

## BOOK REVIEWS

BLACKBURN, ROBIN. *The Making of New World Slavery. From the Baroque to the Modern 1492–1800*. Verso, London [etc.] 1997. vi, 602 pp. Ill. Maps. £25.00.

This is the second publication in Robin Blackburn's study of the rise and fall of New World slavery, although it is first in terms of chronology, covering the years from 1492 to 1800. The previous volume, published in 1988, was entitled *The Overthrow of Colonial Slavery, 1776–1848*, and dealt with the ending of slavery in the British and French colonies, and the related changes in the other remaining slave areas of the Americas.

As did the earlier book, *The Making of New World Slavery* provides a sweeping survey of the various Western European nations and their American offshoots. In rough order of number of pages allocated, there are discussions of the English, Portuguese, French, Spanish and Dutch colonies. The book is loosely divided into two historical periods. The first, from 1492 to 1713, is focused on the struggle of the early modern state to survive, in part via the development of New World slavery. The second, from 1713 to 1800, deals with the establishment of New World slavery on a racial basis, the growth of slave systems, the role of slavery in metropolitan capital accumulation and economic growth, and the emergence of anti-slavery thought.

Blackburn has done extensive reading in most relevant secondary sources in several languages, relating to slavery and to European history, with only limited archival work. While the basic presentation is non-quantitative, there are 35 tables (almost all in the second half of the book) as well as four maps and seven illustrations. The depth of detail is indicated by the almost 1,000 footnotes, some providing references to sources, others being more substantive in presenting arguments.

Blackburn's survey covers just about all aspects of the history of slavery and its impact upon Europe, and does so in a clear and thorough manner. There are generally excellent summary examinations of key questions, and of the sweeping changes that occurred. In many regards this is a very useful guide to the current state of the study of New World slavery. There is particularly good coverage of the Iberian colonies and of the early stages of the rise of New World slavery. And, as we shall see, Blackburn presents some very interesting and original analysis on some issues, adding new variants to current debates.

Several of the key points of Blackburn's analysis fit comfortably with what might be described as the current consensus view of slavery. New World slavery is seen as a key aspect of social modernization, being a flexible and profitable form of economic organization (although in the long run it is argued to be a factor retarding economic growth). The expansion of New World slavery was tied to the growth of consumption demands within Europe, thus providing a link (of uncertain quantitative magnitude) between the Americans and Europe. Slavery had been very widespread in time and place, and few societies existed without there having been some form of enslavement of members of another society. One important difference with New World slavery was the defining of race as the basis of the enslavable outsider. It had been only several centuries before the first transatlantic sailing that the Europeans had, for whatever reason, ended the legal enslavement of other Europeans.

There is less said about the societies within Africa and the African slave trading network, the nature of the extensive Islamic slave trade from Africa, the relations between the colonies in the New World of the various European powers and their settlements in Africa and Asia, and little on the Native-American residents of North and South America at the time of European arrival.

There are three debates of particular interest to which Blackburn provides original analysis. Blackburn's discussion on the myth of Ham and the rise of racism follows upon an article published in the *William and Mary Quarterly* in 1997. The intent is to tie western religions to the expansion of modern slavery, with the argument that without this myth it is probable that New World slavery would have been more difficult; indeed, given alternatives and the coexistence of races elsewhere, all slavery might have been ended prior to New World settlement. Despite this early failure of western religions – Catholic, as well as Protestant, and Jewish – to stop the rise of slavery, the same beliefs were also true for other world religions of the times. It was these western religions that were later to become instrumental in leading the movement to end slavery in Africa and Asia, as well as in the New World.

Blackburn expands upon this counterfactual world without racial slavery in his long chapter on "Alternative to Slavery?", asking about a possible free labor America, without, however, convincingly specifying how it would have been done. He speculates as to whether New World development would have been possible without the use of slave labor. Nevertheless, however theoretically plausible those arguments might sound, there is no full statement of what this different pace of settlement would have been nor the costs to consumers of any such institutional changes. To make these arguments, moreover, Blackburn must argue that the scale of production for sugar and other slave grown crops was limited and those non-pecuniary aspects such as Smith's "love to domineer" and Phillips's "conspicuous consumption", that some believe made slavery advantageous, did not exist. There is a rather curiously undeveloped argument in Blackburn's claim that Islamic control of the African coast could have ended the availability of slaves for the transatlantic trade. While this counterfactual would have meant a limited number of New World slaves, what it would have done to the magnitude of the Islamic slave trade and slavery elsewhere is not discussed.

