

BOOK REVIEWS

BEIK, WILLIAM. *Urban protest in seventeenth-century France. The culture of retribution.* Cambridge University Press, Cambridge [etc.] 1997. xiii, 283 pp. Ill. £40.00; \$59.95. (Paper: £14.95; \$19.95.)

Popular protests in seventeenth-century France have had their share of historians prior to William Beik's detailed new and important book. Boris Porchnev and Roland Mousnier used these uprisings to debate whether the *menu peuple* of early modern France had the practical and mental capabilities to protest abuses and to demand change, or whether they were manipulated by urban mercantile and professional elites. Historians Yves-Marie Bercé and Perez Zagorin, employing sociological and anthropological approaches, analyzed these riots as somewhat repetitive, unsophisticated and even childish pre-political rites, and the protesters' agenda as a return to a mythical golden age. Beik's book offers some suggestive and convincing corrections to all of these views. Drawing on his extensive research in numerous cities and towns, and on *intensive* readings of the documents, Beik shows that early modern French town-dwellers had a clear understanding of governance, acted as political agents, and demanded concrete and practical measures to improve their economic and social positions. Beik names the people's political culture "a culture of retribution". This seventeenth-century culture of demanding a punishment for evils done to the community was distinct, and should be differentiated both from the religious riots of the previous century, analyzed, among others, by Natalie Zemon Davis, Barbara Diefendorf and Robert Descimon, and from E.P. Thompson's moral economy that shaped eighteenth-century crowd mobilization.

Beik classifies urban confrontations of the seventeenth century according to their degree of organization, from a momentary flare-up of anger to the consolidation of political parties. Such ordering of protests necessarily obscures the chronology of events. But this approach enables Beik to offer a typology of unrest, and to show the underlying cultural and social preconditions and assumptions that made urban protest possible. Violence and conflict, he argues, were endemic in early modern French cities. Cities and towns were ruled by small oligarchies, who protected their own economic and financial interests, while the majority of residents lacked political voice. Conflicts pitted these groups against each other, while each of the groups, in and of itself, was far from being a monolithic entity: artisans, merchants and workers created small sub-communities according to professional and neighborhood affiliations, religious loyalties and familial ties, while the ruling elites were divided by different degrees of power and wealth, and according to factions and economic interests.

Such environment gave rise to constant struggles for economic gains as well as for honor and respect. Using insights from anthropological literature, Beik argues that communal anger, just like that of an individual, was based on personal notions of honor and shame. Urban dwellers defended their group honor just as strongly as they protected their personal esteem, and defied authorities whenever they felt that their private space or rights were violated. Thus new taxes or new intrusions by external forces such as the king's representative, or unfair distribution of the tax burden by the city authorities themselves could ignite anger and indignation, followed by resistance. High prices, shortages, tax exaction, and the feeling that the authorities were betraying their commit-

ment to the community could easily spark a riot. Beik is right to point out that such outcries were not merely passive responses by hungry and poor masses. They were also political, in the sense that they articulated, by means of protest, a clear notion of disaffection with the ruling elites, and demanded, in addition to returning to a pre-riot situation, the punishment of those who violated the trust among the city's residents. Thus urban protest was a form of bargaining with the authorities, an opportunity for the voiceless masses to air their grievances and, hopefully, to improve their lot.

The culture of retribution covered a wide spectrum of acts, from benign and symbolic gestures to pillaging and burning houses, to brutal executions and mutilating of corpses. In all such events protesters were careful to target specific individuals, who, rightly or wrongly, were regarded as responsible for the violation: the king's emissaries and tax collectors, their financial backers from within the municipal elites, hoarders and alleged conspirators who collaborated with the intruders. *Partisans*, *traitants* and *gabeleurs* were the usual accusatory attributes of such individuals. *Gabelle*, especially, came to symbolize not merely a specific tax on salt, but "the ultimate assault on the community from the outside". *Gabeleurs* and their supporters within the community – those who housed them, helped them collect the tax, and supplied them with income assessments – were the ultimate traitors, and the protesters' anger was directed at them, at their property and at their agents.

More sophisticated protests demanded advanced preparation and weeks of sparring between tax agents, trade groups, anxious customers and municipal officials. Such larger uprisings were built upon pre-existing sub-loyalties of guilds and neighborhood associations, and in some cases the urban militia, and were led by people with organizational skills. But all urban protests were short-lived. Sooner rather than later the authorities intervened, put an end to the rioting, and punished agitators and organizers. Municipal elites were aware of their fragile position between the people they controlled and the central authorities whom they could not alienate. They, too, opposed many of the intrusive renovations of the growing centralized regime, and they, too, wanted abolition of new taxes. Therefore, they at first chose to let a popular riot run its course and then struggled to preserve their authority and prevent serious destruction. But the masses demanded retribution in addition to abolition, and the municipal authorities, who could not afford to lose control over the populace, sided with the king and implemented his repressive measures against the rioting *menu peuple*.

Urban uprisings were more likely to succeed, or, at least, last longer, when the people's own disaffection coincided with fractional fights within the municipal elite, or when the popular anger was linked to a broader constitutional protest. In these cases the factions or the entire local (and even princely) elite fomented popular unrest, and collaborated in early stages of public mobilization. Beik, however, insists and demonstrates that the people's fury was genuine and was not instigated by the factions or the elites. What did happen in these revolts was a "channeling" of popular protest by a faction, without even being able to control it totally. Urban protests that were backed by a political faction or by notables mobilized more resources, were better organized, and lasted longer. But when push came to shove the elites, again, sided with the crown, and distanced themselves from the unleashed dangerous forces of popular anger which they could not manipulate. Thus, even during the Fronde in Bordeaux, the most successful and best-organized urban revolt in early modern France, the municipal commercial elites, who had prepared people for rebellion and armed them, realized that the interests of the Ormée – the popular movement – were distinct from their own. Urban

authorities and the bourgeois merchants always needed the crown to protect their economic interests, while the crowd's demands for minimal level of subsistence and a fairer distribution of the burden of taxes threatened these very same economic interests.

Beik weaves microhistories or vignettes of specific urban disturbances into his narrative. These detailed reconstructions of events do more than just add flavor to the more theoretical sections of the book. They demonstrate the fallacy of the complaint that microhistory or historical studies "after the linguistic turn" are histories "with the politics left out". Quite the contrary. Beik's stories expand the traditional narrow definition of the political, and show that state building in early modern Europe cannot be told without paying attention to the political aspirations of the lower classes and without taking their forms of resistance into account.

Popular forms of protest, rather than seeking a mythical golden age, expressed very precise class interests and political demands. Rioters and protesters demanded, in addition to annulment of taxes and other intrusive policies, punitive retributions. As such, urban protests were dialogues not only about social issues but also about city governance. By taking seriously the content of the people's grievances and their symbolic actions, Beik resurrects their voices, and presents a very convincing image of early modern city-dwellers as active participants in urban and national politics.

Moshe Sluhovskiy

English population history from family reconstitution 1580–1837. [By] E.A. Wrigley, R.S. Davies, J.E. Oeppen [and] R.S. Schofield. [Cambridge Studies in Population, Economy and Society in Past Time, 32.] Cambridge University Press, Cambridge [etc.] 1997. xxii, 657 pp. £60.00; \$85.00.

Despite the size, complexity and density of this book, its central findings are simply distilled. The underlying empirical data for the study consists of the pooled results of 26 family reconstitutions. These can be roughly divided into manufacturing, rural, handicraft and retail, and "other" communities, and apart from the fact that no large urban areas are included the authors are confident that these places were representative of those communities not under the family reconstitution microscope. In subsequent demographic analysis, however, it is relatively rare for the results of all 26 reconstitutions to be used at once. Concerns over the different start and end points of individual reconstitutions, and over the quality of the reconstitutions, prompts the authors to divide them into four groups – those where evidence is reliable between 1580–1729, 1600–1729, 1680–1789 and 1680–1837. Just four communities yielded data reliable enough to allow them to appear in all four groups, while six could fit into one group only. These overlapping constituencies are used in different combinations to analyse the three key demographic mechanisms – nuptiality (more specifically age at marriage), mortality and fertility.

The mean age at first marriage was stable at just over 27 for men and just under 26 for women in each decade 1600–1609 to 1720–1729. Thereafter, marriage ages for both sexes fell, with the key turning point located 1740–1749. By the early nineteenth century men were marrying at 25.5 years, while women were marrying for the first time at 23.1. The latter fall equates to an increase of completed family size by one quarter. Within this general framework, as other commentators pointed out in the 1980s, the key experi-

ence was a fall in the proportion of people marrying late and a rise in the proportion of people marrying early.¹ Thus, the proportion of brides marrying at 30+ fell from 18 per cent to 10 per cent during the eighteenth century, and there was also a consolidation in the importance of the 20–24 age group. The downward trend in marriage ages was faster in proto-industrial communities than agricultural ones, but all except one of the communities did see a downward trend. The ultimate result of these experiences was a narrowing of the differentials in marriage ages between communities by the early nineteenth century.

Infant mortality also fell, declining from a peak of 190 per thousand legitimate live births in the period 1700–1749, to a low of 136 in the period 1800–1824. These movements chiefly reflected a decline in mortality risks at ages under one month, as endogenous mortality fell off substantially. Between 1825–1837, rates rose slightly. Similar chronological trends were to be seen in the mortality rates for age groups 1–4, 5–9 and 10–14. When smaller groups of parishes are aggregated into economic types, it is clear that manufacturing parishes bucked the national trend, recording rising rates of infant and childhood mortality, while agricultural parishes saw only moderate changes. The bulk of the fall in infant and childhood mortality at the level of the whole sample is thus to be explained by large falls in rates for retail and handicraft communities. Meanwhile, adult mortality appears to have moved in an opposite direction to that of infants and children. Life expectancy at age 25 increased between the late seventeenth century and 1750 (when it reached 36.6). It then fell and stabilized at around 35 years in the late eighteenth century, before rising once more in the nineteenth century even as infant and childhood rates were rising. Such movements in adult mortality take place against the backdrop of an inexorable fall in the maternal mortality rate, from a peak of 17 in 1650–1674 to a trough of 4.7 by 1825–1837.

There is perhaps little new in these estimates. Fertility provides more of a surprise. After almost two centuries of stability, marital fertility began to rise in each age range from 1750. For the age groups 25–35, fecund marital fertility was 4 per cent higher from 1750–1837 than it had been in the previous half century. For the age group 35–39 the rise was 9 per cent and for those aged 40–49 the rise was 31 per cent. These big rises at ages of 35+ are “the most striking feature of the fertility history of early modern England”, reflecting a small extension of the length of the childbearing period and a notable rise in the intensity of childbirth within marriage.² Overall, fecund marital fertility rose by between 6 and 10 per cent depending on period comparison and measure used, with little or no variation in the experiences of communities of different socio-economic type. Fertility rises of this magnitude would have had the same impact on completed family size as a year sliced off the female age at marriage.

