data base on the size of west and central European cities which at some time during the three centuries the book covers had reached a population of ten thousand. He begins his study in A.D. 1500, arguing that by that date Europe had established a permanent urban sector which was probably not too different from the system as it stood at its apex in the fourteenth century. The most urbanized part of Europe in 1500 was the Mediterranean basin, perhaps as a legacy of the Roman Empire, with fully half the European town dwellers concentrated there. The Mediterranean proportion shrank sufficiently over time so that by the end of the eighteenth century its share had declined to a third. The Low Countries remained the most urbanized locale in Europe with not quite a fourth of its population living in towns. Great Britain experienced the most rapid urban growth: the share of its population living in towns increased from 2 percent to 15.6 percent during this epoch.

During the sixteenth century the pace of urbanization was generally rapid. As an indicator one might note that the number of towns whose population exceeded 100,000 doubled from four to eight during this century. During the subsequent century urban growth proceeded, albeit at a slower rate, in spite of the demographic reversals experienced by the rural population. Urban population growth continued in the first half of the eighteenth century but the Romanized parts of Europe were no longer its primary beneficiaries; the lands beyond the Rhine and Danube participated in European urbanization to a larger degree

than they had earlier. The process accelerated in the second half of the eighteenth century but urbanization was no longer concentrated in the larger cities, but rather in the smaller ones. In addition, many new urban settlements emerged in contrast to the previous period when the number of towns with a population of five to ten thousand stagnated. This pattern prevailed first in Northern Europe; by the nineteenth century Mediterranean Europe, too, exhibited a similar pattern. Urbanization during the previous three centuries was more moderate than hitherto believed: the percentage of the population living in towns of over five thousand inhabitants increased only from 9.6 to 13 percent of the population. DeVries also shows that in 1800 the world was probably twice as urbanized as previously thought; Europe had 16 million inhabitants living in towns of five thousand or larger. This fact implies that nineteenth-century urban growth, too, was slower than previous estimates. These, then, are the salient features of European urbanization during this time span.

One should note that DeVries is by no means a mere collector of data, although that by itself is a major contribution. Collecting data on towns with population of five to ten thousand would have been much too tedious and the result would probably not have been worth the effort. Consequently, DeVries proceeded to estimate the populations of this important urban category. He invoked the pattern found in modern urban structures, namely that the plot of the distribution of the population of cities ranked by size is approximately a straight line on a double logarithmic graph over a considerable range. Extrapolating this line from towns with ten thousand inhabitants to towns with a population of five thousand one obtains a reasonable estimate of the urban population falling into this category. To be sure, one has to assume that the graph remains linear within the extrapolated range. This reader appreciates the care with which DeVries explores this phenomenon for the years in which data are extant before performing his estimation, enhancing considerably the reader's confidence in his results. The shapes of the rank-size distributions are interesting in themselves, for they reveal much about the urban system and level of economic integration. In 1500, the distributions show a low level of integration among the various regions in Europe, clearly an example of an immature pre-modern system. A large number of cities with populations in the 20 to 45 thousand range shared the top positions. They were mainly independent entities with administrative, ecclesiastical, and economic influence over a limited hinterland, such as Cologne, Nuremberg, and Utrecht. With the substantial urban growth of the following century the rank-size distribution reveal a higher level of integration. Naples, Paris, London, and Amsterdam emerged with large populations; with the exception of Naples, they began to link regional centres and the large cities themselves were more integrated than before. In the Mediterranean basin smaller cities still dominated their regional world without any one megalopolis exerting primacy over them. Venice, Genoa, Milan, Lisbon, and Seville belonged in this category. By the eighteenth century a hierarchy of cities was in place, with urban populations concentrated in the largest,

integrating "Europe into a single, albeit loosely coupled, urban system." The extent of "urban hierarchy-formation" achieved by 1700 was not exceeded until 1890. Whereas prior to 1750 urban growth was concentrated in the larger cities, without many new cities being added to the urban stock, after 1750 smaller cities grew relatively more quickly and new urban settlements appeared in large number. During the nineteenth century, the data vary by country, and the pattern of urbanization reversed itself once again and growth in the larger cities began to overwhelm growth in the smaller ones. Hence it appears that larger cities already dominated the urban sector in the seventeenth century, and the industrial revolution held their expansion in check until the spread of railways enabled large city growth to reassert itself.

To explore the stability of the urban structure DeVries investigates the so-called transition matrices. These matrices show how many cities in one of seven size categories stagnated, grew, or declined in 50-year intervals to another size category. This analysis shows that during a typical 50-year interval approximately three-quarters of the towns remained in their initial category. Fifteen to 25 percent moved up at least one category and 5 to 7 percent declined to a lower category. The matrix for the interval 1600-1650 stands out as a notable exception; only 60 percent of the cities remained in their initial size category, 10 percent rose, and nearly 30 percent fell in rank. This period also had few cities which crossed the ten thousand population threshold, but many declined below this level.

During the seventeenth and early eighteenth centuries those cities grew fastest which were either capitals — reflecting the importance of the rising nation states — or ports reflecting the importance of long-distance commerce. In fact, during this period, 30 of the 38 rapidly growing cities were in these categories. Capital cities evinced a greater stability of growth than any other type of city, reflecting the "irresistible expansion of government." In contrast, commercial and industrial inland cities were the most vulnerable to the vagaries of changing socio-economic circumstances.

