

BOOK REVIEWS

HOCHSTADT, STEVE. *Mobility and Modernity. Migration in Germany 1820–1989.* [Social History, Popular Culture, and Politics in Germany.] The University of Michigan Press, Ann Arbor 1999. 400 pp. Maps. \$52.50.

JACKSON, JAMES H., Jr. *Migration and Urbanization in the Ruhr Valley 1821–1914.* [Studies in Central European Histories.] Humanities Press, Atlantic Highlands (NJ) 1997. xix, 452 pp. Ill. Maps. \$85.00.

In 1976 Steve Hochstadt and Jim Jackson together dug a number of migration statistics out of local archives in Duisburg and Düsseldorf. At the same time, Jackson stumbled upon the *Meldebücher* of the Duisburg police, which contained nominal-level information on all in- and outgoing migrants in the period 1857–1894. Not long after their discovery these two migration scholars started publishing bits and pieces of their research in various journals and collections, in which they called for a structural integration of internal *and* international migrations in social and demographic history. The two books reviewed here are the end of a long and fundamental research journey.

The most important contribution to the field of migration history, and the same holds true for urbanization and economic growth, lies in the refutation of the modernization theory as propagated by Wilbur Zelinsky.¹ This theory assumes that people only started to move on a significant scale in the era of industrialization in the nineteenth century. Analogous to the demographic transition, Zelinsky's model of mobility transition is based on the assumption that modernizing societies witnessed a change from low to high levels of geographical mobility. The increased (mostly internal) migration was furthermore characterized as a final and massive move from the countryside to cities. This image of cities as irresistible magnets is consistent with the ideas of German sociologists like Simmel, Tönnies and Max Weber, who, at the end of the nineteenth century, drew a bleak picture of the uprooting and marginalizing effects of the settlement process, a picture that later formed an important source of inspiration for the ideas on urban ecology, as developed by the famous Chicago School of sociology. The analyses of Hochstadt and Jackson, and the overwhelming weight of their empirical data, relegated these ideas, which had been criticized for some time, once and for all to the dustbin.

Hochstadt and Jackson show that migration rates were already quite high before industrialization really gathered speed, and Hochstadt found a sometimes stunning “structural similarity” in the trend between cities in the Ruhr area and other parts of Germany. The main findings of his book are summarized in the figure on page 277. Here we see an increase in German migration rates starting in the 1830s, reaching an all-time high between 1880 and 1914, and decreasing again to the level of 1830 during the remainder of the twentieth century. More important than this trend is the fact that immigration to cities was characterized by a huge turnover and that the streams of in- and outgoing migrants almost matched. Thus both authors reject the one-dimensional push–pull model and replace it with a much more interactive and dynamic model of

1. Wilbur Zelinsky, “The Hypothesis of the Mobility Transition”, *The Geographical Review*, 61 (1971), pp. 219–249.

continuous and circular moves between rural and urban areas. Cities did experience net population gain as the result of immigration, due to the settlement of families, but in most cases the efficiency rate of migration was quite low. In particular, young migrants, both men and women, moved frequently and kept returning to their place of origin. Permanent migrants were only the residue from much larger movements. Hochstadt therefore concludes that “The turn of the century city was much more than a vacuum cleaner” (p. 279). He explains the high migration rates in the second half of the nineteenth century in terms of the highly seasonal and irregular rural and urban labour markets. Only when these features became less dominant and stable year-round jobs became the norm, especially after World War I, did the necessity to move from one job to another decrease.

Hochstadt’s study on Germany raises the important question to what extent his general conclusions are applicable to the whole of western Europe. So far, comparative research on long-term mobility rates has been scarce. An exception is the very recent work on mobility in Britain since the eighteenth century by Pooley and Turnbull. Surprisingly, these two migration scholars reached opposite conclusions: in Britain most migration was undertaken in family groups, and no structural change in geographical mobility could be established.² The overwhelming and stunning empirical German data, which are corroborated by Scandinavian findings, give strong reasons to doubt the representativeness of the British dataset, which consists of some 16,000 life histories, assembled by family historians and genealogists. Notwithstanding the somewhat different phasing of the industrial revolution and the possible subduing effects of the English Poor Law system, it seems highly improbable that the trends in these two countries differed so fundamentally. The problem for students of the British situation (and the same accounts for most European countries) is that the type of sources used by Hochstadt and Jackson are not available. But from now on their authoritative German model will be indispensable as a starting point from which to detect and adjust biases in other types of sources.

Both studies not only shed light on migration rates and processes, but also on social and demographic history in general. The consequences are systematically presented in Hochstadt’s concluding chapter, titled “Migration History and Social Science”, in which he relates his migration topic to a number of important issues in the field of social history. Thus he calls for “a social history that is not urbanocentric” (p. 169); migration wove city and countryside together and so historical research ought not to separate them. This insight should be applied to both the history of urbanization and of class formation, so Hochstadt argues, and with good reason. He criticizes the conventional interpretation, which sees class formation only from an urban point of view. The strong ties of many workers to their rural backgrounds does not contradict working-class formation as a concept, but highlights the importance of the peasant background. He criticizes the work of Kocka and Nolan³ who, although recognizing that workers were highly mobile and likely to return to the countryside, define them as not belonging to the working class proper (p. 171). Hochstadt instead introduces the concept of worker–

2. Colin Pooley and Jean Turnbull, *Migration and Mobility in Britain Since the Eighteenth Century* (London, 1998).

3. J. Kocka, “Problems of Working Class Formation in Germany, 1800–1875”, in I. Katznelson and A. R. Zolberg (eds), *Working-Class Formation: Nineteenth-Century Patterns in Western Europe and the United States* (Princeton, 1986), pp. 279–351, and M. Nolan, “Economic Crisis, State Policy, and Working-Class Formation in Germany, 1870–1900”, in *ibid.*, pp. 352–393.

peasant and stresses the importance of integrating both migration patterns in the study of class formation. In this respect he further argues that more attention should be paid to the effects of migration on household structures, especially when men and women occupied different class positions.

Notwithstanding the great merits of these two studies, a number of points need a more critical approach and/or further elaboration. First, both books can be characterized as examples of successful paradigm bashing. Shoulder to shoulder, they unfold their criticism with unflagging zeal. Although, on the whole, I fully agree with their critique the attack is somewhat outdated. Most historians acknowledge the untenability of the paradigms mentioned above. Leslie Moch, for example,⁴ has already shown the importance of migration in the early modern period, while the idea of uprooting has been discarded by many (mainly American) immigration scholars from the 1960s onwards. In this respect therefore neither book is the first spade, but more the finishing stone on the grave of modernization and marginalization theory. And, it has to be said, both graves have been given very fine and solid stones indeed.

More problematic than being somewhat outdated (although new insights were slow to influence German historiography) is that in their zeal to prove their case both authors sometimes tend to overreact. This is especially the case in Hochstadt's study. Of course Zelinsky was wrong in his ideas about migration rates in the pre- and post-industrial phases, but this does not detract from the fact that there is a clear relation between industrialization, or rapid economic growth, and high geographical mobility. The same goes for urbanization. Net migration may not on average have been very impressive, compared with total mobility, Hochstadt nevertheless admits that "a substantial proportion of urban growth in the 19th century was due to net migration" (p. 268). Moreover some cities had high efficiency rates (Düsseldorf and Duisburg fifty per cent, Mönchengladbach sixty-one per cent, and Essen even eighty per cent). Preoccupied with demonstrating general trends and structural similarity, Hochstadt, however, does not explain these diverging patterns. Further, I do not agree with Hochstadt's statement that the streams of single migrants had little to do with urbanization (p. 106). The huge number of temporary migrants must have made an impact on urbanization, not only economically, but also socially and physically. After all, these migrants ("who collectively swelled the city", p. 105) must have lived somewhere. Finally, Hochstadt, in my view, tends to adopt a rather too romantic (and in an ironical sense modernist) position by assuming that most migrants from rural areas deliberately refused to settle permanently in cities in order to resist capitalism, modernization and industrialization (pp. 213–216). Not only do his aggregate quantitative data not allow for such far-reaching conclusions, it is not entirely compatible with his overarching economic explanation for changing mobility patterns. One could also argue that the unstable labour markets left young working migrants no choice and that the combination of rural and urban jobs was a perfect rational strategy.

Jackson has a somewhat similar love–hate relationship with his paradigm. By rightfully attacking the idea that migration in general had uprooting effects he seems to close his eyes too much to possible negative effects (why not make a case study of people who ended up in the gutter?) and moreover seems to give too rosy a picture of the assumed stabilizing functions of networks. Family and ethnic networks can also

4. Leslie Page Moch, *Moving Europeans: Migration in Western Europe Since 1650* (Bloomington, 1992).

have suffocating and isolating effects on migrants, whereas breaking with social control can have very positive effects. By assuming that *Gemeinschaft* is better for people, Jackson in a sense implicitly accepts and reproduces the problematic underlying associations of the *Gesellschaft–Gemeinschaft* dichotomy of Simmel and Tönnies.

