

BOOK REVIEWS

HARMAN, CHRIS. *A people's history of the world*. Bookmarks, London [etc.] 1999. vii, 729 pp. £15.99.

Chris Harman's *A People's History of the World* is an ambitious attempt to provide an accessible single-volume overview of human history from a historical materialist perspective. Harman, a prominent British socialist, explicitly aims to provide a general history that uses class analysis and, for once, brings the subordinate classes and their struggles with the ruling classes to the centre of the historical drama in a real "history from below".

Harman quotes Bertolt Brecht's "Questions from a Worker Who Reads" on the first page:

[...] In what houses
Of gold-glittering Lima did the builders live?
Where, the evening that the Wall of China was finished
Did the masons go? Great Rome
Is full of triumphal arches. Who erected them? Over whom
Did the Caesars triumph? [...]
So many reports.
So many questions.

He succeeds admirably in giving some of the answers in an eminently readable, frankly fascinating survey of humankind's history from preclass primitive communalist societies to the emergence of class societies in the mists of antiquity 5,000 or 6,000 years ago, through the empires of the ancient world, to the birth of capitalism five centuries ago. In each instance, Harman is at pains to show how technologies and class structure and struggles shaped historical outcomes.

The agricultural revolution of ten millennia ago allowed permanent settlements to develop, a slow expansion of the human population from around ten million people to 200 million by 1500 AD, and ongoing technological advance. The early horticultural societies remained relatively internally egalitarian, although warlike towards their neighbours.

But agriculture, by allowing the production of a reliable surplus, made possible the emergence of nonproducing exploiting classes, a core feature of early urban-centred, Bronze-Age civilizations in Asia, Egypt, Central America, and Mesopotamia. Although there were advances in agriculture, metallurgy and writing in these societies, there was also massive class and gender inequality (Harman arguing that gender inequality first appeared in this period).

Harman maintains that many early civilizations closely approximated Marx's "Asiatic mode of production", dominated by a ruling class of priests and kings exploiting artisans, peasants, and slaves through an emergent state machinery in the absence of private property. It was here that the first recorded strike took place, when Egyptian pyramid builders downed tools in 1170 BC over late rations. But these societies were stagnant and tended to collapse or simply reproduce themselves largely unchanged for millennia.

The next phase belongs to the great Iron-Age empires of the Mediterranean basin,

Central America, India, and East Asia such as the Greek city states, Rome, and the Ch'in Empire. Like H.G. Wells, who undertook a similar exercise in socialist history eighty years earlier in *The Outline of History*, Harman is careful not to minimize the "share of the central uplands of Asia and of the Persian, the Indian and Chinese cultures, in the drama of mankind" (Wells). Harman notes that the Ch'in Empire, founded in China in 221 BC, ruled a larger population (and built more roads) than Rome, and was able to put 300,000 people to work on the first Great Wall. The new empire forged by the Sui and T'ang rulers in the sixth century AD was even more impressive, with cities of five million people, bank notes, gunpowder, mechanical clocks, advanced naval techniques, printing presses, and large-scale steel production.

For Harman, the actors in these civilizations were not kings, priests and philosophers, but classes. In ancient Greece, history is partly the history of rebellious Helot serfs in Sparta, and peasant and artisan struggles against rich Athenian landlords (leading to a limited, but real democracy). In Rome, the class wars – between plebeians and patricians in the first republic, the Gracchi brothers' struggle for land reform in the second century BC, and, above all, the awesome revolt of Spartacus, who led 70,000 slaves in 73 BC in "the biggest slave revolt in the whole of the ancient world" – are central parts of the narrative, not extras. In China, peasant rebellions precipitated the collapse of the Ch'in dynasty, and, in other revolts, peasants seized whole provinces again and again, only to relinquish power to class enemies, new emperors and dynasties.

These civilizations broke down through class war and their own limitations. Slave labour, so central to Rome, could only be secured through continual conquest; when the wars ended, the empire faced a labour shortage and leaned increasingly on peasants who revolted and deserted; it collapsed in the fifth century AD through a cycle of war and agricultural decline. In China, peasant rebellions overthrew dynasties but failed to reconstruct society on new lines, whilst the powerful merchant bourgeoisie lacked the capacity to challenge landlords and state officials for power.

The breakdown of Rome, India and early China led to forms resembling feudalism in the fifth century AD (earlier in China), centred on autonomous warrior landlords ruling fiefdoms of unfree peasant labour. The focus of "civilization" in the next millennium shifted to the Islamic world and the revived Chinese empire.

Yet it was from Western Europe, (an agrarian backwater after Rome's fall, but reborn by the thirteenth century), that an entirely new social system was to emerge in the sixteenth century: capitalism. The bulk of the *People's History* is focused on the world historic transformation wrought by this system through conquest, class restructuring, and above all, the industrial revolution that transformed human productive capacity in a way unseen since agriculture's advent. Harman focuses on the role of class and technology in capitalist development, and on how capitalism swept before it the remnants of primitive communalism and the ancient world to create a single world system dominated by competitive commodity production.

His discussion ranges from the nature of plantation slavery and its relationship to emergent racist ideology, to the French Revolution, the Paris Commune, the "Scramble for Africa", May 1968, and the post-Cold-War "New World Order". In the process, Harman makes a convincing case that the former world of "actually existing socialism" was simply a chapter in this process, an extreme form of capitalist revolution from above, not dissimilar to Germany and Japan's hothouse transition to capitalism at the hands of feudal elites.

Accompanying the capitalist revolution was the creation of a "universal class", the

modern proletariat whose birth and struggles and achievements – particularly the revolutions, successful and otherwise of the 1910s – Harman documents in detail. Harman estimates the modern working class at about two billion people, surrounded by a further two billion peasants and petty commodity producers, making it the largest single class in human history. There are more industrial workers in South Korea today, Harman notes, than in the entire world when Marx and Engels wrote the *Communist Manifesto* in 1848. It is this class, including in its ranks the descendants of the most diverse and impressive of past civilizations – from the Aztecs to the Chinese, Egyptians, Italians, Germans, Indians, to the Zimbabweans and Zulu – which Harman contends holds the key to the creation of a better, democratic, socialist, future.

Any work as sweeping as Harman's is inevitably open to interpretive and substantive criticisms. It is a pity that Harman did not dwell more on the important task of developing a theoretical analysis of the modes of production and laws of motion of premodern societies, revisiting debates on the utility of models like the "ancient", "Asiatic", "lineage" and "tributary" modes of production. Material on Africa (outside Egypt), whilst not absent, is somewhat thin for ancient and modern periods. There is a substantial historical materialist literature on Africa, adeptly synthesized in Bill Freund's *The Making of Contemporary Africa: The Development of African Society since 1800* (London, 1984, 2nd edn, Boulder, CO, 1998), which should be read as a companion volume to Harman's work.

Harman is not altogether convincing in his orthodox Marxist contention that no premodern class war could have succeeded in reorganizing society because artisans, peasants, slaves and merchants lacked cohesion and programmes of social reconstruction. As Harman's own material indicates, peasant movements, such as the Taborites in fifteenth-century Bohemia, the German Peasant War of 1524, and the Zapatistas in Mexico in the 1910s, *were* able to at least conceive of a new social order. Is it therefore reasonable to contend that their struggles were doomed?

For a history of class struggle, the *People's History* is selective in its account of nineteenth- and twentieth-century labour and peasant movements. Harman mentions that he relied on Trotsky for the early twentieth century, and it shows. Recent research underlines the enormous influence of anarchism and revolutionary syndicalism between the 1860s and the 1940s in labour revolts, peasant wars, and anticolonial struggles, often overshadowing Marxism as a revolutionary current. But these are absent from Harman's account of European popular movements (besides, predictably, France and Spain), let alone those of Latin America and Asia. The effect is to reduce post-1840 radical labour history to a history of social democracy and Leninism.

For all that, Harman's work remains a monumental synthesis of the first order, of use to everyone interested in the human past and future, whether activist, student or general reader.

Lucien van der Walt

ROSS, ERIC B. *The Malthus Factor. Population, Poverty and Politics in Capitalist Development*. Zed Books, London [etc.] 1998. viii, 264 pp. £45.00; \$65.00. (Paper: £14.95; \$25.00.)

Two essays lurk inside the covers of this book: the first discusses the eighteenth-century origins of Malthusian theories of population dynamics and their transmutation in the

course of the nineteenth century; the second part is about the political economy of late capitalism and, in particular, its attempts to forestall communist/socialist revolution in low-income countries through aid programs designed to provide an alternative road to modernization. Ross's intellectual history of Malthusian theory is much more successful and interesting than his attempts to link this ideology to the practise of international aid policies. To my way of thinking, the second half of the book is essentially a prosopography of the American Cold Warriors who have manned the international capitalist brigade, and is not particularly insightful in linking their supposed Malthusian-inspired ideology with their repressive practises.

Malthus's influence is primarily of importance because he provided a way to link early modern discourses on poverty – which largely blamed the poor for their condition – with the rhetoric of capitalist–labour relations. Of particular importance is his view that the social management of the labour force is intimately connected with the political economy of social protection systems. In his own time, Malthus explained this brilliantly. In response to what alarmed his contemporaries in the landed classes – runaway welfare costs – Malthus's 1798 “Essay on the Principle of Population” displaced the explanation of the problem to forces outside the fabric of their society. His recourse to very simple examples comparing and contrasting the difference between arithmetic and geometric rates of increase – and suggesting that this model could be seamlessly transferred to social relations – had an electric effect on his contemporaries.

Malthus shifted the poverty discourse to a new plane – away from social relations and towards the “nature of things”. In so doing, he neatly domesticated Bernard Mandeville's lesson – public virtues (i.e., indiscriminate charity to the deserving poor) could – and now, manifestly, did – lead to private vices (i.e., indiscriminate reproduction of these same poor people). The lesson to be learned from this exercise was hard-hearted: to be “effective”, charity had to be doled out in a discriminating way. Rather than being an act inspired by Christian ethics, welfare was to be a form of policing the poor, surveying their most intimate behaviours. Assistance should “attempt to better the condition of the labouring classes [...] by cultivating a spirit of independence, a decent pride, and a taste for cleanliness and comfort”. In other words, social assistance was revisioned as a disciplinary technique and, as such, it could be denied to those who refused to help themselves.

The first two chapters of Ross's *The Malthus Factor* splendidly delineate the genealogy of the parson's ideas; the enthusiastic welcome his ideological innovation received, and the kudos that were heaped on Malthus; also his influence on the reworking of the English Poor Laws in the 1830s; and the terrible impact that these new welfare rules had in Ireland, where they were deployed to justify nonintervention during the Famine. This is the best part of the book: it is insightful and studded with marvellous examples of the author's eye for the telling quote.

My only reservation with his first two chapters stems from my sense that Ross might have expanded his discussion by supplementing his account with additional information drawn from the secondary literature. In particular, I was surprised that neither Patricia James's biography of Malthus nor E.P. Thompson's many writings on the social history of the period seem to be incorporated into the account. Indeed, there is no cognizance taken of the mountain of scholarly research that has modified the Thompson version, which is in many ways a rehash of an older school, centring on the Hammonds. That is not to say that these so-called “pessimists” were incorrect in drawing attention to the costs of the first foray into industrialization, but rather that they neglected to balance their account with a

survey of the benefits that issued from new levels of productivity. Ross's social history of early industrialization is, therefore, not only rather oldfashioned in its historiographical understanding but also not especially nuanced. Furthermore, none of the many essays that appeared in the two collections published to celebrate the sesquicentennial of Malthus's death in 1984 seem to have been consulted: J. Dupâquier (ed.), *Malthus Past and Present* (London, 1983); and Michael Turner (ed.), *Malthus and His Time* (New York, 1986).