Transition matrix analysis also reveals that in 1800 of towns with population of ten thousand or more only 41 percent were old towns which had existed above this threshold level in 1500. In Northern Europe the dominance of new cities over older ones was even more pronounced. Yet the towns in existence in 1500 proved very resilient: only 6 of the 154 fell below the threshold level and only 3 declined in population to a lower size category. This means that the process of European urbanization showed much continuity. The post-1800 pattern, too, shows continuity with the past. DeVries concludes: "*ex ante* the fate of a large city of 1800 is difficult to predict; *ex post* the large city of today is very likely to have been a leading city in 1800." In other words, adds De Vries, the industrial revolution was not a watershed in the development of the European urban system. "Its contribution was a gradual process... [and] the construction of an urban system was... a precondition for modern industrial growth."

The chapter on the demography of the early modern city presents a synthesis of the literature without claiming to report original research. It is, however, an excellent introduction to this field of analysis. The chapter on migration and urban growth is to some extent speculative since data on this aspect of population dynamics are quite scarce. Yet this is an important issue, since rural-urban migration was a major factor in urbanization. Growth of cities was, in fact, determined not by their birth and death rates as much as by immigration. DeVries calculates that in early modern northern Europe only about 2 percent of rural births were destined to migrate to the cities. This share increased slowly. By the latter half of the eighteenth century more than 4 percent of a birth cohort ended up in an urban environment. Thereafter the process accelerated considerably so that by the second half of the nineteenth century more than a third of rural births were destined for urban migration. One should add that these calculations are based on thinner evidence than those in earlier chapters. They nonetheless reveal a plausible pattern.

With all its brilliance, this book does not integrate the process of urbanization into the overall framework of economic development. That task, however, has been taken up by Paul Hohenberg and Lynn Hollen Lees in *The Making of Europe, 1000-1950. Harvard Studies in Urban History*. Cambridge, MA: Harvard University Press, 1985. Clearly, DeVries had other goals in mind, and those he did accomplish in an exemplary fashion.

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D. M. GORDON, *Merchants and Capitalists: Industrialization and Provincial Politics in Mid-Nineteenth-Century France*, Birmingham, Alabama: The University of Alabama Press, 1985, 249 pp.

J. M. MERRIMAN, *The Red City: Limoges and the French Nineteenth Century* New York: Oxford University Press, 1985, xix, 332 pp.

Twenty or more years ago, historians used to observe that they had not yet succeeded in rescuing urban history from the hands of amateurs, from antiquarian local chroniclers and their unstructured, anecdotal, isolated ways. Then the new urban history presumed to rectify this situation, putting some economic and social theory into the study, integrating the local experience with the national. Still, something was amiss, notably that politics seemed to find no natural place in this refurbished variety of history. But now the *new* (new) urban history seeks to remedy the matter. Urbanization is closely related to politics at all levels, and both local and national studies are set within the context of "global" social change. It is a tall order, a grasping after "total" history, or, if that sounds a bit frightening, "holistic" history, as John Merriman says in his

new book. Merriman recently edited a collection of essays on *French Cities in the Nineteenth Century* (London 1982) which offered a series of illuminating case studies. Two of them may now be seen to have served as trailers for the present volumes, which extend and elaborate on the themes then announced.

David M. Gordon's examination of Reims and Saint-Etienne from about 1848 to the fall of the Second Empire shows that the *République sous l'Empire* was not only *belle* but well prepared, and that the Third Republic was thus no curiosity or mere creature of the sharp struggles during the quadrennium after Sedan. His contention is that the *bourgeoisie* was well-intentioned toward the Republic long before 1870. To show this, he selects two prosperous cities with a powerful merchant and manufacturing elite and a strong artisanal industry. Each held about 45,000 people, more or less, in 1848; one was ancient, the other much more recent. In each city the elite confidently challenged the July Monarchy and managed its own working-class supporters before having the wind taken out of its sails in the revolution of 1848-49. Quite abruptly, Louis Napoleon appeared and drew to himself the working-class underpinning for a new conservatism, creating a schism between the old liberal manufacturers and their artisanal employees. The bridging, if not closing, of this class rift constituted some major part of the liberal agenda over the next two decades. A social and political feat, it was made possible by the accelerated modernization and industrialization in the two cities under review here, of France's woollen and steel enterprises. A new liberal industrial elite came into being, holding out the hope of better relations with the working class in the 1850s and 1860s, despite divisions between artisans and factory workers, rising strike activity, and a general radicalization of labour.

This was possible because still in 1871, even at the moment of the various Communes, there was so little overall working-class consciousness that the liberal bourgeois elite (perhaps elites is better) was able to draw both worker and middle-class support for the struggle with the older conservative oligarchy. The new industrialists, Gordon tells us, had become optimists, sure of making high profits, ready to bankroll the working of free political institutions, confident of heading off working-class independence by a policy of high wages. It was these people who galvanized the opposition in the last days of the Empire and themselves adopted moderate republicanism as characterizing the regime most likely to guarantee their material interests, secure middle-class values, and preserve a general social harmony. All this is clearly and agreeably set forth in a solidly-researched tale of two cities. The harmony, of course, was not to last. The not less significant story of continuing and growing class tensions is at the centre of the second work here.

Merriman, covering a much broader canvas, seeks to portray some not dissimilar evolution of economic and social events across the whole nineteenth century within a single city. Not steel or wool, but the porcelain industry provided the motor in Limoges. But the enterprise and activity of the new liberal business class are only the inner dynamic of an extraordinarily full recreation of

life in this radical town. Where Gordon's work presents an important thesis about the relationship of political development to economic change, Merriman's study goes a long way to fulfill his ambition to write the "total" history of a significant industrial centre from the end of the *Ancien régime* to the threshold of the contemporary age. The work is rich in its detail of street life, public ceremonies, *fêtes*, artisanal and mercantile activity, sanitary habits and private pleasures, social and cultural organizations, religious observances, strikes, insurrections and the criminal exploits (not many in the earlier period, anyway) of *limougeauds* in the *ville rouge*.