A second point in both studies that deserves closer scrutiny is related to the use of aggregate data. Although I can see why they used them, and the results are often impressive, there is also a problem in using such data: they do not permit one to answer certain basic questions, for which nominal-level data are indispensable.⁵ This has become especially evident in the relationship between the rural and urban labour markets. Both studies show that until World War I migrants used both circuits, but less clear is how these two systems related. As the seasons overlapped almost entirely (demand for work was greatest between March and November) the question is how the work cycle was constructed. In my opinion there must have been at least three different cycles: migrants who concentrated on jobs in the city and supplemented these with work in agriculture; migrants who did the opposite; and migrants who moved from city to city (like journeymen).⁶ Such sophistication will not refute the grand theory about the interactive rural-urban circuit, but it could teach us more about the micro and meso levels of migration and, furthermore, shed further light on the assumed resistance to modernization as well as on the relationship between marginalization and networks.

My third main point concerns the explanation for the decreasing mobility after World War I postulated by Hochstadt. I like his economic argument, but it seems too one-sided. Why not pay more attention to both political–institutional and technological factors? Conspicuously absent for example are the effects of the welfare state on geographical mobility. It is not far-fetched to assume that the emergence of welfare arrangements after World War I, albeit slowly and temporarily, must have decreased the willingness and urge to move to wherever there is work. As a result labour markets became more restricted, because people do not so easily change residence for a short-term, low-paid job. This development is closely linked to the accumulation of wealth and the greater significance of education. Both made it less likely that people would change residence as frequently as before. Another factor that is dismissed much too easily in my opinion is the influence of the transportation revolution, which increased the radius of travel enormously and permitted workers to combine several jobs without having to change residence. In particular, bicycles and intricate transport structures like light rail, which spread from the end of the nineteenth century onwards, must have made a difference. A good example can be found in a recent article by Carl Strikwerda, who illustrated this point in his analysis of the transportation policy of the Belgian government. Cheap workmen's tickets on the national railways and the build up of an enormous system of tramways allowed workers to commute daily or weekly to cities, predominantly in the north of France, and still keep their families and farmland in the small towns or countryside. The number of workmen's tickets issued yearly increased from some 14,000 in 1870 to over a million in 1890 and 4.5 million in 1900.⁷

5. As demonstrated in the impressive recent book by Jose C. Moya, *Cousins and Strangers: Spanish Immigrants in Buenos Aires, 1850–1930* (Berkeley etc., 1998). See especially pp. 411–413.

6. A good example is offered by Sophie de Schaepdrijver in her study *Elites for the Capital? Foreign Migration to Mid-Nineteenth Century Brussels* (Amsterdam, 1990).

7. Carl Strikwerda, "France and the Belgian Immigration of the Nineteenth Century", in Camille Guerin-Gonzales and Carl Strikwerda (eds), *The Politics of Immigrant Workers: Labor Activism and Migration in the World Economy Since 1830* (New York and London, 1993), pp. 101–132.

Finally, I wondered what the effect had been of changes in municipal boundaries, which make a lot of local moves invisible. The constant spatial expansion of cities and their swallowing up of surrounding villages must have decreased the total mobility rate.

These critical remarks do not, however, detract from my great admiration for the hard, exhaustive labour of Hochstadt and Jackson. Their work marks a new milestone in the history of migration, in Germany in particular and western Europe in general. Their conclusions may not always be new, so perhaps the greatest value of their work is the systematic way they link their findings to an array of more general fundamental discussions in the field of social history, building on an intimidating empirical basis. Both books show, as Leslie Moch demonstrated in her *Moving Europeans*, how crucial the study of internal as well as external migration is for a better understanding of larger processes, be they urbanization, industrialization, gender relations, household structures or working-class formation.

Leo Lucassen

GREEN, NANCY L. *Ready-to-Wear and Ready-to-Work: A Century of Industry and Immigrants in Paris and New York*. Duke University Press, Durham [etc.] 1997. xi, 426 pp. Ill. \$59.95. (Paper: \$19.95.)

Historians have often called for a comparative approach in the construction of research themes. The purpose in suggesting this method was to break away from the mould of nation-state histories and the nationalizing functions of history itself. Only by moving out of the local context, they argued, can one define the national or the inherently structural in the fields of study. Nancy Green's *Ready-to-Wear and Ready-to-Work* fully meets that goal. By producing a history of the garment industry in two cities, Paris and New York, over one century, Green is able to redirect her readers' interests from grasping either the French or the American industrial and labour-history components of the stories involved to an understanding of the paradigms of that industry and its labour force that make it similar on both sides of the Atlantic. That in itself is no small accomplishment. Her book addresses the specificity of the garment industry beyond local and national characteristics, thus establishing the inherent factors that have led to its functioning and survival.

Even more striking is Green's success in establishing her study over the *longue durée* of one century. Every US labour historian is familiar with descriptions of turn-of-the-century New York Jewish or Italian workers sewing garments in tenements or in the (in)famous factories of the Lower East Side and knows the drama as well as the heroes and heroines of union formation (International Ladies Garment Workers' Union, ILGWU) in the women's wear sector of the industry. For French social historians the garment industry is perhaps less central to the knowledge of union building than its American counterpart, but *le Sentier* and its concentration of immigrant workers is inescapable. But, although all are regularly struck by the "reappearance of the sweatshop" in their respective countries, on neither side of the ocean has a history been drawn of that industry that spans the full century and combines both historical and sociological knowledge. The comparison therefore is not only spatial, it allows us to compare the industry over time within and across national boundaries. This twofold comparative axis is what enables Green to isolate different variables as well as give a coherent description of the garment industry independent of local and ephemeral

contingencies. The closely detailed analysis, combined with the more distant viewpoint that comparison implies, enables Nancy Green to adopt what she calls a “post-structural structuralist” approach to historical description that focuses on the structure of that industry as well as on the semiology of its differences.

Having to create (or respond to) the fleeting manifestations of fashion, the garment industry is all about flexibility, seasonality, versatility, adaptation to ever-changing clothing styles, and a transient labour force. For that reason it has been left out of the classic historical narratives about industrialization, which are more concerned with smokestacks and heavy metal working. And yet, as Green points out, it is one of the last urban industries, and one which has been the port of entry into their new countries for generations of immigrant workers. Flexibility, now the badge of modernity in labour management, has at all times been the essence of labour and production conditions in garment making. The stability gained through the union movement, especially in the United States, and through the relative concentration of industry during wars and their subsequent periods of prosperity, was in its turn eroded by economic depressions, the arrival of new immigrants, globalization of markets, new methods of production and extended competition. In this perspective, the fragility, and quick turnovers in companies and in the labour force, rather than simply being signs of decline, were the systemic forms of existence of an enduring industry.

Understanding how that industry functioned from the vantage point of the end of the twentieth century gives rise to a sense of succession and repetition. As Green makes clear, it was an industry of passage for all ethnic groups. Germans, central European Jews, Italians and Lithuanians, Puerto Ricans, African Americans and Chinese have succeeded each other in the United States. French *cousettes* and *midinettes* have been followed by Jews, Armenians, Turks, Serbians and Chinese in the Paris context. The sweatshop can serve as a metaphor for the whole industry, although at all times garment making was divided between centralized production and a varying degree of subcontracting. The division of work from factory to workshop, to homework and a dispersed labour force were equally true of the 1900s as of the 1980s. If for nineteenth-century (male) workers the restructuring generally implied a deskilling process from artisanal to mass-production work, for those who started at the bottom of industry, especially for immigrant men and women, succession could also mean promotion from homework to the workshop or to the factory, or from seasonal piece rates to regular wages and employment stability. Thus the fragmented labour markets have reflected the industry’s adaptation to competition but without necessarily meaning starvation wages for all workers.

The perception of the industry’s stability through its very fluctuations allows Nancy Green to deconstruct the many ethnic or gender stereotypes that have regularly appeared when one category of workers was pitted against another. Thus, she says, the constant redistribution of roles within the industry has only been possible through the redefinition of categories concerning skills, machines, and workers. What was perceived and demeaned as “women’s work” often became men’s work and most noticeably immigrant work. And within the latter group “immigrant women in fact bridged the epistemological gap between two categories of analysis (gender and ethnicity)” as the more relevant paradigm of the constantly renewed labour force. In France and the United States, women have constituted the majority of the garment workers, although in the urban contexts of Paris and New York male workers have been more numerous than in smaller

towns. Green's attention to the language used to reify categories forcefully illustrates their contingency and variations across time and space.