The authors explore an enormous variety of other avenues, but this is the basic skeleton of English reconstitution results. Some may be tempted to wonder why it takes 622 pages to make these basic points, though, as the early chapters suggest, in a technical sense dealing with 26 family reconstitutions is no mean feat. Several other problems with content and approaches also spring to mind. Firstly, this summary arises out of a very close reading of chapters which are generally unwieldy, even for someone familiar with historical demography. The fertility chapter alone is 157 pages long, and because

1. J. Goldstone, “The Demographic Revolution in England: A Re-Examination”, *Population Studies*, 40 (1986), pp. 5–33.

2. Wrigley *et al.*, *English Population History*, p. 382.

of the way in which individual reconstitution parishes move into and out of observation it is often difficult to keep track of the changing constituency of the underlying datasets within and between chapters. Secondly, there is a very real sense in which readers will already be familiar with many of these conclusions given similar work on a dozen reconstitutions in the 1980s. One might thus argue that substantial portions of the dense discussion of the aggregated results which runs through each chapter could have been shipped to appendices without really affecting the tenor of the argument, leaving space for more detailed evaluation of the results of individual parish studies. Thirdly, the spatial distribution of parishes remains unbalanced. Most importantly, the underlying data do not encompass a single Lancashire parish despite the eighteenth-century population turmoil in that county. Finally, the authors apply socio-economic labels to communities based upon data contained in the 1831 census. This data may reflect long-term socio-economic type for some communities, but for rural industrial parishes before 1750 the label “manufacturing” is a false one. The authors acknowledge this. What they do not acknowledge is the fact that even after 1750 this approach to labelling may be misleading. Nowhere is this more apparent than in Birstall, where only 8 of the 14 settlements within the parish boundaries might accurately be described as proto-industrial in 1800.

These are relatively minor details. The key question is how we should characterize the volume in broad terms. This is a thorny issue, for there is a peculiar sense in which this book is both too late and just in time. The sense in which it is too late will be familiar to any of those who have read *The population history of England*.³ This review of the evidence from 26 family reconstitutions represents the meat for the bones discovered in 1981 – an attempt to highlight the variety of behaviour within the national population framework and, as the authors suggest, to supplement this national framework with data which could not be gleaned in 1981, such as age specific death rates. In this guise, the book is at least a decade too late. It addresses an agenda – the need to create pure demographic statistics whatever the cost – founded in 1981 and takes only limited account of how far historical demography has moved since then. A bibliography which spans just 12 pages in a 622–page volume is perhaps testimony to this.

In fairness, of course, the authors never claim to do more than provide the raw material which others can then use. The book equates to “laying the foundations and erecting the main fabric of a house but leaving the finishing and furnishing to another day”.⁴ However, in exploring all of the nooks and crannies of the data (many of which are already familiar) and in investigating all of the dark corners of historical demography, the authors have missed an opportunity. They conclude that many of the demographic intricacies revealed by family reconstitution data still wait to be explained. Quite true, but if they had collected and configured their data to look for explanations as well as patterns, and if they had devoted much less space to methodological and empirical minutiae and much more to exploring data at community and sub-community level, English historical demography could have been rocketed to new heights.

Paradoxically, this weakness in the finishing and furnishing department also ensures that the book appears just in time. The authors point to some new directions which might repay more detailed research. What effect did the Marriage Duty Act have on

3. E. A. Wrigley and R. S. Schofield, *The Population History of England 1541–1871: A Reconstruction* (Cambridge, 1989).

4. Wrigley *et al.*, *English Population History*, p. 5.

male marriage ages in the short and medium term? Why did women resident continuously in a community have almost double the risk of celibacy of those who moved? How do we explain the fact that corrected female first marriage rates remained exactly stable at 24 in the key decades 1780–1809? What was it that distinguished spinsters marrying widowers (who usually married in their late twenties) from ordinary spinsters? Perhaps more important are the avenues for further research which arise implicitly from these foundations. Why, for instance, should a late marrying group in the early eighteenth century decline so markedly after 1750? Did the Poor Law have anything to do with such an experience? How significant is it that between 1700–1749 and 1800–1837 female age at first marriage in Alcester fell by 4.2 years, while in Gainsborough the fall was just 0.9 years? Is it significant that in 1675–1749 infant mortality rates in agricultural parishes varied between 92 and 176, and that even as late as 1838–1844 the range was 80–154? While the authors fail to look at how mortality risks were manifested at familial level, more and more studies are observing the concentration of the worst mortality experiences in just a few families. Did falling infant mortality reflect the moderation of risk amongst this group of families at parish level, or did it reflect even milder risks amongst the vast bulk of families? Was rising marital fertility widely spread, or did it involve a change in the practice of just a core of local women? These questions are the future of English historical demography, and the fact that I am asking them suggests that the authors have achieved their stated aim. Thus, while this book will appear on reading lists and bookshelves because it would not do to be seen without it, read imaginatively *English population history from family reconstitution* could make a very real difference to the way in which we think about historical demography and a wide range of related debates on poverty, the nature of rural society, kinship, marginality and the nature of everyday life.

Steven King

Britain and America: Studies in Comparative History, 1760–1970. Ed. by David Englander. Yale University Press, New Haven [etc.] 1997, in assoc. with The Open University, Milton Keynes. xviii, 317 pp. Maps. £35.00. (Paper: £14.00.)

One component of the Open University Course, *Understanding Comparative History: Britain and America from 1760*, this collection brings together articles designed to illustrate and fairly represent “creative comparative research”. Major concern rests with the attempt “to broaden and deepen our understanding of the development of liberal capitalist society in two different settings”. Working upon the assumption that “cross-national comparison is most effective when the focus is upon specific aspects of particular social phenomena”, the editor organizes the articles into five thematic sections: Political Culture and Development; Economic Development; The City; Class and Class Conflict; and Gender, Citizenship and Welfare. The outcome is a study which, notwithstanding its limitations, will be useful to students in history, politics, sociology and gender studies.

The major achievement of *Britain and America* lies in the fact that it presents the reader with a genuinely comparative perspective upon aspects of British and North American society. Rather than descriptively setting nationally-based case studies side by

side under the guise of “comparative” history, all the thirteen explicitly comparative articles in this collection offer sustained and often demanding investigation of the nature and balance of similarities and differences between the two countries. Most successful in this respect is the final thematic section or part, Gender, Citizenship and Welfare, which embraces the essays by David Morgan (women’s suffrage), Ann Shola Orloff and Theda Skocpol (the origins of more advanced and extensive state-sponsored social welfare provision in Britain as compared with the “laggardly” USA), Skocpol and Gretchen Ritter (comparing “paternalist social policy accomplishments in Britain” with “maternalist policy breakthroughs in the United States”) and Anthony Badger (the more extensive development of state capacities in Britain during the 1930s). Mainly concerned (with the partial exception of Morgan’s piece) to demonstrate the superiority of, and to explain, differences between the two countries, these essays make thought-provoking attempts to incorporate politics and gender into what have traditionally been predominantly socio-economic explanations of Anglo-American trends in social welfare and social policy during the late nineteenth and twentieth centuries.

Stimulating comparisons and contrasts are, however, to be found in all the sections. These range, for example, from Ira Katznelson’s well-known theses of “split” class/ethnic worker consciousness in the United States as contrasted with the predominance of class in England, and Jeffrey Haydu’s examination of the competing strategies of defensive sectional bargaining and radical and inclusive “workers’ control” among metal workers in Coventry and Bridgeport (Part IV); to Mark Clapson’s consideration of “the significance of suburbanization to social change in Britain and the United States” and Clive Emsley’s study of the role of the police in industrial disputes (Part III); and the reproduction of Habbakuk’s (1962) and Saul’s (1967) seminal articles on, respectively, the economic effects of labour scarcity and the American impact on British industry (Part II). In addition, Part I includes a spiky contribution from Jonathan Clark – laying claim to the key importance of law and religion to the American Revolution; and, in the only article with a predominantly single-country focus, Mary Geiter’s and W.A. Speck’s challenging thesis that by the 1740s many colonists had already developed a cultural, as opposed to political, “consciousness of their own collective identity as Americans”. Leon Marshall’s 1937 essay entitled, “The English and American Industrial City of the Nineteenth Century”, and David Ward’s 1961 study of streetcar suburbs in Boston and Leeds complement the more recently published articles which dominate this collection. Finally, David Englander nicely sets the comparative focus and brings the various themes together in the introduction and provides useful overviews at the beginning of each section. Notwithstanding the authenticity of its comparative practice and the richness and diversity of its substantive scope, *Britain and America* does, however, suffer from a number of weaknesses. In the first place, and despite its thematic organization, there is no single unifying thesis or idea employed throughout the book successfully to link, and rigorously to explore and derive conclusions from, the similarities, differences and tensions which emerge in the course of the many subject areas and issues covered. Rather the reader tends, somewhat unsatisfactorily, to be left suspended in mid air, left free to draw his or her overall conclusions. Second, and equally frustrating, is the fact that the thematic sections are not congruent with the chronological focus of the book. For example, Part 1, Political Culture and Development, effectively ends with the American Revolution. The themes of Economic Development and Class and Class Conflict are largely confined to the nineteenth and early twentieth centuries.

Gender, Citizenship and Welfare mainly covers the period from the late nineteenth century to the 1930s.

And while one of the essays in the section on The City does bring us up to 1970, no one article or theme in the collection spans the entire chronological period covered by the book, 1760–1970.

Third, and in many ways this is the most damaging criticism, there is relatively little in this collection which is new. Notwithstanding the claim on the dust jacket that *Britain and America* offers “up-to-date analyses” and the editor’s statement that “several pieces have been specially commissioned for this volume”, there is a very real sense in which the collection as a whole is somewhat dated. This is certainly the case with the sections on Class and Class Conflict and Economic Development.

With respect to the ten essays comprising the other three sections, only those by Geiter and Speck and Clapson appear to be entirely new. Of the remaining eight, one, by Badger, is a more recent version of a conference paper presented in 1993. The other seven are either straight reproductions of (Marshall, Ward, Morgan, Orloff and Skocpol, Skocpol and Ritter), an abridged extract from (Clark), or a revised version of (Emsley) previously published work.

Fourth, conscious editorial policy “to omit all original footnotes except where a note qualifies a reference in the text” and to include full references for only “articles [. . .] specially commissioned for this volume”, means that in the majority of cases it is impossible to ascertain whether the author is *au fait* with the full range and depth of primary and secondary sources relevant to his/her chosen subject area. This is, of course, no slight matter in the demanding exercise of cross-national research. Indeed, the appearance is certainly given in some of the articles (for example, those by Katznelson and Haydu) that the author is more in command of the American than the British sources.¹

In conclusion, these weaknesses should not detract from the high quality research to be found in this book. Many of the articles pose very interesting, indeed original, questions to the material and offer stimulating and often controversial conclusions. As such they offer a rewarding and useful read to students and other interested readers, set the reader’s mind fully into analytical gear and should act as a spur to further research.