Limoges seems to have experienced every major phenomenon of the century, from grain riots to neo-royalist manifestations, from Saint-Simonian missions to its very own Commune. Nothing is so stale, one might say, as yesterday's local politics, and it is his artistry and sharp eye for telling illustrations that permits Merriman to carry us cheerfully along through this unfamiliar provincial cityscape of butchers and laundresses and porcelain workers, feeling all the while that in fact we are not total strangers to what he shows us and that the *limougeauds* he holds up to us are in fact quite recognizably first cousins of the Parisians we think we know from that other French history centered so comfortingly on the great capital. In this sense, his book is a brilliant success in extending the familiar range of French written history. We are perfectly accustomed to being taken on brief excursions out into the provinces, to be shown, say, 1830, 1848, even 1871, as more than Paris phenomena. In this work we see that all of them, and very much more associated with the nineteenth century at the country's urban centre, were authentically Limoges phenomena. Nothing can alter the fact that the great occasions were signalled by and from Paris, as Merriman points out, but the innate realities of these Limoges occasions are convincingly demonstrated. In some ways, moreover, this city was ahead of the capital in choosing the future: "The Republic easily found roots in Limoges long before the *seize mai* crisis in 1877 and the national republican electoral successes of the early 1880s. Since the July Monarchy Limoges had never ceased to be republican."

Merriman's history solves about as well as one can imagine the old problem of reconciling analytic description and forward movement in written history. There is no narrative flow really, but one is given the impression of movement forward even as one is being held to be shown closely detailed snapshots of a great many *moments*. The book rests upon an impressive array of archival and printed sources. It is illustrated with splendid period photographs and some rather less successfully reproduced maps. The Introduction shares a number of charming personal recollections of the pleasures, trials, and exiguous economies of doing research as a young scholar in provincial France. This is a major work, exemplary of its kind.

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L. C. HUNTER, *A History of Industrial Power in the United States, 1780-1930*. Vol. II: *Steam Power*, Charlottesville, VA, University Press of Virginia, 1985, pp. 732.

Louis Hunter, the dean of American economic/technological historians, has added the second and final volume to his monumental *History of Industrial Power in the United States*. This volume on steam power is a superb complement to Hunter's 1979 work on waterpower, which is already considered a classic. In many ways, *Steam Power* is an even more valuable addition to the historical literature on industrial development.

Hunter's focus is once again on the nineteenth century and on the many contributions of a prime mover to the complex process of industrialization. He shows in considerable detail how and why the technology of steam power evolved on both sides of the Atlantic. Having already demonstrated the crucial role of waterwheels and hydraulic turbines in "the century of the steam engine," he is free to concentrate on the engines that have always fascinated him. We should remember that one of the early highlights of Hunter's long and distinguished career as an historian was the publication of *Steamboats on Western Rivers*.

The historiography on steam power is rich with works on the achievements of Newcomen and of Boulton and Watt. So much attention has been focused on these English pioneers that few people recognize the inherent limitations of the atmospheric or low-pressure engines which they developed. Low-pressure steam was, in fact, a technological dead end, a safe but relatively weak source of power that held the attention of many English engineers for too long. Hunter is not seduced by the smooth and stately motion of the great Watt engines. His heroes are the men who first harnessed the frightening power of high-pressure steam and those who found ways to use it efficiently.

It may come as a surprise to staunch Anglophiles that many, perhaps most, of these men are American. Hunter places Americans Oliver Evans, George Corliss, Charles Porter, Henry Worthington, Erasmus Leavitt, and Edwin Reynolds in the first rank of great steam engineers. The shift from the use of low to high-pressure steam for industrial power (urban pumping applications are included here) was much more rapid and widespread in America than in Britain. Hunter does not ignore the work of Richard Trevithick, Jonathan Hornblower, Arthur Woolf, and other British innovators in high-pressure technology, but he finds the main line of development on the other side of the Atlantic. Hunter concludes that "For all its origins in eighteenth-century Britain and its early introduction from there in the early 1800s, the advances in the technology of steam power, alike in stationary and mobile applications, were for more than half a century almost entirely an American response to the distinctive conditions and requirements of this once colonial extension of Great Britain."

High-pressure, non-condensing engines were smaller, cheaper, and easier to manufacture than the Boulton and Watt forms with their separate condensers.

Evans and Trevithick developed the high-pressure engine independently and almost simultaneously at the beginning of the nineteenth-century. Although Evans' first engine was for a manufacturing process, he soon saw his high-pressure concept widely adopted for western steamboats. Because of the great abundance of waterpower sites in America, extensive use of steam engines in manufacturing was delayed until mid-century. By that time, American engine builders had made great advances in the design of high-pressure steam engines.

Hunter takes his study of steampower beyond the pioneer period and into the era of the reciprocating steam engine's greatest value, the second half of the nineteenth century. Here the body of historical scholarship on industrial power is thinner, and his contribution is most welcome. He gives much-deserved attention to George Corliss, whose development and promotion of the automatic variable cutoff engine brought a higher level of efficiency to power generation and made possible close control of operating speeds. Corliss was one of the creative engineers who saw great possibilities in the "expansive working of steam." Designers tried in various ways to reduce the waste caused by exhausting steam that still had useful pressure at the end of a piston stroke.