Nancy Green's close analysis of meaningful periods of continuity or change in their national characteristics also isolates the specificity of each context. There are two stories in one, and interaction between them. In France, fashion and *la haute couture* created standards that the United States entrepreneurs tried to emulate. Conversely, American sportswear launched the mass-produced style of the end of the twentieth century which French garment makers have had to adopt. Up to a point, art on one side, industry on the other, were the hallmarks of different conceptions of clothing.

Politics and ethnicity (or race) have shaped the industry's social policies differently on both sides of the Atlantic. And here perhaps lies the greatest challenge to a comparison over a long period of time: political identities cannot easily be abstracted from the larger national environments and specific historical moments. The two segments of garment production did not function in an economic symbiosis or even competition. Green indeed points out that they served different markets. She also posits their different places in the national economies, political worlds and the national union structures. Garment workers in France and the US did not play the same role in labour politics and in the shaping of union cultures. Neither does the language of class and ethnicity function in the same way. Green, however, strikes a balance by stressing the similarity of structure while recognizing the dissimilarity of cultures. Jewishness and anti-Semitism, for instance, had a specific meaning in France, where a whole generation of Jewish workers and bosses was directly under attack and virtually destroyed during World War II; and African Americans' complaint of racism refers to a deep and distinctly American history. Race, class, ethnicity and politics (i.e. communism) reverberate differently in the two countries. And yet one may agree that in France today ethnicity is less subsumed by class than it formerly was in the heyday of union power, and has therefore become more analogous to American versions of ethnic consciousness.

Nancy Green's many talents address the story at all levels: in this bold endeavour she is a historian of technology and culture as well as of labour, and she responds to issues raised by gender as well as immigration specialists in the French and the American historiographical contexts. The result is a many-faceted and dynamic analysis.

Catherine Collomp

SCOTT, JAMES C. *Seeing Like a State. How Certain Schemes to Improve the Human Condition Have Failed.* [The Yale ISPS Series.] Yale University Press, New Haven [etc.] 1998. xiv, 445 pp. £25.00.

The jacket of this book describes it as "a broad-ranging, theoretically important, and empirically grounded treatment of the modern state". These claims concerning the work's merits are not misleading and the reader will not be disappointed. This book may be placed in the tradition of Patrick Geddes, Lewis Mumford, Paul Goodman, Jane Jacobs and Colin Ward. The research includes field work in Malaysia, accounts of Tanzanian and Ethiopian forced villagization, Brazilia and Chandigarh, Soviet collectivization including Potemkin, and The Great Leap Forward in China, as well as first-hand knowledge of French cadastral mapping and modern European "fiscal forestry". There is a wealth of information that is perfectly mastered, without the author becom-

ing pedantic. As is rarely the case for such works, reading this book is a feast. Such a perfect book calls for some criticism, which I will undertake when I discuss its unstated paradigm.

The author sees the state as “the vexed institution that is the ground of both our freedoms and our unfreedoms”. In fact, I have found no example in which the state is the ground of freedoms. And this is not surprising, since Scott’s own critique shows that the state does not look at the complex and multiple activities of nature and society but only at what interests the official observer. There is therefore a radical simplification of reality in favour of formal schemes of order. The author’s demonstration is intelligent, illuminating, well argued, and even accompanied by photographs and maps that illustrate his points.

One strong point of his analysis is that he tries to avoid any overgeneralization by declaring that states are nefarious in two cases – either when they adopt utopian plans or when they have an authoritarian disregard for their subjects. Yet the examples presented appear to reveal that this situation does not occur in those two instances only but with all statist societies, including democracies. For instance, we are told that the state has had to know how much timber its forests would produce: “In state ‘fiscal forestry’ [...] the actual tree with its vast number of possible uses was replaced by an abstract tree representing a volume of lumber or firewood. From a naturalist’s perspective, nearly everything was missing from the state’s narrow frame of reference. Gone was the vast majority of flora: grasses, flowers, lichens, ferns, mosses, shrubs, and vines. Gone, too, were reptiles, birds, amphibians, and innumerable species of insects. Gone were most species of fauna, except those that interested the crown’s gamekeepers.” Thus the natural environment to create monocrop forests was destroyed.

Or take the case of the French Revolution. It simplified human complexities by suppressing all intermediary groups, local nobles and elites through whom it collected the taxes, organized conscription and obtained all necessary information. Instead, it created a new reality, the French “citizen”, reducing the nation to homogenized, uniform beings. Even the emancipation of the Jews undercut the intermediate levels and gave the state immediate access to each individual, since the citizens’s rights were contingent on the state and therefore revocable by the state (p. 364).

The state’s hand extended to all forms of property. In the Soviet Union, “the dream of state officials and agrarian reformers [...] was to transform the open field system into a series of consolidated, independent farmsteads [...] They were driven by the desire to break the hold of the community over the individual household and to move from collective taxation of the whole community to a tax on individual landholders” (p. 41).

This confusion of the elaborate human landscape with the political one has in every statist society the same purpose: legibility. The power elite has to know where each citizen is; for the purposes of taxation and conscription, and thus governments everywhere have characterized each individual by a first and a last name, and a social security number, they have given each building a numerical street address to make the cities more legible, and so on. They have simplified their own views of the world and, thereafter, endeavoured to shape the world itself according to their monochrome visions.

One may note that the desire to break the hold of communal societies on the individual also appeared in so-called democratic countries: in the United States and Australia, for instance, when they refused to respect the indigenous peoples as collective entities with their own rights, or when they denied collective ownership of property by the

utopian communistic associations. The author, who clearly admires Jane Jacobs, nevertheless criticizes her because she seems to fail to see that, in the absence of the state, the urban landscape is shaped by commercial and speculative interests (p. 387). But is there only one alternative, either the state or capitalism? Doesn't Jacobs point to another possibility, that of human groups acting according to their needs? And is the state an alternative to capitalism? Scott's model does not explain why the state, which crushed all independent elites and intermediary groups, was unable to act in the same way with the capitalist enterprise, that other Behemoth.

In spite of this limitation, the model proposed is quite interesting because it may be applied to a large number of situations. It explains adequately the failure of many a global enterprise in low-income countries and offers insightful comments on the effects of this simplified perspective in the new world order. If the state moulds people's unconscious and their imagination, is it surprising that individuals, too, tend like the state to oversimplify their view of nature and of reality? One can no longer hold the same view of the world after reading this book.

Ronald Creagh

Stalinism and Nazism: Dictatorships in Comparison. Ed. by Ian Kershaw and Moshe Lewin. Cambridge University Press, Cambridge [etc.] 1997. xii, 369 pp. £45.00. (Paper: £15.95.)

In September 1991 Moshe Lewin, one of the foremost experts on the history of the Soviet Union, co-organized a conference in Philadelphia. One of the aims of the conference was to explore similarities and differences between Stalinist Russia and Nazi Germany. Fifty scholars from five countries – the United States, Russia, Germany, France and the UK – took part.

The timing of the conference was apposite. The Soviet Union had collapsed just one month before. For the first time both dictatorships could be referred to in the past tense. Though the demise of the Soviet Union could scarcely have been reflected immediately in the debates that took place in Philadelphia, it certainly has been in the subsequent writings of the participants. Both dictatorships are discussed with the same degree of detachment. Before then, it was virtually impossible to escape the Cold War in any comparison between National Socialism and Stalinism. Who can possibly forget the *Historikerstreit*, ignited in 1986 by the celebrated historian Ernst Nolte? The starting point of the debate at the time was not simply whether Hitler's mass extermination of the Jews was a "reaction" to the earlier class genocide pursued by Lenin in the early 1920s in the Soviet Union. It was, above all, a debate about the "legitimacy" of what was then the Federal Republic of Germany. Nolte rejected the oppressive legacy of the Third Reich which continued to burden West Germany. His principal adversary in this debate, the philosopher Jürgen Habermas, insisted, however, that the legacy of the Third Reich had actually yielded benefits. To prevent the repetition of a German *alleingang*, successive chancellors – from Konrad Adenauer to Helmut Kohl – have pursued a policy of integration within the European community and NATO.

Such politicization of the study of Stalinism and National Socialism is inconceivable in the post-Soviet era, and it seems inevitable that it should become "academicized". This volume of essays is an excellent example of this academicization. The nine authors

and two editors (Lewin's co-editor is the Hitler specialist Ian Kershaw) have written excellent, carefully crafted essays, none of which will cause any great furore in the political or academic worlds.

