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BOOK REVIEWS

KAELBLE, HARTMUT. Der historische Vergleich. Eine Einführung zum 19. und 20. Jahrhundert. Campus Verlag, Frankfurt/M. [etc.] 1999. 179 pp. DM 34.00; S.fr. 33.00; S 248.00.

Although historians of all sorts and persuasions practised comparative history during the twentieth century, by the end of this epoch comparative history as a genre still seemed in need of theoretical legitimation and explication. So much can be inferred from the fact that, even in the 1990s, several books and articles were devoted to the subject by practising historians, Kaelble's *Der historische Vergleich*, under review here, being one of them. This observation suggests that comparison as a research field is still *embattled territory* for historians, notwithstanding counterclaims that "*all* history is comparative history" – and the implication that comparative history as such is not worthy of special discussion.

Several explanations for this remarkable state of affairs can be presented (most of them with a long pedigree in historiography), but the most notable is that related to the disciplinary differentiation between history and the social sciences. Since the method of history was defined in the course of the nineteenth century as somehow fixated on the "particularity", or even "uniqueness" of its object, comparative strategies have been regarded by most mainstream historians as lying outside history *proper* and at best have been relegated to the fringes of the discipline (such as *theoretical* or *speculative* history). This tendency was immensely strengthened by the fact that many of the social sciences, especially sociology and political science, claimed comparison as their *own* disciplinary speciality. Therefore, it is by no means accidental that many of the historians who have practised and propagated comparative history have at the same time propagated a *rapprochement* between history and the social sciences.

Hartmut Kaelble, Professor of Early Modern and Modern History at the Humboldt University in Berlin, fits perfectly into this picture. As a former leader of the Arbeitsstelle für Vergleichende Gesellschaftsgeschichte in Berlin, and one of the present leaders of the Zentrum für Vergleichende Geschichte Europas, interdisciplinary and comparative history have been his daily bread and butter for a long time now. He is therefore well equipped to give a sound overview of this field and to act as a guide for beginners. And that is what this short book essentially amounts to; no more, no less.

Being a German professor, Kaelble deals mostly with German and West European history. This German focus might also reflect the fact that for the last two decades or so comparative history in Germany has been closely related to the debates on the alleged German *Sonderweg*. Being linked to the *Sonderweg* debate, most of the comparative history dealt with is related to the national framework and to the usual candidates for comparison with Germany, i.e. France, England, and – to a lesser degree – the US. This fixation on the national framework was also to be expected since the primary unit of analysis of the *Gesellschaftsgeschichte* – i.e. the *Gesellschaft* – was none other than the national state, as was recently observed by both Lutz Raphael and Paul Nolte. However, the "national characteristics" of this book only testify to its concrete, practical character and do not weaken its theoretical argument, for there is no *intrinsic* relationship between the nation-state as an analytical unit of comparison and comparative history; subnational and supranational units are just as comparable as national ones, as Kaelble also agues in his chapter on *Zivilisationsvergleich*.

After an introduction, Kaelble starts off by defining historical comparison (ch. 2) and listing several of its main species (ch. 3), starting with the classic work of Weber, Hintze, and Bloch and ending with modern classics like those of Barrington Moore, Jr and Charles Tilly. Kaelble makes a distinction between two main types of comparative design, one aimed at establishing the characteristics shared by the cases, and one aimed at establishing the peculiarities of each case. However, this contrast is relative, allowing for various hybrids. Along with Theodor Schieder and Jürgen Kocka, Kaelble tends to regard the "individualizing" types of comparison as typical for historians and the "generalizing" types more typical of the social sciences. In contrast to the formative years of the *Gesellschaftsgeschichte* in the 1960s and early 1970s, there is now a clear tendency to distinguish between the methods of historians and those of social scientists.

Kaelble provides further useful subdivisions of comparative approaches within this matrix of "generalizing" and "individualizing" approaches, based on *what* is compared and the intended *result* of the comparison. A first subdivision relates to the contrast between the comparison of cases as a *whole* – *Gesamtvergleich* – (nation-states, for instance) and of *aspects* of cases – *Spezialvergleich* (for instance, the political consequences of various modes of economic modernization). Kaelble, however, rightly points out that it is not possible to compare whole cases without a *selection* of the themes being compared across the cases and without the formulation of questions in search of an answer. Again therefore, the distinction between *Gesamtvergleich* and *Spezialvergleich* is not absolute but relative (see especially pp. 36–37).

A second subdivision is related by Kaelble to the *geographical scope* of comparison, ranging from local to global – with the *Zivilisation* presented as the most promising subglobal unit of comparison. The third subdivision of comparison Kaelble proposes relates to the type of *causal explanation* aimed at, ranging from testing a general causal hypothesis to identifying different causes leading to similar consequences or similar causes leading to different consequences. So although Kaelble too sticks to the Millean idea that there is a "special relationship" between comparison and causal explanation, he explicitly – and justifiably – warns against oversimplifications in this respect.

In chapter 4 Kaelble develops his analytical framework of comparative approaches further by classifying comparisons on the basis of their intended results. He distinguishes four basic intentions behind comparison: the *analytical*, the *enlightening*, the *understanding* and the *identifying* comparison, and presents them as analytical constructs. Therefore each comparative study may combine several of these four intentions, although Kaelble claims that in most cases one intention is clearly dominant.

In the analytical comparison, the basic intention is to analyse causes or develop typologies, or both. This has been the most well-known type of comparison since the 1960s. In the *aufklärende* and *urteilende* (evaluating) types of comparison the basic intention is to compare cases where things went well with those where things went wrong. This type of comparison tells the historian where to locate the causes of *Fehlentwicklungen* in his or her own society and how to evaluate them vis à vis contrasting cases. It is not hard to connect this type of comparison to the *Sonderweg* debate, but Kaelble feels entitled to elevate it to an autonomous type.

In the *verstehende* type of comparison the intention to enlighten and judge one's own society has given way to the intention to understand another society or culture in its

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"otherness". And in the last type of comparison, the *Identitätsvergleich*, the basic intention is to construct a (local, regional, national or supranational, or gender) identity through comparison. Of course, Kaelble hastens to add, these identities are not monolithic but multiple; and, both at the individual and the supra-individual levels, one gets to know their specific identities better by getting to know others.

In chapter 5 Kaelble goes further into the differences between historical comparison and comparison in sociology and anthropology. Not surprisingly Kaelble locates the main difference in the fundamental importance of *temporal* and *spatial* contexts for historians. While sociologists may feel at ease handling a large number of variables over vast tracts of time and space, historians usually do not. And historians usually become suspicious as soon as the relationship between their present-day concepts and the language of their sources becomes obscured or slippery, while sociologists are usually perfectly happy with self-produced "data". This disciplinary *habitus* of historians goes a long way to explain why even most comparative historians feel ill at ease on terrain without clear temporal and spatial contours; for them, basically, *time and space matter*. The same *habitus* explains why the explanatory role of social-scientific theory is likely to remain contested even in comparative history.

In chapter 6 Kaelble presents a practical guidebook to comparative history for the uninitiated. The formulation of research questions, the selection of cases for comparison, and the problem of locating and selecting relevant sources are dealt with successively. Pitfalls are identified and overoptimism is tempered: Kaelble makes no bones about the irritating fact that, though comparative research designs may be intellectually more satisfying than noncomparative designs, their price in terms of time is extraordinarily high; (according to one practitioner comparativists invest at least twice as much time as their noncomparativist colleagues and receive at best half their recognition). He also warns against the idea that comparison is *always* the best approach and against the idea that it is the solution to *all* historical problems.

Kaelble's openness to the problems of comparative history is admirable, although undoubtedly some may find his openness does not go far enough. For instance, the problem of the relationship between comparative history and *Transfergeschichte* is touched on by Kaelble only marginally, while the main problem raised by *Transfergeschichte* goes to the very heart of comparative history, since similar developments in different contexts may simply be due to the *transfer* of specific "items" (such as socialist ideology or trade unionism) instead of to similar sets of variables causing similar "effects" or "outcomes". Here we touch on the presupposition, tacitly adopted from J.S. Mill by much comparative history, that the "cases" compared are, in effect, independent of one another and do not in any way interact with each other. This quasi-experimental condition, however, is seldom met in history, where almost *all* "cases" interact in one way or another.

However, Kaelble has invested his energy in pursuing other tasks, and only time will tell what weight should be attached to the qualifications of the *Transfer* critics. In the meantime, Kaelble has written a lucid and useful analysis. It can be recommended to all with an interest in comparative history.

Chris Lorenz

Namer, Gérard. Le système social de Rousseau. De l'inégalité économique à l'inégalité politique. [Logiques Sociales.] Éditions L'Harmattan, Paris; L'Harmattan Inc., Montréal 1999. xxii, 212 pp. F.fr. 120.00

The re-edition of this study, first published in 1979 by the Emeritus Professor of Sociology at the Paris University VII, Gérard Namer, proves just how important this discipline, long declared dead, actually is for social confrontations and conflicts, even today. For many years, the history of political ideas, particularly in Germany, appeared to have been banished, so to speak, to a "warehouse" (Klaus von Beyme), or at best to a "museum" (Ulrich von Allemann). Whereas its traditional significance as a model and scientific compass toward improving discernment was in danger of being suppressed and forgotten in Germany by social history, this was not quite the case in France, nor for that matter in England (Pocock, Skinner, Baker). Classical French writers of political thought, particularly those of the Enlightenment such as Montesquieu, Rousseau, Mably, Condorcet, and Siéyès among others, have time and again caused sweat on the brow of many a scholar, as a glimpse at the relevant bibliographic data banks reveals. Of course, the majority of work listed there falls into the categories of literary history and the history of political ideas, and seldom into that of the sociology of knowledge.