I don't think that this line of historiographical criticism is simply an exercise in academic point-scoring since the story Ross tells about Malthus and his ideas' reception is essentially one-dimensional. Thus, Ross's Malthus is a political economist, pure and simple; but the historical Malthus was also an ordained minister of the Church of England. This is not a trivial point, since Malthus foreclosed the possibility of birth control to limit population size, not only because it seemed impossible to stem the passion between the sexes, but also because such intervention in the act of procreation was unnatural and therefore immoral. This led him to suggest that education of the masses was the only practical way to inculcate them with moral restraint. Malthus's advocacy of popular education stood in stark opposition to most of his contemporaries, who were haunted by fears of an English rerun of the French Revolution if the lower orders were well-informed. Contemporaries, Malthusians and others, simply neglected this argument, and for another three or four generations there was little willingness on the part of the political elite to countenance a national system of compulsory education.

Furthermore, some of Malthus's ideas may have been appropriated by hard-hearted men to justify their policy of nonintervention in the Irish Famine, but the parson himself was never a poster-boy for racist attacks on the Irish. Indeed, Malthus – an ordained minister of the established Church, a fellow of an Oxbridge College, and a man of the ruling elite – wrote in his *Principles of Political Economy*: “Let the Irish Catholics have all that they have demanded; for they have asked nothing but what strict justice and good policy should concede to them”. Therefore, it is crucial to recognize, as Jacques Wolff notes, that “Malthus was not a Malthusian in the sense that this term has acquired”, because, as their contemporary Robert Torrens noted, “As presented by Mr Ricardo, political economy shows a regularity and simplicity which is more pronounced than that found in nature; as presented by Mr Malthus it is a chaotic assembly of specific and independent facts”.

The book's third chapter, “From Eugenics to Environmentalism”, reinforces my impression of Ross's one-dimensional approach. His analysis privileges the costs exacted by the imposition of neo-Malthusian biosocial engineering at the expense of recognizing the benefits that accrued to working-class people when they were able to restrict fertility – and, correlatively, to restrict the supply of labour. When workers organized to restrict labour supply – to remove women, children, and “blacklegs” from the labour force – they were acting to regain control over their labour. While it might at first sight appear to us that the campaign for a “family wage” was retrograde because it was motivated by antifeminist sentiments, the issue appeared in a rather different guise to working people in the early industrial age because the impact of a runaway labour market had served to provide employers with a stick with which they – and their wives and children – were perpetually being beaten. Seen in this light, the proletarians' practise of birth control was a crucial way in which their politics were incorporated into their private lives.

The casual reader of *The Malthus Factor* would be kept in absolute ignorance of this issue because Ross never probes the massive shift in reproductive behaviour that took place between 1870 and 1930, when life expectation doubled and fertility nosedived to a fraction

of its earlier level. Not only is there no discussion of these readily accessible figures, their implication is silenced in his account. Instead, Ross limits his account of Malthusianism to a discussion of the racist implications of some prominent neo-Malthusians. To be sure, this is a significant issue, but my point is that Ross's account is partial while at the same time denying workers agency in their private struggles to reform public life.

The first half of the book is really a prologue to the author's intention to explicate how "Malthusian thinking" has influenced modern development theory. And, sadly, the book is terribly deficient on this score. Instead of an explication of the impact of Malthusian theory on policy formation, Ross provides us with an extended prosopography of the American Cold Warriors, who launched an international effort to stem the red tide by promoting more compatible forms of development. However, throughout his account Ross never really tells us why, if these Cold Warriors had really believed that "the question of population pressure was rapidly assuming a pivotal role as a explanation of social and economic problems in the developing world", they spent so little money on birth control. "So little money", that is, in relation to the vast sums they were willing to spend propping up corrupt regimes in Brazil, Iran, and elsewhere throughout the Third World. This is, I think, the nub of the problem that Ross has drawn to our attention and one has the nagging suspicion that the club of Cold Warriors were attracted to "Malthusian thinking" as a way of rationalizing their activities. They may have "talked Malthus" but they acted like policemen.

Rather than turning the Third World right side up, "Malthusian thinking" was deployed by Cold Warriors to sustain a repressive status quo that funnelled wealth into the banks and corporations they controlled. "Malthusian thinking" thus provided the window-dressing that made the Cold Warriors' view of things seem so natural that the military-industrial complex's agenda would be accepted as a reason to provide funding for military and industrial adventures that subjugated the very people whose hearts and minds and bodies were supposed to be modernized. Schooling the people in taking control of their own hearts and minds and bodies was thus deemed subversive – and this would have been a far, far better book if Ross had explained to us why and how this inversion took place.

David Levine

Kapital.doc. Das Kapital (Bd. 1) von Marx in Schaubildern mit Kommentaren. [Von] Elmar Altvater, Rolf Hecker, Michael Heinrich [und] Petra Schaper-Rinkel. Westfälisches Dampfboot, Münster 1999. 342 pp. [1 CD-Rom encl. System requirements: Windows 95 or higher, Netscape Communicator 4.07 or other browser.] DM 48.00.

Max Weber compared Marx's *Das Kapital* to some of the greatest creations of European art, including Michelangelo's Sistine Chapel and Bach's *St Matthew Passion*. Marx himself, in his private letters, described *Das Kapital* as a work of art ("ein 'artistisches Ganzes'"). The authors provide us with a guide to this masterpiece for the modern man and woman. The book is accompanied by a CD-Rom that includes the *Communist Manifesto*, the full text of the first volume of *Das Kapital* (the fourth edition of 1890, as published in volume 23 of the *MEW*), the *Theses on Feuerbach* of 1845, the introduction to the first manuscript of the *Critique of Political Economy* of 1857, the foreword to *A Contribution to the*

Critique of Political Economy of 1859, extracts from the *Economic-Philosophical Manuscripts* of 1844, and extracts from various manuscripts documenting Marx's efforts to revise and improve the text of *Das Kapital*, especially the first chapters, after its first publication in 1867. Even an index is provided.

On paper we get an ongoing chapter-by-chapter commentary (by Elmar Altvater) on the text of the first volume of *Das Kapital*, supported by various diagrams to clarify the structure of Marx's line of reasoning. As result of his many years of lecturing on Marx, Altvater succeeds in presenting a commentary that is systematic, vivid, and full of useful hints and illustrations. With care, even parsimony, Altvater refers to later and current debates on political economy as well as to phenomena of contemporary capitalism, explaining the actual use value and the critical potential of Marx's thinking. Marx in Marx's own words is rather puzzling to the contemporary reader, since his work is so clearly at odds with most of the conventional academic and everyday wisdom on economics. Altvater shows how provocative *Das Kapital* is, and elucidates the meaning of the critique of political economy as intended by Marx. Altvater does not shun the argument, and refers to various objections to Marx, explaining why Marx's theoretical action makes much sense in terms of his aim of presenting a thorough critique of capitalist society and the economic categories in which it is, even now, depicted in our minds. As a good commentator should do, Altvater highlights various points – such as property rights, ecological questions, the global dimensions of capitalism – where Marx confines himself to mere hints or allusions. Reading Marx with the help of Altvater, one does not enter a closed universe of “Marxism”. It is possible, and it makes good sense, to go beyond Marx's writings, following Marx's inspirations. Reading *Das Kapital* can intimidate beginners since they know there is a real mountain to climb, volume 1 being just the first summit to cope with. In his introduction, Altvater expounds the structure of the whole work as planned by Marx and goes on to pinpoint the status of various arguments in the context of the whole. Even for the seasoned reader of Marx, his commentary, including the highly useful and clear diagrams, is a great pleasure to read.

The commentary to the text is supported by an annotated bibliography (by Michael Heinrich) on Marx's and Engels's writings on the critique of political economy, on the later writings of various authors – Marxists and anti-Marxists – on Marx's and Engels's critique, and on critiques of Marx today. Of course, this is a select bibliography, far from complete, and biased towards German contributions. Although presented in chronological order, it focuses on debates around theoretical issues – debates Marx had with himself and Engels, debates among Marxists and non-Marxists about the right interpretation of Marx's critique of political economy. The main points of emphasis in this bibliography are the various rediscoveries of Marx's economic theories and the ensuing debates in the wake of the 1968 movements and since. This is quite useful, as most of these contributions have been even more comprehensively forgotten than the few larger studies (by Hilferding, Luxemburg, Bucharin, Sternberg, and others) from the heydays of classical Marxism.

Das Kapital has a long and twisted history of its own. Nobody can read and fully understand it without knowing something of the history of the book and the many manuscripts associated with it, some of them unpublished even now. Rolf Hecker, a member of the small international group of experts working on the MEGA (Marx-Engels-Gesamtausgabe), tells the story of how Marx wrote *Das Kapital*, how parts of the unpublished manuscripts were edited by Engels after Marx's death, what happened to the vast bulk of the unpublished manuscripts after Engels's death, and, finally, when and how

those manuscripts were subsequently published (in various editions, right up to the present day). He adds a brief and very lucid exposition of the plan and the editorial principles of that great work in progress, the historical critical and complete edition of Marx's and Engels's writings, the second MEGA (the first, inspired and organized by David Rjazanow, was aborted during Stalin's purges of the 1930s). Hecker's text is again supported by several very clear diagrams outlining the many manuscripts by Marx in their right order. Altogether, the book provides an excellent guide for all those who want to study seriously Marx's principal work, a work almost completely unknown to most of those who accuse its author of Marxism and other deadly sins.

Michael R. Krätke

DRESCHER, SEYMOUR. *From Slavery to Freedom: Comparative Studies in the Rise and Fall of Atlantic Slavery*. Foreword by Stanley L. Engerman. Macmillan, Basingstoke [etc.] 1999. 454 pp. £55.00.

In 1977 Pittsburgh historian Seymour Drescher, previously renowned mainly for his scholarly work on Alexis de Tocqueville, published *Econocide: British Slavery in the Era of Abolition*. This groundbreaking analysis of the British abolition of the West Indian slave trade was to be instrumental in redirecting the scholarly debate on the eradication of the "odious commerce", and of the "peculiar institution" of Atlantic slavery as such. In *Econocide* – a term first coined in the book and since widely used in scholarly debates – Drescher argued on the basis of extensive statistical evidence that the British abolition of the slave trade had been enacted against the best economic interests of the West Indian planter class and, indeed, of Britain itself. Economic decline followed rather than preceded abolition. Thus Drescher squarely refuted the prevailing dominant economic readings of this history by prominent scholars such as Lowell Ragatz and, particularly, the Trinidadian historian and later MP, Eric W. Williams. Though vulgar economic readings have since been discarded, the debate on the explanation for the British abolition has not ceased since *Econocide*. The book did, however, leave an indelible mark both for its transparent argumentation and its thorough methodology.

After *Econocide*, Drescher addressed the question of British abolition from another angle. If not out of economic self-interest, why did the British stop the trade (1807) and then slavery itself (1834)? In several articles, some of which reappear in the first part of this collection of his writings from the 1970s to the 1990s, he demonstrated the importance of grassroots opposition to the trade in Britain itself, rooted in working-class radicalism and, to a lesser degree, in dissident Protestantism. He proposed another set of arguments (based on political and cultural values rather than economic interests), and instead of "the traditional cast of agents" (planters, merchants), a less elitist force.

So, after the postwar dismissal of the chauvinist "Whig" interpretation of abolition – "the unweary, unostentatious, and inglorious crusade of England against slavery may probably be regarded as among the three or four perfectly virtuous pages in the history of nations" (E.H. Lecky, *A History of European Morals* (London, 1869)) – by the late 1980s the economic explanation seemed, in turn, to have run its course too. Notwithstanding his paradigmatic reorientation, Drescher concurred with his predecessors in highlighting the unique vanguard role of Britain. Why Britain – and to an extent the North American

states too – and why not another European nation, or Latin-American countries? In Part 2 of this collection, Drescher addresses the divergence of the Anglo-American versus the Continental models of antislavery. He discusses at length some of the other candidates and their “failure” to enact abolition at an earlier stage and without decisive British pressure. France, for all its revolutionary fervour, did not outlaw slavery until 1848. The Dutch, supposedly a modern, democratic, and capitalist nation since the seventeenth century, waited even longer, until 1863. Brazil followed, but not until 1888.