The essays are divided into three sections, though that is not apparent from the contents page. The first five essays discuss the position of Stalin and Hitler in their respective autocracies. On what was their power based? Were they powerful at all, or should one regard them – to borrow that famous description of Hitler by Hans Mommsen (who is also represented in this volume) – as “weak dictators”? The next five essays discuss the dictatorships “in action”. Central to all these is the Second World War, which not only saw a clash between the two systems but also the ultimate (and catastrophic) expression of the dynamics inherent in the respective dictatorships. A series of historiographical essays conclude this extremely readable volume. They include a consideration of the German *Sonderweg* debate, which engaged distinguished German historians like Fritz Fischer and Hans Ulrich Wehler for a number of decades. One of the contributors to the present volume, George Steinmetz, demonstrates once again how politically charged that discussion has been.

The debate on the German *Sonderweg* was conducted in public, at least in West Germany prior to 1989; inevitably, for many years that on Stalin and Stalinism was conducted covertly, though no less intensely. Of great significance was the controversy during the 1970s between the then dissident (but loyal Marxist) historian Roy Medvedev and the bitterly anti-communist novelist Alexander Solzhenitsyn. Naturally, these two antagonists are mentioned in Mark von Hagen's informative essay, but unfortunately he fails to say much about the enormous influence of Medvedev and Solzhenitsyn on Western historians and other intellectuals. Opinion, particularly in France, was divided on the issue. The thesis of Medvedev's magnum opus *Let History Judge* (1971) on the genesis and consequences of Stalinism can be summarized: the Soviet system was derailed by Stalin and his clique; no blame could be attached to Lenin and, had he lived longer, such a catastrophe would have been prevented. French historians developed this theme in the 1970s. Jean Elleinstein's *The Stalin Phenomenon* (1976), in which the “innocent” Lenin is also compared with the evil Stalin, is scarcely conceivable without Medvedev.

The communist critique advanced by Medvedev and Elleinstein was rebuffed in Solzhenitsyn's *The Gulag Archipelago*. That extraordinary “experiment in literary investigation” was to prove a revelation to many French left-wing intellectuals. The most discussed were the so-called “New Philosophers” André Glucksmann and Bernard-Henry Lévy, who after 1977 were extremely vocal in their anti-communism, so discrediting fellow-travellers like Jean-Paul Sartre. Von Hagen devotes not a word to the influence of the debate on Stalinism outside Russia and is concerned with the debate only within Russia itself. In itself, that debate is interesting enough of course. The emotional and political revisionism to which communist historiography is being subjected is now in full swing in Russia – and inevitably in such circumstances the finer points of the argument are sometimes lost.

Such crude revisionism is something the present authors eschew. Instead they offer an *intellectual* revisionism and summarize the current state of research in often elegant prose. The comparison between Stalinism and National Socialism is the central theme of most of the essays here. Kershaw and Lewin are very conscious of the risks involved in drawing such a comparison, however. Had not the totalitarianism theory of Carl J. Friedrich, Zbigniew Brzezinski and Hannah Arendt in the 1950s shown these two

systems to be static and schematic, and comprehensively ignored their differences? The major merit of these essays is that they demonstrate that it is possible to compare National Socialism and Stalinism without descending to the simplistic level of equating one with the other.

All the same, this volume is flawed. It is an ambitious collection of essays. It eschews endless anecdotes about Stalin's capricious personality, the curious habits of Hitler or the status and conduct of their respective "courts". Instead, the authors want to understand the *systems* of Stalinism and National Socialism and to situate the two dictators in terms of time and circumstances. In the historiography of the Third Reich this has long been the order of the day. Initially, historians focused almost exclusively on the intentions of those figures central to National Socialism. In the twenty years after 1945 whole shelves were filled with books, particularly on Hitler and his essentially incomprehensible anti-Semitism.

The many biographies of Hitler have attempted to understand his intentions and so penetrate to the core of National Socialism. Following in the footsteps of a number of pioneers (particularly Franz Neumann of the Frankfurt School, whose unsurpassed *Behemoth: The Structure and Practice of National Socialism* appeared in 1942), the "structuralists" began to tackle the issue of National Socialism. The most prominent of them included Martin Broszat and Hans Mommsen, both of whom taught Ian Kershaw. Broszat especially has carried out pioneering work on the chaotic functioning of the Nazi regime. His *Der Staat Hitlers* (1969, an English translation *The Hitler State* appeared in 1981) is an extremely worthy contribution in this respect. But the very title of his book points to an insurmountable problem for structuralists: however skilful the structuralists have been in describing the relationship between the bureaucracy, the *Wehrmacht* and the SS and the struggle for power between them, the Third Reich also remains a personalistic history *par excellence*. And, first and foremost, a history of Hitler. As Ian Kershaw has demonstrated time and again – most notably in his recent biography *Hitler: 1889–1936: Hubris* (1999) – the Führer is also a central figure in the work of structuralists. In his biography Kershaw has attempted to achieve a synthesis of intentionalism and functionalism. In general, he succeeds. But it required a biography.

Stalinism and Nazism too devotes considerable attention to the position of Stalin and Hitler. Ronald Grigor Suny has written a brilliant essay on Stalin the cunning politician, who emerged as a "conservative revolutionary". He carried through a revolution from above and suffused society with his now legendary mistrust. Moshe Lewin has contributed both a psychological portrait of Stalin and a study of the role of the bureaucracy. Drawing on the recently published autobiography of the long-forgotten writer Konstantin Simonov, who participated in a number of meetings with Stalin and was able to observe him at close hand, Lewin provides a portrait of the intriguing dictator. He brings Stalin closer to us, but just as the Georgian was a mystery to his contemporaries, so too he remains a mystery to posterity. At least as interesting is Lewin's essay on the bureaucracy, in which he demonstrates not only how the Soviet bureaucracy was invented by Stalin but also how it managed to survive the leader's paranoia – and how after his death the cult of Stalin was transformed into a cult of the state, with the bureaucracy as victor.

Stalin's bureaucracy survived; that of Nazi Germany did not. That is not a coincidence. The chaotic relationships within the Third Reich, which experienced scarcely any stability, inevitably led to its demise and ruin. Hitler's radical mentality ("*Weltmacht oder Niedergang*") contributed more than a little to this. Hitler could afford such a

mentality better than Stalin. However one analyses Stalin, he was part of a tradition of Marx and Lenin. And however much he perverted Marxism, that tradition set limits to his capriciousness. In contrast, National Socialism had neither tradition nor future. Hitler was the Marx, Lenin and Stalin of National Socialism. For twelve years he directed a revolution against Judaeo-Christian civilization that was unparalleled in world history. Ultimately, what he did is incomprehensible. The contributors to this volume should not interpret this as a criticism however: they have written a magnificent book, and every scholar of contemporary history would do well to read it.

Wim Berkelaar

DENECKERE, GITA. *Sire, het volk mort. Sociaal protest in België 1831–1918*. Hadewijch, Antwerpen [etc.]; AMSAB, Gent 1997. 416 pp. B.fr. 1290.00; D.fl. 64.90.

Gita Deneckere has written a richly documented and theoretically informed study of Belgian protest using sociologist Charles Tilly's model of changing repertoires. Tilly argues that protest or collective action, as spontaneous as it may look, usually takes place according to certain "repertoires", which change with socio-economic trends and political opportunities. In the mid-1800s, food riots, turnouts or marches by workers, and shaming rituals (*chivaris*) gave way to demonstrations, organized strikes, and political petitioning.

Belgium provides an excellent test for Tilly's model. It was the second country to industrialize. Yet democratization proceeded in an uneven fashion. A democratic constitution was granted with independence from the Dutch in 1830, but universal male suffrage was implemented only in 1894, and even then with plural voting which was only eliminated in 1918. Belgium, too, has had a long history of street actions – "*commotions populaires*" – and general or nationwide strikes. In applying Tilly's model, Gita Deneckere provides an impressive overview of the whole range of socio-economic and political protest in Belgium, drawing on a wide variety of sources – newspapers, personal papers, police reports, and internal political party documents.

Deneckere shows clearly that Belgian government leaders, after independence from the Netherlands, reacted to pressure from below by pro-Dutch or "Orangist" forces and by discontented factory owners and their unemployed workers. In 1834, the government banned Orangist demonstrations in order to curb anti-Orangist agitation which might get out of hand and threaten the maintenance of order (p. 35). It also split potential opposition by winning over Orangist factory owners with subsidies (p. 38).

Even though Belgium was relatively industrialized and urbanized, this interaction between government and protest from below took place in the mid-nineteenth century, Deneckere argues, within a largely traditional repertoire of paternalism and customary popular action. Protests in Ghent during the 1830s demonstrated, she says, "vertical solidarity" between workers and employers on a number of issues (p. 57). Cities distributed bread as a means of controlling riots in the 1840s (p. 106). When industrialists pushed for free trade around 1860, "*de intermediare groep van meestersgasten en klerken speelde een sleutelrol in de mobilisatie van de arbeidersmassa*" (the intermediary group of foremen and clerks played a key role in the mobilization of the mass of workers) (p. 169).