This historiographic perspective makes the study by Gérard Namer all the more interesting. Namer, who has previously produced sociological studies on the theories of Machiavelli, Montesquieu, and Maurice Halbwachs, reads Rousseau from a new, less common angle: Rousseau as a precursor of sociology! At the same time, Namer imposes his methodological approach on the function of the history of political ideas in an effort to resolve political questions still at issue – be it in the era of "neoliberalism" or of "civil society" or within the model of a "new middle." Who would want to deny that social inequality is no longer a problem challenging politics? Is there not more than material poverty hidden behind the facade of wealth even in industrialized societies? This is the perspective of political perception that concerns and motivates the sociologist Namer. Rousseau is not stuffed into any of the once common categorical boxes. He is not considered to be a precursor of modern theories on freedom and alienation or of constitutional models of soviet democracy, let alone of totalitarianism. What is pointed out instead is that Rousseau, unlike Montesquieu, was not concerned with forms of government, be it monarchy, tyranny, republic, or democracy, but focused instead on overcoming the consequences of conditions of inequality in society. Social inequality is not perceived as a problem of an agrarian society, as described through the lenses of social history, but as a theoretical problem within the context of its political consequences. The crux of the problem lies in the connection between social inequality, poverty, the underprivileged, exclusion, and deprivation linked to waning social mobility that is rooted in conditions related to poverty. Important to Rousseau, as Namer elaborates, are the consequences arising from this, particularly the advent of oppression and political violence. Namer shows that Rousseau thoroughly grasped the economic, political, and social realities of his time and that, consequently, Rousseau's sociological theory is not at all abstract and unhistorical, as could be assumed if, as is often the case, one possesses no more than a superficial knowledge of his theories on social contract, the "*volonté générale*" and "*volonté de tous*". Rousseau is not interested in an apolitical context, philosophical speculations, a fantasy of ethical conviction about paradise on earth, but about a fundamental, empirical, and sociological theory for shaping a modern civil society based on human rights. According to Namer, Rousseau's theory makes it possible to contrast

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permanently the failings in a society (inequality, exclusion, injustice, misuse of power, corruption, etc.) to the ideal type of a thriving and humane constitution. It is the responsibility of politics to implement this idea. If politics does not fight to eradicate inequality, it destroys democracy and the basic civil principles of modern societies; this is the virtually the same message found repeated later by Tocqueville, Karl Marx and Max Weber, Emile Durkheim and Karl Mannheim.

A historian would possibly like to see this new, empirical-sociological method discovered in Rousseau's work being supported concretely by more examples from the time period itself and from the more recent and quite abundant scholarly research that has been done on this topic and also on the sociology of knowledge. The theorists of the modern representational, constitutional state will note that Rousseau underestimated the need for institutional controls over political power, unlike his contemporary Gabriel Bonnot de Mably, and that this also is largely ignored by Namer. Still, in the reading of the paperback published by Editions L'Harmattan in the *Collection Logiques Sociales*, and prefaced with the eloquent foreword by Francis Farrugia, an old idea comes alive, an idea that Albert Otto Hirschmann, for example, in his work on passion and interests in the eighteenth century, once emphasized: it is still worthwhile to study time and again the theories of the classic philosophers, to reread them from varying theoretical perspectives. Such study is not valuable because it will settle controversies, but because the quality of the controversy can be thereby stimulated and perhaps improved. This may be a more convincing argument for interpreting Rousseau anew from a sociological perspective than Namer's focus on the regulative idea of equality under conditions created by globalization processes. And this perspective might well be more fruitful politically than the method favoured by social historians merely to provide a description, and a more or less Eurocentric one at that, of social inequality and its consequences.

Peter Friedemann

Moulier-Boutang, Yann. De l'esclavage au salariat. Économie historique du salariat bridé. [Actuel Marx Confrontation.] Presses Universitaires de France, Paris 1998. 768 pp. F.fr. 168.00.

More than 100 years ago the exploitation of "guest workers" caused the labour movement to envisage them as unfree rather than as free labourers. As Claudie Weill has shown in *L'Internationale et l'autre*[1] (not quoted in this book), the Second International were doubly indignant: about the appalling working conditions endured by these immigrants, and about the unfair competition the immigrants seemed to represent. The revival of this type of migration in postwar Europe gave rise to similar discussions, including one on the exploitative nature of capitalism. This time the discussion was dominated not so much by trade unionists but by activists and scholars. One of them is Yann Moulier-Boutang, a French social scientist who was born in 1949 and has taught at the Institut d'Études Politiques in Paris since 1979. He has been wrestling with the issues raised in these discussions since the early 1970s.

His three-volume doctoral thesis, based on an extensive reading of the mainly French, Portuguese, and English literature, was entitled "Le salariat bridé, origines des politiques

1. Claudie Weill, *L'Internationale et l'autre: les relations inter-ethniques dans la IIe internationale* (Paris, 1987).

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migratoires, constitution du salariat et contrôle de la mobilité du travail". In this slightly abridged and revised version, published in 1998, he analyses the conditions for the emergence of unfree labour since the Middle Ages, particularly in Europe and its colonies. His chronological and spatial framework resembles that of Lydia Potts's *The World Labour Market*.[2] By *salariat bridé* he means "all types of contractual labour relations that are constraining in form and in the content of what is sold (i.e. more than pure free labour)" (p. 16).

His objective is ambitious. He wishes to offer an alternative to Karl Polanyi's *Great Transformation*.[3] Polanyi's equation of capitalism with a free labour market is incorrect, says Moulier-Boutang, who points not only to "the methodical and continuous fight against human mobility by the first forms of capitalism, whether based on mercantile interests, landed property or on mercantilism", but also and most of all imperialism, with its forced labour in Africa and coolie labour in Asia, and, subsequently, labour migration under special legal conditions (p. 380).

To formulate an alternative, he begins with the seminal article by Evsey D. Domar,[4] which in its turn builds on the insights of the Dutchman, H.J. Nieboer,[5] and the Russian, V. Kliuchevski.[6] Domar suggested that a high land-to-population ratio explains the emergence of unfree labour, but only if there was government intervention to enforce it. However, according to Moulier-Boutang, this does not explain the abolition of unfree labour, neither in Russia, nor in the US. Therefore he proposes (p. 672) to take up a suggestion by Nieboer, who wrote "that slavery cannot exist to any considerable extent among peoples with closed resources",[7] and to expand it by including not only land but also capital and external labour supply as a resource. In doing this, Moulier-Boutang concludes that "on an international scale the availability of quasi-unlimited numbers of people for the immigration countries leads easily to an unfree labour market" (p. 673). In this way the author thinks his original question about the recent emergence of *le salariat bridé* can be answered.

And so it can, partly, if only because of the broad definition used. It is also a truism that capitalism and unfree labour can go hand in hand, as many historians have shown. Nevertheless, the author has not been able to escape the fate, summarized by himself in his introduction with the proverb "*qui trop embrasse mal étreint*" (p. 14, "he who grasps too much, loses all"). Instead of concentrating on an extensive body of historical evidence, he should have given more thought to frameworks of analysis that are not purely economic. This might seem a strange reproach to a scholar who has made his career at a prestigious institute of political studies and who tries to include many different approaches in the numerous analytical diagrams that illustrate his final chapter and the appendices. Nevertheless, as Marcel van der Linden has shown, the limits to the freedom of labour under capitalism cannot be explained by economics alone.[8] Micro- and macroeconomic

2. Lydia Potts, *The World Labour Market: A History of Migration* (London, 1990).
3. Karl Polanyi, *The Great Transformation* (New York, 1944).
4. Evesey D. Domar, "The Causes of Slavery or Serfdom: A Hypothesis", *Journal of Economic History*, 30 (1970), pp. 18–32.
5. H.J. Nieboer, *Slavery as an Industrial System* (The Hague, 1900, 1910).
6. V. Kliuchevski, *A History of Russia* C.J. Hogarth (transl.) (London [etc.], 1911).
7. Nieboer, *Slavery as an Industrial System*, p. 442.
8. Marcel van der Linden, "The Origins, Spread and Normalization of Free Wage Labour", in Tom Brass and Marcel van der Linden (eds), *Free and Unfree Labour: The Debate Continues* (Berne [etc.], 1997), pp. 501–523, 521.

factors do play a role, but so do politics and morals in politics. Only in combination do they offer the sort of explanation Moulier-Boutang is looking for. At the same time, they would have led him to a different appreciation of the legal restrictions on labour migrants in late twentieth-century Europe. Different to such an extent that it would have influenced the straightforward suggestion now implied in the author's subtitle.

Jan Lucassen

GESTWA, KLAUS. Proto-Industrialisierung in Rußland. Wirtschaft, Herrschaft und Kultur in Ivanovo und Pavlovo, 1741–1932. [Veröffentlichungen des Max-Planck-Instituts für Geschichte, Band 149.] Vandenhoeck & Ruprecht, Göttingen 1999. 680 pp. DM 142.00.

This is an ambitious work. Not only does it attempt to capture almost 200 years of Russian social and economic history, it aims to do so on a topic left virtually untouched by earlier research. In spite of the fact that almost any work on Russian economic history mentions the existence of cottage industry, or *kustar nye promysly*, in passing, there are very few articles, and even fewer monographs, specifically devoted to the topic. Gestwa tries to fill this gap, and as a starting point for this endeavour he takes the protoindustrialization model developed in the 1970s and 1980s by Schlumbohm and others. Despite having found widespread application since as an analytical framework for understanding the early phases of the industrialization process in various regions, the model has never really been used for describing the development of similar processes in Russia, even though it was generally assumed that patterns of development east of the river Elbe must have differed from those in Western and Central Europe in fundamental respects. Gestwa's aim therefore is not just to enrich our understanding of Russian social and economic history by applying the protoindustrialization model, but also deliberately to place the Russian case in an international context, and possibly refine the model on the basis of the findings of this study.