In the light of the foregoing, one anticipates the recurrent conclusion of Drescher’s meticulously researched studies of these national histories: in all these Continental cases, the decisive weight of a nonelitist popular antislavery movement was conspicuously absent. To various degrees, a philosophy of antislavery developed among intellectual circles in all these countries. However, these intellectuals, whether reasoning from a moralistic or an economic perspective, never made it into the mainstream of society or politics. Antislavery therefore remained isolated and ineffectual. This, then, is forcefully confronted with, in Drescher’s view, obsolete economic theorizing (for example, “the ‘Dutch test’ turns out to be a killing field for any of the hypothesized casual linkages between capitalism and antislavery”).

The third and last part of *From Slavery to Freedom* comprises six articles addressing a variety of themes in the historiography of slavery. Tracing the connections between abolitionism and the evolution of European scientific racism, Drescher once again concludes that there was a contrast between the Anglo-American and the Continental model, with the former demonstrating a far greater interest in and indeed an inclusive attitude towards Africans. In passing, the abolition of the slave trade receives a remarkable but actually rightful epitaph (“one of the most successful human rights movements in history”). To the sceptically minded, this may be conspicuously reminiscent of Lecky’s praise. Drescher clearly has no qualms about throwing in more than just the occasional sweeping statement.

The next two chapters address the subject of Jewish involvement in the Atlantic slave trade, which was actually of limited importance, except for the Dutch colonies in the Americas. The extensive attention Drescher pays to this theme seems less inspired by its immediate historical relevance than by the need to redress recent often virulently anti-Semitic “scientific” reappraisals proposed in the United States by African-American radical leaders such as Louis Farrakhan.

The collection closes with two chapters on Eric Williams’s *Capitalism and Slavery* and, finally, a comparison of free and slave labour. The chapters on Williams’s 1944 classic provide, in fact, a comprehensive overview of several of the historiographical debates on the slave trade and slavery in the last half century. The closing chapter addresses the disillusioning economic consequences for both the antislavery ideologues of the times and the slaves. The former had believed, or at least pretended they had, that emancipation would demonstrate the economic superiority of free over slave labour. For the ex-slaves, freedom soon turned out to involve anything but a sudden increase in their standard of living – though, as Drescher rightly hastens to add, freedom is about far more than just that.

From Slavery to Freedom is a most welcome collection of erudite writings by one of the most influential contemporary scholars of the Atlantic slave trade and slavery. One need only read the praise lavished on its author in the accompanying “blurb” by Robert W. Fogel, Stanley L. Engerman, David Brion Davis, David Eltis, and Howard Temperley, to

appreciate the importance of Drescher's work. Of course, that same gallery of prominent scholars also betrays a certain hegemony in this field of study, which may be intimidating and/or irritating to sceptics. ("One gets the feeling that Britain invented West Indian slavery for the sheer pleasure of gloriously enacting its eradication afterwards.") However, one thing anyone familiar with Drescher's writings and public performances will confirm is that the eagerness, scrutiny, and wit he brings to the subject is hard to emulate. One trusts his scholarly career will not find its conclusion in this selection of previously published articles.

Gert Oostindie

Nationalism, Labour and Ethnicity 1870–1939. Ed. by Stefan Berger and Angel Smith. Manchester University Press, Manchester [etc.] 1999; distr. excl. in the USA by St. Martin's Press, New York. xii, 292 pp. £47.00.

Reviewing multi-author collections is always a difficult task. It is often impossible to give due weight to all the contributions, and frequently the case that authors fail to work to a common agenda, thus impairing the coherence of any consistent argument within the volume. By and large, the editors of this volume have sought to overcome this habitual weakness. They have constructed a set of questions which the majority of the contributors have engaged with to greater or lesser extent and thus established a framework for the comparative dimensions of the topic.

The essential structure of the volume is a general introduction and nine case studies – five European (Britain/Germany, Spain, France, Poland and Russia) and one each on the USA, Australia, South Africa and India. The main themes range around the debates concerning the apparent tensions between what has historically been seen as the competing claims of right-wing nationalism and the emerging forces of socialist internationalism within the nation state. As Berger and Smith point out in their introduction, such a simplistic analysis has tended to devalue the historical dynamics of specific national and chronological change. Differing political and cultural traditions and the timescales of national and institutional development have played a significant part in the variety of experiences which are discussed within this volume. In general, the studies examine the extent to which sections of the labour movements were drawn into the national state. Although the language of incorporation is sometimes employed in these analyses, what most of the authors identify is a process of negotiation and concession between state and organized labour. Certainly, in the period up to the end of the First World War, there was the need within a number of the more "advanced" nation states for the commitment of labour to that process of state-building. The 1920s and 1930s then see more fundamental clashes between state and labour as political agendas and requirements change. Most of the contributions also focus on the clashes between organization and ideas formulated around right-wing nationalism and the rise of social democratic parties and political ideologies. One of the more interesting dimensions of the discussions highlights the ways in which ethnicity and "race" were discourses which created tensions within concepts of the nation and also within the notion of national labour movements. Patterns of migration and immigration, across not only nation-state boundaries but also across continents, created particular, often racist, responses. These in turn posed problems in the creation of a

coherent labour movement, and exposed the language of internationalism and solidarity in ways which were often difficult to combat.

It is these themes which feature in most of the chapters. Of course, each case study has its particular features and there are variations in the patterns of these processes and debates. In the one directly comparative study, that of Stefan Berger on Britain and Germany, it is interesting to note the ways in which the Left attempted to construct what he defines as an “oppositional nationalism” in order to respond to the broader patterns of nationalism and racism which became significant features of a broader German and British culture. It is this continual confrontation between these different forms of nationalism which broadly constitute the study. It was an uneven struggle with the “traditional” forms of nationalism and racism acting as a constant fracture within the attempts to create and impose a “good” democratic nationalism. Berger notes that, in Germany, the earlier establishment of aspects of a welfare state may have acted to draw in elements of labour in a more conclusive fashion than in Britain. However, it is also made clear that the British working class were no less susceptible to the siren call of racism and popular nationalism, at the expense of a more ideologically distinct democratic national identity. Labour leaders in both countries were essentially uncertain as to how such challenges should be constructed, and the history of these years is of a complex series of attempts, mostly unsuccessful, to break the hegemonic processes constructed by and around the nation state.

Elsewhere in the volume, non-European experiences provide an interesting contrast. This is not to argue that labour is able necessarily to negotiate more powerful forms of democratic nationalism and to slough off the challenges from popular racism and right-wing nationalist discourses. It is the case, however, that we are confronted with quite different sets of historical and cultural processes and these allow a broader reflection on the complexities of the evidence. Neville Kirk’s study on the working class in the USA argues very powerfully for a revision of the simplistic notion of American “exceptionalism” in this respect. This is not the same as dismissing the specific and particular sets of forces at work within the American situation, but to suggest that the processes of nation-building, the definitions of what constituted an “American”, and the interrelationships between class, ethnicity, and national identity have distinct parallels in this period with those seen in Europe and elsewhere. Kirk is particularly keen to emphasize that, alongside the history of exclusionary and exceptionalist attitudes and practices, sat radical strands of a more democratic nationalism and a more inclusionary sense of American identity. This was most evident in the 1930s, as demonstrated by the CIO’s efforts to return to a more radical attempt to draw in immigrant workers, blacks, and women. Again, this is not uncontested within both the wider labour movement and political structures, but it is clear that it has a degree of success and, perhaps more importantly, a symbolism which ought to be acknowledged.

Other studies offer equally important insights. In his piece on Australia, Terry Irving concentrates upon the processes of state- and nation-building during these years, and the contributions of the labour movement within these broader patterns. His essential argument is that the Australian experience provides an illustration of state-building from below and, as such, reveals a distinctive radical/democratic nationalism, free from the “patriotic excesses” seen elsewhere during these years. In what is sometimes a difficult argument to follow, Irving seems to play down the overall significance of labour movement racism, although not denying its existence, and to articulate a much more positive role for labour in the construction of a national identity which was essentially

democratic. He looks in some detail at the constituent elements within the movement and the different moments of the “forging” of the nation, in a chapter which deserves careful reading. Its challenges to a history constructed around “labourist myths” and his efforts to argue for a more practice-based approach is bound to provoke responses, but it clearly opens up the opportunities for a more constructive dialogue around the themes of nationalism and ethnicity.

Finally, Rajnarayan Chandavarkar’s work on the Indian working classes and the engagement with nationalism and ethnicity offers a very different experience from that of the western European studies. Workers’ politics in the period 1870–1947 saw a complex interplay between class and caste, language and nation, and between religion and region. In a very detailed study, what emerges is that both class identities and a sense of national identity could, and did, serve as focal points for a recognition of interests above and beyond those of caste, local community or religious groupings. However, class and nationalism could also serve as conflicting ideologies, fragmenting the working classes and adding to the tensions inherent within the temporary alliances constructed at various times during the period. These were frequently informed by broader political debates on the nature of the state and the growing nationalist critique, from the 1920s onwards, of the colonial influence and the desirability of political change. Chandavarkar is keen to emphasize the sterility and limitations of more conventional analyses of the relationship between class and nation, and his wide-ranging evidence offers a perspective which reflects more astutely on both the complexities of Indian society in these years and also on a more sophisticated theoretical approach to the general topic.

Overall, then, it is this kind of conclusion which can be derived from this volume. Individually, the necessarily shortened case-studies can only serve to provide basic sketches of the national experiences. Collectively, what they offer is a more challenging perspective on the interplay between identity, nation and labour in the late nineteenth and early twentieth centuries. As Eric Hobsbawm has demonstrated, this was a crucial period in the formation of nation states and in the shaping of the ideological forces that were to determine the nature of those states. It is appropriate that Berger and Smith dedicate the volume to Hobsbawm, in that they justifiably see their work and that of their fellow contributors as developing the study of these aspects of nationalism in a worldwide comparative approach.

Kenneth Lunn

POOLEY, COLIN G. and JEAN TURNBULL. *Migration and mobility in Britain since the eighteenth century*. UCL Press, London [etc.] 1998. xix, 419 pp. £18.95.

This book has already become a major work of reference in recent migration studies. It fills long-standing gaps in our knowledge of migrations in the past, thanks to an amazing dataset, careful analysis, and convincing conclusions. Based on all known moves of more than 16,000 individuals living between the 1750s and the 1990s, the book offers a kaleidoscope of migration experiences, ranging from simple changes of address within communities to emigrations to Australia and back. In their concluding chapter, the authors assert emphatically that “migration should be viewed as a basic human event” (p. 330).

Pooley and Turnbull's gold mine, the dataset with 16,091 individual life stories, is the result of exemplary cooperation between social scientists, family historians, and genealogists. Almost 1,400 volunteers filled in forms with detailed information on all the migratory moves and occupational changes of their ancestors. They were also asked to impute, to the best of their knowledge, the motive for each particular move. They could draw on interviews, their own recollections, or written material such as letters or diaries. The combination of moves and motives makes this dataset unique. Obviously, genealogical material contains biases, which are meticulously spelled out and analysed by the authors. From the onset, Pooley and Turnbull make clear that their dataset should not be seen as a representative sample of the population. Because genealogies are focused on male ancestors, who married and had children, the dataset has a clear gender bias and contains little information on people who died young or single. Also, because no genealogists from Ireland were contacted, the experiences of Irish immigrants are missing. Finally, the relative difficulty of tracing proletarian ancestors leads to their underrepresentation in the dataset. Aspects of "proletarian mobility" are hardly touched upon in the book, although the wanderings of farmhands and domestic servants, as well as the relocations of urban paupers from one cheap lodging to another, must have made up a good deal of all residential moves in the past.

The core of the book is formed by extensive cross-tabulations of the moves (totalling 73,864) made by the ancestors. The migrations are unfolded by motive, by distance, and by the characteristics of the movers (age, sex, position in the family, occupation, etc.). In this way, the authors are looking for spatial variation in mobility patterns and for changes between periods. These variations and changes may reveal how migration was linked to the processes of industrialization and urbanization. Also, this method is used to establish relationships between internal migration, overseas migration, and return migration. A number of chapters are devoted to the dominant motives for migration: work, family, housing, and personal and societal crisis. In a final chapter, the authors summarize the impact migration has had on individuals, on places and on society at large. The study is enriched with excerpts from diaries and life histories with which the authors unravel the decision-making processes and assess the individual emotions and meaning involved in making migratory moves. Finally, Pooley and Turnbull present a large number of maps depicting moves across Britain. However, these rather inky maps, with their myriads of lines, have no added value and would have been better left out.