Neville Kirk

CRUZ, JESUS. *Gentlemen, Bourgeois, and Revolutionaries. Political Change and Cultural Persistence Among the Spanish Dominant Groups 1750–1850*. Cambridge University Press, Cambridge [etc.] 1996. x, 350 pp. £35.00; \$59.95.

Two levels of analysis may be clearly distinguished in the work of Jesus Cruz. First, a study of the elite of Madrid – not of the whole of Spain, as the title might suggest – by means of a statistical sample of 549 families which held important positions in the financial and commercial world, the state bureaucracy, the professions and the political

1. See, for example, Jonathan Zeitlin’s review of Jeffrey Haydu’s book, *Between Craft and Class. Skilled Workers and Factory Politics in the United States and Britain, 1890–1922*, in *International Review of Social History*, XXXVI (1991), pp. 275–277.

sphere during the period 1750–1850. The second level relates to the discussion of the model of the bourgeois revolution that has so far prevailed in the Spanish historiography of the period. The author considers both levels to be complementary – the first being the empirical basis on which the second is grounded – and argues (p. 6) that the central objective of his study is the revision of the existing paradigm of the bourgeois revolution, which he regards as a cliché (a theoretical model lacking empirical evidence). Thus, his argument will always refer, somewhat reiteratively, to that same point.

Splitting these two levels of analysis in this review might seem to be a somewhat artificial device. Nevertheless, there is a point to it, not just because the appraisal of each of them differs broadly, but also because their connection is not as self-evident as the author assumes. In fact, as is the case with the advocates of the paradigm that he criticizes, Cruz inserts an intermediate level between the empirical data and the theoretical conclusions, to frame his prior interpretations of what the bourgeoisie is and what is to be understood by the term revolution. Unfortunately, these interpretations are not clearly stated and, consequently, the reader has to infer them from the text.

The empirical study of just over 500 families is a valuable contribution to our knowledge of the Madrid elite during the second half of the eighteenth century and the first half of the nineteenth century. Although great political changes took place in Spain during those one hundred years (a fact the author highlights by defining the period as Spain's liberal revolution), what characterizes the development of these groups is persistence rather than change. All of them, merchant families (202 cases), members of the financial elite (27 bankers), bureaucrats and professionals (191 families) and politicians (129 individuals who held significant office during the first half of the nineteenth century), conformed to norms of behaviour that changed but little, and never in the abrupt and sudden way normally associated with revolutions. This persistence is reflected in their economic strategies, their social behaviour and even in their cultural attitudes (in other words, the *habitus*, as defined by Bourdieu).

Let us consider first the economic field. The fortunes of these sections of the elite – inherited in most cases – did not suffer substantial changes in distribution throughout this hundred-year period. Even more significantly, there are no signs of an increase in industrial investment, which remained negligible in comparison with other forms of investment such as government stock, domestic goods or the purchase of urban or rural property. Social behaviour did not go through significant changes either: the members of the dominant groups – a high percentage of whom came from families already well-off during the *ancien régime* – were able to join the elite, shape it and remain within its bounds, thanks to ties of family and friendship, the protection of powerful patrons, or the solidarity which derives from sharing common geographical origins. All of these account for the importance of kinship, which exceeded the strict limits of the nuclear family, and matrimonial strategies, which often resulted in “marriages of convenience” where free choice was disregarded – a practice contrary to the “individualistic affectivity” typical of bourgeois society.

Concerning the cultural sphere, the analyses of some of the families (particularly the Cabarrús family) reveal the contradictions between a public discourse, unmistakably liberal, and a private discourse in which the old traditional values gained the upper hand. Such old values – as a letter written by Domingo de Cabarrús to his son in February 1816 shows – materialized in a defence of the supremacy of lineage over egalitarian principles, the stress laid on compliance with social rules – as opposed to liberal individualism – or the upholding of social pre-eminence both in its symbolic aspects

and in those that Cruz defines using the term empirical (p. 201) and whose distinctive feature is the perpetuation of authority and patronage relationships.

The descriptions I have been summing up so far, together with the emphasis laid on the persistence of social practices, are the major contributions of Jesus Cruz's study. However, problems arise when the author goes on from empirical study to theoretical analysis. In order to deny that these groups, so deeply rooted in the past, amounted to a new social class capable of carrying out the "bourgeois revolution" that has been the subject of endless controversy, the author is compelled to tell us – albeit implicitly rather than explicitly – what he understands by the terms "bourgeoisie" and "bourgeois society". Although the text makes reference to the difficulty of defining the term "bourgeoisie", and goes so far as to consider irrelevant the discussion of whether such a term might be applied to the Madrid elite (pp. 268–271), there can be little doubt that the idea behind the author's argument is that the prototypical bourgeois is the modern, self-made capitalist entrepreneur (p. 116), of whom few instances may be found in the sample. Similarly, the only bourgeois society proper seems to be that which is ruled by the impersonal relationship imposed by the rule and by the power of money and not by the personal relationships, the bonds of loyalty and fealty, and the servile dependence which characterize traditional societies (p. 169).

The dichotomy reflected in such categories has little to do with historical facts. The impersonal relationship established by the rule and the power of money is not the only factor that lies behind the process that gave rise to the capitalist and "bourgeois" elites. Indeed, social origins, as well as ties of family and friendship, also played an important role (a fact which may even have applied to US society, as Wright Mills argued more than forty years ago). The differences between *ancien régime* societies and bourgeois societies cannot be explained by opposing familism to meritocratic social mobility or marriage strategies to "individualistic affectivity". Such dichotomies are only to be found in the most rudimentary sociological models, and, besides, it is never advisable, from a methodological point of view, to contrast a particular historical situation with a simplified sociological model.

It is precisely from an approach of this kind – which I consider to be misguided – that Jesus Cruz's general analysis derives. According to this point of view, the persistence of *ancien régime* social practices must lead us to the conclusion that no significant social changes took place during the period, let alone a bourgeois revolution furthered by an emergent and revolutionary social class. Legal changes were skin-deep and did not alter old society's mechanisms of social reproduction. Therefore, the whole period should be defined as an era of political revolution within the framework of social continuity and slow economic development (p. 276).

But that is too hasty a conclusion to draw. One gets the impression that, on formulating it, the author has become trapped by the opinions of the writers whom he criticizes, and particularly by the idea that a social revolution must necessarily be a "bourgeois revolution". Not being able to find the protagonists of the latter, he denies that the former occurred and, consequently, shows no interest in the social changes that took place during the first half of the nineteenth century: the end of the privileges of the nobility and of the seigneurial regime, the abolition of the *mayorazgo* and the tithes, the seizure of church properties and the establishment of free trade and industry. Such an approach overlooks the importance of all these radical (one may even say revolutionary) measures. Furthermore, it fails to explain why and how such political revolution came about; in this respect the author simply mentions a conflict between

sections of the elite, without giving further details, and the appeal the new liberal ideas had among the nobility and the bourgeoisie.

Better results might have been obtained by breaking away from the above identification. In my opinion, defining this process on the basis of the traditional Marxist framework of ideas concerning the bourgeois revolution, a framework which has increasingly fewer adherents in the field of historiography (even in Spain), is not a good choice. Unfortunately, in criticizing such a conception, Jesus Cruz seems to mistake a radical social change, which actually took place, for the concept which has been used to explain it so far.

Manuel Pérez Ledesma

PROTHERO, IORWERTH. *Radical artisans in England and France, 1830–1870*. Cambridge University Press, Cambridge [etc.] 1997. xvi, 424 pp. £50.00; \$74.95.

The author has studied the early labour movement for thirty years, initially the English one and now for some time also the French. In this book, his second, he concurs with the bulk of recent research in refusing to attribute the political radicalism of many artisans in England and France to the material situation or the artisan class. But at the same time he also turns against the tendency, following on from the linguistic turn, to decouple a discourse of radicalism completely from the living and working environment of its proponents. Rather, he attempts on the basis of a wide range of source materials (above all the extensively quoted labour press) to clarify what contribution the craft unions, certain forms of protest and socialization, etc., made to the emergence of a radical movement. In a similar meticulous vein he elaborates the contents of the radical creed, convincingly situates early socialist movements within the radical spectrum, and discusses knowingly the high value attached to education and the role of religious attitudes. In an area that has been so well researched, the resort to familiar material is unavoidable, but the author is able to offer new insights on many aspects, from the literary efforts to the affinity between homeopathy and radicalism.

That reading this book is still often frustrating is largely due to three factors. Firstly, the endless differentiations and qualifications bury the theses of the individual chapters. Secondly, the results are not brought together into an overall picture; too many observations remain “local and episodic” (p. 215). That is no doubt because, at the level of two nation states and on the basis of mostly printed sources, it is barely possible to grasp the interplay of workplace experiences, religious influences, scientific interests, associative organizational behaviour and political convictions. Concentration on the already prominent capital cities might have allowed an alternative research design. And thirdly, the study might have benefited from a more rigorous comparative perspective. It is true that the individual chapters contain a wealth of comparative explanations of, for instance, the repressive association policy in France; but these never seek to explain the emergence of an artisan radicalism in England and France, and merely refer to the framework conditions for the development of a phenomenon that is taken as given. Thus the author barely mentions the specific characteristics of the English artisan socialists or the French guild tradition, which have been the focus of a number of recent comparative studies. And the discussion of the languages of class virtually posits an

international language of radicalism, without even asking about the concretely intended class demarcations.

Yet, despite these problems, Prothero's book is a treasure of interpretive insights into a key political discourse from the mid-nineteenth century, which would-be specialists should take note of.

Friedrich Lenger

HEID, LUDGER. *Maloche – nicht Mildtätigkeit. Ostjüdische Arbeiter in Deutschland 1914–1923.* [Haskala, Band 12.] Georg Olms Verlag, Hildesheim 1995. 683 pp. Ill. DM 118.00.

As part of the great Polish labour migration and desperate to escape the persistent pogroms, East European Jewish workers from Galicia immigrated to the Rhenish and Westphalian industrial areas from the 1880s onwards. By the First World War around 30,000 had made the journey. During the war their numbers rose by more than 100,000, as a result of mostly coercive recruitment measures by the German Labour Agency in the German-occupied General-Government of Warsaw. Shortly after the end of the war around 150,000 East European Jewish workers lived in Germany. Most of them were exploited in the chemical, mining and construction industries. After the collapse of the November Revolution the East European Jewish workers became the object of barbaric nationalist-antisemitic campaigns and pogroms. Hence they could not and did not want to stay in Germany. Most of them emigrated to North America and the neighbouring countries of Western Europe. Those who stayed were forced to return to Poland and deported across the German-Polish border in the mid-1920s.