It is to the credit of the author that this ambitious research agenda is brought to such a fruitful end. In almost 600 well-argued pages, a remarkably detailed picture is painted of the development of two protoindustrial regions in Russia over the one and a half centuries between the start of the reign of Catherine II and World War I. The first is the Ivanovo region, with its textile production, which started as a form of cottage industry and became factory-based towards the end of the nineteenth century. The second is the village of Pavlovo and the surrounding area, where household-based metalworking developed into a thriving form of cottage industry during the second half of the eighteenth and the first half of the nineteenth century, producing locks, knives, scythes, and other such items for both urban and rural markets. In both regions these particular industries had a much longer history, but it was only towards the mid-eighteenth century that they developed into protoindustrial regions in the full sense of the model, i.e. with the systematic production for external markets as their main economic activity. Unlike similar regions in other parts of Europe, these were interregional rather than international markets. Indirectly, though, international trade did play a crucial role in the process of protoindustrial expansion through exports of agricultural products, bringing in revenues that fuelled home demand for products of the Russian cottage industry. In response to this demand, production steadily expanded, involving larger and larger areas, as well as growing numbers of people.

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At the peak of the development of these regions, around 200,000 people were wholly or partially employed in the textile industry of the Ivanovo region and some 20,000 in metalworking in Pavlovo and the surrounding villages. What is striking is that this economic dynamism emanated almost entirely from the countryside, which is in stark contrast to the traditional image in historiography and popular writings of a backward, primitive and virtually ossified rural sector.

In fact, this dynamism was such a strong force that cottage industry was very slow to be replaced by factory-based production. In the Ivanovo region it was only towards the end of the nineteenth century that, following an intermediate stage of a putting-out system, the gradual development from household production to industrial manufacturing reached its conclusion. In fact, the transformation was such a drawn-out process that Gestwa convincingly argues it is perhaps not very fruitful to adopt the perspective of a transition. The period in which it coexisted with the first industrial establishments was one of the heydays of the cottage industry in the region, as the two forms of production lived in a peculiar symbiosis, in which manufacturers relied on the system of putting-out as a buffer to make up for shortfalls in industrial production, and to respond to unforeseen upsurges in market demand. In the Pavlovo region, there was scarcely any trend towards mechanization and industrialization, and household production for the market continued to dominate until it was crushed as part of the offensive against private economic activity that accompanied the industrialization drive of the First Five-Year Plan. In both cases, however, the main factor that underpinned the resilience of cottage industry was that it offered comparative advantages over factory production in a market characterized by an unstable and unreliable demand. Home producers could fall back on agriculture in times of failing demand, or, conversely, take on the extra work that the factories could not handle in times of high demand.

Having completed his analysis of the development of cottage industry in the two regions, Gestwa then proceeds to explore further aspects of the protoindustrialization model in four well-structured chapters, dealing with, respectively, market development, local power and feudalism, demography, and the social and cultural aspects of the development of cottage industry in the two regions. Some of these chapters will primarily be of interest to those readers curious about the validity and applicability of the protoindustrialization model, but others, like the one on markets, will appeal to any scholar of Russian social and economic history.

As regards the wider aims of this study, the author concludes that the protoindustrial development in Pavlovo and Ivanovo differed in several respects from the experiences of other countries and regions to which the model has been applied, but that these differences are small enough for Russia to fall firmly within the diversity allowed for in the model. Of these differences perhaps the most significant is the fact that in Russia the impulse towards protoindustrialization came almost entirely from the countryside itself, whereas in the model's mainstream cases it was urban entrepreneurs who took the lead. As to the contribution this study makes to existing knowledge on Russian social and economic history, the author concludes that the application of the protoindustrialization model does not yield any dramatic challenge to conventional wisdom, but that it does enrich our understanding of the complex roots of the industrialization process in the Russian empire. While correct, this claim is also perhaps slightly too modest, since Gestwa's work does offer some important insights.

Above all, it draws our attention to the role of the rural economy in the process of industrialization in Russia. Gestwa shows how, in the Ivanovo and Pavlovo regions, industrialization was driven by the rural sector, which provided not only the resources and markets for industrial expansion but also the bulk of the entrepreneurs. As the author concedes, this experience might not have been characteristic of all regions, and different paths towards industrialization can be observed in other sectors and in other parts of the country. Nonetheless, rural protoindustrialization was an important phenomenon in this sense, as is evident from the fact that in 1902 the majority of industrial establishments were located in the countryside. This central role of the rural sector left its mark on industrialization. The dependence on the rural market made protoindustrial and industrial expansion a slow and brittle process because of the relatively modest and sharply fluctuating demand in this market. As Gestwa points out, the development of a rural market in Russia was much more a process of "monetization" than "commercialization", in the sense that it consisted of an increase in cash at the disposal of rural households rather than of a market-led process of economic expansion. Thus, in the end, it is the weakness of market demand that comes to the fore as the decisive factor in Russia's slow pace of industrialization, a finding that corroborates much of the literature on modernization processes in other sectors of the Russian economy, notably in agriculture.

In some parts of Gestwa's study the text is marred by lengthy citations from the works of the main theoreticians of the protoindustrialization model. These might safely have been dispensed with, since they disrupt the flow of an otherwise well-structured argument. Also, a few of the chapters lack any real function other than to test some of the assumptions of the model. On the other hand, there are some advantages to using a model that is so comprehensive in its approach and that has been so widely applied elsewhere. One of the very clear findings of this study is how cottage industry developed in close conjunction with the other economic activities of the rural household, ranging from agriculture to wage work in manufacturing, without the one completely replacing the other for most of the period being studied. Finally, by providing a readily accessible basis for international comparisons, the protoindustrialization model has prevented the author from falling into the trap of interpreting the specifics of protoindustrialization in the Ivanovo and Pavlovo regions as being characteristic of a "uniquely" Russian path of economic development. In fact, his study shows quite clearly how the Russian case is consistent with the experiences of other "late industrializers" on the European continent.

Gijs Kessler

Kharkhordin, Oleg. The Collective and the Individual in Russia. A Study of Practices. University of California Press, Berkeley [etc.] 1999. xii, 406 pp. Ill. \$50.00; £40.00.

In this big, noteworthy, and problematic study, Oleg Kharkhordin constructs a grand framework tracing the relationship of the individual to the collective throughout the course of Soviet history. Pursuing a modified Foucauldian framework, the author, a professor of political science and sociology at the European University of St Petersburg and a Berkeley Ph.D., puts "background practices" of self-fashioning and individualization at the center of historical and sociological explanation. If, following Foucault, the *locus classicus* for the rise of Western introspective individualism was private Christian confession, the author argues that the individual in Soviet society was created out of a Russian Orthodox cultural matrix. In the place of confession, Kharkhordin places Russian group penitential practices based on "revelation" of sins.

According to Kharkhordin, then, by going beyond a focus on ideology, institutions, and other "foreground" phenomena and instead uncovering the deep logic of these Russian-Soviet practices, it can be concluded that the secular Bolsheviks unwittingly resurrected and modified the basic patterns set by Russian Orthodox ecclesiastical courts, largely defunct since the seventeenth century. The dynamics of party disciplinary hearings, purges, and other related Soviet methods of individual and group formation operated in harmony with what Kharkhordin identifies as the Orthodox triad: "reveal – admonish – excommunicate". The Great Purge under Stalin, his argument continues, radically furthered individualization within the context of the group by enforcing Soviet-style self-fashioning on a mass scale in the midst of terror. This set the stage for "mutual horizontal surveillance among peers" (rather than Western-style hierarchical surveillance) to emerge full-blown under Khrushchev as the key dynamic in the ubiquitous collectives (*kollektivy)* of mature Soviet society. As Soviet culture became increasingly ritualistic in the late Soviet period, in turn the Soviet-style practices tying the individual to the group were replicated in informal, nonofficial, and dissident settings. The bulk of the book is devoted to substantiating this overall framework, to examining the pre-Soviet origins and Soviet-era evolution of an array of practices connected to the individual and the group in Soviet society, and to interpreting the major periods of Soviet history in their light. Because this is so clearly an original and intellectually powerful work of scholarship that will deservedly gain a broad audience, I will devote this review primarily to criticism, and will register the work's most noteworthy contributions only toward the end.

Kharkhordin's book adds to a burgeoning, if long-standing, fascination with the analogies between Soviet communism and religion. Others have sought homologies between theology and ideology, found quasireligious phenomena filling the voids left by secularization in revolutionary or modernizing society, or traced connections between Russian Orthodoxy and communism in the lives and thoughts of historical actors. Kharkhordin makes the link into the linchpin of his broader historical-theoretical edifice by positing a direct historical genealogy between disciplinary institutions (particularly the ecclesiastical courts and the Central Control Commission) and specific practices (such as *oblichenie*, or the accusation and public revelation of sin, a term also used during the purges). In the undaunted manner of much post-Soviet "culturology" he compares the Protestant Reformation with the Russian Revolution.

Luther made Augustine's solitary confession into the central practice of what became Protestant culture. In so doing, he received credit for helping develop the individualism of Western culture. The Bolsheviks similarly radicalized those ecclesiastical practices that were available to them in their culture, based on Eastern Christianity and the centrality of public penance. Instead of using the aristocratic models of individualization copied from Western models of confessional practices and solitary self-reflection, they turned the Orthodox practice of *oblichenie*, nearly defunct in the official Russian Church, into the predominant Soviet mechanism of bearing witness to one's achievement of lay sainthood and "assurance of grace" (p. 228).

Precisely how and why church mechanisms that became archaic in the course of the eighteenth, nineteenth, and early twentieth centuries revived under the Bolsheviks is not a problem that preoccupies this work. Kharkhordin only briefly observes that in the imperial period they were perpetuated in "discourse". The question arises as to whether, propelled onward by his grand scheme, he has sought out elements of similarity in pre-Petrine Orthodox and twentieth-century Bolshevik practices at the expense of other features that might detract from the sweeping elegance of his deliberately provocative construct. His ubiquitous deployment of religious terminology to describe Soviet-era phenomena – such as his translation throughout of the key Bolshevik term *soznatel'nost'* (consciousness) as upper-case "Conscience" – seems to bolster this suspicion. Despite its flaws, the standard translation dismissed by Kharkhordin does capture elements of "awareness", political education, enlightenment, rationality, advancement on a political hierarchy and cultural level, the first elements of which have an analogue in the Western left-wing phrase, "consciousness-raising". Indeed, Kharkhordin's pursuit of Orthodox roots of Bolshevik practices leads him virtually to pass over far more immediate and extensive roots of Soviet behavior in the culture and group dynamics of the revolutionary movement. It also prompts him to marginalize or minimize manifold modern and scientistic aspects of Soviet development, not to mention such factors as the exposure of leading Bolsheviks to Western life and culture, the role of professionals and the non-Party intelligentsia in shaping the Soviet order, the legacy and rediscovery of prerevolutionary cultural models, the ideas and culture of Russian Marxism and social democracy, and so on and so forth. A reductionist urge lies at the heart of Kharkhodin's schema.