As he does in all his books, Hunter clearly identifies the trends and patterns in the material he presents. He finds five important trends in steam-engine design during the nineteenth century: the use of (1) higher pressures, (2) expansive working, (3) variable cutoffs for the admission point of steam, to improve expansive working, (4) higher operating speeds, and (5) compounding. The last was also a type of expansive working in which steam worked first in one cylinder at high pressure and then in one or more additional cylinders of increasing size at decreasing pressure.

The author's coverage of developments in steampower is international in scope and is not limited to mill engines, or even to engines in general. This is really a study of a steampower *system*, and it has a much wider focus than most readers will expect. Hunter examines steampower within the broader contexts of industrialization and urbanization. He is concerned with the entire power plant, including the boilers that most historians ignore. He is also in marine and stationary applications and his attention to the transatlantic diffusion of steam technology are other examples of the systemic nature of his work.

One of the most impressive chapters describes the process of engine building "before Corliss." Hunter gives us wonderful descriptions of hand filing and cold chisel work, while demolishing the idea that highly-mechanized production came rapidly to this heavy industry. Even after the 1850s, when machine tools were common in engine and boiler shops, hand methods persisted. Hunter's coverage is not as thorough on engine production in this later period, when the "American system of manufacturing," with its emphasis on interchangeable parts, began to have an effect.

The only serious weakness in this fine work is that it provides little information on the installation and operation of steam engines in their industrial settings. Perhaps Hunter was going to cover those topics in the final volume he

had projected: "The Transmission Revolution in Industrial Power: 1850-1930." He died unexpectedly in 1984, before he could finish the entire series.

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J. JACKSON, *The Politics of Depression in France, 1932-1936*, Cambridge: Cambridge University Press, 1985, pp. x, 303.

Faced in late 1931 with unmistakable signs that France, too, was going to suffer the onslaught of depression, her rulers should have devalued the franc, detached the franc from the gold standard, and worked to give the French public the impression that, whatever the political drawbacks of France's revolving-door ministries, the economic and financial leadership of the country was in good hands. It all seems so obvious now. What has puzzled historians, from the collapse of the Popular Front to today, is why — until 1936 when it was too late — France's governments did none of the above; with the result that the country, which had withstood the depression between 1929 and 1931, was then condemned to seven years of economic stagnation and increasingly corrosive social unrest. Julian Jackson's excellent book attempts to tell us the reasons.

Dr. Jackson is careful to explain in his Introduction that his approach is that of an historian not an economist, and that he has restricted himself almost entirely to the political debate over economic policy. Those who use this book, however, will quickly be reassured — from the ease with which he handles economic and financial data and economic and financial concepts — that he is quite at home in using those components of an economic historian's explanations.

The organization of this work is partly chronological and partly thematic. In Part I, "The Background," Jackson's object is to explain why policy during the 1930s was so deeply constrained by experiences between 1924 and 1926. At that time the anguish with which France reacted to the 1926 devaluation of the franc forced the parties of the Left (which might have been expected to promote policies of economic expansion during depression) to commit themselves, virtually irreversibly, to never again undertaking drastic monetary "amputation." Part II deals with reasons Right and Right-Centre governments between 1931 and 1936 were so tragically committed to budgetary, price, and monetary deflation as the main mechanism for dealing with the depression. Part III takes up what Jackson calls "non-conformist" alternatives put forward by Gold Bloc advocates, by *planistes*, and by "super-deflationists" for dealing with the Depression short of devaluation. Almost all the French — including virtually all Socialist and Communist leaders — thought of devaluation as a terrible mistake.

Crucial to understanding this complex picture is the mish-mash of ideologies, party traditions, and interest group needs that went into the Radical party's

make-up. Legislative majorities these years, and therefore governments, were impossible without the participation — or at least the parliamentary complaisance — of this large and inchoate agglomeration. While Radical party congresses repeatedly intoned their belief in “the death of the free market,” and the consequent need for an “*économie dirigée*,” they could never bring themselves to endorse *any* tough, comprehensive set of economic directives for dealing with the depression. This left the party, and governments which depended on it, particularly vulnerable to groups like rentiers, small businessmen, bureaucrats, and others who gave the party votes but had their minds dead set against devaluation.

There are so many useful insights and other attractive features of this work it would be impossible to list them all. Jackson shows, for example, that the Treasury and the Bank of France, though of course they reacted to every expansionist economic initiative by condemning it as an attack on the budget, were not a “*mur d’argent*” against which the French people hurled themselves in vain, but rather were interpreting quite faithfully the visions of France’s political leadership of the Right and Centre (plus many on the Left). Jackson’s terminal date is September 1936 (the doomed Popular Front devaluation); but enough of an epilogue is provided to take the reader very capably all the way to World War II. The intricacies of Blum’s self-imposed role as only an interpreter of socialism, his inability (or refusal) to innovate bold new socialist economic policy, are admirably explained here. The book also has the best explanation I have seen of the Communist rationalization for rejecting devaluation. Jackson also proves beyond a doubt what seems so hard for us to understand — that the political leadership of the Left as well as the Right did indeed believe — in the trough of the depression! — that a devaluation would bring about a disastrous inflation along the lines of 1924-26.

All through his book, Jackson’s use of quotes from speeches and writings by his key protagonists gives the reader many illuminating insights into their reasoning and their values. And he has accomplished remarkable feats of interpretive scholarship. In addition to working through newspapers, parliamentary records, journals, and all the massive amounts of secondary literature on his subject, he used dozens of archival collections, many in private hands. All his points are meticulously and extensively footnoted, an unusual performance for a work by a British scholar.