Deneckere documents the change in collective action that Tilly sees in the mid-nineteenth century. She argues that workers, artisans, and the lower middle class transformed the repertoire of protest in a variety of ways in the period bracketed by food riots in 1846 (p. 117) and 1861 (p. 124) and labour strikes between 1861 and 1886 (p. 191). By 1861, protests over food were not only over bread, but other food items as well, and their target was as much wages and the cost of living as bread prices. Strikes gradually lost their ritualized or paternalist character; instead, workers made clear their economic claims upon employers in more instrumental terms. This transformation, Deneckere argues, paved the way for political mobilization by workers themselves rather than middle class intermediaries. Even then, she points out, socialists could still weave in revolutionary emblems with traditional symbols of urban independence. Internationalists marching in Ghent in 1875 sang the *Marseillaise*, waved red flags, but also “*brachten een groet aan het standbeeld van Jacob van Artevelde*” [saluted the statue of van Artevelde, the medieval hero of Flemish cities] (p. 203).

Deneckere’s treatment of the socialists appears a bit less original since this is a tale that has often been told before, but she makes interesting points nonetheless. Even as the Socialist party leaders channelled protest into more institutionalized forms to extend the right to vote, workers could still use political slogans as though they were traditional or religious symbols: “*Vive le SU*” [Up with “*Suffrage Universel*” (universal suffrage)] had an “eschatological” hold on many workers (p. 257). She does an excellent job in showing how socialists moved from anti-royalist protests in the 1880s and 1890s (p. 207) to relying in the general strikes of 1902 and 1913 on the possibility of the king pressuring the Catholic government to institute pure universal male suffrage (pp. 338–339). Deneckere’s sensitive juxtaposing of memoirs and personal papers of the elite with police reports on street actions is an impressive achievement in the historiography of collective action. The new socialist tactics, she shows, meant breaking with revolutionary traditions: “[...] *de Jacobijnse traditie werd in het laatste decennium van de 19e eeuw verdrongen uit het socialistische repertoire*” [The Jacobin tradition was pushed out of the socialist repertoire in the last decade of the nineteenth century] (p. 255). Her introduction to this last phase of collective action before World War I could serve as a summary of her larger argument: “[...] *oproer en stakingen aan de basis lagen van de geleidelijke uitbreiding van de democratie. De maatschappelijk-politieke integratie van de arbeiders kan niet aan de beslissingen van verlichte staatsmannen worden toegeschreven, maar aan de druk van onderuit*” [Insurrection and strikes laid the foundation for the gradual extension of democracy. The socio-political integration of workers cannot be attributed to the decisions of enlightened statesmen, but to pressure from below] (p. 230). As rich and extensive as Deneckere’s uncoverings of lower-class protest are, they do have some limitations. Her work largely concerns Ghent, Antwerp, and Brussels, with only small attention paid to Liège, Deneckere’s only example from the heavily socialist coal and metallurgical areas of Wallonia. The coverage of Liège seems uneven, in part perhaps because she apparently used Leon Linotte’s inventory of social protest in Liège which, in the reviewer’s research on Liège, turned out to be unreliable. The initial socialist mobilization of Liège, in this reviewer’s opinion, was almost completely devoted to electoral politics and did not transform strikes or workers’ collective action. In contrast to Deneckere, this reviewer would suggest that the repertoire of Walloon strikes and protests took longer to become institutionalized: not until 1912 did Liègeois strikers adopt reforms which *Gentenaars* had in 1895. Analysing religious conflict apart from socio-economic and political battles as Deneckere chooses to do is also problematic.

Socialism was almost always vehemently anticlerical and anti-Catholic. Socialist workers themselves combined attacks on Catholics with their demonstrations for universal male suffrage. Deneckere, however, has written a parallel volume on anti-religious collective action in the same period, *Geuzengeweld. Antiklerikaal straatrumoer in België (1831–1914)* (Brussels, 1997) which, if read together with the volume under review presents more of the full story.

None of these, however, are serious flaws in what is undoubtedly a major contribution to Belgian political and social history. Deneckere's extensive research and theoretically-informed conclusions should encourage other work on Belgian collective action by scholars, and her results should be conveyed to historians of other European societies.

Carl Strikwerda

KALB, DON. *Expanding Class: Power and Everyday Politics in Industrial Communities, The Netherlands, 1850–1950*. [Comparative and International Working-Class History.] Duke University Press, Durham [etc.] 1997 [recte 1998]. xi, 341 pp. Ill. \$21.95; £19.95.

In *Expanding Class* Don Kalb argues strongly that class is an important analytical tool in social and historical analysis. He does so oscillating between high theory and two case studies of industrial and social relations in the province of North Brabant in the Netherlands in the first three decades of the twentieth century.

In the theoretical chapters Kalb takes on both reductionist class analysis and post-structuralists who also criticize economic reductionism but who throw out the baby with the bath water. In doing so, Kalb invokes Norbert Elias, the Tillys, Alf Lüdtke, Eric Wolf, Ira Katznelson, Ron Aminzade, Eric Hobsbawm, Edward Thompson and the principal protagonists of numerous other strands of social analysis. This opens up many avenues for research. Some of these Kalb walks down in his empirical chapters, others remain as interesting suggestions and options for the reader to follow. Kalb especially criticizes an approach in which industrialization leads automatically to the formation of an industrial working class which then proceeds to form a socialist movement based on its working-class consciousness. Instead of taking this preconceived model as their starting point, scholars should ask themselves why historical actors did what they actually did. Kalb tackles this question at the level of the working-class household. His analysis points to matters as diverse as the spatial organization of industrial towns, the gender division of labour within the family, and civilizing coalitions in which the intended objects of civilization participate. All of these elements are to be applauded. However, in the process *class* has become charged with so many suggestions, ideas and meanings that it becomes an unwieldy research concept. If class is to contain so much, it becomes very plausible that we must analyse labour relations and social relations in the light of class, but it also becomes less clear what that statement actually means.

The empirical analysis is of shoemakers in the vicinity of Waalwijk and the early years of the Philips multinational electrical manufacturing company in Eindhoven. The Philips case is a good example of Kalb's approach, in which he combines a re-analysis of the existing literature on Philips with oral history and articles published by the trade-union press to reconstruct Philips's labour policy and its consequences.

North Brabant used to be a peripheral low-wage area. This was why Philips located its light bulb factory there in the first place. Philips produced mainly for an international market, in which it could compete only by keeping its production costs low. It did so by having the bulbs assembled by low-paid peasant girls who lived in scattered villages around Eindhoven. As this was only one of several sources of income, local peasant families were willing to supply their daughters for low wages. These daughters were already disciplined to contribute to the family income. Used to the authority of their fathers at home, they easily adapted to workplace authority.

As Philips grew, it had to import skilled workers (mainly metalworkers and glass workers) from elsewhere. To attract these, Philips had to pay high wages. The firm also required more low-wage labour. By 1924 Philips was using the local supply of unskilled cheap labour fully. This became an acute problem when Philips became very successful in the production of radio sets from 1927. For some time the firm tried to compensate by bringing in commuters and lodgers. These, however, proved both less malleable as workers and created unrest locally. Lodgers were seen as a threat to morality and Philips was supplying the Catholic Church with an argument against the firm. Philips therefore resorted to another policy. It brought in what one could call industrial peasants in the form of migrant families from Drenthe, another poor sandy region of the Netherlands. The male heads of these households were offered a relatively well-paid job with Philips, on condition that they also provided the firm with enough low-paid daughters. These families were housed in new neighbourhoods built by Philips, thus giving Eindhoven the appearance of a standard industrial town. Philips not only provided jobs and housing, but also social provision and leisure opportunities. In this way the migrant families had an important stake in the firm's wellbeing, and a socialist labour movement did not take root. When the crisis of the 1930s struck, Philips' management discovered to its surprise that its labour recruitment system, which was built to maximize growth, was in fact very flexible. If necessary, Philips would fire one of the members of a family, but by keeping other members on its payroll it was supplying an extremely cheap form of poor relief and tied labour to the firm. Especially in the chapter based on oral history, Kalb shows how the tensions and the opportunities of working at Philips marked the relations between fathers, daughters and mothers of the families employed. The sons, however, are conspicuously absent from the analysis.