A second feature of the study, the author's preference for breadth over depth, is reflected in the source base of the study. The book interprets a significant array of published primary sources, most strikingly Soviet handbooks and instructions on organizing group meetings and purge sessions. These are fascinating documents rarely used before, and they are supplemented by revealing excursions into belles-lettres, Stalinist pedagogy, and, most originally and effectively, fresh readings of late-Soviet sociological and psychological studies. The greater number of extant windows into the role of the individual in post-Stalinist society make Kharkhordin's readings of the early Soviet and Stalin periods the most provisional. How can one write a "study of practices" using handbooks about how purges and meetings should be conducted? One is tempted to call the result a study of discourses about practices. Even a modest attempt to consult the voluminous files of the Central Control Commission, open since the early 1990s (not to mention available records of party cells and other "collectives") might allow a deeper exploration of group dynamics – if only in the context of a single concrete group. It is clear that the author is no archival historian. Yet one cannot square his logocentric analysis with his simultaneous claim: Soviet "background practices" have been ignored by Western scholars busy analyzing surface phenomena, oblivious to those deeper "everyday" practices that gave them meaning. This "failure" on the part of all previous historians ("with a few exceptions"), he claims, suggests that scholars would do well to study background practices as he does before proceeding to "comparisons of foreground phenomena like the content of discourse or the official form of institutions" (p. 361). While some might claim that all records of group behavior are discursive representations, it does appear that Kharkhordin has relied on discourse to get at everyday practices, and then maintained he has penetrated far deeper than discourse. At the very least, he has juxtaposed the prescriptive literature on group behavior with societywide behavior itself.

Kharkhordin's handling of the Great Purge of the late 1930s can serve as an illustration of how his approach and source base interact. His basic argument is that the "admonish" part of the traditional Orthodox triad temporarily vanished, and a novel and deadly configuration of practices emerged: self-criticism (denunciation) and purging (excommunication) were fused into a single process as both were assigned to grassroots "collectives" around the country. Now, the whole group had to participate in a process of deliberation about its members. The notion that the crucible of terror imposed on a mass scale "practices of individual self-care" through "collective self-analysis and collective self-

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criticism" (p. 236) seems a provocative and convincing argument. Yet the whole thesis is also revealing on another level. The deeper meaning of the purges, in the author's fertile yet schematic mind, is related solely to a self-referential shuffling of the various practices and elements of his own broader framework: "admonition" disappears, self-criticism and purging fuse. One is not overly impressed with Kharkhordin's familiarity with the historiography on the 1920s and 1930s; "with a few exceptions" he relies heavily on a handful of obvious studies by Fitzpatrick, Kotkin, Getty, and others. Even in terms of his own framework he is thus not aware (see p. 155) that purging and self-criticism were in fact intertwined at a major turning point before, in the all-union self-criticism campaign beginning late 1928 and the simultaneous battle against the "Right Deviation", as well as in the subsequent purges of party cells and state institutions the next year.

The achievements of the study are several. Most broadly, Kharkhordin has convincingly suggested that individuality in the Soviet Union arose with its own particular features and should be considered in the context of collectives. His stimulating analysis of a wide range of concepts (such as *lichnost* and *kollektiv*), connected to the individual and the collective, is carried out at a high level. Of great interest to historians will be his novel treatment of the Khrushchev Thaw, which is described less as liberalizing anti-Stalinism than a realization of Stalin-era projects. A broadly based "collectivization of life" campaign under Khrushchev is interpreted as a series of far-flung efforts to extend social pressure against deviance and further aspirations of total "horizontal" control in Soviet society, so that the pervasive coercion of "comradely admonition" would replace outright terror. Suggestive also is the way he suggests how official Soviet practices were internalized and replicated by unofficial groups and subcultures under Khrushchev and Brezhnev. At the beginning and the end, Kharkhordin states that one of his goals is to move beyond tired and blunt comparisons of the individualistic West and collectivist Russia, and in the case of Russia he certainly has succeeded.

For 360 of the book's 362 pages of text, however, this reader kept wondering if there were not certain obvious oversimplifications in Kharkhordin's repeated arguments about Russia and the West. For isn't it difficult to maintain that Western culture rests on hierarchical rather than horizontal surveillance and on confessional, contemplative techniques rather than any "penitential" analysis of deeds? Correspondingly, isn't the notion that hierarchical surveillance in the Soviet Union was of dramatically less significance than "horizontal" a bit hard to swallow? Only on the last two pages of the book, although he has given intimations of it before, does Kharkhordin reveal that he considers his own theses "rather crude" (p. 361). "Comparisons – if they are to be drawn as meticulous comparisons, rather than initial crude generalizations – should perhaps [...] note that subjects in Russian culture rely on penitential modes of self-knowledge in 70 percent of their life situations, while in the balance they use confessional techniques. By contrast, Anglo-Americans may turn out to rely on confessional techniques in 70 percent of life situations, and on penitential techniques in 30 percent." (p. 362). Some cultural historians, who seem to have increasingly moved away from imposing schematic models on the historical process and whose mode of cultural analysis seems more than ever to value thick description, may nonetheless find it a relief to discover that Kharkhordin's oversimplifications were not unintentional.

Michael David-Fox

Koch-Baumgarten, Sigrid. Gewerkschaftsinternationalismus und die Herausforderung der Globalisierung. Das Beispiel der Internationalen Transportarbeiterföderation (ITF). [Quellen und Studien zur Sozialgeschichte, Band 17.] Campus Verlag, Frankfurt [etc.] 1999. 578 pp. DM 148.00; S.fr. 137.00; S 1080.00.

Trade-union internationalism, and globalization in particular, are in again as topics of social and historical research. Sigrid Koch-Baumgarten, however, is not just surfing a trend. For many years now she has been studying various aspects of international trade unionism and the history of the ITF. Her present book focuses on the conditions for the constitution and functioning of trade-union internationalism – still, she argues, an underrated field of historical analysis (p. 12). Koch-Baumgarten begins with a thorough presentation of different approaches towards trade-union internationalism in historical research, before going on to explain her own. Hitherto, questions borrowed from research on national trade-union organizations have too easily been transferred to the international arena. She outlines the particular framework in which international trade unions operate: competition between capital in one country and that in another as a major element blocking any harmonization of working conditions; international antagonism between communities of interest of capital and labour concluded at the national level; and, due to the absence of state-like structures, the marginal institutional representation of trade unions at the international level (pp. 35 ff.). One may doubt whether there really is competition between national, i.e. French and German, capital, but certainly the reader is more than willing to share Koch-Baumgarten's scepticism as to whether there were ever collective interests at the international level, at least in the past. She relies heavily on the approach of the *Regimeforschung*: at the international level there is little, if anything, comparable to the representation of interests at the national level. There is pragmatic cooperation in certain specific areas, where national actors are dependent on what others are doing beyond their own frontiers: "Als Gewerkschaftsinternationalismus wären damit auch informelle Normen, Regeln und Verfahrensweisen zur Konfliktregelung oder zum gemeinsamen Management von problematischen Interdependenzbeziehungen im engeren Gewerkschaftsumfeld zu begreifen, auf die sich Verbände in spezifischen Politikfeldern einerseits und in den gewerkschaftlichen internationalen Binnenstrukturen andererseits einigten" (p. 39). It seems to me that such an approach is more useful than the romantic fiction of an international working class with unified interests, but still I doubt whether the *Regimetheorie* can help us understand all aspects of modern trade-union internationalism. In recent years we have seen major European mobilizations, manifestations, and sometimes strikes and other forms of protest that can hardly be explained by an intention simply to manage problematic areas of mutual interdependence.

Koch-Baumgarten takes the International Transport Workers' Federation as an example for her study, since the ITF has been one of the most influential International Trade Secretariats, and since the transport sector is particularly influenced by its international structures. She warns the reader that it is not her intention to offer a comprehensive presentation of the organizational development of the ITF through its entire history, but rather to offer a mix, with chapters dealing with the organizational development of the ITF in certain periods alternating with examples of international trade-union cooperation.

The foundation of the ITF she defines as a long process in which national actors from completely different backgrounds became acquainted with one another and developed elementary informal rules concerning the exchange of information and mutual support in the event of strikes. It was on this basis that the ITF could build in the period after the Great War. Koch-Baumgarten is right in stressing the tremendous difficulties in overcoming national cultures of industrial relations. One might ask, however, whether the ice was not thinner than she pretends: the records of the ITF secretariat in the 1920s are full of complaints about member organizations not sending in any information. During the interwar period, in the Fimmen era, the ITF was basically a European organization "mit vereinzelten Außenposten" (p. 67) in other parts of the world. Koch-Baumgarten explains the extraordinarily active role of the ITF by referring to the international structure of the maritime transport sector, although she herself underlines the fact that over half the ITF's members were in the railway sector. The main novelty in the organizational development of the ITF after the Great War was the creation of the secretariat, which received its legitimacy directly through the international organization (pp. 69 ff). What member organizations expected from the ITF was above all information and assistance in the event of industrial action. One might doubt whether the campaign of solidarity organized by the ITF during the British General Strike of 1926 really demonstrated that there was a well-functioning system and well-respected rules for such cases inside the ITF. Fimmen's own report was rather pessimistic. Conflicting political options and particular national interests often blocked effective international solidarity, as Koch-Baumgarten underlines. When the Second World War started, the ITF, now financially exhausted and deeply divided over the policy of nonintervention in the Spanish Civil War, and over appeasement, was no longer an independent actor. During the War the ITF changed its policies and came out openly in support of the allied countries, to defend trade-union liberties along with parliamentarian democracy. British trade unions were heavily dominant in the ITF, and it was therefore no surprise that the ITF tried to impose guidelines for collective bargaining which were influenced by British standards. However, the ITF did not fully succeed in exporting the British model of labour market regulation in wartime; Norwegian seamen, in particular, defended their freedom to engage in collective bargaining tooth and nail. Koch-Baumgarten gives a short but comprehensive analysis of the ITF's efforts to support the allies during the War, from anti-Nazi broadcasts on the BBC to cooperation with the secret services of allied countries.