Until now virtually nothing was known about the “phased migration” of this proletarian minority, which despite its relatively small size became a dominant theme in the domestic political struggles after the war and became victim of a fatal interplay between nationalist-antisemitic propaganda and institutional antisemitism. When this minority was mentioned at all in the historical research, it was merely as a footnote to the migrant- and forced-labour history of the late Wilhelmine era and early Weimar Republic. This is surprising because its fate was a warning, marking a decisive stage on the road to the genocide of the Jews during the Nazi era. The Nazis' pogrom on 9 November 1938 had been preceded several weeks earlier by the deportation of the remaining members of the East European Jewish communities in Germany.

Ludger Heid has set himself the task of filling this gap in our historical knowledge and also of erecting a monument to the Polish-Jewish migrant workers. He sees their history as “Jewish history” of “an independent, original, proletarian or proletarianized social group”: “of Jews whose Jewish self-consciousness was rooted in the proletariat and in Zionism” (pp. 9f.). Beyond this central aim the study also focuses on the relationship between the East European Jewish workers and a hostile German society and the “complicated interdependencies with the German-Jewish community” (pp. 10f.). Furthermore, Heid wants to lay to rest the once again virulent antisemitic prejudice that Jews are a “work-shy” people, intent then as now on avoiding the hard discipline of factory labour. Slavery, not charity: that according to Heid was the underlying motivation behind Polish-Jewish day labourers, proletarianized artisans and small traders

leaving, under the formative influence of the socialist-zionist associations, the mass poverty and orthodox-religious rabbinical culture of the *shtetl* for the great German industrial centres.

For his study Heid has tapped an astonishingly wide range of source material. This broadly falls into two types. At one end are the files collected across the world of the Jewish sections which from 1915 were formed within the labour and social administrations in Germany and in the German-occupied East European countries and which survived within the German-Jewish associations after the war. At the other end there is the evidence from below, the oral, familial and administrative reminiscences and documents of the East European Jewish workers. In this context the author benefited from the fact that the Polish-Jewish minority like no other proletarian social group tended to bombard its official supervisors and tormentors with complaints and petitions, which were meticulously recorded by the police authorities and have survived in some municipal archives in the Ruhrgebiet. These finds are new and have enabled the author to make accessible a largely unknown and yet highly significant historical context, which touches equally on Jewish, Polish and German labour and social history.

This monumental study is divided into five major sections. The first section consists of a range of methodological, source-critical and historical introductions. They illustrate the East European Jewish emigration movement from all possible angles and lead up to an explosive first excursion, which casts a critical eye on the defence mechanisms used by the German Jews against the Polish-Jewish immigrants (pp. 64f.). According to Heid, the East European Jews were seen by many assimilated Jews as “ghosts from the past” who disrupted their own bourgeois integration and as “nobodies” at the lowest rung on the social ladder who provided excuses for antisemitism. Only the zionist associations welcomed the East European Jewish workers as allies in the fight for control of the Jewish communities.

The second section is devoted to the public observation and the labour-political “administration” of the Polish-Jewish immigrants (pp. 79f.). Here Heid presents a mine of information and makes important historical discoveries. Taking the example of the leading zionist official Julius Berger, he reconstructs how the German-zionist organizations were able from 1915/1916 onwards to establish themselves as intermediaries with the German Labour Agency, which was responsible for the mobilization of Polish forced labour, and to transform its wild violent actions of the early war years into an efficient and rational practice of official worker transfer (pp. 85f.). In another excursion (pp. 234f.) Heid proves that in the system of forced mobilization for the German arms industry the East European Jewish workers were pushed down to the lowest rung of tied working conditions, and that from 1920/1921, in an interplay of nationalist-antisemitic propaganda and official antisemitism, they were stigmatized, confronted with border closures, penned into concentration camps (this term was used as early as 1920!) and eventually expelled (pp. 162f., 192f.). There are also detailed analyses about East European Jews decrees (*Ostjuden-Erlasse*) of 1919–1923 (pp. 146f.), the relevant debates in the Reichstag and in the Prussian parliament (pp. 172f., 178f.), and the public identification of East European Jews as a supposed core of and actual participants in revolutionary movements (pp. 245f., 266f.), as well as case studies which impressively document the decision-making processes in the phased migrations (pp. 289f.).

The third section (“Welfare”, pp. 303f.) shows how the German-Jewish communities, associated since January 1918 in a “Workers’ Welfare Centre” (*Arbeitsfürsorgeamt*), tried

in vain to avert disaster and to grant the East European Jewish workers, who were excluded from the public unemployment insurance schemes, a minimum of social security.

The most detailed are the fourth and fifth main sections, in which Heid deals with the working conditions (“Work Environment”, pp. 343f.) and the everyday life of the East European Jewish workers (pp. 475f.). With the help of numerous individual examples, he reconstructs the recruitment practices, the dreadful discrimination at work, the ambivalent attitude of employers and trade unions, the struggle for recognition of religious holidays and cultural identity, the wage conditions and the relationships between forced tenementing and boarding. Impressively Heid shows how the East European Jewish workers defended themselves against the heavy piecework, the miserable food, the bullying treatment, the inhumane accommodation and the tight restrictions on their movements. Refusals to work and breaches of contract became a widespread phenomenon from 1917 onwards.

These findings, worked up almost entirely from individual records, do indeed erect a monument to the East European Jewish workers. But they also document in a depressing way how their indomitable rebelliousness reinforced antisemitic stereotypes among senior managers in the German arms industry – for instance, it was thought that spotted fever was a “Jewish disease” (p. 564) – and thus paved the way for the extermination syndrome evident during the Nazi dictatorship. In an impressive epilogue Heid compresses these findings into “three theses on the situation of the East European Jewish proletariat” (pp. 585f.).

With this monumental study Heid breaks new ground. What is more, the study is not so much a self-contained piece of research, more a mine of information for other researchers to explore.

It must be said, however, that Heid does not make things easy for his readers. The underlying concept is not systematically thought through. There are unnecessary repetitions in all the passages. Instead of a synthetic approach, the work is dominated by a form of presentation in which the interconnections are fragmented and re-examined from many different angles. The author also touches only sporadically on the history of Polish immigrant workers in Germany, which is close to his theme. By not doing so he avoids problems which could question his undisguised identification with the history of labour Zionism. At no point does Heid address the fact that within the Polish-Jewish labour movement there were also currents which pursued a revolutionary anti-war policy and which firmly rejected the cooperation between labour and socially active Zionism with the migrant- and forced-labour policies of German imperialism. In this respect Heid presents a closed world of labour Zionism, which did not see any problems in a tactical accommodation with the nationalist stance of the German labour movement and in cooperating with labour agencies completely subservient to the needs of the arms industry.

To discuss this further would not be appropriate at this juncture. Nor does it detract from the significance of this study. In this book Heid has charted a *terra incognita* for the first time. We can look forward eagerly to the reception of this contribution to an intercultural history of the European working class, viewed from the perspective of its most discriminated and persecuted constituent group.

Karl Heinz Roth

LASCHITZA, ANNELIES. *Im Lebensrausch, trotz alledem. Rosa Luxemburg. Eine Biographie*. Aufbau-Verlag, Berlin 1996. 687 pp. DM 68.00; S.fr. 64.80; S 503.00.

When Annelies Laschitza published her second biography of Rosa Luxemburg in 1996 after 25 years (the 1971 version was a collaboration with Günter Radczun), the “socialist” German Democratic Republic had disappeared. Released from her role of representative of the oppressive state, she was castigated by the eminent West German reviewer Volker Ullrich for not facing up to the “ideologically distorted representation of the past” (Ullrich, “Trotz alledem?”, *Die Zeit*, 1 March 1996, p. 30). He claimed she offered neither an admission of guilt – except for a “convoluted, insincere formulation” (Ullrich) – nor a feminist perspective.

Of course, prophets are never honoured in their own land. Laschitza, a Luxemburg researcher with a worldwide reputation, shows that she is willing and able to reassess both her own views and the main research preoccupations. She bases her work on many new unpublished sources, above all statements from Luxemburg’s friends and acquaintances, which create a profound and lively picture. The biography paints the life of Luxemburg (1871–1919) within a matrix of clashing personal and political desires and hopes. What emerges is a comprehensive picture of a woman who was active in both the Polish and German social democratic movements, who was a co-founder of the Communist Party of Germany (KPD) and who was murdered during the Spartacist Revolt. It is the author’s aim to create a new biography, which unites the personal and the political.

Elzbieta Ettinger, in her biography of Luxemburg (Bonn, 1990), reported in detail about Luxemburg’s childhood in Poland, but Laschitza is able to show that political events played a significant role in Luxemburg’s life from a very early age. Her strong sense of justice and her empathy with all oppressed and exploited people found expression through political activity even in her youth. Laschitza accurately recounts Luxemburg’s close relationship with the Russo-Polish labour movement and especially with the Social Democratic Party of Poland. Although, as Laschitza shows, Luxemburg was concerned above all with developments in the revolutionary movement at home during her student years in Zurich and stays in Paris, and although this was also the defining aspect of her early activities in the Second International, I cannot agree fully with the author’s contention that Luxemburg’s Polish roots influenced her very deeply. “In mentality and culture”, Laschitza writes, “Rosa Luxemburg remained, despite or rather because of her humanist cosmopolitanism and proletarian internationalism, a proud Polish Jew” (p. 62). There is no question that Luxemburg was a very proud woman, but it should be stressed – as her writings during the First World War illustrate – that her spiritual home was in the international labour movement and more generally in universal humanism.

Completely new is Laschitza’s emphasis on Luxemburg’s period as lecturer at the party school from 1907–1914. In the past these years were almost seen as a wasted time in political terms (see e.g. the Luxemburg biography by Paul Nettel, 1965), or as a frustrating period in her private life (Ettinger). But the author shows how Luxemburg was able to develop her political economic theory during this time. She highlights not only Luxemburg’s *The Accumulation of Capital* (1913), but also and above all the *Introduction to National Economy*, which is based on the teaching material. Its theses on the

need for a more just social order and on the development of colonialism and imperialism remain highly topical today, Laschitzka contends (p. 330). Although the interpretation of this almost forgotten text is refreshing, it must be pointed out that Luxemburg herself considered the completion of *The Accumulation* (as well as the critique of her critics, *Antikritik*, Gesammelte Werke, vol. 5) to be rather more important. What mattered to her was not only the theoretical development of Marxian theory, but above all political practice. *The Accumulation* was written because she saw a need for a theoretical foundation of developing imperialism, so that she would be able to revise the political principles (i.e. criticism of the SPD in the run-up to the war, emphasis on active revolutionary tactics). Laschitzka describes Luxemburg's anti-war struggle in detail and how she gradually lost the goodwill of the party executive, but the precise way in which linked theory and practice could have been better presented. Thus the interpretation of Luxemburg's attitude to the Spartacist Revolt remains unclear.