Some features of this book might be improved in a second edition. There is not enough here on how France’s “collective social reflexes” grew out of her economy’s strong performance in 1929-31 as well as out of the near-disaster of 1924-26. The extensive passages on whether or not the French knew enough about New Deal or Keynesian (pre-1936) economics to have made any difference seem out of proportion to their significance in this study. And I feel that French hopes for economic help from the Gold Bloc or from better handling of the imperial economy are slighted here.

On the whole, Jackson’s treatment of the French leaders responsible for

France's stagnation (and, at least partly, for her military failure in 1940) is remarkably restrained if not downright kindly. There are no villains here, not even Laval. In his conclusions Jackson raises the remarkable question of whether a devaluation, if it *had* come about in, say, 1932, might not have caused quite as much social disruption as anything we can observe from the harm caused by *not* devaluing before September 1936. It is true, he seems to be saying, that while France's economic agonies were in part inevitable outcomes of political "immobilisme" and mismanaged economic policy, they also resulted from bad luck in the form of quite understandable misinterpretations of the economic history of 1924-26. He never specifically claims that looking at the structures of France's political life is the only way to understand the failures of her economic policy in this era; but the evidence he presents for pointing the finger at political failure rather than at evils in France's economic or social structures seems conclusive.

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C. KEYDER, *The definition of a peripheral economy: Turkey 1923-1929*, Cambridge University Press, 1981.

This is an ambitious study. The author tries to do too much while asserting the modesty of his objective as represented in the title. A definition is usually derived from the observable characteristics of the defined, in contrast to the specification which begins by assigning a meaning or criteria by which an object is recognized. This study attempts both: it specifies a model of the peripheral economy and then uses it to define that type of economy as illustrated by the economy of Turkey between 1923 and 1929. The author is apparently testing the model, though in less rigorous ways than the word testing implies. The results support the contention that Turkey was a peripheral economy, as specified in advance.

According to the model, the relations between the periphery and the centre are based on the world division of labour, and those economies which enter the market lose their "internal articulation." [p. 1] The unit of analysis is the "entire capitalist market," and accumulation on the "world scale implies the extraction of maximum profits by capital..." [p. 1] Merchant capital initiates the peripheralization process by creating an export market "that produces commodities for the world market" as it begins to dominate the newly integrated pre-capitalist economies. [p. 1] Penetration into these economies would eventually lead to the creation of an infrastructure and a set of institutions which complete the process of peripheralization and incorporation of the periphery into the "world economy as a subordinate component of that unit" [p. 2]

That is what happened to Turkey, admittedly before 1923, but at this stage of

the process, (1923-1929), peripheralization was accelerated and entrenched by the "Open Economy" policies of the Ataturk regime. The process of peripheralization is consummated with the aid of credit, the manipulation of agriculture, and the adjustment of manufacturing to keep it in consistency with the peripheral status of the economy. Trade and the merchant capitalists are the main forces of the process. Data on the expansion and composition of trade, the changes in agriculture, and the continued backwardness of industry and manufacturing are presented as illustrations of the process, rather than as rigorous evidence of its existence.

The conclusion that Turkey was or became peripheral is not in question. In fact to suppose otherwise would be unwarranted. It is not evident, however, that Turkey became a peripheral economy in the period specified, nor that the observations of that short period would be sufficient to illustrate the peripheral status of Turkey. Indeed, it is possible to argue that Turkey (the Ottoman Empire) became progressively more peripheral beginning in the XVIIIth century, as the technological gap between its economy and those of the industrializing nations became more conspicuous. However, its peripheral or dependency status was virtually institutionalized in the 1838 and 1867 agreements which rendered the Ottoman economy more free and open to foreigners than those of the capitalistic centre. That status had persisted through 1923-1929; it probably persists through the present day.

In the "open economy" of the XIXth century the Ottoman rulers lost all control over their own economy. Agriculture, manufacturing, and trade were dominated by representatives of the centre, with the help of merchant capital, the capitalist governments, and the capitulations. Ataturk, of course, helped by complementing Turkey's economic peripheralization or integration in the capitalist market with cultural and socio-political integration. He adopted foreign institutions, such as the fashions, the script, the organization of the army, and the system of government, at the expense of Ottoman or Turkish institutions. The changes introduced were imitations which could only increase the dependence so long as the economy was on a lower technological level than the centre.

This brings me to the most glaring gap in the study, namely the little attention given to technology in the larger sense: as knowledge, as a source of productivity, and as the basis for competition in the capitalist market and domination of the weaker by the stronger. Why did technology in Turkey remain backward? What methods were used to prevent technical transfer? Why did the peripheralized Turks (and Ottomans before them) agree to the division of labour which kept them subordinate — even after independence?

The process of integration in the world market cannot mean that the natives should be passive; why were they? Was it possible to enter the world market and not be integrated in it? Did the centre countries also lose their "internal articulation" as did the countries of the periphery?

Other problems in the study are generated by the model itself. If the capitalist market is the unit of analysis, what role does the national economy

play? If merchant capital and profit are the main (objective) forces, why differentiate between native and foreign, or between members of the different religions? [pp. 6-9] Does integration in the world economy necessarily lead to dependence or peripheralization?

These comments notwithstanding, the study is important in that it may be the beginning of a series of serious and rigorous studies to answer the multitude of pending questions, regarding peripheralization in general and of the Ottomans and Turks in particular. I hope the author will face the challenge and proceed to fill some of the gaps in knowledge he has helped to identify.

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R. A. SOLOWAY, *Birth Control and the Population Question in England, 1877-1930*, Chapel Hill, North Carolina, The University of North Carolina Press 1982, pp. 418.