Also incomplete is the counterfactual regarding the basis for the absence of enslavement of Europeans by other Europeans. In criticizing the questions posed by David Eltis concerning why this transformation in European ideas had occurred, and why slavery of Europeans was not reintroduced, Blackburn assumes slavery's absence more than he explains why it happened. He goes on to argue that more expensive free labor would have done more to stimulate the English economy than did the slave economy, but little is said on what its presumed impact on the costs and output of colonial crops would have been. This section contains a number of quite provocative ideas, but a precise, acceptable, set of conclusions is not so easy to discern.

In the final, and longest, chapter of the book, Blackburn turns to the long-standing debate on the role of the slave trade and slavery in the process of British industrialization. This relation could be based on the Marxian claims of the nature of primitive accumulation as well as the more recent argument of Eric Williams on the impact, specifically, of the expansion of British New World slavery. These two arguments differ somewhat since, to Marx, the primitive accumulation influenced all the nations of Western Europe, while Williams (and Blackburn) argue that the dramatic effect on industry comes only in one nation, and that the last to be effected. Blackburn's general

claims, however, include a mixture of factual and counterfactual arguments that may leave the reader somewhat less certain about the answer than the author seems to be, although his conclusions are sufficiently hedged and qualified that the differences with others in the debate are rather less clear than seems to be argued.

As a disputant in this debate I should note that I am sometimes uncertain how the works of my coworker, Patrick O'Brien, and myself are interpreted. Part of this confusion no doubt has been due to our imprecise wording in several places. Still, to note that we now accept a stimulant role for slavery in the Industrial Revolution, could leave a rather large range for its contribution. In particular, Blackburn at several times points to the importance of internal economic developments within Britain that permitted the slave trade and slavery to be profitable and industrialization to emerge. He thus implicitly plays down the long-standing debate on the British standard of living with its argument for the effects of capitalist exploitation. The impact of the "drain" from India is briefly discussed, but considered of lesser importance for the eighteenth-century British growth than was New World slavery.

As is familiar to followers of this debate there are many assumptions and assertions about which disagreements persist and where still there seems no firmly agreed upon empirical evidence. The growth of exports from the cotton textile industries, for example, does not really occur until the 1790s, while a significant displacement of the French sugar trade awaited the Haitian Revolution. The usual uncertainties about the possible existence of unemployed resources in Britain remain, as do the questions of what adjustments might have been possible in the absence of slavery. More puzzling is Blackburn's claim that private profitability is a more important measure than social (or national) profitability, which means that the costs of empire, including defense, and the price of protection of British colonial goods from cheaper sources of supply, are excluded from consideration. Yet with all the interesting calculations, the gains imputed to slavery and the slave trade remain a relatively small number compared to national income. We are back to the issue of whether small causes can have very large effects (and, presumably, larger causes therefore have small effects). Thus while the analysis that Blackburn presents is quite interesting and makes some very useful points, the ongoing debate about empirical magnitudes still remains open.

In a thoughtful epilogue Blackburn points to links between slavery and the development of modernization in regard to changes in economic rationality, industrial discipline, and alternative means of motivating labor. And he points out that by the end of the eighteenth century Enlightenment ideas, plus the growth of industrialization, were apparently becoming incompatible with slavery, although the changes were not to be immediate. For that story, therefore, it is necessary to turn to his *The Overthrow of Colonial Slavery*.

Stanley L. Engerman

Social Security Mutualism. The Comparative History of Mutual Benefit Societies. Ed. by Marcel van der Linden, in collab. with Michel Dreyfus, Bernard Gibaud and Jan Lucassen. [International and Comparative Social History, 2.] Peter Lang, Bern [etc.] 1996. 707 pp. S.fr. 98.00; DM 123.00; S 817.00; \$78.95; £52.00; F.fr. 392.00.

Marcel van der Linden and his associates have succeeded in assembling a monumental compilation on mutual benefit societies, a little-studied but quite important aspect of social history. Twenty-six essays focus on national case studies; while Europe is covered in the most detail, there are important essays from South America and Asia. Two essays and the introduction complete the collection; one of these essays examines efforts of mutual benefit societies to construct international organizations in the late nineteenth and first half of the twentieth centuries, and the other two, the introduction by Marcel van der Linden and a concluding essay by Michel Dreyfus, place mutual benefit societies in the larger context of labor history. The quality of the individual essays is universally high; all are workmanlike and competent and some are little gems; I have in mind here particularly the outstanding essays by Bryan D. Palmer on Canada, Erik Olssen on New Zealand, Luigi Tomassini on Italy, Jacques van Gerwen and Jan Lucassen on the Netherlands, David Ownby on China, and Sergio Grez Toso on Chile.