Although the authors emphasize that their work is a first, tentative, exploration of the dataset, their conclusions are highly significant. Contrary to expectations implicit in modernization theories, the frequency and distance of migrations increased only gradually over the past 200 years. The revolutionary changes in transport, communication, and information did not lead to structural changes in mobility. Conversely, the impact of mobility on macrolevel changes should not be exaggerated. For instance, rural to urban migration was less important in explaining urban population growth than fertility. Also, depopulation of rural areas was a very gradual process and therefore had little impact on daily life. Another interesting finding is that the migration behaviour of men and women was very similar. Finally, although work was the predominant reason for moving, this motive always interacted with family interests and with the attachment to familiar places. The authors explain the similarity of migration patterns between different areas and different periods in terms of this lack of correlation with economic factors. They surmise: "migration was a process which was primarily affected by individual decisions, related to

personal aspirations and life-course factors rather than local or regional economic and social conditions” (p. 308).

In demonstrating that migration cannot be reduced to economic stimuli, *Migration and Mobility* serves as an indispensable correction to the traditional “push and pull” approach to migration. However, this conclusion is coloured by particular biases in the data as well as by the deliberate choice of “straightforward” methodology. To begin with, the motives that are imputed by the family historians for the moves of their ancestors reflect their own preoccupations with family-based events. They have less knowledge of the economic problems facing their ancestors and thus are less likely to impute an economic motive. Likewise, local economic conditions (prices, wages, etc.) will have interacted with life-cycle events but are not likely to be described accurately in autobiographies or interviews.

To some extent, the conclusion that there was little variation in migration patterns derives from the way the data are handled and presented. Lumping together all moves, including the numerous moves within a locality, makes it hard to discern significant variation in, for instance, the percentage of medium- or long-distance moves. Also, a specific migratory move is hardly ever related to the persons under observation in the dataset. It is therefore difficult to control for changes in the demographic and social composition of the population. For instance, the increase in longevity means that the more recent birth cohorts probably contain more elderly persons who were likely to have made more moves during their lifetimes. Perhaps tables giving the incidence with which persons with particular occupations, household situations, ages and so on moved within say the first ten years might suggest different conclusions as to temporal, social, and regional differentiation.

Another methodological choice with strong implications for the conclusions is that moves of individuals are analysed in isolation from one another. The association of these moves with other factors is sought in terms of immediate effects. Individual *trajectories* are evoked using the life histories, but no attempt is made to study them more systematically. This leaves no room for long-term planning or for the cumulative influence of past experiences on present decisions. For instance, occupational mobility is discussed solely in terms of the direct improvement of a position simultaneous with a migratory move. However, long-distance migration may well have been an investment in future opportunities. It is not realistic to assume that such a migrant would have achieved a better position immediately upon arrival, but in the end he or she may have been better off than the stayers. A comparison of trajectories is quite different from the comparison of sequential moves (first move, second move, etc.) made in some tables. These tables are difficult to interpret as these moves are not related to specific stages in the life cycle. It is therefore not clear why shifts in motives or characteristics occur.

In stark contrast to the quantitative analyses, which show so little variation in mobility patterns across time and space, the life histories described in the book show both the enormous variability in migration experiences as well as the complexity of the decision-making involved. The authors themselves find that explanation and interpretation at the individual level is more “effective” than at the aggregate level that dominates the book (p. 322). Clearly, the database can be subjected to a more elaborate analysis in order to test hypotheses deriving from the biographies. For instance, little or no use has been made of the family connections between members of the dataset. Yet the case studies show that the presence of family in distant but prospering areas was of crucial importance for the young, seeking housing and employment opportunities. The database might be reworked to investigate this phenomenon of “chain migration”. Also, for those family groups that are

complete, the variable of sibling position could be included in the analysis of individual migration behaviour. If the dataset is oriented towards individual trajectories, more light can be shed on return migration. This reveals much about the nature of individual planning, the quality of information on other places, and the nature of migrants' ties to their former place of residence.

Hopefully, the authors will find the energy to refine their dataset and methodology and extend their analysis with trajectories. It could only strengthen the impact of this already highly impressive work.

Jan Kok

SHAPIRO, GILBERT and JOHN MARKOFF, with contrib. by Timothy Tackett and Philip Dawson. *Revolutionary Demands. A Content Analysis of the Cahiers de Doléances of 1789*. Foreword by Charles Tilly. Stanford University Press, Stanford 1998. xxxi, 684 pp. \$75.00; £45.00.

This book has a long history. It is a report, covering some fifty years of research, trying to establish a communication port between historiography and quantitative approaches to the social sciences. Its historical background is France before the introduction of a system of contemporary political representation at the end of the eighteenth century. Since 1302 the King had summoned an assembly of the States General on a regular basis. Each of the three estates in every *baillage* was entitled to draw up a *cabier de doléances*, a list of grievances, describing the prevailing state of affairs in its parish and demanding assistance, solutions and reforms. Deputies were elected by each of the estates to represent them in the States General. These deputies adopted a collated and condensed version of these lists, one for each estate. Finally, the King considered a synthesis of these three lists. He would take note, and respond efficiently to the demands of his kingdom's estates, and perhaps levy extra taxes in return. This system became impossible with the French civil war.

Despite calls, at the end of the regency of Marie de Medici, for the States General to be summoned, after 1614 it was a dead letter for almost two centuries, while provincial estates and other forms of local and regional assemblies survived and flourished, as did the French *parlements*, a kind of constitutional court under the *ancien régime*. Exactly 175 years later, Louis XVI was forced to summon a new – and final – convocation of the States General. It was held in 1789, but instead of debating a synthesis of grievances it declared itself the *Assemblée générale* and introduced a system of abstract political representation. This marked the start of the French Revolution.

The lists of grievances were archived nonetheless and, after de Tocqueville's *L'Ancien régime et la Révolution* (1856), became part of the Revolution's historiography. Subsequently, historians such as Gustave Bord in *La conspiration révolutionnaire de 1789* (1909) and Augustin Cochin in *Les sociétés de pensée et la démocratie* (1921) saw in the *cabiers*, as a complete corpus, a valuable source of information. They detected several identical patterns and, impressed by the contemporary development of political parties, launched a new thesis concerning the revolutionary process as being both intellectually and socially organized. Later research focused on local studies of the *cabiers de doléances*. Some of them were largely ignored; others, like those by Régine Robin (*La société*

française en 1789 (1979)), were more or less passively rejected by the historiographical mainstream. Several projects came to nothing or were simply abandoned.

Another approach was conceived, and finally implemented, by the authors of this book. Their aim was to create a comprehensive computerized database of the 40,000 *cabiers de doléances* and to produce a coherent analysis of their content. This was only part of a larger research programme, entitled “Quantitative Studies on the French Revolution”. Any historian working in archives and creating databases based on archive material will readily admit how difficult and demanding this is; that one needs tremendous enthusiasm and perseverance; that ultimately all you might discover is what you forgot, or did not know should be included at the start; that the results do not correspond to your questions, or vice versa; that any number of seemingly intractable technical problems will continue to frustrate you. Such historians could easily be forgiven for believing they are wasting their scholarly time and energy, that they would have been better off taking refuge in the archives of a forgotten monastery somewhere, or even devoting themselves to the tedium of administrative committee work (expecting little in the way of results of course, but at least ensuring better salary prospects). In other words, this kind of research can become a frustrating experience, and the results are never exactly what you expected. Even worse, since software is constantly developing, you ultimately realize that most of your problems are not actually due to technical questions. So you run the risk of having to start all over again. All the more reason then why this research programme, and the courage, the skills, and the historiographical advances it represents, deserves our respect.

The book begins with a history of content analysis in the social sciences, followed by a discussion of the sources and a demonstration of how the authors have analysed the *cabiers*. Finally, the authors provide a number of examples of their research. Nine of the twenty-one chapters have already appeared in print elsewhere. Some 250 pages (of the 684) are devoted to methodological questions (parts 1–3), another 240 pages to appendices, notes, a bibliography, and an index.

There have been many different attempts to produce and analyse samples of the *cabiers*. Shapiro and Markoff have opted to produce a national sample and to code all the grievances contained in the texts. The crucial issue is one of coding, and its consequences. “We know that some of the meaning of the text may be altered in this, as in any, translation”, they readily admit (p. 216). One problem occurs where “the text is too complex for the simple semantic structure of the code, so that its meaning can not be contained without severe distortion of meaning within our simple Subject–Predicate structure, even with qualifying Remarks”. This happens commonly where “grievances are in some way semantically coupled”. Coding does not just involve translation into another language – where it is possible to find equivalents for complex semantic structures – but also abstraction, without the possibility of decoding unless you return to the original texts.

However, this abstraction relates not only to the complexity that results from semantic coupling. Take, for example, the following statement, which can be found in the *cabiers*: “Either expel the Jews from the kingdom, or treat them just like other Frenchmen.” Another example: “The Nobility’s ceremonial prerogatives of deference should be retained, provided that their tax privileges are abolished.” Now it is “evident that treating the two clauses [...] simply as independent demands would violate the meaning of the document”. There is, the authors argue, “no basis for judging whether the writers preferred either to expel the Jews or to treat them equally”. Shapiro and Markoff’s solution is “to code each of the coupled demands independently, while indicating [...] the manner in which they are related to one

another". ("Do this unless [...]") Furthermore, these kinds of "theoretical and historical decisions [...] are best not left to coders" (pp. 186ff.). So what is lost as the result of coding are not only complex expressions, but also contradictory ones, as well as all that has not or has only allusively been said. And, of course, contexts and co-texts may differ regionally.

Furthermore, coding is indifferent to social and political praxis too: "we code only 'grievances' or 'demands' ". The authors "ignore the reasons and arguments given in support of demands, including the information conveyed by the community [...] about their life-situation so often dispersed among their grievances" (p. 83). One might call this "concrete". No one grievance is necessarily identical to the same grievance expressed in different circumstances and supported by different arguments or even in pursuit of different aims. The authors of the *cabiers* wanted to provide evidence to support their demands, and this evidence might actually be more interesting than the grievances themselves.

In fact, the 1789 *cabiers* are complex and difficult to understand. The list of grievances was not organized as a kind of multiple choice. Very often, the demands made were conclusions to lengthy complaints, and it is these complaints that are important, not the grievances themselves. Even more problematic is the fact that some grievances are difficult to understand since the arguments "supporting" them point in quite a different direction.

One might well conclude then that coding should be accompanied by the complete texts in a database that allows one to analyse complex semantic structures. As *Begriffsgeschichte* turns out to be the history of discourse and of political semantics (Reinhart Koselleck, Rolf Reichardt, Niklas Luhmann, Dietrich Busse and many others), we are confronted with the fact that the meaning of a text may be quite different from, and actually contrary to, what is actually expressed, and that paradoxical discourses are central to these revolutionary movements. To give just one example: at the beginning of the French Revolution many popular texts, pamphlets, and newspapers called for an end to all elements of the *ancien régime*, while at the same time refusing to accept the new form of political representation and insisting on re-establishing immediate and direct personal links with their "*mandataires*" or "*commis*" in the assemblies, and even with the King. This political discourse was successful and fully accepted by the revolutionary masses. Later, Robespierre was to become a master of this kind of ambiguous political semantics. And, as François Furet and Denis Richet have stressed, this communication supported and, ultimately, became the Revolution.

Sociologists are aware of this fact. Historians still need to be acquainted with it. Databases will help them pursue their research, but only if ambiguous, contradictory and paradoxical discourses are allowed to play a full part, and not stripped away by the coders.

Fred E. Schrader

GOURDOT, PAUL. *Les Sources maçonniques du socialisme français 1848–1871*. [Franc-Maçonnerie; Humanisme et tradition]. Editions du Rocher, Paris 1998. 313 pp. F.fr. 150.00.