The author shows how during the war Luxemburg was able to maintain contact with her colleagues and friends from her prison cell. The formation of the Spartacus League was a sign of the insurmountable differences within the party, a majority of which continued to support the imperial war policy. She evocatively describes Luxemburg's moments of exhaustion and despair, which were brushed away with the outbreak of the Spartacist Revolt. But she does not discuss how irritating the whole situation was for Luxemburg: on the one hand there were huge anti-war demonstrations before the war, on the other hand the party executive stressed the duty to defend the homeland and the threat of invasion (see Wolfgang Kruse, *Krieg und nationale Integration*, Essen, 1994). Laschitzka does not appreciate – and this is very important – how for Luxemburg the conditions for revolution in Germany outlined in her writings disappeared during the war. Unlike many other Spartacists, Luxemburg did not overestimate the revolutionary situation in 1918 (see p. 607, and e.g. Luxemburg's speeches at the KPD's founding congress). For her the conditions for revolution were not fulfilled, because an influential revolutionary party which could have led the semi-educated working masses did not exist, and because the class-based agitation had been abandoned years earlier. It is true that the impact of the Russian Revolution could also be felt in Germany, but even at the time Luxemburg doubted the German revolution's chances of success.

The re-evaluation of the relationship between Luxemburg and Lenin gives an indication of Laschitzka's changed stance since the first biography. She describes how already at the start of the century Luxemburg tended to side with the Mensheviks, and how the differences with Lenin were significant in the debates on the role of the party and the masses, the right to national self-determination and the progress of the proletarian revolution. Although Laschitzka regards Luxemburg's pamphlet "On the Russian Revolution" as a critique of Bolshevik tactics, but also as an appreciation of the brave revolution in Russia, Luxemburg's fundamental rejection of the Bolshevik strategy deserved a more explicit presentation. The conflict between Luxemburg and Lenin is described well, and in this way the new political evaluation of Luxemburg's theory is also clarified. Laschitzka notes aptly how the party conceptions of Luxemburg and Lenin led to two different party types. Luxemburg's predictions "about the inappropriateness of Lenin's party type for the construction of socialism have come true with a vengeance" (pp. 203f.). Luxemburg's concrete observations on democracy and the dictatorship of the proletariat certainly underlined her humanistic outlook, according to Laschitzka.

Laschitzka has been able to paint a profound and very lively portrait of Luxemburg, in which her problems and preferences and her love of nature and for her fellow human

beings are brought out with sensitivity. She shows that for Luxemburg life was a totality, in which the inexhaustible beauty of existence and the promotion of the broadest possible freedom were interrelated.

This biography is not only a great boon for experts, especially Luxemburg researchers, but because of its still topical political themes it offers many fruitful impulses for the general debate on the nature and future of socialism.

Virve Manninen

BERMANI, CESARE, SERGIO BOLOGNA [und] BRUNELLO MANTELLI. Proletarier der "Achse". Sozialgeschichte der italienischen Fremdarbeit in NS-Deutschland 1937 bis 1943. Übers. von Lutz Klinkhammer. Mit einem Vorwort von Karl Heinz Roth. [Schriften der Hamburger Stiftung für Sozialgeschichte des 20. Jahrhunderts.] Akademie Verlag, Berlin 1997. 418 pp. DM 84.00.

HERBERT, ULRICH. Hitler's foreign workers. Enforced foreign labour in Germany under the Third Reich. Transl. by William Templer. Cambridge University Press, Cambridge [etc.] 1997. xxi, 510 pp. £50.00; \$79.95.

The publication of an English translation of Ulrich Herbert's standard work on compulsory labour in Nazi Germany fills a gap in the literature on the history of the Third Reich. Herbert's study has been preceded only by the book by Edward L. Homze, *Foreign Labour in Nazi Germany*, which appeared in 1967 and relied on but a small percentage of the rich documentation provided by Herbert who presents a comprehensive picture of the deployment of foreign labour by Nazi Germany, including an introductory chapter dealing with the experiences of World War I and the development of the German labour market under the strains of the emerging rearmament programmes after 1933.

In spite of the official ideology of the Nazi regime which was in principle directed against the deployment of foreign workers, the regime relied even in its early years on labour imports from the Benelux countries and Italy, but the use of foreign labour, particularly of enforced labour, climaxed during the later war years.

Herbert describes the different stages of the deployment of compulsory labour, starting with the Polish campaign when the use of Polish workers was installed on an experimental basis. He shows that the regime, due to its reluctance to break with its own principles of racial and ethnic homogeneity first tried to restrict the deployment to agriculture and the mining industry, thus achieving a certain social isolation of the foreign workers, but then deploying foreign labour from France, Belgium and even Germany's northern neighbours all over the industrial realm.

As compensation the regime invented a system of dissimulation and discrimination of the foreign workforce, partially in order to neutralize the harsh criticisms by the party hard-liners, partially for security reasons. Especially the Poles were exempted from the ordinary jurisdiction and submitted to special regulations, preventing any closer social contact with the German population, especially any sexual intercourse. Simultaneously the regime did not hesitate to exploit the Polish labourers by reduced wages and by excluding them from any social security payments and similar privileges of their

German colleagues whether they started as prisoners of war or had voluntarily signed labour-contracts.

This still rather civilized picture changed completely after the extreme labour shortage compelled the regime to admit, first, Soviet prisoners of war as workforce and, since October 1941, when these prisoners were no longer available because millions of them had died on account of hunger and diseases, to deploy Russian and Ukrainian civil workers, called *Ostarbeiter*. In the beginning, they were still hired on a formal voluntary basis, while later on they were forcibly collected, brought into transfer camps and transferred to the German industrial sites, where they used to live in rather primitive barracks strictly separated from the majority of the population and without the possibility of leaving their camp without being accompanied by official guards. After 1944 the regulations became less harsh and some improvement of the living conditions and the personal treatment came into being.

Herbert's book provides a systematic insight into the methods of deployment, deportation, the poor "housing" conditions, the wages and financial compensation, the totally insufficient provision with foodstuff and sanitary goods, the surveillance by the police and Gestapo units, the role of the labour education camps and the relations between foreign labourers and German workers in the workplace. With respect to the later war years, he displays the impact of Allied air raids, the emerging bartering and black marketeering, the increasing system of premium payments for those who were able to perform maximum outputs at the cost of those who were no longer able to do efficient work during the then fourteen hours workday and with respect to the lack of leisure time even at the weekends.

The book relies heavily on the experiences of the Krupp company, and therefore partly focuses on the metal industry, and partly also mining, while other industrial sectors, such as chemistry, the automobile industry and machine production, are less well explored. It can claim, however, to present a sufficiently representative picture of the fate of enforced foreign labour within the Old Reich, while the conditions in the occupied countries, especially in Poland, the militarily controlled parts of the Soviet Union as well as the conditions in France, are not included in this otherwise indispensable scholarly analysis, which contains a series of tables and statistics in order to get an understanding of the quantitative aspect of the deployment of foreign workers.

Herbert, however, does not describe the fate of Jewish compulsory workers, who were either concentration camp inmates and therefore do not belong technically to the specific group of compulsory labourers or were living in so-called privileged marriages which possibly did not exceed several thousand people. Furthermore, the deployment of Jewish concentration camp labour in German industry in the Reich after 1943 is not the object of his analysis, certainly on the grounds that they do not belong to the category of compulsory labour. Anyhow, this group was quantitatively, in relation to the about eight million enforced workers, not too significant and did possibly comprise not more than 100,000 men.

While Herbert provides an illuminating and extremely well-researched survey on the foreign workers' deployment, the publication by Cesare Bermanni, Sergio Bologna and Bruno Mantelli deals with the fate of the Italian workers in Nazi Germany and presents hitherto unknown empirical documentary evidence especially on their everyday experiences in Germany. The book contains three parts: the first is written by Sergio Bologna and presents a survey on the impact of workers' migration in Italy since the nineteenth century, in particular the interrelation between overpopulation, emigration and re-

migration. He shows that the fascist regime already stood in a longer tradition of exporting the Italian surplus labour force, which became more and more directed by official institutions.

Bologna, an experienced historian of Italian labour and protagonist of *operaismo* points out, that, on account of the long-standing migration tradition in Italy, a specific “culture of emigration” could arise which explains why the fascist regime did not have too many difficulties in complying with the accelerating German demands for labour migrants. In conjunction with the clarification of the historical background of labour migration, Bologna presents a detailed report on recent research developments and the origins of a special migration history which is closely connected with his scholarly initiatives as editor of the journal *Primo Maggio*.

The ensuing contribution by Bermani intends to describe the day-to-day experience of the Italian workers in the Third Reich. Starting with a survey of general workers' migration during the inter-war period, Bermani displays the increasing weight of the migration to Germany, including the workers of Italian descent who were living in France and shows that the higher payments in Germany were the main reason for the fact that to work there, at least for a certain while, appeared to be extremely attractive, which was significantly corroborated by the widespread unemployment and part-time work particularly in the rural areas.

Bermani writes in a rather essayist way by choosing the perspective from below, quoting throughout reports of Italian labour migrants dealing with their individual experience, work conditions, wages and accommodation, the food (which was all the time the outstanding element of complaints) and the treatment by German colleagues and superiors. His description offers some insights into the motivations and reactions of the Italian workforce in the Third Reich, of disciplinary problems, irregularities in the transfer of the wages to their relatives at home, partly because of the bureaucratic behaviour of the German banks and the customs officers, the conflicts with German employers and fellow workers, and also the penal system with respect to the treatment of Italians and the respective intervention of the fascist state. Moreover, he depicts the increasing use of coercion in order to prevent the habit of many Italian workers of leaving their contract and choosing a more convenient employment.

Although Bermani's contribution throws new light on the living conditions of the Italian guest workers, it does not show precise results and consists of a multitude of individual testimonies and reminiscences without any consideration for the extent to which the respective details can claim to be representative. This means that the multitude of details and subjective reminiscences, being the outcome of systematic use of oral history, only resembles a collection of documentary material, and this is therefore more or less disappointing.

The third part of the handsome publication, whose German edition was published by the Hamburger Institut für Sozialgeschichte and excellently translated by Lutz Klinkhammer, himself a specialist for the German occupation of Italy after July 1943, provides a systematic history of the German deployment of Italian workers during the period between 1938 and 1943 by Bruno Mantelli. He presents an illuminating and brilliant picture of the topic, and thus compensates for the lack of methodological strength and the historiographical shortcomings of his co-authors.

After displaying the main obstacles against the deployment of Italian agrarian and industrial workers Mantelli's analysis is focusing on the increasing pressure of the Reich government on its Italian ally to expand the workforce in Germany even if this was

accompanied by a certain labour shortage in Italy. Mantelli provides a clear picture of the difficulties of the German partner to cope with the Reich's negative balance of payments which actually resulted in the phenomenon that even the wage costs had to be borne by the Italian government. This phenomenon helps to explain why the increasing pressure of the fascist regime, and in the last run Mussolini himself, to take the Italian workers back was reluctantly accepted by Hitler, although the German officials did what they could to obstruct the respective Fuhrer order.