M. TEITELBAUM, *The British Fertility Decline; Demographic Transition in the Crucible of the Industrial Revolution*, Princeton, New Jersey, Princeton University Press 1985, pp. 269.

The social science traditions from which each of these books stems are sufficiently different that it would be appropriate to review them separately; considering them together, however, focuses attention on an issue that has been central to attempts to understand the fertility decline in Europe. Teitelbaum documents the timing and pace of the fertility decline in Great Britain: he calculates that in England most of this decline occurred between 1871 and 1931. Soloway's book shows that in England the decline occurred in the absence of any organized distribution of contraceptive information or techniques, and indeed in the face of the concerted opposition of most of the nationwide organizations that were accorded legitimacy to speak on the topic. By 1930, when Earl Russell exclaimed "the walls of the citadel had fallen" (Soloway, p. 316) the story of one of the major social revolutions was essentially over.

Teitelbaum's book is one in a series of monographs that constitutes the Princeton European Fertility Project. The aim of the project was to describe and explain the fertility transition in Europe over the last century, using the province (count in England) as the unit of analysis, and its findings are summarized in a recent volume edited by Coale and Watkins. In England, as elsewhere, the fertility transition was an unprecedented and irreversible decline in marital fertility. It is currently thought that the initial stages of the European fertility decline were characterized by the adoption of a specific kind of fertility control, the termination of childbearing before a woman's reproductive capacity was exhausted; in demographer's jargon, this kind of fertility control is called

parity-specific control, or family limitation. Age-specific marital fertility rates for England and some other European populations show an absence of family limitation in pre-transition populations; thus, earlier stopping can be considered to be an innovation. Although the requisite data are not available for England during the period 1870-1930, it is likely that the early stages of the fertility decline there were also marked by the rapid and widespread adoption of this particular behaviour.

The fertility transition in England was also similar to that in other European countries both in the timing of its onset and in the speed of the decline. Wrigley and Schofield have shown that marital fertility was rather stable for the two centuries between 1600 and 1800 (after which the data available to them are inadequate for their analysis). Since once a 10 percent decline in marital fertility had occurred the decline was not reversed until very low levels of marital fertility were reached, the data of the 10 percent decline are a convenient marker of the onset of the transition. Teitelbaum shows that the first county in England to experience a 10 percent decline was the West Riding, in 1886; the last county was Monmouth, in 1902. Considering national measures of marital fertility, by 1891 marital fertility had fallen by 10 percent of its 1871 level; by 1921 marital fertility had fallen to 55 percent and by 1931 to 43 percent of the 1871 value. Compared to the rapidity with which marital fertility has fallen in some currently developing countries, this is a relatively leisurely pace; compared, however, to the centuries of stability in marital fertility which preceded it, it appears rapid.

Most recent interest in the fertility transition has been directed at understanding why it occurred, and Teitelbaum's is no exception: it fits comfortably into the work of other social science historians in its emphasis on quantitative explanations. Soloway's work permits us to consider more carefully *how* it happened. Drawing from another historical tradition, one which relies primarily on qualitative information, he documents the role of the birth control movement. Despite the early attempts by contemporaries to explain the declines in fertility by changes in the physiological capacity to reproduce, by the turn of the century it was clear to some (and by 1916, when the National Birth-Rate Commission issued its report, to most) that the declines were due to the deliberate limitation of childbearing within marriage. Given the paucity of information on contraceptive use in these decades, Soloway wisely does not attempt to evaluate the relative importance of the various methods available. But it would seem that withdrawal, abstinence and abortion were the most important, with a lesser reliance on barrier methods such as the condom, the sponge, and the various types of pessary.

Those who publicly advocated the use of the birth control within marriage were in few number. Because Soloway begins his story with the formation of the Malthusian League in 1877, he ignores earlier advocates such as Francis Place and Richard Carlile. Members of the working class themselves, they took as their mission the dissemination of birth control information to the masses, and in some of their writings (for example, the handbills distributed by Place in

1823) explained in detail the use of the sponge and withdrawal. For decades the Malthusian League was the only *organization* in England advocating birth control. Their interest, given prominence in Soloway's title, was in population. Like Malthus, they believed that only by voluntary restraint of procreation could individuals (and in the aggregate, populations) improve their material well-being; they differed from Malthus primarily in finding methods other than delayed marriage acceptable (thus the term Neo-Malthusians). They did not, however, offer how-to-do-it advice until 1913.

The Malthusian League was, I think, most interesting for the opposition that it provoked. Soloway devotes a chapter each to the objections of the spokespersons for labour, for religion, for medicine, and for women. With the exception of a relatively few individuals, most shared a belief that interference with the natural consequences of sexual activity was immoral; underneath their objections, one gets a sense that many felt that were couples able to indulge their passion without fear of pregnancy, unbridled excess would be the consequence. To this general view, each group added objections that stemmed from their own interests. Socialists and leaders of the labour movement argued that to view poverty as ameliorable by individual effort, as the League did, was not only incorrect but would divert attention from collective action. The medical profession added that the predictable result of contraceptive use was unbridled passion, which would result in physiological enervation. The bishops of the Anglican communion, meeting at the Lambeth conference in 1908, called upon "all Christian people to discountenance the use of all artificial means of restriction as demoralising to the character and hostile to national welfare", buttressing their arguments with the testimony of doctors that preventive practices would lead to "serious local ailments" as well as to moral decay (Soloway, pp. 99-100). In general, delayed marriage and abstinence were considered acceptable, mechanical methods of birth control unacceptable, and most spokespersons demonstrated ambivalence about withdrawal. Some individual feminists supported birth control publicly, and many more presumably practised it privately; feminist organizations "did not object to family limitation, but they considered it an inappropriate, divisive issue that could only weaken their struggle for the vote and for greater economic and educational opportunities" (Soloway, p. 134). In addition they too feared the consequences of passions unleashed by artificial preventatives: "...prudent family restriction based upon continence and self-restraint not only conformed to popular morality but to feminist sensibilities as well" (Soloway, p. 135).