This is essentially the text of a doctoral thesis defended at the University of Poitiers in 1987. Paul Gourdot, who was Grand Master of the *Grand Orient de France* in 1981–1984, is a proponent of "liberal freemasonry", a current that has shed much of the esoteric characteristics of the Craft and is well represented in the *Grande Loge mixte de France*, which he helped found in 1982. The *Grand Orient* is not without interest to French policy-

watchers, as it can boast a considerable number of influential socialists among its members. Indeed, in the spring of 2000, it suffered a serious crisis over the way some of them used it in trying to solve the Corsican problem.

In the light of this, it comes as no surprise that Dr Gourdot's book is in many ways a contribution to the debate on the political course of the *Parti Socialiste*. Its main thesis is that French freemasons played a major role in the creation of a *socialisme humaniste* that was destroyed, together with the Paris Commune, and replaced by Marxism, with its scientific pretensions and authoritarian reality. The message is that after the downfall of Marxism and the rise of a ferocious liberalism, the process should be reversed. Put differently, this is yet another *plaidoyer* for a "Third Way", whose tradition Gourdot is inventing.

In the style of many such exercises, he offers a roughly chronological story sustained by long document summaries and extensive quotes. Although these are sometimes derived from little-known masonic sources, their value is too often limited by a lack of context, thus depriving them of much of their power. In another *topos* of the genre, Gourdot presents his book as a series of revelations, on the assumption that few, if any, socialist freemasons have ever been identified as such, let alone taken seriously, in the historical literature. Yet this illusion results at least in part from the fact that he entirely neglects the work of foreigners such as Max Nettlau and Boris Nikolaevsky – and, for that matter, of compatriots such as Maurice Dommanget and Jean Bossu.

Since this book is about tradition, almost a third of it is taken up by the history of freemasonry prior to the arrival of socialism. We encounter a delicately revisionist treatment of l'Abbé Barruel, whose denunciation of freemasonry as the secret source of the French Revolution is strangely supportive of Gourdot's argument. Interestingly, this point of view is itself part of a tradition, as some nineteenth-century conspirators already took pride in such roots. When he arrives at the age of socialism proper the author becomes predictably inclusive. Charles Fourier is generously turned into an honorary freemason, as is any early socialist – and there are many of them – who might conceivably be described as *initiaticque*. Even Filippo Buonarroti, whose masonic links are well established but who hardly fits Gourdot's conditions, is said to have aspired to "*un état initiaticque, dirigé par de grands initiés, qui ne peuvent que vouloir le bonheur pour leurs frères*" – and, consequently, was not so black as he was painted. The result is a band of Brethren that may be historically incongruous, but makes a handsome lineage for today's humanist socialism.

For the historian, there are, of course, lots of raw little facts, though they are often poorly documented. The absence of a bibliography is all the more keenly felt, as it is frequently impossible to establish what exactly the author is quoting in the notes. Sadly, as is the rule in France, there is no index either. There is, however, an amusing reference to the *IRSH* as the *International Review of Secret Societies*.

Jaap Kloosterman

ANZ, CHRISTOPH. *Gilden im mittelalterlichen Skandinavien*. [Veröffentlichungen des Max-Planck-Institut für Geschichte, Band 139.] Vandenhoeck & Ruprecht, Göttingen 1998. 326 pp. DM 72.00.

Anz's study of guilds in medieval Scandinavia was written under the direction of Otto Gerhard Oexle and submitted as a doctoral thesis to the Fachbereich Historisch-

Philologische Wissenschaften of Göttingen University in 1996. Its publication in the series edited by the Max-Planck-Institut für Geschichte is a logical sequel to the stipend with which this same institute enabled Anz to conduct the research on which his thesis is based.

Like his mentor, Anz adopts a wide definition of “guild”, which includes not only professional corporations (merchant and craft guilds) but also religious confraternities and various types of organization aimed at mutual help. The distinctive features of the guild are the reciprocal oath sworn by its members and the voluntary character of their adherence. Essentially, the guild is a *Schwurbrüderschaft* (*coniuratio*) and a *freie Einung* (p. 61). The reasons for choosing this definition are given in several preliminary chapters in which Anz considers the historiography of guild studies, focusing on Germany, and explains his own theoretical point of view.

Using this definition Anz investigates the guilds in Norway, Sweden, and Denmark. Though he has done some archival research himself, the great majority of the sources are available in reliable editions. This is especially true for Denmark, the country with the richest material. Anz takes account not only of previous “continental European” (mainly German) publications on Scandinavian guilds, but also of the extensive output on the subject by Scandinavian authors. Anz seems to have read scores of publications in all three Scandinavian languages, and the scholarly world owes Anz a considerable debt for making accessible in a major European language the work of his northern European colleagues.

The earliest Scandinavian guilds that fall within the compass of Anz’s definition date from the twelfth century (Norway and Denmark). This in itself is significant. Earlier references to guild-like associations are discarded by Anz because they lack the essential characteristic of the oath. Because Scandinavia had been more or less converted to Christianity by the twelfth century, Anz can afford to ignore the thorny problem of a possible pre-Christian, Germanic origin of the guilds. Though he never explicitly says so, for all practical purposes Anz considers the guild as an essentially Christian phenomenon. Moreover, in his chapter on the historiography of the guilds he is extremely hostile towards the hypothesis of the guilds as evidence of Germanic continuity throughout the Middle Ages. This approach, of course, has become suspect because of its contamination with Nazi ideology. Because Scandinavia, for obvious reasons, played a major role in this hypothesis, Anz’s break with older German historiography could not have been sharper. The way he argues his case, however, leaves one with the impression that his reasoning is not entirely noncircular.

Three separate chapters, one on each of the three Scandinavian countries, contain extensive treatments of the evidence on the guilds. These chapters follow the same pattern. First, the material is presented in its chronological setting, with special emphasis on the structure of each of the guilds. A question Anz tries to answer as well as possible in each case is that of guild membership: the geographical and social origin of members, the presence of and degree of participation by women (which, generally, appears to have been high). Next, separate sections are devoted to three important aspects of guild life. The guild as a *Schwurbrüderschaft* is supposed to function not only as an organization for mutual help and protection, but also as a community educating its members to behave morally and to ensure peace between each other. An indispensable feature of guild life therefore are the statutory obligations (*gewillkürtes Recht*) it enforces on its members. Anz analyses the working of guild judicial institutions and the changing relationship between the guild statutes and the public law of the state. Over time, the space given to the administration of justice by the guilds declined in favour of the growing force of the state’s judicial

apparatus. Anz's approach is to minimize the importance of clashes between the two legal systems and to emphasize the positive contribution guild law made to the establishment of law and order. The guild was also a religious community, devoted to the cult of the patron saint and the remembrance of the dead. *Memoria*, manifesting itself in the custom of reading aloud the names of deceased members at the annual guild banquet, is an important aspect of the role of the guild in shaping its own identity and that of its members. A third section examines the political and economic significance of the guilds; the latter includes the financial position of the guilds and their property, with special emphasis on the presence or otherwise of a guildhall. Oddly enough, Anz does not deal with the craft guilds, though he concedes that they fall within his definition. Readers who are interested in guilds in the context of the corporate organization of labour can therefore skip *Gilden im mittelalterlichen Skandinavien* altogether. The book continues with a chapter containing a cross-Scandinavian comparison of all the guilds treated earlier, with respect to their relationship to secular and ecclesiastical authorities and to their place in the evolution of law. Anz's final conclusion, that Scandinavian guilds were similar to Continental European ones, is plausible. It should not be taken for granted though: not even Oxle had expected them to be so.

The sources used by Anz include both documents originating from the guilds themselves, such as statutes, lists of members, inventories, and account books (which are very rare), and sources produced by external bodies, mainly charters. However, he prefers the first type, and especially the statutes. This is consistent with his interest in the inner workings of guilds as peacekeepers and as institutions aiming to improve the morals of their members.

Anz's book makes interesting reading, particularly when he is discussing some of the more extraordinary guilds. The three Norwegian guilds he examines were conspicuous in being rural (seafaring). The Stockholm Corpus Christi guild included members of the royal family, of the Swedish state government, and of the Stockholm city council. Anz rightly stresses the political potentialities of this guild as an unofficial meeting place for those in power. A Danish guild in Schleswig (c. 1170) came close to functioning as a de facto city government, showing characteristics of the other type of *coniuratio* that Anz discerns, namely the commune. What Anz has to say about such special categories as the Knut's guilds (prominent in many Danish and Swedish towns and consisting, but not exclusively, of merchants), the journeymen's guilds and the Confraternities of the Rosary is very worthy.

Some reservations are warranted nonetheless. They relate to the circumscription of the subject and the way Anz handles his source material. In itself, the choice of a broad definition is unobjectionable, the more so as Anz is perfectly clear about his decisive criterion (the oath) and why he chose it. I even see a great advantage in treating professional corporations and religious confraternities as specimens of one broader category. The characteristics they shared are more important than their differences, at least they were during the late Middle Ages. The emphasis on the mutual oath and its religious and social corollaries focuses attention on the religious aspects of the guilds, which in the case of merchant and craft guilds might easily be overlooked if treated separately. So, basically, Anz's approach is commendable. However, omitting the *Gilden* (in the narrower sense of merchants' guilds) and *Zünfte* (craft guilds), though understandable from a practical point of view, is a serious flaw in Anz's approach. Did oaths have the same significance in establishing a sense of community among professional guilds as they did in

those described by Anz? Did the occasional prohibition of guilds (Anz mentions such a prohibition in Bergen and an abortive attempt by the bishop of Roskilde to prohibit them in Copenhagen) not prove to be more the rule than the exception? Certainly one might expect this to have been the case if one compares the many difficulties guilds experienced in continental cities. And, perhaps, if one were to include the craft guilds one would discover that the law-enforcement activities of the guilds and the secular authorities coexisted far less harmoniously than Anz suggests. Most of the book focuses on religious confraternities. It is deplorable therefore that no mention is made of the existing literature on brotherhoods in the rest of Europe. There is no reference at all to the work of scholars such as Le Bras, Meersseman, Vauchez, Weigand and Miri Rubin, neither in the text nor in the extensive bibliography with which the book concludes.

Anz applies to the Scandinavian guilds the concepts developed by his teacher Oexle. In turn, Oexle pays tribute to the great nineteenth-century legal historian Von Gierke and to Max Weber's sociology of law. From Von Gierke Anz inherits, along with his broad view of medieval group formation as a whole, the strong emphasis on voluntary association expressing itself in the founding of guilds and the all-embracing character of guild membership. Originally, those who joined a guild did not do so to pursue a narrowly defined goal, but to find a new identity and achieve lifelong solidarity. Weber's ideas on the implications of swearing the membership oath went far in the same direction. For Oexle, guilds should not be understood as utilitarian devices having a single purpose, but as communities absorbing one's whole personality. In that sense, the emphasis on the guild's statutory obligations is quite logical. In the early to high Middle Ages, Oexle's favourite field of research, such an approach is fruitful in view of the low degree of law enforcement that could be guaranteed by the state. Anz's period, however, is the later high and late Middle Ages, and though state development and urbanization started later in Scandinavia than elsewhere in Europe, the circumstances were not the same as in, for example, the Carolingian and Ottonian periods. Therefore, Anz has to make some effort to make Oexle's concepts work. His predilection for statutes is easily explained by the emphasis on the guild as an "all-life" community, but it has the great disadvantage of focusing too much on normative texts. Who can tell us whether the rules laid down in the statutes were actually put into practice? It is obviously true, as Anz contends, that several rulings were devised to tackle problems recent in nature. But to what degree was this the case? Quite a few paragraphs in the statutes deal with the way in which the annual banquet was to be held. This may be taken as an expression of the importance of the banquet as a festival of the fraternity, but it says nothing about the level of fraternal coexistence in the period between such festivals. The detailed rulings concerning the attendance at funerals of co-members and about memorial masses seem to prove the centrality of this aspect of brotherhood life. But in Scandinavia, as on the continent, these rulings were increasingly accompanied by the stipulation of fines: this may be an indication that guild members were not spontaneously willing to meet their fraternal obligations. Anz's method involves the meticulous philological exegesis of statute texts, which, in combination with his wish to discover Oexle's *Schwurbrüderschaften* in late medieval Scandinavia too, easily leads to overinterpretation. Another example of this tendency is the great importance attached to the fact that guildhalls were quite often used for political meetings. But does this really prove the political influence of the guild, or was its hall simply chosen for practical reasons?