This is an astute analysis of what happened at Philips between the 1900s and the early 1930s. But is it also, as Kalb claims, an analysis of a special system of "flexible familism" or "Philipsism"? Here the argument is less convincing. As Kalb acknowledges (p. 151), it was in fact a series of solutions for different problems. Philips would pay skilled workers whatever it took to import them from elsewhere. The firm got unskilled workers for low wages first from the neighbouring agricultural villages, then through commuters from further away and then again by importing migrants from still further away. If the study had covered a longer period it would have shown Philips setting up factories in other low-wage areas inside and outside The Netherlands and importing cheap labour from around the Mediterranean. Similar policies to form a local wage proletariat from among peasant households, to bring in migrant labour, to contain socialism and to preserve the established hegemony of the Roman Catholic Church were employed around the same time by the Dutch state and private mines in the nearby Limburg area, and, with less close co-operation from the Belgian state, by the coal mines in nearby Belgian Kempen. All succeeded in keeping out socialism. If "Philipsism" was thus not confined to Philips, even Philips could not keep it up much longer

than the period described here. After the exceptional periods of the crisis of the 1930s, the war and the post-war rebuilding of the Dutch economy had passed, Philips had to raise its wages just like all other employers. But even if there is no need to apply a special term to a system of labour recruitment which was to a large degree contingent and which consisted of elements employed by comparable employers under comparable circumstances elsewhere, this should not keep us from welcoming a good analysis of this system.

Lex Heerma van Voss

ECKERT, RAINER. *Arbeiter in der preußischen Provinz. Rheinprovinz, Schlesien und Pommern 1933 bis 1939 im Vergleich*. Peter Lang, Frankfurt/M. [etc.] 1997. 778 pp. S.fr. 119.00.

This is an important contribution which focuses on a number of key debates concerning the nature of German fascism and, in particular, the reaction of the working class to the National Socialist regime. The regular Gestapo reports have already been used by other authors to explore the changing levels of support for the Nazi dictatorship. Eckert is fully aware of the potential difficulties in relying on this type of material as a source of "objective information", and has sensibly extended the range of his investigation by utilizing additional reports from other administrative agencies, including senior police and district officials. Eckert's approach is also deliberately comparative: he has chosen to develop his research agenda by examining working-class attitudes in three border areas, namely the Rhineland, Silesia and Pomerania. Regional policy during the Nazi period still remains a relatively neglected research theme and Eckert's analysis of "*der Praxis im kleinen Bereich*" is also to be welcomed on this basis. Basic information is provided on each of the three provinces, in terms of their economic and social structure, as a prelude to a very detailed analysis of the overall position of the working class and their reported attitudes to the regime during the prewar period. The situation in each of the provinces is examined in turn within the framework of designated sub-periods: from the Nazi seizure of power to 30th June 1934; from the Röhm affair to September 1936; and finally from the start of the Four-Year Plan to the immediate pre-war period. In each case, Eckert provides a detailed analysis of working-class attitudes within the specific context of economic and employment conditions in the individual provinces. In Pomerania labour creation schemes were of central importance in reducing rural unemployment and generating "peaceful" conditions; in Silesia selective price controls on products purchased by the working class contributed to growing complaints against the regime; whereas persistent shortages of butter and margarine in the Rhineland fuelled local resentment. A final section highlights some of the major findings from this study: working-class attitudes at the provincial level were affected by such factors as the level of industrialization and urbanization, confessional affiliation, the extent of politization before 1933, and by the existence of a collective sense of identity. Although dissatisfaction with the regime persisted during the prewar period it became less extensive over time and was invariably caused by material factors, including poor housing, price increases and food scarcity. There was no evidence of a class-based opposition to the Nazi state, which was never able to control effectively the conditions of daily life.

Despite the undoubted merits of this book, it fails to develop a convincing compara-

tive approach. The background information on the three provinces is derived from a limited range of secondary sources and the decision to examine working-class attitudes during three distinct sub-periods precludes any rigorous analysis of the impact of Nazi policy on regional development, sectoral growth or employment. Too often aggregate data are used to illustrate contemporary problems: the trend in net additions to the housing stock in Pomerania, for example, is used to illustrate the persistence of a housing shortage in the mid-1930s. But the key issues remain unexplored and there is insufficient evidence to make a meaningful comparison between the three selected provinces. Moreover, the overall structure of Eckert's study often militates against effective comparison. No attempt is made to synthesize on a comparative basis the discussion of working-class attitudes during the three sub-periods between 1933 and 1939. Pomerania suffered from territorial readjustment in 1936 and the lack of both Gestapo and local administrative reports inevitably restricts a comparison with the Rhineland and Silesia where the record basis is considerably superior. Finally, it might be argued that an analysis based on specific sectors or individual communities might have provided a more appropriate starting point for this study; although the Rhineland and Silesia shared some common features, specifically in terms of the relative importance of heavy industry and secondary sector employment, they were quite clearly distinct as far as the pattern of long-run development was concerned, and Pomerania retained a strong agricultural base. Nazi policy from 1933 onwards would therefore have had a differentiated impact in these three provinces and attitudinal responses would have varied accordingly, particularly as these were determined primarily by economic considerations. Nevertheless, this study is to be welcomed: it offers a detailed analysis of changing working-class attitudes to the Nazi regime at a provincial level and should stimulate further research on the economic and political impact of Nazi regional policy.

W. R. Lee

OSGERBY, BILL. *Youth in Britain since 1945. [Making Contemporary Britain.]* Blackwell Publishers, Oxford 1998. xiii, 256 pp. Ill.

This book in the series *Making Contemporary Britain* offers a general overview of changes within British youth culture after World War II. It sets these developments within the context of British social, economic and political life. The author gives concise descriptions of the various youth subcultures in successive postwar periods, finishing with today's "postmodernist", eclectic and hybrid lifestyles among youth. In a clear attempt to avoid presentism, Osgerby also points to continuities and discontinuities with British youth culture in the nineteenth century and between the two world wars. In addition, he surveys continuity and change in social representations of youth and strategies of social control by governmental and private agencies in the postwar era.

After a brief introductory chapter, chapter 2 covers the development of youth cultures in the UK before 1939, and examines the impact of World War II on youth and the post-war stereotyping of young people by moral authorities as the embodiment of both the best and the worst aspects of social and cultural change. Chapter 3 looks at the ways in which, during the immediate postwar period, "youth" became further institutionalized as a distinct age grade, a process enhanced by economic trends that increased demand for young people's labour and delivered greater financial power to

especially working-class youngsters. Chapter 4 deals with the consequences of these changes, focusing on the expansion of the commercial youth market and the rise of the “teenager” as a social construct. This is followed by a chapter on the significant changes that took place in the lives of young women during the postwar period, especially the gradual emergence of more proactive versions of feminine identity. Chapter 6 surveys the developments in subcultural styles in the late sixties and the rise of sociological approaches which highlighted social class as a key determinant of the lifestyles of British youth. Chapter 7 concentrates on middle-class youth and the various counter-cultural movements that manifested themselves during the sixties, and examines these developments against the backdrop of the wider transformation of British cultural and political life at the time. Chapter 8 describes the confrontational subcultural styles of the late seventies and the early eighties, particularly those of punk, whereas chapter 9 explores the history of black youth subcultures, and the impact of race and racism on British youth culture. The next chapter traces the postwar history of the juvenile justice system and those agencies involved with the social control of young people. It highlights the way in which a reformist spirit of rehabilitation and welfare was replaced, during the seventies and eighties, by a more punitive set of intervention strategies, as a change towards a more authoritarian form of political state took place. Chapter 11 turns towards changes in lifestyles among youth again, especially the impact of the “acid house” and rave dance phenomena and the rise of the New Age traveller movement during the late eighties and early nineties. It also shows how once again moral panics regarding youth behaviour arose, and how political authorities met these developments by the passage of the 1994 Criminal Justice Act, according to the author “one of the most repressive pieces of legislation to be introduced by a modern British government” (p. 3). The book concludes with a brief sketch of the social and economic conditions that young people have to face as Britain enters the next century. The last chapter also emphasizes that the forms and styles of contemporary youth culture have become characterized by new, “postmodern” features of fluidity and cultural hybridity, in a societal situation of increased risk and uncertainty that has come to distinguish young people’s position in the labour market.

The book is written in crisp prose, refreshingly free of jargon, and is accessible to undergraduate students and the general reader. It is based entirely on secondary sources (the most significant are mentioned in a brief annotated bibliography at the end of the book), as one may expect given its survey character. One of the book’s strengths is its incorporation and application of a cultural studies approach. The author borrows from the various contributions made by associates of the Birmingham School of Cultural Studies (the co-called CCCS approach as initially developed at the Centre for Contemporary Cultural Studies). His summaries of the findings of these studies are generally accurate and sharp. Osgerby has also incorporated criticisms levelled at this strain and recent insights concerning the relationships between subculture and commerce and mass media respectively. According to this view, youth subcultures are not unmediated social formations, nor are they autonomous grassroots cultures which only meet the media when they are in the process of “selling out”, or evoking moral panics. Rather than processes of “containment” having been accelerated during the eighties and nineties, youth subcultures have always been intertwined with the institutions of the market in an ongoing relationship of exchange (p. 201). However, the book’s forte is counterbalanced by its weakness, that is, the highly descriptive nature of much of the rest of the content.