The perception of trade-union internationalism changed little after the Second World War. Again, what were demanded were complementary measures to safeguard the capacity of national trade unions to act and to campaign for reforms. The chapter on the World Confederation of Labour underlines the fact that the ITF was far from being principally responsible for the failure to integrate the ITSs into the WCL. The ITF was representative of a tendency that existed among all the ITSs. Their organizational concepts were incompatible with the WCL; no interface could be found. Astonishingly enough, given its record in supporting anti-Nazi resistance from 1933 to 1945, the ITF had almost no influence on developments in postwar Germany. The allies were not at all interested in working with the ITF, and its member organizations, particularly the British, showed little enthusiasm either. The predominant influence of the British organizations was broken in 1947, when the US railway trade union joined the ITF, but by then it was already too late for the ITF to have a decisive influence on political developments in postwar Germany. The Marshall Plan was one of the major challenges of international trade unionism after the war, and the ITF was the first international organization to openly cooperate with the AFL and CIO. However, its policy towards the Marshall Plan mainly served US interests. Employment on US cargo ships to Europe was assured, the US model of industrial relations was promoted, unitarian trade unions split up, and the dreams of socialist alternatives came to an end.

A logical follow-up to the rollback and containment linked to the Marshall Plan was the creation of so-called vigilance committees from the spring of 1949 onwards, mainly in Mediterranean harbours. Their mission was to collect information on communist activities and ultimately to take action. The vigilance committees, a trade union "Gladio" of the 1950s, were influenced mainly by the AFL and the FO. The ITF secretariat tried in vain to introduce a degree of rational politics, and was forced to stand by, amid growing desperation, while the activities of the vigilance committees (which included transporting weapons for the French war in Indo-China) increasingly became a burden to the ITF. Fortunately, what threatened to become uncontrollable states within a state were dissolved in the mid-1950s.

From the vigilance committee Koch-Baumgarten turns her attention to the organizational development of the ITF up to the 1990s. She rightly characterizes the ITF as an international organization with regional disparities, with strongholds in North America, Europe, and Asia. The globalization of the ITF finally created a kind of forum of very different interests no longer linked by generally shared values. There was not even a comprehensive organization (*Gesamtorganisation*) or policy, only different policies for different parts of the organization with very different actors (p. 318). A programme for the international harmonization of wages and working conditions was adopted, but never implemented. The only results in terms of setting international standards were achieved through the ILO.

European integration, and the emergence of a European Transport Workers' Federation, is seen by Koch-Baumgarten as weakening the international organization. Certainly this rather narrow perception of European integration is the "official" view in some ITSs, but it is far from correct. We have examples enough where rights acquired in the European Union could be used to strengthen global cooperation.

The last chapter of her book deals with the international Flag of Convenience Campaign, which resulted in the hitherto unparalleled international regulation of one sector, the maritime transport sector. The creation of a system of international collective bargaining was possible only due to the absence of trade unions in countries where the ships concerned were registered. As there was no system of collective bargaining at national level, there could be no conflict of interests between bargaining levels either, and it was not necessary to transfer any bargaining power from the national to the international level, (Koch-Baumgarten does not believe that such a transfer would have been possible anyway). Beyond maritime transport, the results achieved in the area of international cooperation have been rather poor. The multitude of interests represented in the ITF is increasingly a barrier to the formulation and implementation of an international trade-union policy. The prospects for democratic and effective international cooperation in the age of globalization are not, Koch-Baumgarten believes, very good.

Willy Buschak

Douglas, R.M. Feminist Freikorps. The British Voluntary Women Police, 1914–1940. Praeger, Westport (Conn.) [etc.] 1999. xv, 171 pp. Ill. $55.00; £46.50.

The headline title of R.M. Douglas's monograph suggests a book aimed at an audience that

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one would not automatically take to be academic. Moreover, Douglas's central character, Commandant Mary Allen, with her cropped hair, her monocle, and her predilection for appearing in uniform with highly polished leather riding boots, appears a grotesque stereotype. But as the eye-catching title and the odd central character also suggest, Douglas has an interesting story to tell; moreover, he tells it soberly and well.

Douglas sets out to chronicle the history of the Women Police Service (WPS), through its various manifestations, from its first appearance during World War I to its demise as the Women's Auxiliary Service (WAS) on the outbreak of World War II. One of two female police organizations established during World War I – the other was the Voluntary Women Patrols of the National Union of Women Workers – the WPS grew out of the militant suffragette movement, and particularly that group determined to establish a new moral and social purity within British life. Under the firm hand of Margaret Damer Dawson, seconded by Allen, and partly financed by wealthy benefactors, the WPS provided women police for the Ministry of Munitions's wartime factories, and for any chief constable and watch committee prepared to employ them. Dawson had hopes for a national women's police force directly responsible to the Home Office and composed of educated, essentially middle-class women. Such an organization would have required major legislative action and significant Treasury funding, since, with the exception of the Metropolitan Police of London, all police forces in England, Scotland and Wales were responsible to local police authorities; moreover, the recruits to these forces were drawn generally from the unskilled working class. Neither the legislative action nor the Treasury funding was likely in the aftermath of the War. In addition, the high-handed behaviour of some of the WPS, and especially its leaders, in pursuing courting couples, raiding brothels, and professing legal powers which they did not have, did not endear them either to the Home Office or to successive Commissioners of the Metropolitan Police. Indeed, towards the end of 1918 when the Metropolitan Police established its own, official women police patrols, it became patently clear that the Commissioner did not intend to have anything to do with Dawson, Allen, and the WPS. Dawson died suddenly in 1920, and Allen assumed command. In the following year Allen and four other members of the WPS were prosecuted, convicted, and fined ten shillings each at the Westminster Magistrates' Court, for wearing uniforms that could be taken for those of the Metropolitan Police Women Patrol. Allen consequently modified the uniform and changed the organization's name to the Women's Auxiliary Service. But the prosecution provided no check to her aspirations.

After World War I, the WPS supplied officers to assist the British forces during the conflict in Ireland. Allen subsequently negotiated a WAS presence with the British Army of Occupation in the Rhineland and, in 1930, she arranged for two veterans to be sent to assist the commander of the English police in Cairo. The India Office turned down her offers to provide women officers who might help with the suppression of civil disobedience in the subcontinent. During the interwar period Allen became more and more attracted to right-wing politics. The WAS spawned its own paramilitary wing, whose members were drilled and learned the use of firearms, again prompting concern among the Commissioners of the Metropolitan Police.

Douglas has usefully trawled a variety of archives in England, Ireland, and Scotland, and he tells his story economically but vividly. It may be, as he suggests on several occasions, a problem of the lack of sources, but the book increasingly focuses on the antics of Allen, travelling the world in her uniform, visiting various congresses and national leaders – Hitler, she concluded after a brief meeting, was "an enduring friend of England" – and proclaiming herself as the representative of Britain's women police. The authorities appear to have thought that, if they ignored her, she might go away; but by ignoring her, they did not give her, or the WAS, any additional publicity. It would be interesting to know a little more about the women who served in the organization, about their motives and their conduct when on duty. Douglas has found some material that relates particularly to wartime, but he provides only tantalizing glimpses of their conduct in Ireland, even less on their activities in occupied Germany, and nothing at all on the two veterans who went to Cairo. It is, perhaps, understandable why women joined the organization during World War I. But what did they think they were joining after the war? Especially when it was becoming clear that the WAS was not going to be an official police institution, and when its Commandant persisted in making such an odd exhibition of herself? Douglas makes some telling points about the way in which elements of militant, early twentieth-century British feminism could feed into the British variant of fascism, but the voices and experience of the rank-and-file are missing.

A splendid, and hard-hitting, left-wing journal article is reproduced (sadly undated and with the journal unnamed) among the book's fine illustrations: "Come off it 'Commandant'. What are you 'Commandant' of? And who appointed you? Did you appoint yourself? [...] Why do people take you so seriously?" Having finished Douglas's fascinating book, some readers might share the journalist's professed bewilderment.

Clive Emsley

LAYBOURN, KEITH and DYLAN MURPHY. Under the Red Flag. A History of Communism in Britain, c. 1849–1991. Sutton Publishing, Stroud 1999. xix, 233 pp. Ill. £25.00; $34.95.

Writing communist history used to be the province of frustrated people – including polemicists indifferent to the documentary and other sources, academics constrained by their inability to get access to such sources, and the occasional Party hack intent on avoiding all reference to the sources. Today archives are open and former members are prepared to speak more honestly than ever before – when they are not actually writing their memoirs. None of these developments guarantee that a worthwhile communist history will be written. As the present volume illustrates, it is still possible to know too little of one's subject to say anything of interest about it. Archives can be used for largely cosmetic purposes, as would seem to be the case here, though it is perfectly possible that the authors improved their own knowledge of communist history in the course of using them. There is certainly evidence that they began this project in a state of political innocence. What else are we to make of their "discovery" that the Comintern "dominated" the leadership of the British Communist Party?

A greater familiarity with the secondary literature would have helped Laybourn and Murphy to make much better use of the documents they consulted. But time seems to have militated against this course of action. The numerous errors in this text – some so comical that they relieve the dull, repetitive, and consistently shallow narrative – testify to haste as well as ignorance. Many communists are carelessly renamed in the process or relocated to universities and places that they had no significant connection with. Thus R.P. Dutt was educated at Cambridge, according to the authors, rather than Oxford; he and his wife lived in Amsterdam, rather than Brussels. There are errors of this sort scattered throughout the text, together with others attributable to poor proofreading and general sloppiness. It is true, however, that none of these mistakes seriously detract from the book's central thesis.