In short, one cannot avoid the impression that Anz's argumentation suffers from a certain dogmatism. One final point might be made in connection with this. Although in

several late medieval statutes the oath is no longer explicitly mentioned, Anz clings to the central importance of this sacral element in constituting the unity among guild brothers and sisters. But is it inconceivable that another sacral ritual supplanted the oath in the fourteenth and fifteenth centuries, when guilds and confraternities increasingly took on the form of altar communities? The fascination for the Eucharist, which more than anything else characterizes late-medieval popular religion, was canalized in the burgeoning confraternities. This resulted in the Eucharist becoming the spiritual and moral centre around which guild life revolved.

These critical remarks are not intended to suggest that Anz's book is unimportant. Anyone wishing to learn about guilds in Scandinavia – except insofar as they organized labour – would do well to start here.

Koen Goudriaan

JACOBSON, MATTHEW FRYE. *Whiteness of a Different Color. European Immigrants and the Alchemy of Race.* Harvard University Press, Cambridge (Mass.) [etc.] 1998. xi, 338 pp. £18.50.

“Inconclusively white”, a court in Alabama ruled in the 1920s in the case of a Sicilian woman. The ruling meant her “black” partner could not be accused of miscegenation. If not even the courts could decide who was white and who was not, who could? *Whiteness of a Different Color* tells us about the varying, and inevitably failing, attempts to come to terms with the concept of “whiteness”, which, despite its vicissitude and inconclusiveness, was, and still is, one of the most important notions in American political culture. Jacobson's study is thus part of the burgeoning literature on the construction of whiteness, a literature that provides an analysis of and insights into the fact or idea that being white – as much as being black – is the result of much sociohistorical construction. As such, the present study is akin to those of Roediger, Barrett, Ignatiev, Allen, and Brodtkin. Jacobson, however, relies less on the analysis of the material inputs and outputs of being constructed as (or participating in the construction of being) white, as on the analysis of public images and public exchanges in the process itself. It is the vicissitude of whiteness, its changing content and meaning that Jacobson wants to outline, because “its definition, its internal hierarchies, its proper boundaries, and its rightful claimants” are essential to American culture (p. 5).

The object of Jacobson's analysis is European mass immigration, and his analysis is guided by two assumptions: race is central to the understanding of European immigration, and race resides not in nature but in politics and culture. As such, racial categories reflect competing notions of history, peoplehood and collective destiny (p. 8). Jacobson sifts through literary, legal, political, graphic, and visual evidence to map out patterns of seemingly natural differentiations based on race. His book is thus about a system of difference by which one might be both white and yet racially distinct from other whites. What he has discovered is that people who were considered white in the early years of the republic became Anglo-Saxons, Celts, Hebrews, Slavs, Mediterraneans, etc. in the later part of the nineteenth century, only to become Caucasian in the mid-twentieth century.

In the first part of the book this three-phase pattern is outlined, and here Jacobson is most convincing. The first phase focuses on the first naturalization law of 1790, which

allowed only free white men to become citizens. The law favored the exclusion or inclusion of people on the basis of their “fitness for self-government”, in other words on whether they could be relied upon “to fight the Indians” or “to put down a slave revolt”. Thus, Jacobson argues, citizenship was intertwined with whiteness. Legal exclusion on grounds of race and gender was not just a flaw in an otherwise perfect system of democratic principles, but rather an inseparable aspect of republicanism. Unfortunately, though he elaborates extensively on the issue of exclusion on grounds of race, he says nothing about exclusion on grounds of gender. While the Euro-American conquest of “Indians” and “Negroes” initially relied on the logic provided by religion, science soon offered an additional or alternative vocabulary to justify the “white man’s burden”, and it changed the epistemological basis of whiteness. The issue was no longer whether whites were superior, but whether their supremacy should be benign.

The second phase spans the period of Irish (and German) mass immigration in the 1840s to the Quota Law of 1924, a period in the history of immigration when white Europeans became more and more differentiated into a hierarchical order of white races. With the arrival of the Irish, “whiteness” came to signify Anglo-Saxon, and the Irish were basically regarded as unfit for self-government. The discourse about the growing differentiation among the European races was aided by a combination of scientific doctrines of race and political concerns. A new paradigm of plural white races ranked by shades and degrees of human difference had been established. The refinement of this differentiation is best expressed in the “Dictionary of Races”, published as part of the Dillingham Report in 1911, which became extremely influential in providing the eugenics-based arguments for the Quota Law. For the “traditional” historian of immigration this analysis will be of interest. Jacobson uses three very telling incidences of racial violence by whites against whites to exemplify his arguments: the 1863 “draft riot” in New York, the 1891 lynching of Italians in New Orleans, and the 1915 lynching of the Jew, Leo Frank, in Atlanta. None of these events are new to historians. However, the discussion in this particular context is interesting. Drawing comparisons with the frontier tradition of vigilantism would have added additional depth to the analysis, though.

The third phase began in the 1920s and ended with the Celtic John F. Kennedy taking office and the elimination of race from immigration legislation in 1965. (It was in the mid-1950s that the term “free white persons” was eliminated from the naturalization law.) A growing concern with “race relations”, which came to mean the socioeconomic position of African Americans, and changing paradigms in sociology in the wake of Nazi horrors, precipitated a more culturally-based discourse on race. Ruth Benedict’s verdict on the existence of only three world races – Caucasian, Mongolian and Negroid – rendered all other distinctions cultural. The Cold War, which left no room for white–white differentiations, African-American participation in World War II, and the emerging Civil Rights movement, all helped firmly entrench American society in a biracial structure. “White” people, previously perceived as greatly varied, were unified in the Caucasian “race”. In addition to these political insights, Jacobson draws on evidence from music, film and literature to argue his case.

In the second part of his book the author attempts to explore the fluidity of race by focusing on one particular year – 1877. There is nothing especially significant about that year, as he points out, but it was a year in which racial issues surfaced in every region of the country: “civil strife over Negro rights in the South, anti-Chinese agitation and Indian Wars in the West,

labor agitation and violence in the Midwest and East [...]” (p. 137). Each of these issues generated its own particular discourse, and these were not always mutually constructive.

The third part, again, is quite convincing to the social historian. Here the author seeks to answer a number of persistent questions: What are the driving forces behind the malleable concepts of whiteness? Which interests drive these changes? And what circumstances account for the transformation? Jacobson chooses three areas to analyse the process of reorganizing a unified whiteness: American imperialism, when in fact it was anti-imperialism which brought forward the most racist arguments; naturalization cases, which forced judges and courts to develop criteria for whiteness (it was the “in-between cases”, the Hindus, Syrians, Armenians, which presented the most difficulties); and civil-rights politics, which, in the process of coalition building among liberal-minded people, rendered Jews once and for all white. The choice of these critical areas is most convincing.

True to his professional “identities” as historian and American Studies scholar, Jacobson’s sources are tremendously varied, ranging from novels, films, print journals, to legal records, colonial charters, and state constitutions. It is this broad range of sources which makes his analysis both challenging and somewhat inconclusive. Sometimes his arguments are hard to contextualize and his findings difficult to evaluate. The book’s argument is most convincing though. Most of us will have had a vague notion about the constructiveness of “whiteness” and its changing content, but now we have a much better understanding of how the process worked and how and why it changed. However, a number of issues remain unaddressed, and some questions unanswered. The discussion of the 1790 naturalization law could have benefited from some “internationalization” by making connections to the French Revolution, which, after all, made people draw opposite conclusions. By the same token, the concept of Anglo-Saxonhood could be contextualized by considering discussions north of the Forty-Ninth Parallel. In Canada, simply “being British” turned a person into a prospective citizen, but that Britishness was very inclusive; it even included runaway slaves. Who is the Anglo-Saxon who suddenly becomes the norm? One gets the feeling that the insertion of class as an analytical concept may have helped. In the discussion of the various naturalization cases, it would have been useful to know to what extent class (i.e. wealth, property, education, economic clout) determined whether a person was more or less white. The total neglect of any consideration of gender, however, is particularly disheartening. It becomes most obvious in Jacobson’s analysis of lynchings, but it is also neglected in his analysis of citizenship issues. Did Jacobson assume that Linda Kerber had said it all? Hardly a valid conclusion.

Last but not least, one would like to know just how malleable the concept of “blackness” was, and whether there is a future beyond race. But these, of course, would be the themes of other studies.

Christiane Harzig

BASKERVILLE, PETER and ERIC W. SAGER. *Unwilling Idlers: The Urban Unemployed and Their Families in Late Victorian Canada*. University of Toronto Press, Toronto [etc.] 1998. 294 pp. C\$55.00. (Paper: C\$24.95.)

“Unwilling idlers” was the usual description given to the unemployed in late nineteenth-century Canada. The fortunate circumstance that the census of 1891, and especially that of

1901, provide information about employment status, made it possible to study the profile of the unemployed in that period, to assess the impact of unemployment on living conditions, and to understand the way the unemployed tried to cope with their situation.

In the preface and the introduction the authors, Peter Baskerville and Eric W. Sager, point out that their book is more than a standard study of unemployment. They have used social science to provide more than just another historical description, and to offer comparisons with other studies and countries as well. They explicitly defend the use of tables and graphs as the best way to summarize the data, and remark that some historians know too little about statistical methods. Their tribute to the publisher, who had the courage to publish the book, despite a refusal on the part of the Aid to Scholarly Publications Program to award it a publication grant, is also indicative of the significance of this book: it is more than a study of unemployment; it is also intended to demonstrate how quantitative history should be used to achieve a better understanding of the past.

The authors' analysis is based on a ten per cent sample of the households (as defined in the census) of six cities: Montreal, Vancouver, Hamilton, Victoria, Halifax, and Winnipeg. Because awareness of unemployment as a social problem and the identification of someone as "unemployed" was mainly an urban phenomenon, only cities were sampled. These cities were chosen because of their differences in terms of socioeconomic characteristics, such as the percentage of inhabitants born outside Canada, industrial- as opposed to service-dominated economies, and the number of inhabitants: the sample includes large cities, but also medium and smaller ones. Since Montreal had more inhabitants than all the other five cities put together, over half the dataset stems from Montreal, rather excessive from a statistical point of view (at least for comparisons at the level of the city). In total the database, which subsequently became part of the database of the Canadian Families Project, covers almost 15,000 households containing about 80,000 individuals (not including the oversampling of the unemployed in 1891).

Baskerville and Sager start by elaborating the concept of unemployment, which only obtained its modern sense towards the end of the nineteenth century. Canadian workers no longer regarded unemployment as a seasonally determined state of idleness but as a consequence of modern capitalism, with its business cycles and, as it appeared to them, its effect on stimulating immigration to create permanent resources of surplus labour.

In the third chapter the authors start their analysis of the censuses by making an extensive comparison of the social and economic characteristics of the unemployed and the employed. In 1891 the unemployed were those out of work the week before the census took place, which was just after the winter. The authors conclude that unemployment was not restricted to just some occupational groups. Even groups like professionals had an unemployment rate of 2.4 per cent. However, the risk of unemployment among industrial workers and general labourers was much higher. Multivariate analysis reveals that city, sex (women were more vulnerable to unemployment than men), age (the youngest and those over fifty had above-average rates of unemployment), and household status (those living on their own were more likely to become unemployed), also influenced the risk of unemployment. Baskerville and Sager reject the "lottery hypothesis" of Keyssar, which states that class background rather than personal characteristics were responsible for the misfortune of being unemployed, and conclude that some groups within the working class were much more vulnerable than others.

The census of 1901 included fourteen questions on employment status. The question on unemployment now related to the entire preceding year. New, too, were questions on the

average monthly income of all individuals in the household, and the degree of schooling. One in five employed persons had been unemployed at least one day during the year. Making a dichotomy between people unemployed for three months or more and the fully employed, the authors repeated their analysis on the 1901 data and reached more or less the same conclusions. Not very surprising is the connection they found between the risk of being unemployed and low income. However, a further important finding is that illiteracy clearly increased the risk of unemployment. Especially vulnerable were workers who accumulated unfavourable characteristics: being nonwhite, illiterate, over fifty, having recently immigrated, *and* being a general labourer or industrial worker.