The single most valuable contribution of this collection is its help in identifying large-scale trends in mutualism; by identifying mainstream aspects of mutual aid development and less typical patterns, this study allows students of institutional trade unionism and popular life to better locate their studies comparatively. Van der Linden defines mutual aid societies as “associations formed voluntarily for the purpose of providing their members with financial assistance in case of need” and, although this definition is not always followed consistently, the contributors adhere closely enough to provide a rather thorough treatment of the history of these organizations. Clearly, contributors have been encouraged to provide statistical documentation on mutual aid societies and, while few contributions meet the high standards set by Tomassini and van Gerwen and Lucassen, the many tables and appendixes do provide some gross measures of the magnitude of the societies’ adherents and financial strength for most countries.

Almost all of these essays converge to reveal a similar pattern of mutualist development. Originating in the nineteenth century, early or late, mutual aid societies were part of the complex of formal and informal popular institutions including debating and lecture groups, demonstrations and petitions, cooperatives, and trade unions that coincided with the birth of popular political consciousness and action. Whether trade unions and mutual benefit societies enjoyed formal ties or not, both organizations participated in a common wave of rapid growth that occurred at roughly the same time in industrial regions of individual countries. As trade unions and socialist parties began to back compulsory social insurance, whatever relations there might have been between the two movements weakened. The growth of national welfare states undermined the appeal of mutual aid societies and helps explain a general decline that began in the 1920s and 1930s.

In some cases, mutual aid societies were able to survive by dramatically reconfiguring themselves or because of the inadequacy of particular welfare states. In France and Sweden, mutual aid societies succeeded where their British comrades failed, and became an important part of the welfare state’s administration. In Belgium, Catholic suspicion of the central state led to a strong role for mutual aid societies, although in the modern period they have more or less become assimilated by the state administration. In Australia, mutual aid societies endured for a time by creating a specialized niche for themselves; they negotiated contracts with doctors and druggists to provide cheaper services but, in the 1950s and 1960s, aggressive associations of doctors and druggists more or less undermined their ability to provide such specialized services. In Portugal mutual aid societies have survived into the present as a means of supplementing the loopholes

in the welfare state. Much the same is true in Chile where the existing welfare state is being dismantled in favor of private insurance programs, and it is possible that this will create a revival of mutual aid functions. In most cases, however, the decline of welfare states is more likely to benefit private insurance companies than popularly-controlled mutual aid societies.

The great strength and principal weakness of this collection is that every single essay is written from the perspective of labor history. This is a great strength because it gives the collection thematic unity. For example, each contributor discusses the issue, first raised by the Webbs, of whether mutual aid should be seen as the initial stage in the development of trade unions. In general, the contributors challenge the Webbs' linear interpretation. In Russia, Spain and Congress Poland, the cross-class character of mutual aid societies and the presence of clergymen, employers and government officials ruled out from the first any close association with trade unions. In other countries such as Chile, mutual aid societies flourished more vigorously than trade unions and took on an early independent political role, escaping trade unionism and the Webbs' classification system all together. In addition, little support is provided for William Sewell Jr's suggestion that early mutual aid societies inherited guild traditions and artisanal attitudes. Sewell's view has been challenged by French historians who find little relationship between the first mutual aid societies and collapsing guilds, and there is little evidence elsewhere that mutual aid societies perpetuated guild outlooks; in any case, the democratic character of Western European mutual aid societies and its contrast with typical guild practices should by itself raise doubts about persistence. This collection demonstrates conclusively that no simple connection exists between labor movements and mutual aid societies but, in the process of this demonstration, almost all of these essays provide a relatively coherent account of relationships between mutual aid societies and labor movements.

While the concerns of labor history give a necessary unity to this collection, they also limit and narrow the investigation of the rise and fall of mutual aid. For example, labor historians ignore demography. Not one of the authors in this collection – even the quite useful article by David Neve on Great Britain – discusses the evolution of life expectations over the course of the nineteenth and twentieth centuries. Yet the lengthening span of human life in the period may be as important in the collapse of mutual aid societies as the advent of national welfare states, and a grand comparative study such as this one might have helped us disentangle this relationship. Most of the contributors mention that in the nineteenth century government regulation required mutual aid societies to base their schedule of benefits on actuarial statistics; but actuarial estimates were calculated on the contemporary regime of mortality, and made no allowance for the greatly diminishing toll of mortality at all ages that affected Europe and North America in the late nineteenth and twentieth centuries. By the early twentieth century in Great Britain, even actuarially-conscious mutual aid societies found themselves in trouble and willing to contemplate social insurance that would relieve them of some of their responsibilities.