Nevertheless, Osgerby offers the general reader a clear overview of the relevant developments in postwar British youth culture within the broader context of British society. In this he is also up-to-date; even the latest developments of the current New Labour government are covered. Moreover, the general reader derives a better understanding of the lives of British youngsters and the ways in which they are contained and policed today. The theme is also enhanced by the discussion of regional differences which contrast the north west with the south east, as well as the characterizations of local situations in specific cities and towns. Yet, for subject specialists Osgerby offers little that is new regarding developments in postwar youth subcultures in Britain, but in all fairness they are not his target audience. Another flaw is the overexposure of spectacular or eccentric youth subcultures at the expense of more mainstream trends. However, this is not surprising since it is a reflection of Osgerby's references.

The book also contains a minor factual error that should be addressed in subsequent editions. In chapter 7 the author refers to the rise of the Beat Generation, and suggests that the immediate origins of this literary-expressive movement in the US lay in postwar Paris and the avant-garde intelligentsia of the Left Bank (p. 83). Although there were cultural exchanges between French existentialists and American Beats in the late 1940s and 1950s – and Beat writers have been inspired by French existentialism in addition to various other sources – the Beat movement was actually the first Bohemian group in America that *did not* emulate French Bohemia. Characteristic of the (American) Beats were their pronounced and creative synthesizing of European and American art forms and cultural styles rather than being a mere emulation of European artistic tendencies. In the latter respect the Beats made a clear break with a long-standing tradition among American Bohemians of simply following primarily what went on in France. After all, they embodied a quintessentially *American* protest against the American Way of Life. Furthermore, Osgerby underexposes the way in which American Beat culture was selectively borrowed and adopted by British youngsters. They made this into their own lifestyle, thereby also incorporating indigenous British components about which we do not learn very much. One may also ask whether the number of Britain's Teddy Boys who, before the arrival of rock 'n' roll in the mid-fifties, were dancing to the "throbbing sounds of hot jazz" in the local dance halls (p. 120), was of any significance. Or did contemporary reports of this phenomenon confuse middle-class youngsters who were against the grain with Teddy Boys? I also disagree with Osgerby's interpretation of the Spice Girls as some kind of girls' empowerment in the late 1990s (pp. 61–62). He seems to read more assertiveness and resistance against traditional feminine identity in their public performances than actually was the case.

Notwithstanding these criticisms, Osgerby's book has obvious merits as a survey for the general reader. I can also recommend it to students, those in the fields of contemporary history, the social sciences and cultural studies in particular. Because of the radiation of British youth cultures abroad, the book is relevant as well for students of popular culture in other countries who explore the local appropriations of these cultural forms.

Mel van Elteren

Namibia under South African Rule: Mobility & Containment 1915–1946.
Ed. by Patricia Hayes, Jeremy Silvester, Marion Wallace [and] Wolfram

Hartmann, with Ben Fuller Jr. (The “Trees Never Meet” Project.) James Currey, Oxford 1998; Out of Africa Publishers, Windhoek; Ohio University Press, Athens (Ohio). xx, 330 pp. Ill. Maps. £15.95.

The seventy-five years of South African rule and occupation (1915–1990) have been notoriously under-researched. This long-awaited volume, which examines the first three decades of that period, is an important step in overcoming this situation, however. It originated in a conference held in 1994 in Windhoek entitled “Trees Never Meet”, a quote from a Namibian proverb that goes on to add “but people do” (p. viii). Indeed, much of pre-colonial and, certainly, the colonial history of Namibia can be read as an intricate process of the movement of individuals and of communities – and of attempts to restrain and regulate such mobility. This process was linked inseparably to the one central problem faced by the colonial administration and economy: mobilizing labour power and conveying it to where it was applied in mining and settler agriculture. The labour problem was structurally related to issues ranging from the set-up and performance of the colonial administration to the assertion or (re-)assertion of collective identities and the struggle for land, but also to experiences of urbanization and to indigenous attempts at development initiatives.

While having very diverse focuses, the contributions to this volume are given coherence by this theme. But they also demonstrate a great variety of experiences that are conditioned by the marked regional differences in colonial experiences in Namibia. The southern and central regions, which were also known as the Police Zone, were characterized by systematic white farmer settlement and large-scale land dispossession. In contrast, the north, which includes the population centre of Ovamboland, was controlled effectively by the colonial state only after the advent of South African rule and administered for the most part by “indirect control” (p. 17). Here were the main recruiting centres for (male) migrant labour. Besides this, Namibia also presents a great variety of precolonial social and cultural conditions and of ecological habitats, the South and centre being suited to pastoralist ways of life only, while the Ovambo and Kavango regions sustain rainfall agriculture. Thus, as the editors point out in their extensive and seminal introductory essay, Namibian historical experience contrasts with the more familiar “South African grand narratives of ‘dispossession’” and its linkage to the themes of development, modernization and capitalism, bound together in the “even more familiar narrative [...] of ‘historical transition’”: “such a model splinters when transposed to Namibian soil” (pp. 18ff.). This invites closer inspection of received wisdom, as well as careful periodization. At the same time, the Namibian experience stands out as the result of this rather unique constellation, representing a kind of paradigmatic kaleidoscope of trajectories that can also be found in varying shapes in the experience of other parts of southern Africa and indeed in the experience of other former colonies.

Periodization therefore takes on a particular significance. The editors divide the period under study into three distinct phases (see pp. 22ff.): the first is that of martial law and “tentative liberalism” (A. Emmett) from the military occupation in 1915 up to the finalization of the League of Nations Mandate late in 1920, which for black Namibians inside the Police Zone brought a palpable relaxation of the repression and exploitation they experienced; the second is the period from 1921 onwards, when a whole range of laws and regulations were put in place and accompanied by increased efforts at white South African settlement and infringements on the living space of indigenous communities, and occasional savage repression, including the infamous bombing of

insurrectionary Bondelswarts in 1922. Attempts to seal off the north from agitation within the rest of the country were frustrated by the effects of labour migration, “providing a human and economic chain linking the Police Zone to the areas beyond” (p. 30). But “efficient repression” (p. 25) set in only by the late 1920s, when the editors note a third phase marked by attempts at systematic control of the north. Such “utter control” (p. 33) by the colonial state remained in fact tenuous, in particular on account of the effects of the migrant labour system, which “effected a union of the territory that was deeply imprinted in the bodies of [...] black men” and thereby in fact “made the nation” (p. 34). These considerations point to two further central themes of the volume, the low degree of competence on the part of the administration when it came to effective action, and the deeply gendered nature of all the processes and structures concerned, epitomized certainly in predominantly male labour migration but also apparent in reserve and township regulations, in the construction of traditional communities framed into the counterparts of indirect rule, and in the modalities of missionary and schooling activity. All these issues are broached and detailed in twelve individual contributions which are grouped into sections on “Construction of People – Construction of the State”, “The Reserves – Contesting Containment”, and “Beyond the Police Zone – Ovamboland”.

The close interrelationship between the control and construction of social groups and gendered concepts and practices is brought out graphically by Marion Wallace, who demonstrates the linkages between the control of venereal disease in Windhoek and administrative preoccupations. The impact of VD control went far beyond merely “greater control in the Windhoek location” (p. 80). It concerned “fears of black – in particular female – sexuality” (pp. 82f.) and provoked a resistance particularly by Herero women which was linked to a re-evaluation and re-formation of Herero ethnicity, while Herero elders tried to grasp this chance to control their (young) women. An even closer look at the reconstruction of both gender relations and ethnicity is afforded by Patricia Hayes’s analysis of the effects of the great drought and famine in Ovamboland during the “watershed years” (p. 120) of 1929/30. The crisis shattered the system of control over “junior males and women” (p. 124) agreed on by colonial administrators and Ovambo elders. This meant young men moving out, while immobilizing women, thus barring them from cash income. From the famine onwards, it became more difficult to keep women “from seeking mobility which transcended the limits imposed by the state” (p. 128). Those who stayed became subject to enhanced control, based on the linkage between relief and labour for dam building, largely mobilized from among “women and children” (p. 132). This also established patterns of labour division, according tools to men and load-carrying to women and children. Again, this led to the region “becoming an oversupplier of migrant labour” (p. 146) by the 1960s, with men controlling the privileged avenue to cash income. A corollary is provided by Meredith McKittrick’s account of the dynamics of intergenerational power relations in a more westerly part of Ovamboland.