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This is the notion that the Party's fatal weakness was its "close association" with the Soviet Union. It is not an original idea, though it might be claimed that it is rendered singularly sterile in this particular application.

No party has prospered in Britain to the left of the Labour Party, and it might well be argued that of all the attempts to sustain such a party the CPGB was the most successful – by far. Would this have been possible without Soviet support? Would the communists have made a bigger impact if Soviet support for the CPGB had not entailed Stalinist domination? It is futile to speculate. But at no point does it occur to the authors that the West European communist parties (to look no further) shared the debility to which they attach so much importance in the case of the CPGB. This did not prevent them from becoming major actors in the politics of numerous countries – Italy, France, Finland, Portugal, Spain, Greece, Czechoslovakia, and Weimar Germany immediately spring to mind. But there are many other cases – such as Sweden – where the communists made a bigger impact in the parliamentary mainstream than they did in Britain. This is admittedly only one of the ways the significance of a party can be measured, but it is one which the authors attach importance to. It might have helped them to identify some of the relevant peculiarities of British political culture, and some of the powers of attraction of the communists, had they noticed the multinational nature of the phenomenon they are concerned with. Unfortunately the "close association" thesis is an *idée fixe* and everything is bent to its service. The consequence is that in this blinkered approach there is no very close probing of communist history, or of British history, or of the interesting places where the two meet.

An innocent reader would never know from this account, for example, that for several decades the Soviet Union was held in high esteem by many socialist activists who had no connection with the Communist Party. It was not just the drama of the Revolution or the fight against fascism which captivated this noncommunist milieu. The USSR was also thought to be laying the foundations for socialism, if not already benefiting from them. Throughout the 1950s, socialists of all persuasions believed in the strength and dynamism of the Soviet economy. Nye Bevan was merely the most articulate of the Labour Left in his conviction that such an economy must lay the basis in the medium term for a Russian socialist democracy. Here was a reason to be hopeful about the USSR, both before and after 1956. Richard Crossman, Ian Mikardo, and other Bevanite MPs expressed the same judgement. *Tribune*, and many of its readers, took a similar view. The communists do not seem nearly as marginal when one considers for how long belief in the efficacy of Soviet central planning captivated the world after 1929. In many ways, more than we have space to recount here, the communists shared the same intellectual and political milieu as Labour and trade-union activists. Furthermore, in the first two decades of the Communist Party's existence – when its membership was at its smallest – it was far from obvious that the road taken by the Labour Party was more likely to achieve "socialism". The authors seem to forget that the first half of the twentieth century saw two world wars, postwar economic dislocation, a world economic crisis in the 1930s, and the rise of fascism. There was no social democratic model of governance in these years, and no record of social democratic achievement.

Despite all of this, the Communist Party failed to recruit, and it is of interest to know why, especially in the light of what has just been said. The reasons for this may be far more complex than Laybourn and Murphy allow. How popular was socialism of any description in Britain? It is not a question that the authors ask, but it might be relevant to their account of the CP's failure to "penetrate the mainstream". After all, this failure was already evident before the destructive zigzags of Comintern policy had done their work. The Labour Party itself was unable to win over millions of working class voters in the 1920s and 1930s and it was the Conservative Party which established its hegemony after the introduction of universal suffrage. The conservatives were able to command up to fifty-five per cent of the Left's "natural" constituency, despite mass unemployment, poor housing, and widespread poverty. Was the Labour Party also hampered by its close association with Moscow? Was the ILP after 1932? Or were there other factors at work in explaining their failures, which might also be relevant in explaining the poor showing of the communists (or for that matter the British Union of Fascists)? Laybourn and Murphy lack the curiosity and historical imagination to find out.

One unfortunate consequence of this is that they do not concern themselves with aspects of communist history – however central to the Party's life they might be – unless it suits their narrow purpose. Thus the Party's colonial work is excluded, as is the peace campaigning which ran from the late 1940s to the 1980s in one guise or another. There is nothing about the organization's intellectual life, or of its interventions in the many fields of culture where it occasionally made an impact. The authors have not troubled to make use of the many published memoirs, obituaries, and autobiographies which shed light on these questions and related issues, such as the mentality of the membership and the ethos of the organization. The result is a one-dimensional account which adds nothing to the existing literature on the British Communist Party and tells us little about the history of British socialism and trade unionism.

John Callaghan

Kimeldorf, Howard. Battling for American Labor. Wobblies, Craft Workers, and the Making of the Union Movement. University of California Press, Berkely [etc.] 1999. x, 244 pp. \$45.00. (Paper: \$17.95.)

The starting point of Howard Kimeldorf's stimulating book is an enduring question about American exceptionalism. How can we reconcile the political quiescence of the American working class with its record of industrial struggle? Kimeldorf's answer is that no reconciling is required. Yes, American workers did not go in for European-style labor politics. But that not does make them conservative. They preferred to fight at the point of production, and what characterizes the labor movement, American Federation of Labor (AFL), no less than Industrial Workers of the World (IWW), is this syndicalist bent. Here is Kimeldorf in a nutshell: "Washing across the industrial landscape of early twentieth-century America, syndicalism represented a fluid mix of the institutional brawn of pure and simple trade unionism with the mobilizing muscle of contemporary working-class insurgency to produce a kind of 'syndicalism pure and simple' – defined by its point-of-production focus, aggressive job control, and militant direct action." (p. 15) Under this definition, Kimeldorf posits two forms – the business syndicalism of the AFL crafts and the industrial syndicalism of the IWW. The historical thrust of Kimeldorf's book is that business syndicalism gave way to industrial syndicalism, not of course by the triumph of the IWW, but by the absorption of Wobbly practice into mainstream unionism in time to catch the wave of insurgency of the 1930s. Others have noted American labor's syndicalist bent – Kimeldorf cites the suggestive work of Melvyn Dubofsky and David Montgomery – but no-one has developed so sustained a syndicalist thesis or applied it so fearlessly to the question of American exceptionalism.

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The empirical grounding for Kimeldorf's thesis are case studies of two IWW-inspired unions, one of Philadelphia longshoremen, the second, of New York culinary workers, both triggered by fierce strikes in 1913. In the case of the culinary workers, the IWW tie itself quickly dissolved, but the independent union that emerged remained every bit as committed to industrial syndicalism as did the Wobbly longshoremen. What most interests Kimeldorf is that both developed viable, ongoing organizations based on direct action, mass mobilization, and labor solidarity. These tactics worked, Kimeldorf argues, because of the vulnerability of wharves and hotels to disruption. Why be tied down by a contract when more was to be gained by hitting the dining rooms on New Year's Eve or the docks when the ships were stacked up?

In the process of exploring these struggles, Kimeldorf uncovers rich collateral veins. Historians of gender will find much of interest in Kimeldorf's account of the New York culinary union. Appeals to their "manhood" always figured prominently when downtrodden restaurant workers took on employers, but the result was a union that barred women from the culinary trades. Only when they forced their way in during the World-War-I years, often as strikebreakers, did the union open its doors to women and finally embrace Wobbly inclusivity. In his longshore history, Kimeldorf offers a provocative instance of the confounding crosscurrents of working-class race relations that Eric Arnesen, Bruce Nelson and others have been exploring. Because the racist AFL longshore union never got there, the Philadelphia waterfront remained wide open to black workers, whose allegiance the IWW union courted and won. No union was a more robust practitioner of interracial unity. In the aftermath of World War I, however, the rival AFL union countered by establishing an all-black local, seeking to exploit the explosive racial tensions that swept Philadelphia (and the entire country). It was a shrewd move, attracting many embittered black longshoremen and significantly weakening the IWW union. By 1926, it was out of business, and within a few years so was the New York culinary union. But both lived on in the AFL unions that succeeded them, Kimeldorf suggests, by virtue of leaders still in place and industrial syndicalism still embraced.

It might seem churlish, in view of this rich and supple history, to suggest that Kimeldorf has given us too much of a good thing. He is employing a particular variant of comparative analysis – Theda Skocpol's parallel demonstration – "in which the objective is to build a generalizable argument by demonstrating its 'common applicability' to a wide range of contexts" (p. 7). But Kimeldorf offers only two cases, and while his brief methodological discussion stresses their differences, his substantive analysis – as already noted – actually turns on a key similarity, namely, that longshoring and the culinary trades were vulnerable to direct-action tactics. In any event, a larger pool of case studies, abbreviated though they might be, would surely have served Kimeldorf's purposes better than his two fully realized historical accounts. Kimeldorf is, one might say, a sociologist who has been seduced by the charms of history.

The question is, can the trajectory toward industrial syndicalism that he identifies be replicated in transportation, or coal mining, or, most particularly, in the mass-production sector, which was just coming into its own in Kimeldorf's period, and which would constitute labor's biggest challenge for the next quarter century? In passing references, and in a summary chapter on the 1930s, Kimeldorf says Yes. He can make that claim only by conflating two distinct phases of industrial syndicalism. Organizing struggles most certainly did involve direct action and mass mobilization. But, past that point, the issue becomes much cloudier. In mass-production industries, there is little to suggest that organizing struggles aimed at anything other than union recognition and a contract.

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Kimeldorf's longshoremen and culinary workers rejected contracts altogether, but he mentions instances in which militant unions signed contracts and still embraced direct action. Once the contract defined the right to strike, however, industrial syndicalism in Kimeldorf's meaning had dim prospects. The outcome was likely to be – to take the best known case – what the miners experienced, retaining as they did the right to strike over grievances, but only after exhausting an elaborate grievance procedure. As for the CIO industrial unions, they rarely sought that right anyway, preferring instead (as soon as they could get it) final-step arbitration. As I have argued elsewhere,[1] workplace contractualism took hold virtually at the outset and slowly but surely strangled the syndicalist impulse that had set it in motion. In his account of the 1930s, moreover, Kimeldorf is silent about the impact of New Deal collective-bargaining legislation. Once the representation election became routinized, an environment emerged that was deeply hostile to rank-and-file mobilization in organizing struggles. Only today, in its distress, is the labor movement finally moving to extricate itself from the representation election and reclaim the syndicalist energy that had once driven organizing campaigns.