Only after World War I did the opinions expressed by leaders of these groups change. During the 1920's, feminist and labour organizations increasingly expressed open support of family planning. The Anglican Church and the medical profession followed suit, but somewhat later. A conference of bishops in 1920 (also held at Lambeth Palace) reaffirmed the conclusions of the 1908 conference; not until 1930 did the Anglican bishops sanction the use of contraceptives in limited circumstances, conceding that "in those cases where there

is such a clearly felt moral obligation to limit or avoid parenthood, and where there is a morally sound reason for avoiding complete abstinence... other methods may be used, provided that this is done in the light of Christian principles" (quoted in Soloway, p. 252). Even then, however, they added that the preferred form of limitation was abstention. In the same year, the Ministry of Public Health agreed that the medical officers in their maternal and child health clinics could provide contraceptive advice in limited circumstances, and that medical schools began to instruct students in contraceptive use.

In the context of Teitelbaum's dating of the decline in marital fertility, Soloway's story is provocative. By the time that the major opponents to birth control had capitulated, marital fertility was already low in all of the counties of England as well as in virtually all social groups, including the working classes. Soloway concludes, rightly I think, that it would be hard to credit the small Malthusian League directly with responsibility for the decline of fertility in England. More interestingly, the vocal opposition to the Malthusian League also appears to have had little direct impact; their condemnations of birth control may have slowed but it certainly did not stop the limitation of childbearing within marriage. (Indeed, Soloway notes that whatever members of the medical profession and clergy were saying publicly, the 1911 census shows that their family sizes were smaller than average).

In accounting for the onset of marital fertility control, social and economic factors clearly played an important role. Teitelbaum can account statistically for a substantial part of the variation among counties in the timing of the onset of the fertility transition. He proposes five alternative models, and uses step-wise regression to evaluate their explanatory power. He distinguishes between a set of variables that capture economic and social circumstances, and those that attempt to capture a more broadly cultural context. In the first are included education (proxied by the ability to sign the marriage register), urbanization (the proportion living in towns of 20,000 or more), occupational structure (the proportion in agriculture, manufacturing, civil service, professional and commercial, and female domestic occupations, as well as the proportion of women in the non-domestic non-agricultural labour force), general economic conditions (per capita income and level of employment) and infant mortality. These provide the basis for the "transition" model, and a second model includes these variables plus the proportion in coal mining. The third, "cultural", model tests the explanatory power of an index of ethnic diversity, the proportion of Celtic speakers, the proportion born in England, Wales, Scotland, Ireland or France, the proportion born in the county of enumeration, and measures of religious affiliation for 1851. A fourth model includes these cultural and ethnic variables and the proportion in coal-mining, and the fifth model combines all the variables. In the best of all possible worlds, other variables would be desirable, but Teitelbaum has chosen well from among those available; given the constraints imposed by the data, his analyses offer fairly strong support for the role of social and economic change in the fertility transition in England. Male literacy,

proportions of females in the modern occupational sector, and urbanization, he concludes, show a consistent relation with the date of the 10 percent decline, but other key variables, such as the level of infant or child mortality, the occupational structure (including the proportion in coal-mining) do not. In England and Wales the "cultural" variables generally explain less of the total variance than do the socio-economic variables but they are more adequate when Great Britain as an aggregate is analyzed (Teitelbaum, pp. 182-183).

It is likely that the controversy between Neo-Malthusians and their opponents, waged in the courts (notably in the Bradlaugh-Besant trial of 1877), in church pulpits, in editorials in the *British Medical Journal*, and in the public press also played a role in the fertility transition in England and Wales. In a 1973 article, Ansley Coale pointed to three pre-conditions for fertility decline: (1) that fertility control be within the calculus of conscious choice, (2) that it be seen as advantageous to couples, and (3) that techniques are known and available. Most scholars now accept Himes's conclusion that in Europe methods of fertility control were always known, at least by some, before the fertility transition. Currently, the debate within the field is between those who emphasize the transition as stimulated by changing socio-economic circumstances that rearranged the relative costs and benefits of children, making diminished family size advantageous, and those who emphasize the precondition that fertility be within the calculus of conscious choice. The latter argue that although techniques of fertility control may indeed have been known by some, and used in pre-and-extra marital liaisons, or even to space births, their use *within* marriage to terminate childbearing was not considered licit; thus, what tipped the balance was the willingness to consider techniques of fertility control to stop childbearing before menopause. If parity-specific control of marital fertility was indeed unthinkable before the transition began, the public debates that the Neo-Malthusian League provoked probably did play a role in its adoption by some. The onset of the fertility decline in some groups precedes public debate, and was probably stimulated by changing social and economic circumstances; once the decline had begun, however, this debate may account for some of the spread of fertility control to other sectors of the society.