Restrictions experienced in the reserves were due to a large extent to the state-sponsored competition for the same resources by pastoral settler agriculture, as detailed by Jeremy Silvester. In some cases, this could be overcome partly by ways to generate cash income. Ben Fuller recounts the battle residents of the Otjimbingwe reserve waged over decades against the consequences of the small size of their reserve. They bought additional land, moved into creamery schemes, but eventually floundered through internal conflict articulated in ethnic terms but traced back to a range of origins, which

include colonial ethnography, the consequences of incipient apartheid policies, and the ubiquitous mover of things in Namibia, drought. Michael Bollig, dealing with supposedly remote Kaokoland, demonstrates the intervention of colonial policies in shaping ethnic identities; these policies “literally” forced the Himba/Herero, one of Namibia’s current, supposedly pristine tourist attractions, from “economic diversification [...] into the role of ‘traditional’ livestock keepers” (p. 192). These studies are linked together by Robert Gordon’s stimulating account of the ambivalences of enforcing the vagrancy laws, a cornerstone of efforts to control black Namibians within the Police Zone. While failing “from a labour instrumentalist point of view” (p. 68), the impact of these provisions lay in the “routinisation of terror in day-to-day interaction” (p. 74) they entailed. Gordon goes on to conclude that “Contrary to Foucault and especially Giddens, disciplinary power does not depend on information retaining surveillance, but rather on a set of documents that legalises officials’ and settlers’ autonomy” (p. 75).

These glimpses can only hint at the richness of these and the other contributions that could not be mentioned here for lack of space. A particularly commendable feature of this publication is the provision of carefully selected photographs that function as a visual commentary to many of the arguments presented.¹ The volume covers just over a third of the period of South African rule in Namibia and shows that there is still ample opportunity for extensive and innovative research. But it provides guidelines for such work which one will not be able to ignore, and it gives important hints for comparative perspectives, especially on “native” policy and the role and dynamics of traditional communities.

Reinhart Kößler

MORI, HIROMI. *Immigration Policy and Foreign Workers in Japan*. Macmillan, Basingstoke 1997; St. Martin’s Press, New York. xiii, 227 pp. £42.50.

It is almost twenty years since the wave of global migration first reached Japan in the early 1980s. The population of non-permanent residents (including the authorized and the illegal) rose significantly, from approximately 170,000 in 1984 to more than 1.1 million at the beginning of 1997. This figure includes professional and skilled workers in possession of work permits (excluding entertainers they numbered between 70,000 and 80,000 in the mid-1990s) and students, but most non-permanent residents were unskilled workers engaged in the manufacturing, construction, and service industries. The illegal immigrants, most of whom were from other Asian countries, tended to stay increasingly longer but to decline slightly in number. On the other hand, the number of foreigners legally engaged in unskilled labour increased, despite a policy designed to prevent foreigners gaining access to unskilled jobs. Why? What made the Japanese government adopt a policy of admitting foreigners through the “side door”, and what was the result?

The author, who has made a special study of the social statistics, has analysed this

1. For a kind of companion volume, see Wolfram Hartmann, Jeremy Silvester and Patricia Hayes (eds), *The Colonising Camera: Photographs in the making of Namibian History* (University of Cape Town Press, Cape Town, 1998; Out of Africa Publishers, Windhoek; Ohio University Press, Athens, Ohio).

new trend in co-operation with the Migration for Employment Branch of the International Labour Organization, which was also interested in the new destination for global migration. The key concerns of Mori's book are how the transformation of Japan's labour market gave birth to factors "pulling" foreign workers into the country and what impact their massive inflow had on the labour market (p. xii). The author further describes the effect of the mechanism described above on immigration policy at the end of the 1980s, and how policy reactions influenced migration flows to Japan and segmented Japan's labour market between the indigenous population and immigrants, and between migrants themselves (*ibid.*). This book is valuable for clarifying, by means of an empirical study of Japan's experience during recent years, the impact of immigration policy on the mechanism of labour allocation, and for providing the basis of an argument which allows a comparison to be drawn with the experience of western Europe.

Mori begins by explaining the nature of government control on immigration and the restrictions on foreign residence in Japan. He presents some useful statistics and categorizes the foreign residents. He makes two important points. One is the importance of the distinction among Japan's entire foreign population between the "old-comers", descendants of immigrants who came from former Japanese colonies (mainly Korea and Taiwan), principally during the 1920s and 1930s, and the "newcomers", most of whom came to Japan during or after the 1980s. Another is the distinction among the foreign population entitled to stay between those authorized to stay because of their activities ("permissible intake" group) and those on whose activities no restrictions were imposed ("permissible establishment" group). Although the "old-comers" are not Japanese, they are not labelled as "foreign workers" because they have been in Japan for a considerable time and have been integrated into Japanese society. "Foreign workers" in Japan include not only those with a work permit, but also foreign college and pre-college students (who are allowed to work part-time), and trainees receiving on-the-job training. However, most foreign workers are either *Nikkei* (descendants of Japanese emigrants), who fall in the category of unspecified "permissible establishment", or illegal immigrants.

Chapter 2 mainly discusses the structural changes in Japan's labour market in relation to both labour demand and labour supply, changes that led to large numbers of foreign workers being pulled into unskilled jobs. The second half of the 1980s saw the wage allocation mechanism that had hitherto been effective in adjusting various labour imbalances begin to malfunction. So-called "3D" (i.e. dirty, dangerous or demanding) jobs and "undesirable jobs", whose status was low and where working conditions were poor, suffered from a chronic and serious shortage of workers since, despite offering higher wages, they failed to attract indigenous labour. Japan began to draw immigrants, and this is surveyed in chapter 3. As for the placement of foreign workers in the labour market, chapter 6 provides a general picture of sectoral and occupational profiles and employment status, and chapter 7 summarizes recent research and the segmentation argument in relation to the labour market and foreign workers like the *Nikkei*, foreign students (part-time workers) and clandestine workers.

In many ways Mori's book breaks new ground, though a number of scholars have already examined the impact of immigration policy on the process of the segmentation of the Japanese labour market. In this sense, the core of this book is chapter 4. Here the author reviews the history of Japan's immigration policy, focusing on the 1990 reforms that dichotomized Japan's foreign workers. This reform was intended to acknowledge the fact that increased opportunities for international travel in the 1980s

made it impossible for existing regulations to cope with the various activities of foreigners and that tough immigration control was failing to prevent a sharp increase in the number of “illegal overstayers” from Asian countries who were employed in unskilled jobs. The discussions in the late 1980s focused on what response Japan should adopt to control those foreign workers entitled to work as unskilled labour. Mori does not explicitly mention the fact that the Immigration Department strongly opposed granting “illegal overstayers” the amnesty that many groups supporting migrant workers were arguing for. It also opposed admitting foreign workers on a “rotation system” basis because of the difficulty in preventing them from staying permanently. Japan was eager to avoid western Europe’s experience in this respect in accepting foreign workers.

Eventually, the government succeeded in creating an institutional mechanism whereby a large number of foreign unskilled workers were admitted legally, but without this compromising the principle of not allowing foreign workers to fill unskilled jobs. One tactic was to grant the status of “permissible establishment” to descendants of Japanese emigrants and their non-*Nikkei* spouses; their ethnic origin was distinguished from that of other foreigners and they were relatively few in number (though some estimates put the size of the *Nikkei* population in South America at over two million, this was still less than the size of the Asian population that may have intended to enter and work in Japan). Another tactic was to take advantage of the trainee system, which could be regarded as an aid programme, and allow small- and medium-sized companies to take on trainees (a “technical intern training programme” was adopted later on, allowing trainees to work for a designated period after passing an examination). As a result of these reforms migration flows changed significantly and Asian migrant workers were quickly replaced by *Nikkei* from South America. Nevertheless, this does not mean that the “illegal overstayers” decreased in number, but rather that many of them became even more marginalized, forced to work in conditions that were even worse and where pay was even lower.

This “side door” policy has been accompanied by unexpected and increasing social costs. Chapter 8 considers the effect of this policy on the provision of public services for immigrant residents. It points to the serious problems associated with “illegal overstayers”, who have no access to public services (not even to health care). These problems include the high cost of health care, and the increase in the level of unpaid medical bills, which medical institutions then have to bear. Furthermore, many of the *Nikkei* who were able to migrate to Japan as members of the “permissible establishment” took with them other members of their family, including their children, with concomitant problems for the education system. For example, there has been a growing problem of foreign children refusing to attend school because of the difficulty they face in mastering Japanese customs and the language. Japan is experiencing something very different from the four-step migration process defined by Castles and Miller.¹

Mori describes Japan’s experience as a new labour-importing country since the mid-1980s. Japan has accepted unskilled foreign workers in a way quite different from how western Europe approached its labour shortage problem in the 1960s. His book should be useful to those interested in the comparative study of the acceptance of foreign workers.

Tatsuya Chiba

1. Stephen Castles and Mark J. Miller, *The Age of Migration: International Population Movements in the Modern World* (London, 1993).