Mantelli, who is well known for his outstanding contributions on the Italian workforce in Nazi Germany and the origins of Italian fascism, appears to be the genuine expert on this subject, and his contribution seems to be indispensable if one tries to study the German-Italian relations during the Second World War. His study is signified by a tremendous knowledge of the archival sources, by an admirable precision of his argumentation and by a remarkable literary quality, displaying it as a model for its combination of social and political history.

Hans Mommsen

The Hungarian Revolution of 1956: Reform, Revolt and Repression 1953–1963. Ed. by György Litván. English version edited and translated by János M. Bak [and] Lyman H. Legters. Longman, London [etc.] 1996. xv, 221 pp. £12.99.

This book is an important contribution to the considerable historiography on 1956, for at least two main reasons: it is the first assessment ever of the Hungarian Revolution which relies on a whole range of previously classified documents that have become available in the last decade or so; and it is the first scholarly account written by Magyar historians living in their own country and freely published in Hungary. The original edition dates back to 1991; it was intended as a high school textbook.

The authors' background deserves to be mentioned in some detail. The editor, György Litván, has been the director of the 1956 Institute in Budapest since its founding in 1989 and belongs to the older generation of historians who were trained in the years following World War II. Along with three other co-authors, namely Bak, Hegedüs and Kozák, he actively took part in the Revolution and, under difficult circumstances, contributed to keeping its memory alive until 1989; he and Hegedüs even served a prison sentence in the late 1950s. As for Csaba Békés, János M. Rainer and the four assistants, they were all born after the events covered in the book and were strongly influenced by the revival of 1956 – animated by Hungary's democratic opposition from 1979 onwards – which significantly contributed to the toppling of Kádár's communist regime in the late 1980s. The volume concludes with contributions from two well-known Hungarian émigré scholars: a historical introduction by George Schöpflin, and an afterword by Pierre Kende, who in 1994 became chairperson of the 1956 Institute.

The Institute itself represents something rather unique in central and eastern Europe, with hardly any equivalent in Poland, the former Czechoslovakia, Romania or Bulgaria. This is not to suggest that in countries other than Hungary the main issues of contemporary history, particularly of the communist period, are not being thoroughly and scholarly researched; but rather that there are no institutions linked to any such circum-

scribed event. This leads us to the conclusion that the 1956 Hungarian Revolution does indeed represent a watershed not only in the history of the Soviet bloc and the international communist movement (“the Hungarian source”, as the late Franco-Greek philosopher Cornelius Castoriadis called it) but also, as István Bibó¹ first noted, the main founding act of modern Hungarian democracy, one of the most important events ever in the eleven-century-long vicissitudes of that country.

When founded in 1989 the 1956 Institute proclaimed itself the successor to the Imre Nagy Institute for Political Sciences, which had been set up in Brussels in 1959 – a year after the execution of the former prime minister – by a group of Hungarian émigrés. They were mainly intellectuals who had been part of the reform communist group supporting Nagy ever since his first premiership in 1953–1954 and throughout his marginalization in 1955–1956 and the revolutionary uprising, until the latter was brutally suppressed by the Soviet Union.

The Nagy Institute, which ceased its activities in 1963, had developed – chiefly through its trilingual quarterly journal² – important analyses of the Revolution, presented as a mass movement largely inspired by democratic socialist principles; one that had radically criticized and then destroyed Hungary’s Soviet-style bureaucratic regime by creating an alternative based on freedom, independence and a multi-party system, along with an impressively extended network of workers’ councils and national committees demanding social control of the means of production.

The importance of the book edited by György Litván lies in its being the product of such a rich experience, along with its taking into account all previously published material on the 1956 Revolution. For the first time, the deep complexity of this event is analysed in all its aspects and in a broader context: from the failure of the Stalinist model imposed by Rákosi to the intertwining of power struggles in the Soviet Union, the GDR and Hungary after Stalin’s death in 1953; from the combination of intellectual and workers’ rebellions to the intricacies of world politics during the Revolution’s quest for independence; from the impact of the fight for freedom on Hungary’s neighbours to its short- and long-term consequences on the international communist movement.

Among the many recently declassified documents that were consulted when preparing this study, one should first mention the Hungarian ones. In fact, Magyar historians affiliated to different research institutions deserve praise for the enormous efforts made over the past decade in rendering a whole range of primary sources available to the wider public. Quite surprisingly, given the rather confusing state of the archives in the former Soviet Union since 1991, the Russian contribution too has been remarkable in providing very important materials on Moscow’s policies, manoeuvres and actions throughout the crucial year 1956. The first step was undertaken by Boris Yeltsin during his state visit to Hungary in November 1992, when he handed over to Göncz a selection of previously top secret documents, later published by the 1956 Institute under the title *A Jelcín dosszié* [The Yeltsin Files]; two other volumes followed, in 1993 and 1996

1. István Bibó (1911–1979), a distinguished Hungarian political thinker still largely (and undeservedly) unknown in the West, was a minister in Nagy’s revolutionary government, which was overthrown by Soviet tanks on 4 November 1956. He wrote several important analyses of the events in 1956 and was sentenced to life imprisonment in 1958 along with Hungary’s current president, Árpád Göncz.

2. The original Hungarian edition was entitled *Szemle*, the English edition *The Review*, and the French *Études*.

respectively, that almost entirely completed the picture – not only of Soviet strategy and behaviour, but of Kádár's too.³ The declassifying of documents by US archives, notably the National Security Archives at George Washington University, as well as of the Radio Free Europe files at the Open Society Institute has also contributed considerably to a better understanding of American priorities in the middle of the Cold War. What do we learn that is new?

First, that the Soviet leadership was indeed seriously divided over what to do with Hungary. Second, that Kádár was hesitant about which option to take. Third, that the main Western powers and the world's most prestigious communist parties *de facto* cooperated in encouraging the Kremlin to quell the Magyar insurrection.

No one has yet been able to provide a conclusive account of the meaning of the famous Soviet declaration on relations within the Eastern bloc, issued on 30 October 1956. We now know that it was meant to ease tensions, and that it was drafted two weeks earlier, even prior to the dramatic change of leadership in Poland on 20–21 October. Nor has anyone yet been able to say whether the Soviet leaders were unanimous and only waiting for the most opportune moment to strike at the Hungarians, or if they were divided. We now know that the latter is true. No one has yet been able to determine whether Kádár had been a willing Soviet agent since the beginning of the upheaval. We now know that he had not, and that he was finally persuaded to switch sides as late as 2 or 3 November, probably during that night and by Khrushchev in person. Last, but not least, no one has yet been able to discover exactly what made the Soviets finally decide to invade Hungary. We now know that it was a combination of America's open expressions of disinterest towards "foes",⁴ belated Anglo-French colonial ambitions in the Middle East, and a bitter hostility towards the Revolution expressed by all world communist leaders. In this latter respect one should emphasize the great importance of a message sent to the Soviet Praesidium by Palmiro Togliatti, the Italian Communist Party chief, on 30 October, which sounded like a sharp reproach for the amateurish handling of the crisis and urged action.⁵ Coming as it did from a former close collaborator of Stalin and Dimitrov at the head of the Third International, it certainly had a strong influence on Moscow's decision-making.

Much, but not all, of the above information has been included in the book edited by Litván, so one can only hope that a second updated edition will be published soon.

Federigo Argentieri

3. These volumes, jointly edited by Russian and Hungarian experts, were published in Hungary under the following titles: *Hiányzó lapok 1956 történetéből* [Missing Pages from the History of 1956] (Budapest, 1993), and *Döntés a Kremlben, 1956: a szovjet pártelnökség vitái Magyarországról* [Decision in the Kremlin, 1956: Soviet Praesidium Debates over Hungary] (Budapest, 1996).

4. The fact that in 1956 the United States still considered Hungary as an enemy country was very clearly pointed out by the late Dr William Griffith, former director of Radio Free Europe, at a conference held in Budapest in late September 1996 entitled "Hungary and the World, 1956: the new historical evidence" and jointly organized by the US National Security Archives, the Hungarian Academy of Sciences and the 1956 Institute.

5. The text of the letter was published in the *Cold War International History Project Bulletin*, 8–9 (1997), p. 357.

FAIRRIIS, DAVID. *Shopfloor Matters*. Labor-management relations in twentieth-century American manufacturing. [Routledge Studies in Business Organizations and Networks, 5.] Routledge, London [etc.] 1997. xiii, 234 pp. £45.00.

“Following roughly three decades of declining shopfloor safety and experience with systems of shopfloor governance that are unresponsive to workers’ shopfloor concerns, a shift in the distribution of power is required to restore justice, legitimacy, and efficiency in U.S. production” (p. 178). This judgment, offered a few pages from the end of economist David Fairris’s ultimately disappointing monograph, aptly summarizes the destination toward which the study’s arguments have drawn the reader. Fairris outlines a broad trajectory, commencing about 1900, in which American labor’s workplace authority increases as unions organize, then wanes in the post-war era of contractualism and national bargaining, before fragmenting in the face of declining unionization rates. Seeing the present period as a crisis and an opportunity, he urges adoption of the German “works council” system to foster shared labor management decision-making about plant conditions. Management would reap increased productivity while workers would gain improved health and safety, along with a measure of dignity and rising morale.

However worthy this goal, Fairris’s study is not addressed to those who might have a hand in realizing it – union, business, or political leaders. Rather its audience seems to be economists, industrial relations specialists, and labor historians. Inexplicitly, Fairris adopts an epistemological hierarchy which privileges “hard evidence” and statistical “confirmation” over “anecdotal” sources. For example, he notes that “There is plenty of anecdotal evidence to suggest that workers benefited from company unions” (p. 35). This conclusion, judged vulnerable given its lack of mathematical rigor, needs buttressing through analysis of accident trends during the company union period, in this case at the Amoskeag mills, before and after 1924. The numbers behave naturally: once the company union arrives, accident rates decline. To be sure, tucked away in an endnote, Fairris observes that “Other explanations for the reduction in injury rate cannot be ruled out, of course.” What has been achieved then? Indeed, what he does present is no explanation, but a correlation standing in for an explanation, based on statistical work which is neither extensive nor robust enough to exclude alternative hypotheses. This pattern is repeated throughout the book, as Fairris relates the generally well-known contours of American unionization, selects a shopfloor element for statistical analysis and imagines that he has improved our collective understanding thereby.