Kimeldorf takes pains to distinguish his syndicalist stance from the "economism" of Selig Perlman's *A Theory of the Labor Movement* (1928). The lynchpin of Perlman's exceptionalism is that American workers were peculiarly resistant to the blandishments of the intellectuals and hence free to build a movement in their own image. Kimeldorf takes the ideologues less seriously, but not with any less consequence for his argument. It was a matter of indifference to the rank-and-file what the IWW leadership theorized. Workers embraced syndicalism because it worked for them. But if, as Kimeldorf says, the appeal was "situational", nothing in his argument precludes an opposite course, which was in fact what the modern industrial environment mostly dictated.

In his case studies, Kimeldorf has demonstrated empirically that industrial syndicalism existed, not, however, as a manifestation of an abiding trade-union radicalism – American syndicalism pure and simple – but more modestly as a subset of Perlman's pure-and-simple unionism.

David Brody

Purnell, Jennie. Popular Movements and State Formation in Revolutionary Mexico. The *Agraristas* and *Cristeros* of Michoacán. Duke University Press, Durham [etc.] 1999. x, 271 pp. £34.00. (Paper: £11.95.)

One of the principal debates among scholars of Mexican history and politics has been over the extent to which peasant grievances and mobilization were decisive in shaping events between 1910 and 1940. Within this debate, Jenny Purnell's study of peasant mobilization in revolutionary Mexico fits into the analysis advanced by the most recent scholarship on Mexico, which, by placing emphasis on regional history, posits that there were "many revolutions" as well as a good number of counterrevolutions during the period in question. Accordingly, Purnell inserts her work between traditional and revisionist interpretations of the Mexican revolution. Traditionalists have depicted the revolution as a massive and undifferentiated popular rebellion in which the political and economic power of the landed

1. David Brody, "Workplace Contractualism in Comparative Perspective", in Nelson Lichtenstein and Howell Harris (eds), *Industrial Democracy in America: The Ambiguous Promise* (New York, 1993), pp. 176–205.

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class was destroyed, ultimately benefiting the peasantry. More recent interpretations, however, counter that the revolution was little more than an intra-elite power struggle, in which the popular values inherent in the *Zapatista* peasant movement were thoroughly defeated.

But both camps of interpretations treat the *cristero* rebellion of the 1920s as an instance of church–state conflict, thereby dismissing its popular character altogether. Instead, they attribute popular support for the Church to fanaticism and false consciousness, resulting from the long-term ideological hegemony exercised by the Church over the Mexican peasantry. Without question, for centuries the "official" Mexican Catholic Church kept the peasantry in a state of passivity and poverty, in the name of God and on behalf of its landlord allies and benefactors. And, that during the era of revolutionary anticlericalism and agrarian reform in which state-building elites attacked the Church, it responded by mobilizing the peasants most firmly under its ideological domination.

Purnell's study, however, goes far beyond this superficial treatment of probably Mexico's most poorly understood social movement of the twentieth century. Purnell's work is valuable to Mexicanists because she successfully demonstrates that the *cristero* rebellion entailed much more than the defense of the institutional prerogatives of the Catholic Church. On one level the rebellion was part of a broader set of struggles between peasants and revolutionary elites over the determination of the contours of the new state. These involved competing conceptions of property rights, popular culture, religious practice, and local political authority. By employing the use of rich correspondence between state officials and peasants written during the nineteenth and twentieth centuries, Purnell demonstrates how, as Catholics and *agraristas*, different peasant communities contested and shaped the process of revolutionary state formation in Michoacán (center-west) Mexico. In so doing, Purnell's study also addresses broader questions: Was the Mexican revolution indeed a popular revolution? What impact did popular movements and cultures have on revolutionary state formation? By examining the movement of the *cristeros* and *agraristas* of Michoacán, Purnell reveals much about the origins and construction of the political identities mobilized in rural collective action and their application to the larger issues surrounding the debate over the Mexican revolution.

In her analysis of rural collective action, Purnell stresses the centrality of legacies of agrarian and political conflict at the local level within a historical context. For example, revolutionary anticlericalism and agrarianism (with which state officials of the 1920s attempted to build a base of popular support, as well as lay Catholic organizations in the center-west, which provided ideology and resources to antistate peasants) can only be understood with reference to the local agrarian and political conflicts in which peasants had been embroiled in the decades, and sometimes centuries, prior to the revolution.

Purnell's analysis of key episodes in Mexican history prior to the *cristero* rebellion indicates that provincial elites in Michoacán, much like the rest of Mexico, developed over an extended period of time into semiautonomous political groups. Repeatedly, throughout the nineteenth century, they resisted attempts by the authorities of Mexico City to impose centralized power. Increasingly intrusive national governments frequently displaced local elites from political power, while depriving the peasants of what they regarded as their land. The author's overview of nineteenth-century liberalism with respect to property rights, anticlericalism, and state formation validates this point. Liberal land reform laws that privatized both Church property and Indian community lands were the source of much of the agrarian conflict in Michoacán prior to the revolution. The policies of the Porfiriato (1876–1911) accelerated these developments and generated three basic patterns

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of agrarian transformation: the consolidation of a system of great estates; the general disintegration of communal property; and the proliferation of peasant smallholding communities (*rancheros*), which occurred at the expense of the Indian community and the great estates. In the course of describing this process, Purnell identifies two distinctly different ideological currents within nineteenth-century liberalism. She argues that this dichotomy extended into the twentieth century and shaped the struggles over agrarianism and anticlericalism in revolutionary Mexico. Both popular and elite revolutionaries emphasized a strong state to generate economic prosperity, citizenship and a just political order. Catholics chose to draw upon the liberal current, which favored a non-intrusive state and individual liberties, both of which would prohibit regulation of property rights, and the mandatory secularization of society. Thus, in the final analysis, the *cristero* rebellion of the 1920s reflected ongoing conflicts over the meaning of the past, as they pertained to the future of Mexican culture, social relations, and the political configuration of the state.

It is in this context where Purnell makes a strong case for Michoacán becoming one of the "laboratories of the Revolution". Looking at the activities of the state's 1920s governors, Francisco Múgica and Lázaro Cárdenas, the author examines how these revolutionary leaders implemented a series of agrarian, educational, labor, and anticlerical reforms, all of which were aimed at transforming Mexican society from the top down. Most importantly, Purnell analyzes how resistance to those measures, namely the violent and radical *cristiada* of 1926 to 1929, forced state-building elites to reshape those policies, which were intended to establish new and stronger links between centralized authority and rural Mexico. Accordingly, the conflict ultimately produced compromises on the regulation of property rights, cultural forms, and the power of local political authority.

But the heart of Purnell's contribution to our understanding of the *cristero* rebellion lies in her analysis as to why the rebellion was limited to Mexico's center-west. She argues that issues of class, ethnicity, and the Church's strength provide only a partial explanation as to why popular rebellion sustained itself only in the center-west region. In this reader's view, Purnell's answer is found in the predominantly *cristero* highlands of northwestern Michoacán, an area with the largest concentration of *rancheros*. These modest peasant farmers differed from their rural counterparts of central and southern Mexico in their more Hispanic origins and culture, and, most importantly, in their reliance on private property. Their social life revolved around the Church and family ties with sharecroppers and day-laborers.

During the 1920s *rancheros* became victims of limited and politicized land redistribution. Because *rancheros*, sharecroppers, and seasonal laborers often were members of the same families facing economic difficulties, a Mexico-City regime claiming itself to be revolutionary and threatening the landed and religious foundations of communities, emerged as oppressor rather than liberator, and became a target of rebellion and insurrection. The regime's attacks on the social and moral authority of the Church served to galvanize the people of northwestern Michoacán into action against the "revolutionary" state.

Purnell's work is important to scholars of Mexican history and rural social conflict because it welds the economic and noneconomic factors to explain the formation, substance, and mobilization of political identities in understanding peasant collective action. Moreover, by comparing the *cristero* rebellion and other popular antistate movements of the revolutionary period, the author demonstrates how resistance, rebellion, and insurrection result, not just from short-term grievances and the ability to marshal

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immediate strategic variables such as allies and resources, but also how they interface with historical memory rooted in long-term legacies of conflict.

Norman Caulfield

AHUJA, RAVI. Die Erzeugung kolonialer Staatlichkeit und das Problem der Arbeit. Eine Studie zur Sozialgeschichte der Stadt Madras und ihres Hinterlandes zwischen 1750 und 1800. [Beiträge zur Südostasienforschung, Band 183.] Franz Steiner Verlag, Stuttgart 1999. x, 389 pp. DM 130.00; S.fr. 130.00; S 949.00.

This study is the doctoral thesis that Ahuja submitted to the University of Heidelberg in Germany in 1997. It addresses the question of colonial state formation from the point of view of the city of Madras, as present-day Chennai was known during the period studied. The dilemma the author takes as his point of departure is not one of conquest or commodity trade, but of manpower and employment. This proves a fruitful approach. Ahuja wonders how a few thousand Europeans managed to establish the English East India Company (EIC) as the dominant power in the Indian subcontinent, i.e. over the approximately 180 million people that could be called Indians at that time. It is, he argues, first and foremost a question of labour. He does not seem very keen, however, on the concept of a labour market, which perhaps he regards as too modern and undifferentiated a term. Instead, his study looks for a variety of forms of social organization that pre-existed the colonial state, informed and transformed it during decades of Indian–British interaction, and adapted themselves in the process to the new tasks required of them.