Although both proponents and opponents of birth control probably played a role in accounting for the decline, the importance of these groups should not be overemphasized. Teitelbaum makes rather little use of the findings of other examinations of the demographic transition in other countries, but they are useful in putting the English changes in a comparative perspective. Considering that mid-nineteenth century England was the most industrial and most urban of the European countries, it would be reasonable to expect that the decline would have been earlier in England than elsewhere. This is not the case: marital fertility declined first in France, and the timing of the English transition is similar to that in other European countries. Taking the more than 600 provinces of Europe together, marital fertility declined by 10 percent in more than half of them in the three decades between 1890 and 1920 (Coale and Treadway, 1986). In terms of

timing, and speed, then, there is nothing special about the English decline. The most obvious difference from other European countries is in the degree to which all parts of the country went through the decline at roughly the same time: England (but not Great Britain as a whole) was demographically more homogeneous during the transition than other European countries. Thus, although both the Neo-Malthusian League and its opponents may have played some role in legitimizing the adoption of parity-specific control of marital fertility, the European-wide nature of the fertility transition makes it unlikely that explanations that rely heavily on factors specific to England are critical to understanding the fertility transition there.

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P. THANE, G. CROSSICK, R. FLOUD (eds.), *The Power of the Past, Essays for Eric Hobsbawm*, London: Cambridge University Press. 1984, 308 pp.

As a rule, *festschrifts* are difficult to review. This collection of essays presented to Eric Hobsbawm on the occasion of his retirement from a chair in history at Birbeck College proves no exception to that rule. In this case, however, the editors did try to influence their contributors to follow a common theme, which, in the editors' words, "was the dialectical relationship between capitalism and its pre-capitalist heritage... the process by which, within certain limits, the pre-capitalist heritage shapes and informs economic, political and cultural change among all social groups, and within the society a whole" (p. 1). Like the historian to whom the book is dedicated, the essayists define the past in the broadest manner, taking structures, institutions, movements, ideologies, rituals, and vocabulary (discourse) as the province of social history. All of them also devote special attention, as editors put it, to how power is constructed, maintained, and wielded in different societies at different times (p. 2). As a group, moreover, the essayists are as cosmopolitan in their nationalities (North American, British, French, German, Hungarian, Polish) and in their subjects (English, French, German, Spanish, Central and Eastern European history) as Hobsbawm has been in his scholarship.

The essays can be separated into two clear groups, those which are based on primary research and those which are interpretative and draw heavily upon published literature. Three of the empirical essays focus on a similar theme, the impact of artisan culture and sensibility on the emergence and history of modern worker movements. Maurice Agulhon analyzes how "sociability" among French artisans in their cafes and clubs during the 1820s and 1830s created a common culture and a collective anti-capitalist mentality, which leaped into public life during the revolutions of 1830 and 1848. His essays clearly establishes the dynamic role of skilled workers in building trade unions in France. Similar-

ly, Jurgen Kocka describes the contributions German artisans made to establishing the labour movement in Wilhelmine Germany, a movement which, in Kocka's words, "was not primarily a defensive protest against threatening innovation and change, but... a movement of emancipation that fought not so much against progress as for a fair share of its rewards" (p. 112). And Richard Price shows how the traditions of English building trades workers produced a century of conflict with their employers, who sought to gain fuller control of the labour process in construction. These three essays are thus additional evidence of the vital role the most skilled workers played in forming labour movements. Joan Scott traces a different aspect of the culture of skilled workers, examining changes in how male and female clothing workers in Paris dealt with the relationships among gender, work, and family. She discloses how and why women workers lost their places as accepted and respected skilled members of the work force as well as essential contributors to the family wage economy between the 1840s and 1870s. Finally, Michelle Perrot, with great skill and subtlety, and Tamara Kaplan, more crudely and loosely, analyze respectively how French and Spanish workers used the ancient custom of mass public rituals to create a new workers' holiday, May Day.

The more interpretive essays are harder to classify and summarize. Two of them, F.M.L. Thompson's discussion of the English landed gentry, and José Harris and Pat Thane's treatment of the economic role of merchant bankers, examine the other side of the class equation, the values and behaviour of the ruling classes. Thompson demonstrates that landlords often promoted economic growth and that many were entrepreneurs as well as aristocrats. Harris and Thane credit merchant bankers with an equally dynamic economic role in the late nineteenth and early twentieth centuries and see them as occupying a place between the old aristocracy and the new bourgeoisie. These two essays demonstrate, in the words of Harris and Thane, that "'class' should be seen as open-ended and capable of unforeseen metamorphosis" (p. 229). The three essays on Central and Eastern Europe discuss an altogether different set of issues. Peter Hanak, for example, analyzes the age-old question in Jewish history of assimilation v. separatism, as he describes the history of Hungarian Jews. Ivan Berend provides a variation on the history of fascism during the interwar years. He suggests that the fascists proved effective politically because they never served as puppets in the hands of aristocrats or capitalists. Instead, throughout Central and Eastern Europe, the fascists modernized backward and partly backward societies in a manner which appealed to the masses. And, finally, Antoni Maczak argues forcefully that the Polish communists have fostered a new form of feudalism in that troubled land.

One essay defies easy categorization, Eugene D. Genovese's evaluation of Hobsbawm's scholarship. In it Genovese reiterates his demand that social history stress power and politics. And he credits Hobsbawm both for delineating internal divisions within the working class and for realizing that the state often acted independently of the "ruling" class, although it usually tended to

serve the interests of dominant groups. Hobsbawm, Genovese suggests, "implicitly recognizes the state as a terrain of class struggle... and as a political centre with considerable autonomy yet ultimately compelled to act primarily in the interests of the propertied class" (pp. 25-26).

Taken together, the essays are indeed a fitting tribute to Eric Hobsbawm. The authors share his appreciation of "the power of the past", the complexities of class, and the need for a more universal and multifaceted history. In this book in fact, the authors examine the European past standing on the shoulders of a giant of historical scholarship, Eric Hobsbawm.

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