The most baffling exercise in this array is the author’s attempt to account for the gradual increase in US manufacturing injury rates after the mid-1950s, whereas German and Japanese rates continue declining, as American rates had done in the immediate post-war decade (pp. 128–135). Fairris believes that “the institutional arrangements of shopfloor governance” which “promote or discourage expressions of shopfloor power by workers” are central here. Seeking in vain “evidence of a statistical sort” for “a measure of shopfloor power”, he was reduced to writing to twenty colleagues in labor history and industrial relations, asking them to rank unionized workers’ post-war “shop-floor power” in nineteen two-digit US census industrial classifications (pp. 130–131). As the endnote reveals, only four replied (p. 214). Did our author drop the idea, extend the survey, rethink what, other than averaging a few colleagues’ subjective guesses, might be undertaken to generate data points on this count? No, he built a table, did

some regressions, got a few “significant” results, and moved on. That this inquiry makes no effort to gauge changes in workers’ shop authority, *c.* 1945–1960, in various trades is mentioned, but is also set aside. So we have a vague term, “measured” and ranked by four scholars using unexplained criteria, encompassing nineteen “industries” (some of which are a vast mix of trades), rendering static a dynamic process, and yielding statistical “confirmation” of an author’s hypothesis. Hard evidence indeed! This is dreadful scholarship. The writing is also tiresome, infested with passive voicing, and Fairris’s use of Albert Hirschman’s notions of “exit” and “voice” to structure early chapters is mechanical, then the concepts fade from sight. I regret to say that there is very little that historians might gain from reading this monograph, at least in my judgment.

Philip Scranton

CHAQUERI, COSROE. *The Soviet Socialist Republic of Iran, 1920–1921. Birth of the Trauma.* [Pitt Series in Russian and East European Studies.] University of Pittsburgh Press, Pittsburgh [etc.] n.d. [1995.] xxvii, 649 pp. Ill. \$70.00.

Cosroe Chaqueri’s history of the Jangali rebellion, which spread across the Caspian Sea region of Iran during the First World War, is undoubtedly the most detailed and well-researched work ever published on this movement. As a long-standing archivist of the Iranian leftist movements, Chaqueri has consulted the most accessible archives in a number of countries, though a number of potentially useful archives (he mentions the archive of the Ministry of Foreign Affairs in Tehran and some Soviet archives, including those of the ministries of Foreign Affairs, Defence and the Interior (pp. xxvi and 407)) remained closed to him. Chaqueri has also made use of many published and unpublished books, documents and periodicals in a number of languages in order to present a most colourful picture of this episode in Iranian twentieth-century history.

The years following the Constitutional Revolution (1906–1909) are best known as the period of political disintegration in twentieth-century Iranian history. With the outbreak of the First World War and the growing interference of foreign powers in Iranian affairs, political chaos and confusion swept across the country and long-standing political rifts became exposed. Successive Iranian governments proved incapable of solving the country’s escalating problems, and fundamental reforms first demanded during the constitutional movement continued to be ignored. However, in the north of the country – in Azerbaijan, Gilan and Khorasan – there were reform-minded individuals who believed that if they succeeded in launching regional campaigns to initiate change and establish stability in their own region, the same reforms would gradually spread throughout the whole country. The agenda of these regional campaigns did not include the call for secession but was confined instead to attempts at establishing a centralized and stable political power in Iran, while maintaining a fair and reasonable degree of political power in the hands of local authorities throughout the country.

In 1914 Mirza Kuchek Khan, a young charismatic constitutionalist with a traditional religious education, became disillusioned with political developments in Tehran and launched an anti-Russian and anti-British campaign in his birthplace Gilan. Although the Jangalis sustained their limited and rather fragile control of the province and appeared to challenge the authority of the central government, generally they remained

in a defensive position, unable to introduce any significant changes in the province during the early years of the war. Their sporadic guerrilla warfare against the central government and Entente interests in the region turned out to be less effective than expected. In seeking an alliance during the war, Kuchek Khan, like many other reform-minded Iranian political activists, chose what might be considered to have been the safest path. Germany, because of its geographical distance and previous non-colonial policies in Iran, and Ottoman Turkey, Germany's tactical ally, appeared suitable candidates. And although the Jangalis provided both Germans and Ottoman agents a safe shelter for their anti-Entente sabotage activities, the military support the Jangalis expected in return did not fully materialize.

Following the Russian Revolution of 1917 and the disappearance of Czarist imperialist interference in Iran, the Jangalis reshuffled their alliances and considered the Bolsheviks as potentially their most favoured ally. However, if one excludes some preliminary conciliatory signals shown by both sides, there was no significant step taken by either to reach an understanding or collaborate. Yet, flush from its successive victories in the Caucasus, the Red Army crossed the Caspian Sea and landed in Gilan in May 1920. Although its main objective was to threaten British interests in India in order to persuade Britain to end its political hostility to and its economic embargo on the young Soviet regime, there were some Bolsheviks, if not in Moscow, who sincerely believed in the world revolution and were in favour of raising the red flag throughout the East.

The period 1920–1921, to which Chaqueri has devoted much of his study, was a crucial one, not only in the history of Soviet-Iranian relations and the Iranian communist movement, but also in the early history of Soviet foreign policy. By assisting the local Iranian communists, the Bolsheviks inspired a loose alliance between Kuchek Khan and the Iranian communists. However, with the communists longing to practise the very revolutionary socialist codes, such as the rather unqualified abolition of private ownership, the ill-fated alliance between the two came to an abrupt end. The escalating hostilities between them dramatically weakened their military potential, as well as the public support they had previously enjoyed. Meanwhile, by eventually signing treaties with the British and Iranian governments the Bolsheviks left the Jangali movement isolated. It did not take long before Kuchek Khan's decapitated head was being presented to Reza Khan (later Reza Shah Pahlavi), who, according to Chaqueri, enjoyed not only the support of the British but also the Soviets (p. 368).

Though the main crux of Chaqueri's study is an analysis of the one-year history of the Jangalis' collaboration with the Iranian communists and the establishment of the "Soviet Republic of Iran", he provides not only a comprehensive picture of the social structure of Iran prior to the Constitutional Revolution of 1906–1909 (chapters 2 and 3), but also an analysis of the political changes in northern Iran since the formation of the Jangali movement (chapters 3–8). He assesses too the early period of Soviet foreign policy and its endeavours to establish "socialism in one country" (chapters 7, 15, 16 and 17). The closing chapter of Chaqueri's study is an epilogue evaluating the "belatedly acquired" documents kept at the Comintern archives in the former Soviet Union. The book concludes with a useful appendix of biographical notes, which helps to provide a better understanding of the Jangali episode.

As an Iranian political dissident during both the Pahlavi era as well as in recent years under the Islamic Republic, it is not easy for Chaqueri to distance himself from his subject when assessing the modern political history of his homeland. Yet, although he makes no effort to conceal his compassion for movements such as the Jangali and

regards them as attempts to “reestablish constitutional government in Iran, to promote progress and welfare, and to free the country from foreign domination” (p. 376), at no point in his study does he idolize such movements; indeed, he is often critical of them for their obvious deficiencies. For him the defeat of the Jangali movement is an example of the failure of a series of anti-imperialist, nationalist semi-uprisings in twentieth-century Iranian history, which eventually led to the establishment of the autocratic rule of the Pahlavis, and later the theocratic rule of the Islamists. Hence, in his study Chaqueri is preoccupied not only with providing readers with a chronological as well as an analytical picture of how the movement proceeded, he often portrays the Jangali movement in the light of what it could have become had its leaders adopted more appropriate and effective measures.

In his conclusions regarding the fall of the movement the author not only observes the external factors that paved the way for its downfall but examines also the shortcomings of the movement and especially its leadership, which eventually brought the movement to its knees. According to Chaqueri, the Jangalis predominantly relied on tribal chiefs and failed to mobilize the peasants, who “constituted the principal force of the movement”, by organizing them in appropriate “unions” and implementing land reform (pp. 386–387). Furthermore, the lack of adequate “knowledge of power” on the part of Kuchek Khan and his colleagues left them with “a formalistic, superficial understanding of the concepts of independence and freedom”. According to Chaqueri, the Jangali “were simply against outright and brutal interference by foreign powers” and failed to grasp that “for Iran to be truly independent, social and political structures had to be fundamentally re-formed so as to remove the internal elements that facilitated foreign intervention” (p. 391).

Of the external factors that contributed to the fall of the Jangalis, Chaqueri highlights their betrayal by the Bolsheviks. He claims that with the Bolshevik invasion of Gilan “the Jangali movement acquired new momentum” (p. 393). However, the Bolsheviks’ earlier policy of cooperating with the movement and even promoting an alliance between the Jangalis and Iranian communists, which Chaqueri sees as nothing other than the movement’s “kiss of death” (p. 374), was later substituted by a policy designed to “liquidate the revolutionary movement in northern Iran”. This in turn was the direct outcome of a change in Bolshevik foreign policy aimed at opening channels of negotiation with the British and acknowledging the power of central government in Tehran. Thus, when in February 1921 the Iran-Soviet Friendship Treaty was signed and followed by the ratification of the Anglo-Soviet Trade Agreement in March of the same year, it became obvious that “a national movement was crushed in the vain hope of building socialism in one country” (p. 374).

Concerning the sources used by Chaqueri in his study, what strikes one is the author’s generally unbiased approach and impartiality in employing historical data and information originating from various political camps. There are a few exceptions, however. Chaqueri’s reference to the Jangalis’ links with the Germans during the First World War is one. To counter the argument that during the war the Jangalis collaborated with the Germans against the Entente, Chaqueri claims that the first contacts between the Jangalis and Germans were as late as the summer of 1917. He argues that “there had been no contact between the Jangalis and Germans before the summer 1917” (p. 130). However, the personal account of one Jangali member, Mirza Ali Khan Taliqani, leads one to doubt this. Taliqani was a member of the Jangali delegation that visited the Ottomans and the Germans in their Tehran embassies. And their first meeting took

place as early as November 1915 (Muhammad Ali Gilak, *Tarikh-i Inqilab-i Jangal (Bi Ravayat-i Shahidan-i 'Ayni)* (Rasht, 1992), pp. 24–29).

In conclusion, *The Soviet Socialist Republic of Iran, 1920–1921. Birth of the Trauma* is lively, well written and a thoroughly researched work, based in part on original materials. It is highly recommended for students of modern Iranian history and those who seek a better understanding of the perplexities that have burdened the Iranian communist and liberal movements in the twentieth century.

Turaj Atabaki

CHAMBERLAIN, MARY. *Narratives of Exile and Return*. [Warwick University Caribbean Studies.] Macmillan Press Ltd, London [etc.] 1997. xii, 236 pp. £14.95.