Ahuja assumes, with good reason, that centuries of commercialization, largely the result of increasing global trade, had exercised a dynamic impact on labour relations in the subcontinent long before 1750. Thanks to the nature of the historical sources available, one can trace the particular impact of these dynamics on, for instance, relations in the military labour market and on military matters in general. This is also the case with various categories of transport workers and construction workers. In other fields much information has been lost. The tools the East India Company used to impose its will on India (including peasant recruits, but also army bullocks – crucial to military logistics – a monetized economy, and a sophisticated system of espionage) were all available to any entrepreneur, whether Indian or otherwise, who was willing to adopt them. Ahuja endeavours to do justice not so much to groups of people neglected in earlier studies, like the peasant-soldier or weaver-soldier, but to trace all forms of labour relations involved in the rise of the colonial state. Yet, he states, the period he has studied was one of intense militarization, not only in the Madras area but also in practically all of India, and the demand for military (but also civilian) labour was much greater than in earlier centuries. I am not entirely convinced that the military dynamics Ahuja considers a new phenomenon were not actually a much older one. It is no doubt true, however, that regional powers like the EIC were compelled to employ large numbers of men, even unproductive men, to maintain their position in the highly competitive politics of the time. A large share of the county's resources was sent – or diverted – to military channels. The demand for labour was often intense, and agricultural production suffered as a result.

Ahuja's study is both wide-ranging and meticulous, and the thorough manner in which he has made use of the London and Chennai archives lends authority to his conclusions.

The problems of the recruitment, discipline, and payment of labour occupy a prominent place in these records. Before the middle of the eighteenth century, textile workers demanded most of the EIC's attention. Subsequently, a much larger number of coolies, sepoys, boatmen, etc. had to be engaged and managed. By 1790, the administration turned to agriculture and to the peasant as the source of labour on which the state primarily depended. However, the military-administrative colonial state came into being in the period 1750 to 1790, and can only be understood by taking into account the patterns that emerged when Indians and Europeans worked together in finding out where their labour institutions and cultures were compatible. Ahuja finds that these institutions were compatible to a large extent, but at the same time emphasizes the violence that was a feature of this process of reshaping social relations in Madras and its hinterland. In itself, the participation of the EIC in these labour relations, military and otherwise, did not represent a fundamentally new departure from the patterns of social organization prevailing in India during the second half of the eighteenth century.

The forms of social organization that were at the basis of colonial state formation cannot, the author argues, be described in terms of the simple opposition of "free" urban to "unfree" rural labour. Already during the long-term process of commercialization that preceded the period of militarization – the nature and especially the origin of both, I believe, remain somewhat hypothetical; but that, of course, falls outside the scope of this study – collective ties between rural labourers and *mirasidar* communities had gradually loosened, without completely dissolving. These relations were characterized by ambivalence and flexibility, negotiation, and the attempt to keep open more than one employment option, which have always been features of Indian peasant survival strategies. Ahuja suggests that these rural labour relations require further research, and important research it would be too.

On the other hand, intensive warfare and the unbearable pressure on the countryside led to a crisis in late eighteenth-century Madras, culminating in the demographic catastrophe of the famine of the early 1780s. This, together with the novelty of a monopoly of arms such as the EIC succeeded in imposing on Madras and its hinterland, resulted in a clear break with the past. Typical for this period of crisis were paradoxical phenomena like the coincidence of low prices on the market for slaves and high prices for hired labour. After the crisis, a long period of peasantization confirmed dominant agricultural groups in their position of control, while the demand for labour and peasant mobility decreased. Ahuja does not for a moment deny that this meant radical change. The colonial state, however, continued to be informed by the knowledge it had imbibed of the forms of Indian labour organization. That state, in other words, did not come into its own in about 1830, when its agrarian fiscal procedures were canonized. For some of its most typical characteristics one must look to the second half of the eighteenth century. Ahuja has convincingly shown this, and one must therefore hope that his book will be translated into English. It certainly has something of considerable relevance to contribute to our rethinking the origins of the colonial state and, therefore, of the modern state in India.

D.H.A. Kolff

Sen, Samita. Women and Labour in Late Colonial India. The Bengal Jute Industry. [Cambridge Studies in Indian History and Society, vol. 3.] Cambridge University Press, Cambridge [etc.] 1999. xx, 265 pp. £35.00; $59.95.

For most of the period between 1890 and 1940, the percentage of female workers in the Bengal jute industry was between twelve and seventeen. In the postcolonial period, the corresponding figure declined to an insignificant two per cent. This drastic reduction is not a consequence of decolonization but an outcome of multiple and increasing marginalization: within factories, families, the political domain, and even labour unionism. Deconstructing an image of a united jute workers' class on the one hand and a united front of employers and the colonial state on the other, Samita Sen focuses on how the constant devaluation of women's work dovetailed with hierarchical arrangements on the shop floor. She relates this trend to the deployment of a family ideology of motherhood, sacramental marriage, and chastity. These had become prominent in the antiimperialist discourse of the high caste elite of late-colonial India and placed women workers more or less outside the state. With the help of an impressive range of government reports, newspapers, and interviews with some of the women involved in the strikes and workers' resistance of the 1930s, Sen has been able to reconstruct how the female workforce of Calcutta's jute mills became progressively marginalized.

Until the ascendancy of Gandhian nationalism as a mass movement around 1920, the *bhadralok* (the Bengal urban elite consisting of those castes that abstain from manual work) were the leading ideologues of nationalism. Though their anti-imperialism was politically radical, it was socially conservative. The *bhadralok* feared the migrants of lower castes and the Muslim communities of Bengal who were flocking to the city. The general impoverishment of Bengal caused a continuous devaluation of women's labour, which was reflected in a shift from the practice of bride price – to be paid to the bride's father to compensate for the loss of her labour – to dowry. This in turn caused additional hardship for widows – a quarter of Bengal's female population – regardless of whether or not caste customs barred them from remarriage. Whereas the *bhadralok*'s elitist perceptions of marriage became practically official, the working classes could hardly be expected to meet the requirements of what were considered to be decent marriages. Particularly in industrial Calcutta, which had an overwhelmingly male population, "unofficial" or temporary marriages, promiscuity, and prostitution were difficult to distinguish from one another. That explains why Bengal's social elite and government went much farther than considering poverty as a source of prostitution, but were inclined to cluster poverty, gender and immorality.

The nationalist noncooperation and mass mobilization of 1920–1923 deeply affected the political scene in Bengal, where both Hindu and Muslim leaders tried to involve the working "masses", and made their lot the subject of propaganda targeting European industrialists and colonial rule. Yet anti-imperialism and labour unionism were uneasy partners. This incompatibility worked both ways. In Bombay, communist labour unionism was strong, militant and pitted against employers, who were, ironically, mostly of local descent. In Bengal, where politics were riven by communalism from the 1920s to the province's tragic partition in 1947, the provincial government enfranchized the lower classes in an attempt to diminish the influence of the *bhadralok*. Though this contributed to the increasing effectiveness of Bengali labour activism, unionism hardly reached out to women. Meetings and classes were held at times when women had to care for their

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children, and gender solidarity on the work floor was seriously hindered by the fact that women were dispersed over various parts of the mill. In the 1930s, some matriarchal leadership was provided by *bhahadramahila* unionists – the female kin of the *bhadralok* – but their involvement was insufficient to empower women. Sen venomously remarks that the women's movement of India, and Bengal in particular, was simply too middle-class to become seriously involved in working-class issues. The image of motherhood that had become an icon of the anti-imperialist struggle was, in fact, a serious obstacle to the women's movement developing into a mass organization. Sen's verdict on the Indian nationalist ideology may nonetheless be too harsh, if we bear in mind that other women's movements in colonial and postcolonial societies that were less heavily inscribed by metaphors of motherhood carried the same elitist stamp.

An important factor in the marginalization of the female workforce in the Calcutta jute industry was its casual character. In contrast to other industries or mines, the Calcutta jute factories resisted stabilization of their female labour force. In fact, a demarcation line was drawn between skilled work, the weavers, and the other tasks that became more equivalent to coolie work. In this respect the employers were aided by the weavers, who were anxious to bar women from their ranks, since the entry of women would have been detrimental to their salary. Though unsuccessful in keeping workers from other castes or religions out, they effectively blocked the entrance of women. Attempts by the Bengal authorities to improve the conditions of women workers, through such entitlements as "maternity benefit", for example, militated against employers' efforts to keep the workforce flexible. Eventually, the employers had to give in as a result of pressure to accept ILO conventions, but in unison with the labour unions they pointed to the Maternity Benefit Act as the prime cause of the decline in the size of the female labour force. In fact, in Bengal this law only came into operation in 1940. By then, the labour-force participation of women had already been steadily decreasing. In the 1930s, retrenchment and mechanization brought about a declining number of women in the jute mills.

The book is densely written and offers a wealth of information. Sen carefully avoids bifurcations or reductions to either communalist, class, or gender categories. This accords with present insights that reject monolithic concepts of class and fixed gender categories, and perceive genderization on the work floor, at home, and in society at large as mutually dependent. Sen successfully integrates these refined theoretical positions in a comprehensively documented narrative. But, by doing so, she has burdened her book with digressions into many dimensions of labour and gender in late-colonial Bengal. The composition of the book is also constrained by the centrality of the question of how social constructions of gender shaped the lives of women workers. In this, she is taking an approach that is currently considered legitimate by many historians in Anglophone academia. However, one might object that their social constructions logically do not shape human lives, at least not unmediated. If the book had been organized around the diverse historical agents instead of intellectual constructions, it would have read more smoothly, and its main argument, that the advance of modernity and the general impoverishment of Bengal effected a progressive marginalization of women workers, might have been elaborated more clearly.

However, the author deserves all credit for not having fallen into the trap of erasing tensions and victimizing the female workers in the jute industry. Her book offers many instances of critical questioning. Sen shows, for example, that protective legislature did not decrease the number of women employed by the jute mills, for the simple reason that employers successfully lobbied for exemptions and evaded what remained of labour

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legislation. Another example is female antagonism to trade unionism, which can easily be explained by the fact that labour activism and strikes are at odds with the constant pressure on women to feed their children. But Sen transcends the obvious by focusing on strikebreaking behaviour and by analysing anecdotal stories about women chasing union leaders with brooms. Here, she highlights traces of women's solidarity and networks, of which only "the faintest whispers" survive in the official records. According to Sen, continuing with the broom metaphor, their agency has been swept under the academic carpet by middle-class historiography. Samita Sen has provided an intellectual tour de force by carefully mapping a grid of oppression from which she has uncovered women's resilience.

Ulbe Bosma

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