In several further chapters the authors continue exploring the relationship between unemployment and seasonality, spatial differences, household composition, and living strategies. Most workers were vulnerable to unemployment, though rates differed from season to season. Given the high degree of mobility, it is interesting to see that workers who were least mobile were most likely to be unemployed. The authors explain this by the high costs of mobility and the fact that unskilled workers did not expect to have better chances of finding suitable employment elsewhere. Given the high investment necessary to make it a success, the "open frontier" was more an ideological option than a real one for unemployed workers.

Rates of unemployment differed between cities and between suburbs. Analysing the differences within and between cities by means of a weighted dissimilation index, Baskerville and Sager found few differences in class composition. Although their concept of class may be politically correct, it is quite surprising to see the special class of "housewives" added to the usual groups of employers, self-employed, white-collar, blue-collar, etc. However, one cannot expect much from this way of analysing the spatial effects of social class, since housewives, who accounted for over forty per cent of the index, tended to be spread very evenly within and over all six cities. To assess the enormous differences in income between cities one needs to evaluate differences in living standards. For each city, the authors therefore calculated the minimum costs of survival for each family (the figures are given in a substantive appendix). About fifteen to twenty-five per cent earned less than this subsistence level. Based on the results of their regression analysis, the authors conclude that, generally, the level of family income depended not so much on the city one lived in but on the life-cycle stage.

The concept of the life cycle was operationalized by dividing the households into five groups, each representing a stage in the life cycle: young couples without children, couples with young children, with children ten to fourteen years old, with older children, and older couples without children. By exploring the relationship between these stages on the one hand, and income levels and employment status on the other, the average risks associated with each life-cycle stage become clear. The young, experienced workers earned well, but income per household member fell rapidly as the number of dependent children grew. It fell, in many cases, to below subsistence levels until the children were old enough to start work. Welfare improved then, but not for very long. By the time the children left the parental home, their now elderly parents would have lost much of their earning capacity and become more vulnerable to unemployment and dependence on charity. The authors have shown clearly that only by placing individuals within the context of the household can we gain a better understanding of the workings of the labour market and the effects of the life cycle on material welfare. Baskerville and Sager found cumulative effects too: household members were more likely to be unemployed if the household head was also unemployed.

Unwilling Idlers is a very abundant book, and no single review can do full justice to it. Some of the authors' other important results deserve to be mentioned, however. Seasonal work, such as construction, paid much better than nonseasonal. Being a seasonal worker did not necessarily mean being unemployed during the winter; many of them found other employment. The authors show that average income depended much more on wage levels than on the total number of months worked. So, a century later, the authors succeed in rationalizing the labour union's policy of concentrating on higher wages instead of full employment.

By connecting household strategies to unemployment, Baskerville and Sager forge a link between labour history and the history of the family. By exploiting the microlevel data from the Canadian censuses of 1891 and 1901 they shed new light on the history of unemployment. Their book is remarkable because of its success in integrating qualitative and quantitative history, and in general the authors have used the statistics very efficiently, and very comprehensively.

Kees Mandemakers

KALE, MADHAVI. *Fragments of Empire. Capital, Slavery, and Indian Indentured Labor Migration in the British Caribbean.* [Critical Histories.] University of Pennsylvania Press, Philadelphia 1998. v, 236 pp. \$37.50; £29.50.

The normal writing of history follows chronology. Most historians, in the nature of the discipline, are structured to say when an event started, to trace its movement in time and to discuss consequences. The best example of this genre of writing in relation to the Indian diaspora is Hugh Tinker's *A New System of Slavery*, published in 1974. Tinker's work is of course, a valuable tome of information and a compulsory base for all students of that diaspora. Similarly there are other competent studies which take a microview of Indian immigration to particular territories. In this regard the works on Indians in Fiji by K.L. Gillion and Brij Lal are noteworthy, as is K.O. Laurence's work on Indian immigration to Trinidad. These individual area studies were necessary since there was so little on this sector up to the seventies. Over the last two decades, however, the pace has quickened and much is now known about the *girmityas* (agreement signers) who left India for Mauritius in 1834, and for the Caribbean four years later. We know about the push and pull factors, the conditions on board the indenture ships, and the terms of the contract in various plantation colonies. What we need now are larger macrostudies which minimize chronology and looks at the body trade more globally, seeking to discern the larger imperial ideology which motivated this new form of bondage. There is the need to take up the bits and pieces of useful information from manifold sources and work these into a coherent whole, tracing the linkages which demonstrate common purpose in the whole exercise. This is precisely the approach which Madhavi Kale follows in her work. Here is an author who is not tied by chronology in her search for thematic relevance. The result is a rather different book, unusual in its presentation but all the more interesting for that.

What are some of the major themes which are taken up in this study of imperialism in its full-blown phase? One that is of considerable interest is the careful ideological tracing of the Imperial ideal. The work highlights the never-ending efforts on the part of British

administrations to paint the life of the African or the Indian in the colonies with a rosy brush. Immediately after Emancipation, Foreign Secretary, Lord John Russell, gloated over the transformation which had taken place with that event. In quick time, he noted, the freed Africans had not become robbers or bloodthirsty insurgents but were now “shopkeepers and petty traders, hucksters and small freeholders, a blessed change, which providence has enabled us all to accomplish” (p.76). No mention was made by Russell of the ongoing displacement of Africans on the plantations by Indians and Portuguese, nor of the favouring of the latter by the colonial authorities in the establishment of business enterprise. Similarly, soon after Indian indentureship had been abolished, a white Guianese, who had earlier sojourned in India, pointed out wistfully that many Indians who had returned from the colonies now wished to be reindentured once again. Now, however, the selection process had become very tight and the many who were refused had to remain in Calcutta to bear “the excessive heat, the hard work, the small wage, the never-ending dhal-bhat and chupatties for food”. These hapless Indians would now brood over the loss of a better life in the colonies, where they enjoyed morning coffee and creole vegetables “with which they could vary their diet” (p. 150). Kale continuously juxtaposes this Imperial glorification of the life of the freed slave or the time-expired Indian, with evidence of a different reality, all of it properly documented over an extensive period of time. At the same time the book delves into many of the internal arguments of the British decision-makers. On the one hand there are many British liberals arguing for civilized usage of Africans and Indians as suppliers of cheap labour. On the other hand there are the more level-headed business types who argue that in the light of economic necessity, the philanthropy of the liberals had simply to be abandoned. In any event, the men of the world argued, these benighted labouring people ought to be thankful that Christian missionaries were being sent to rescue them from their heathenism and usher them into a sweeter servitude. As one English Baptist explained, the Indians in Trinidad were being delivered from the “intolerable yoke and curse of caste” in place of which they were receiving the Gospel “with all its unspeakable blessings” (p. 148). The book demonstrates the triumph of Utilitarianism over the Utopianism of the opponents of either the old or the new system of slavery.

Another area which is given large prominence in the book is the detailed examination of the degraded position into which women were demoted during their long years of banishment to the various *tapus* (islands) of indentureship. *Fragments of Empire* provides us with the grim statistics that out of the 19,050 indentured Indian migrants to Mauritius between 1834 and 1843, only 205 were women. Chapter 7 of the book makes for painful reading as the author demonstrates the predominant male view (Indian and British) of women, not as persons in their own right, but rather as vessels through which prescribed practices such as homosexuality and interethnic sex could be controlled. At the same time women were at the root of all evil: prostitution, promiscuity, and the fateful consequences of these practices, namely jealousy and violence among the men. C.F. Andrews, a major disciple of the Mahatma, saw them as “rudderless vessels” being passed from one man to another. Similarly George Grierson, a well-respected British official, argued condescendingly that the Indian woman was “not an unreasoning brute, if she be a little dull of comprehension” (p. 166). In the final analysis however, in yet another spin of this quaint logic, much use was made of the idea that the enslavement of Indian women was a grave affront to national pride. This provided a potent rallying point for the national movement from the late nineteenth century and a major argument for the abolition of Indian

indentureship. This chapter of the work (chapter 7), which discusses the particular problems of women, is the book's strongest section. What comes out most clearly is the male-dominated establishment's ambivalence, and its consequent hamfistedness in dealing with what was arguably the most fragile aspect of the colonial attempt to solve the labour problem through unfree labour.

The question of labour has been the most persistent in colonial history. Unable or unwilling to do the necessary manual work themselves, the European powers sought to devise creative ways of solving that problem. At first it was the use of Amerindian labour through *repartimiento* and *encomienda*. Subsequent to this they used *engagé* or indentured European labour which did not last long. Some European nations even bypassed this resort as they proceeded directly to the use of African labour, which soon became the accepted mode, lasting for some four centuries. Following this, from 1834, there was a return to indentured labour; this time from Asia. *Fragments of Empire* focuses on two dimensions of this long story of labour, namely the African and Indian, dealing with the *problematique* in a thematic manner, providing much that is new from varied archival sources. It is not only history, but rather it interweaves sociological theory with gender considerations in what can aptly be described as a proper subaltern perspective.

The study could have benefited from the inclusion of comparisons with the other Asian group which emigrated, namely the Chinese. This was a very similar – and very different – experience and an exploration of the mechanics of this other form of Oriental servitude would have enhanced the quality of the work. The virtual absence of Chinese women on these indentureships, and the ways in which the Chinese men coped with this loss, would have been enlightening. Some maps indicating the major areas importing these bonded labourers and the routes taken to the Americas over time, would have complemented the up-to-date and exhaustive bibliography which comes at the end of the text. As a total piece of work, however, *Fragments* offers a new and refreshing perspective, taking us beyond chronology to a thorough examination of some of the macroconsiderations which tied together an early attempt at globalization, linking East with West, the Old World with the New. What happened subsequently was a development of these patterns in a more sophisticated manner. Any attempt to understand this present must be based on that past. *Fragments of Empire* successfully unravels much of that complicated past, making sense of a tangled maze of imperialistic devices. In this sense it is a very useful continuation of our understanding of worldwide diasporas.

Brinsley Samaroo

CASANOVAS, JOAN. Bread, or Bullets! Urban Labor and Spanish Colonialism in Cuba, 1850–1898. [Pitt Latin American Series.] University of Pittsburgh Press, Pittsburgh 1998. xiii, 320 pp. Ill. \$45.00. (Paper: \$19.95.)

This study is among the most valuable works on the Cuban labour movement.¹ It also covers Cuban social and political history and the Spanish colonial system on the island,

1. Recently a Spanish edition of the book has been published: *¡O pan, o plomo! Los trabajadores urbanos y el colonialismo español en Cuba, 1850–1898* (Madrid, 2000).

describing their variations and consistencies in the last quarter of the nineteenth century. Significantly, however, this is one of the few studies – despite the increasing trend toward these kinds of analyses in Spanish historiography – that does not perform a linear analysis of Spanish colonialism, but explores the reasons for its actions and changes, compares its mechanisms, and examines the consequences of these changes and reforms. Casanovas shows how the consequences of the reforms introduced by the Spanish government from the 1860s onward exceeded the scope anticipated and instigated revolts among bourgeois and proletarian associations. Some of these associations struggled against the *Partido Español* to achieve greater reforms, although they did not always pursue independence during these years. By participating in the *Partido Reformista*, the elite white artisans protected their class interests and sometimes set aside the racial barriers. The 1860s also contained the roots of the proletarian struggles of 1880. Authors such as Aguirre, Plasencia, and Hidalgo have overlooked this continuity.