The Caribbean provides an exemplary case for the study of how international migration is used as a key part of a household's strategy for survival. Migration is important to the Caribbean for two principal reasons. First, having wiped out the indigenous peoples (the Caribs and the Arrawaks) the mercantile powers imported migrants from Africa, Asia and elsewhere. Thus, nearly everyone in the Caribbean is a descendent of migrants of the modern age. Second, the commodity that they were shipped in to produce – King Sugar – was frequently dethroned on the international market. The alternatives (bananas, nutmegs, cocoa and small-scale industrialization) barely sufficed – so regional migration and out-migration from the Caribbean has been common for well over a century. Yet, despite often being brought to the Caribbean as compelled labourers and despite often being forced to leave for economic reasons, the islands exercised an extraordinary compulsion of their own. People from the Caribbean in foreign climes tended to look back more with longing than in anger. At the level of the wider kin group their means of survival have, of necessity, become globalized.

These features are the starting point for Mary Chamberlain's account. Using oral histories as her evidence, Chamberlain argues that independent family objectives drove migration within and from the Caribbean quite as much as the demands of international capital or the force of British colonial policy. A migratory "culture" developed, with Caribbean people moving to and from the islands in a continuous flow. The relationship between "home" and "away" became fluid and complex. The great virtue of Mary Chamberlain's book is that she is able to integrate the migration history of her case study, Barbados, into contemporary theories of globalization, "hybridity" and diaspora. Yet, as Alistair Hennessy mentions in his preface, she wears her theory lightly, preferring to let her respondents do the talking. Her data are derived from 85 life-stories over two or three generations of migrant families – 39 with migrants to the UK, 23 with their parents in Barbados, 20 with children born or raised in Britain and three with children left in Barbados.

The author lays special emphasis on the family – "the conduit, not merely the bridge, between the individual and the collectivity" (p. 9). And, of course, "family" has to be used in a broad sense to include "inside" and "outside" children and the wider kin network. Memory, social consciousness and a collective psychology are formed within this nexus. The rationalization and reconstruction of motive are important parts of her account. In keeping with current ways of undertaking social history, these understand-

ings and imaginings are seen as “narratives” – ways of explaining the world that become as important, or more important, than conventional documentary evidence. In a telling phrase, she maintains that migration became “the social history of international relations” (p. 32). Unlike in most of the other islands, during various periods of Britain’s 350-year-old association with Barbados the government wanted to encourage emigration because of fears of overpopulation. However, Chamberlain suggests, such initiatives could not have succeeded if they did not conform to family goals, the narratives of previous adventures and the need for mutual family support.

Her discussion is supported by vivid quotations. For example, one of her interviewees who came to Britain in 1961, proclaimed that he “loved listening to the old fellas telling stories about how they went off to Curaçao and they went off to Panama and they was building the canal, and they went off to Cuba and Aruba and they found oil [. . .] The new place on stream [in the 1950s] was Britain” (p. 52). Another migrant was planning to go to England for five years, then to the USA and Canada, finally to return to Barbados to build “a right, nice house” (p. 61). Because of this sense of history and the awareness other migratory avenues, identity and kinship have been constructed in a transnational way. Scarcely a family in Barbados is not touched by migration, while women in particular find it difficult clearly to reconcile their spatial mobility with their personal, domestic and professional lives. The culture of those at home is replete with the idea of moving, while the culture of those abroad is permeated with the idea of return.

Temporary migration for the purposes of sustaining a family “back home” is by no means solely a characteristic of the post-war period. Already by the end of the nineteenth century remittances from Barbadian migrants were used to improve family circumstances. One young woman “Elaine” had a private secondary education supported by her father’s remittances. In “Olive’s” case, her grandfather and uncles had worked in Cuba and Curaçao which allowed them to build a family house with an “upstairs” and to acquire the three fishing boats that allowed them to make a living back on the island.

Part 2 of the book comprises five family narratives reproduced in considerable detail. Chamberlain consciously echoes Oscar Lewis’s well-known work on Mexico, *Five Families* (1959) which pioneered the use of the tape recorder to capture the raw material provided by people whose culture is primarily oral rather than literary. Though citing Lewis, Chamberlain is cautious of accepting a naive “social realism” which proclaims a superior authenticity to such data. While she accepts there is a further task of reinterpretation, the family accounts provided have a wonderful immediacy. They show that good qualitative research cannot be learned from a manual. It is only someone with a sympathetic ear and an empathetic personality that can bring the best out of people.

Chamberlain does not provide a formal conclusion, which perhaps is a pity. There remain some questions in my mind after reading her account. Is the material too centred on Barbados and would other islands have yielded other imperatives? Is the expression “exile” in the title – with its association of being forced to flee for political reasons – appropriate or suggestive? Rather than exile, her data suggest bi-locality and transnationalism. Despite these minor doubts, Chamberlain gives us an instructive and picturesque account that students will like and should appeal to comparative labour and social historians as well as those concerned more narrowly with migration and Caribbean studies.

Robin Cohen

FERNANDES, LEELA. *Producing Workers: The Politics of Gender, Class, and Culture in the Calcutta Jute Mills*. [Critical Histories.] University of Pennsylvania Press, Philadelphia 1997. xvii, 199 pp. £40.00. (Paper: £21.50.)

Leela Fernandes' book is an important contribution to the study of working-class formation since it highlights how workers' identities are shaped through the interplay of religion, ethnicity and gender in the jute industry of Calcutta. This study clearly represents a stream of thinking that places a strong emphasis on the cultural dimensions of power and politics, distinguishing itself from the traditional political economy of the 1970s and 1980s. On the other hand, unlike some of the main proponents of cultural studies found particularly in Indian scholarship she steers away from the more static categorizations of "class" and "community". She argues in favour of breaking down rigid boundaries dividing class and religion, "objective" and "subjective" experiences, the politics and economics of labour control and subversion. She also attempts to bridge the gap between hegemonic representations and the diverse ways in which these representations are subverted. Following Bourdieu, she stresses the importance of showing the hybrid and plural identities of the social actors, but also the existing forms of economic, social and cultural capital available to them. Social relations and the nature of discourse are continually being contested in the multiple sites of power.

These main tenets pervade her narrative, with different degrees of intensity. We see how differential access to the different kinds of capital are essentially embodied in four main social actors, namely the state, industrial capital, unionized male workers, and non-unionized female workers. Chapter two, for instance, focuses on the politics of state and unions and how women became marginalized in the process. State ideology is not seen as being static: it experienced shifts in its attitude towards women. In the 1960s and 1970s state attempts to secure hegemony were based on a secular ideology that attempted to deal with religious primordialism. Within this ideology, however, women were defined more on the basis of biological determinism, and legislation concerning women emphasized legal protection. This changed in the 1980s when greater emphasis was placed on economic advancement, educational advance, and access to health care and family planning.

However, this shift was not followed by a progressive change at the workplace or at the level of the community since traditional gender-biased assumptions continued to shape the organization of industrial relations. The concept of a monolithic working class which emerged in the political discourse between state, unions and political parties paradoxically facilitated the reproduction of hierarchical relations within the working class as the interests of female workers then became juxtaposed with the general interests of workers, and women were therefore excluded from the negotiations between unions, the state and the company (p. 42).

The exclusion and marginalization of women from the process of production is demonstrated in chapter three, where Fernandes shows how the internal labour market is created. She identifies three primary characteristics defining the internal labour market in the mill: job differentiation, recruitment and the allocation of particular occupations to workers (p. 65). Contrary to the homogenizing nature of state rhetoric regarding the working class, job differentiation in the workplace reflects a highly elaborate system of job classifications and ranks. This system of classification results in the creation of significant hierarchies within the workforce, hierarchies that are shaped by patron-client relations based mainly on kin and particularly male-kin relations. This, once more,

implies that women are often excluded from these ties. In the existence of these joint family ties the line between the “public” (factory) and the “private” (family) is transgressed. The “community” also impinges on the labour process as certain tasks are thought to be performed better by workers from a particular community. These kinds of category are also reflected in the recruitment of workers. Since labour is recruited through the mediation of unions and leaders the same biases also exist in this process, resulting in the compartmentalization of workers from different social backgrounds. The association of certain kinds of work and skills (or the lack of them) with certain categories of workers are then naturalized in the discourse. The jute industry expanded rapidly between the late nineteenth century and the early twentieth century and then faced a decline in the 1930s. The proportion of female workers in the labour force gradually but steadily declined from the 1950s to the 1990s. Fernandes shows how this decline in female labour was linked to the decline of the industry and to male workers’ attempts to take over the niches in the industrial labour force by adopting the “male-breadwinner” ideology.

This exclusion of women from the public sphere can also be seen outside the realm of the workplace, or of state legislation, as Fernandes shows in her fourth and fifth chapters. The fourth chapter concentrates on the interweaving of religion and class in public discourse. As she states, “on the one hand, the religious ritual delineates a sphere of politics distinct from the ‘formal’ realm of industrial politics, on the other hand the ritual is politicized by the actions and interests of unions and managers in the factory” (p. 103). Here again she shows that workers’ attempts to subvert the discipline of time and space allow unions to represent and consolidate their presence and influence among their constituents; it is, however, based on a gendered public space where women are excluded. This is also the main thrust of the fifth chapter, where she argues that “the jute workers’ public sphere attempts to represent the general interests of the worker’s identity with a particular construction of masculinity on the one hand and exclude the participation and interests of female workers on the other” (p. 109).

Fernandes’ book is compact and her arguments coherent. There are two main problems, though. One is the general tendency inherent in this type of theoretical approach. Her focus on discourse and the subjective experience in working-class formation and her collapsing of politics and economics into one seems to result in the neglect of political economy. Because it is mainly through existing social classifications that labour demand and supply are created, too little attention is given to how economic calculations are embedded in the creation of markets and policies. How do economic factors affect the labour process in the jute mill and how is it shaped by the exigencies of the industry? How does employment in the jute industry compare with employment in other sectors? How are the different ethnic groups in the community numerically and socially represented in the labour market? How is one’s position in the factory affected by migration and rural-urban links? Fernandes gives some consideration to these questions, but on the whole her account lacks breadth and scope. By focusing on discourse and identity formation she fails to pay sufficient attention to household dynamics; she is content to offer the narrative of just three women from different social backgrounds.

The second problem derives from the way Fernandes has operationalized her approach. In looking at hegemonic representations she has been unable to avoid being trapped in a relatively homogenized “reality”. Although Fernandes points to the diversity of workers’ groups based on place of origin, religion, ethnicity and workers’ status at work, this diversity is downplayed by the homogenizing nature of political discourse.

Her narrative would have been much richer had the contradictions within these representations been expressed more clearly. The historical dimension that would have given a dynamic and dialectic context to the discourse could also have been emphasized more. At times, the four main social actors identified in her politics of discourse seem to have been operating within a relatively timeless space.

The merits of this book should not be understated, however. It is an example of critical scholarship: it challenges the reader to appreciate the benefits of understanding cultural processes in relations that are too often regarded as purely economic. It would, however, have benefited had the author's analysis showed a better interaction between political economy and cultural studies, the reproductive and productive spheres, the past and the present.

Ratna Saptari