I believe that understanding the true value of this book requires situating it in Cuban historiography, and especially in the studies of Cuban society in the period between 1868 and the first decade of the twentieth century. Comprehensive studies on this period are scarce and have begun to appear regularly only recently. This study by Casanovas is interesting for several reasons. For nearly a century, for example, Cuba was a slave society. This situation led to very peculiar intra-ethnic and interethnic relations. As Dr Casanovas has mentioned in a few articles, this condition heavily affected the island's politics until 1898. Both the Negro element and the growing white influence weighed on the society, which was conditioned both by its demographic evolution, by the strengthening of proletarian movement, and by the configuration of the nation that was conceived largely by the elite. Nonetheless, the coloured population gradually became incorporated in the country's civil and political affairs. The progressive abolition of slavery, the continuous arrival of immigrants, and the growth of the proletariat were major factors in this society. Some scholars have described it as a class society, while others refer to a caste society within a class society.

In recent years the panorama of Cuban social history has been enriched through partial contributions, including studies on immigration, race, the proletarian movement, artisans, banditry, and, to a lesser extent, gender, prostitution, crime, settlers, everyday life, establishment of patronage, catechism, and rural workers. Over time, contributions from the history of science and mentalities have gradually been added to both social history and the general history of the island. As with cultural history, which elucidates social conduct, these contributions have not been elaborated. Nor has their scope had the deserved impact of broadening and complementing knowledge on the theme.

Cuban history during the last century under Spanish rule, when consensus was lacking about the presence and definition of the social classes, or concerning their production system under capitalism, has become more complex with the application in recent decades of working methods that, because of their rigidity, often present a rather schematic and reductionist panorama. This impression highlights continuities with the present, by decontextualizing the objects of study and overlooking periods in history that do not substantiate these continuities. On the other hand, most authors consider these approaches too simple to explain a complex process such as the changes that affected the island and transformed it from a caste society into a class society throughout the nineteenth century, and especially during the last three decades. After all, this transition was neither linear nor perfect. Many factors and interests converged, for example, to avert the replacement of slavery by proletarianization or to counteract the weight of the Negro population.

The above factors enhance the value of this book and add to its importance. This study by Casanovas is conducive to a debate about other studies on the labour movement that stress its revolutionary potential and make it virtually dependent on the power of summons by José Martí. In my opinion, however, Joan Casanovas has demonstrated that reformism and anarchism were the labour movement's chief ideologies, as they promoted the measures and solutions that best suited the workers. This has led him to contradict one of the most managed theses, which attributed the influence of anarchism to the prominence of immigrants, especially Spanish ones, as confirmed by Olga Cabrera in her discussion of the Hispanicization of the labour movement. Her archival research and her knowledge of colonial politics enable her to oppose most of the historiography emanating from Cuba, and to demonstrate that the strength acquired by the labour movement deeply transformed colonial society, in addition to contributing to the victory of the independence movement, notwithstanding her criticism of Cuban nationalism.

The effort in most of this historiography to identify continuities with the present has motivated many authors to lapse into reductionist explanations. Regarding the reformist movement of 1860, some authors have maintained that their ideology prevented the workers from developing a national awareness and class consciousness. Compared with these arguments, the work of Joan Casanovas explores the social and political conditions of the final decades of the nineteenth century and re-evaluates the role of anarchism within Cuban separatism. His position teaches us the meaning of anarchism, mutilated and manipulated by a large share of this historiography that focuses on the Hispanicization of the labour movement, noted above as one of the factors that prevented this group from taking power and underlay the lack of national awareness. On the other hand, Joan Casanovas refutes more official theses, by showing that the changes within the Cuban labour movement in the late nineteenth century enabled and caused anarchism to become the leading syndicalist ideology on the island. Anarchism remained important until the 1920s, when the syndicalists – who were mostly anarchists – began to be persecuted, assassinated, and expelled.

Associationism plays a key role in every social structure and provides document registers of groups that would otherwise not have left any historical evidence. This is especially true for associations formed and cultivated by people born abroad and indicates the types of interest and problems that arise within a community, its hierarchy, the measures taken to address them, and the breakdown of the spheres of action between political and civil circles. This study has revealed new analytical perspectives about Cuban civil society, which, as Dr Casanovas submits, experienced major political changes at the end of the nineteenth century. The resulting legalization of certain forms of expression has enhanced awareness of their discrimination, which underlay the emergent nationalism that confronted Spain in 1895.

Finally, this work's importance derives both from its presentation of a different valuation of the Cuban labour movement, and of anarchism, and from its analysis of the changes in Cuban society and the colonial regime in the last quarter of the nineteenth century, which paved the way toward twentieth century society. Some changes were achieved directly by the subordinate Cuban elements, whose ideology was based on anarchism.

Consuelo Naranjo Orovio

DEUTSCH, SANDRA MCGEE. *Las Derechas. The Extreme Right in Argentina, Brazil, and Chile, 1890–1939*. Stanford University Press, Stanford 1999. xviii, 492 pp. \$60.00; £110.00.

Although the Right has ruled Latin America more often than the Left during the twentieth century, and even when in opposition, profoundly influenced the course of the continent's politics, hitherto the scholarly literature has paid relatively little attention to it. The Left has been more carefully and closely studied than its opponents on the Right. Particularly noteworthy is, as Sandra McGee Deutsch correctly points out in the introduction, the lack of comparative works on rightist movements. With this book Deutsch addresses this imbalance. Based on an impressive amount of primary sources, both unpublished and published, she compares rightist organizations, ideas and activities in Argentina, Brazil, and Chile between the turn of the century and the outbreak of the Second World War in Europe in 1939. (In the final chapter she also briefly discusses the legacy of the extreme Right in all countries.) As part of her undertaking Deutsch looks at the composition of the various movements – both in terms of class and gender – traces their programmatic changes over time, and assesses their relationships with the Catholic Church, the armed forces, the conservative sectors, and labour.

The first part of the book, covering the period between 1890 and 1914, deals with the antecedents of the radical Right. Deutsch convincingly argues that a widespread feeling of apprehension about the dissolution of traditional socioeconomic and political arrangements, caused by the economic domination by European powers and the rise of organized labour, was crucial for the emergence of extreme rightist sentiments at the time. Nationalist authors and intellectuals began to articulate this unease about the current situations of their nations, and the first precursors of the far Right appeared on the political scene, particularly the Chilean *Ligas Patrióticas*, the Brazilian *Jacobinos*, and the Argentine vigilantes. They generally linked immigrants, and in the case of Chile Peruvians, with the incipient labour movement, and denounced them, and the foreign owners of the export-based economies, as the greatest menaces to the established order. Still, only in Argentina, where foreigners dominated the work force and the most important workers' organizations, the Right successfully tied antilabour sentiments to antinationalism. The Argentine incipient far Right also differed from its counterparts in both Chile and Brazil as regards its social base. While in the former it was mostly a characteristic of the elite, in the latter it also took roots in middle-class sectors, which were more willing to denounce the existing economic model, based on the export of primary products. In general, however, the differences between the forerunners of the far Right and the conservative sectors were not yet clearly developed. Even in Argentina, which had the strongest extreme Right at the time, the new current could not unite its forces.

During the second period, which covers the years between the outbreak of the Great War and the mid-1920s, the extreme Right become more coherent, moving from rhetorical agitation and occasional acts of violence to the establishment of extraparliamentary groups. At the same time, the perceived threat of the Left, represented by the labour mobilization at home and the revolutionary changes in Europe, replaced the foreign presence as the principal catalyst of rightist activities. Radical rightists, Deutsch points out, "soon interpreted their nationalist mission as one of preventing labor unrest", and fighting leftist utopias, which they regarded "as destructive, misguided, and un-Christian – and a threat to their own positions" (p. 137). Notwithstanding these ideational similarities, Deutsch shows that the strength of the extreme rightist groups differed markedly, reflecting the

peculiar national circumstances. The Chilean paramilitary groups of the far Right, which lacked a carefully elaborated ideology, remained ephemeral phenomena. The factions could not convincingly tie labour militancy to the small Jewish community or the tiny foreign presence amongst workers, and the common action of the Chilean governments and the traditional elite and business leaders against workers and leftist students deprived them of a cause to rally around. The preconditions for the Argentine radical Right were, as Deutsch demonstrates, far more advantageous. The uncertainty about the labour policy of President Hipólito Yrigoyen's administration, together with the perceived severity of the threat posed by militant workers, created a fertile breeding-ground for extreme rightist militants. Especially the *Liga Patriótica Argentina* benefited from this situation. The conservative sectors of Argentine society – e.g., the parties of the moderate Right, the Catholic Church, employers, and the members of the armed forces – shared the fundamental objectives of the group and supported its violent struggle against labour militants. In Brazil, too, the moderate Right and the establishment initially seconded the radical Right, particularly the *Ação Social Nacionalista*, but due to the weakness of the labour movement the group soon faded away. In contrast to both Chile and Argentina, where the organizations of the extreme Right functioned as the armed wings of the oligarchy, the Brazilian groups were, however, not the prolonged arms of the upper class. They were also less elitist and their ideologies distinguished them from the conservative sectors.

The “era of fascism”, the period between the late 1920s and the start of World War II in Europe, saw the emergence of new radical rightist organizations on the political scene, namely the *Ação Integralista Brasileira* (AIB), the Chilean *Movimiento Nacional Socialista* (MNS), and the Argentine *Nacionalista* movement, which united various factions and intellectuals. Influenced by the impact of the Great Depression, which caused economic instability, military involvement in politics, and the mobilization of the left, as well as the rise of integral Catholicism and European fascism, these groups and factions dominated the right end of the political spectrum in their countries. Deutsch generally identifies them as fascist. All organizations continued to oppose the Left, and their militants were frequently involved in violent confrontations with members of labour organizations, but one can at least see, as Deutsch carefully points out, that some groups also attempted to attract workers to their cause. The Argentine *Nacionalistas* was the most powerful and pervasive movement. Despite internal divisions, their essentially elitist orientation and a traditional Right that consolidated its forces and regained some of its power after the putsch of September 1930, the *Nacionalistas* “built upon their longstanding identification with the church, the military, and nationalism to increase their influence”, and, unlike the other groups of the extreme Right, briefly “gained access to power at the national and provincial levels” (p. 193). The AIB, in terms of members the single largest faction, initially benefited from the absence of nationally operating movements, the groups tied to the Catholic Church and the ephemeral *Aliança Nacionalista Libertadora*, the Brazilian Popular Front, being the only exceptions. In the end, the strength of conservative sectors, which enjoyed the crucial support of the military and the hierarchy, found the authoritarian rule of Getúlio Vargas's *Estado Novo*, however, more reassuring than a revolutionary *Integralista* regime. Together they prevented the AIB from achieving power. In comparison with both Argentina and Brazil, the basic conditions for the MNS were far more disadvantageous, because it operated in a relatively well-established party system. In addition, the traditional Right could preserve its strength

and the Left had already embraced ideas of economic nationalism; both factors limited the political space of the faction. But Chilean *Nacismo* was, Deutsch claims, “more worker-orientated, violent, extreme, and opportunistic” than other factions (p. 191).

Overall, Deutsch’s book is highly recommendable. It is lucidly written, convincingly argued, and impresses with the amount of archive sources used. One of the great merits of *Las Derechas* is that it consistently addresses the social and gender composition of the various movements of the far Right. In each chapter Deutsch discusses the role of women in radical rightist activities and assesses the importance the different organizations ascribed to women in their propaganda efforts. Although in general they only played minor roles, reflecting the persistent strength of traditional value systems and the extreme Right’s emphasis on virility and violence, until now the secondary literature on the Latin-American right has completely ignored these aspects.

Only two shortcomings have to be mentioned. Firstly, I think that Deutsch should have paid more attention to the political developments in Europe and demonstrated their impact on Latin America. During the 1930s, for instance, the discussions about the alleged threat of the “fifth columns” to the integrity of the various countries lastingly influenced the perception of the different fascist groups and affected their propaganda. Secondly, the book would also have benefited from a clearer conceptualization of the term fascism. Deutsch falls back upon Stanley G. Payne in the case of the AIB and the MNS, but also uses Ernst Nolte’s definition for the Argentine *Nacionalistas*. Their concepts differ markedly, however, and the latter’s approach, with its focus on anti-Marxism, has long been discarded as too narrow by the literature. Despite these objections, the work fills an important gap. For those interested in the twentieth-century Latin American Right, it contains a considerable amount of new information as well as stimulating interpretations.

Marcus Klein