Similarly, although many of the essays discuss the exclusion of women, blacks and immigrants from mutual aid societies, the emphasis of this collection is heavily on production – the principal subject of labor history. The lack of consideration of reproduction means that proletarians are considered separately in isolation from family or household. Burial insurance, perhaps the oldest and the most common of the mutual aid benefits, was intended mainly for family members. How mutualism fitted into

family and household economies remains unclear. Did opportunities for the employment of women and children diminish the involvement of male workers in mutual benefit societies? How is the mutualist expansion connected with the character of industrial change? Did the "Second Industrial Revolution" of the late nineteenth century spur on the movement by diminishing the role of child and female labor, thus making the continuous employment of males all the more central to families?

While emphasizing the role of the national welfare state in the demise of mutual aid societies, the collection more or less ignores those institutions concerned with social reproduction that provided the local context for the development of mutualism. With all its documentation this collection casts little light on how proletarians survived accident, illness, and unemployment and old age in a pre-welfare state world. Mutual aid societies are one part of the answer to this question, but a fuller picture requires attention to religious charitable institutions, local government provision, employer paternalism and the help of rural kin. Changes in any of these factors would influence the attractiveness and desirability of mutual aid societies, yet contemporary labor history has systematically isolated factory workers and artisans from the rural world and the public and private welfare institutions that surrounded the nascent proletariat. This omission of such factors is brought out in Ranajit Das Gupta's study of India where kinship ties to the countryside served as the major mechanism for workers' relief.

Finally, the emphasis on voluntary mutualism stresses those welfare institutions in which labor had the maximum independent role. Should this be the object of "labor history"? Labor historians such as E.P. Thompson and Eric Hobsbawm have long reminded us that labor history was inextricably bound up with the history of capital. Alongside the voluntary mutual aid societies organized by workers were welfare societies dominated by employers, clergymen and government officials. The role of employers, state and church in the regulation of working-class reproduction was always contested and remains a largely untold story, ignored by labor historians of all schools. It cannot be told by simply focusing on the most autonomous element in the workers' struggle to survive in a fierce and threatening industrial world.

Marcel van der Linden's accomplishment must be applauded. The collection he has assembled casts valuable new and important light on a largely ignored aspect of the working-class world. But his sweeping searchlight only reveals whole portions of working-class life relegated to the shadows by our labor history. Let us hope that his work inspires historians to pursue the search and to launch their own exploratory expeditions.

*Michael Hanagan*

WINCH, DONALD. *Riches and Poverty. An intellectual history of political economy in Britain, 1750-1834.* [Ideas in Context.] Cambridge University Press, Cambridge [etc.] 1996. xi, 428 pp. £50.00; \$64.95. (Paper: £16.95; \$22.95.)

*Riches and Poverty* is a rich and fascinating book. It represents an enormous knowledge of the literature as well as a sophisticated treatment of an intriguing theme. Winch explores the intellectual history during 1750-1834 of Adam Smith's contribution in terms of analysis and policy to linking the fortunes of the rich and poor in commercial

societies. An essential objective of this book is to do justice to the heterogeneity of political economy both as science and art, as Winch puts it. In this sense the book differs from doctrinal histories of the development of economics during this period, by broadening the scope through moral and political considerations as a reflection of the approach taken by the key figures, Adam Smith, David Ricardo and Thomas Robert Malthus, themselves.

The first part of this book deals with Adam Smith's science of the legislator. It is concerned with the propitious legal and political circumstances that generate irreversible social change and potentially unlimited growth on the basis of freely and optimally employed capital, throwing light on the secret concatenation. The second part is devoted to a thorough comparison of the works of Adam Smith, Edmund Burke, Thomas Paine and Richard Price. The four essays in this part deal with contested affinities, the loss of regal government, Burke's creed on politics, chivalry and superstition, and with the labouring poor. The third part contains a lengthy discussion of Malthus as a political moralist. Winch points out that morals connoted something far more inclusive to Malthus than sexual mores only. In this respect he mainly followed Paley, except in his diagnosis of and practical remedies for mass poverty. Malthus entered the field of political economy as a result of his concern for the relationship between population and subsistence. However, the moral component in his thinking is intimately connected with his natural theology. According to Malthus there was no guarantee that capital accumulation would always improve the living standards of the poor. Indeed, there is in Malthus's view a distinct possibility that when economic growth is at the expense of agriculture and entails living in cities, the material gains might be purchased at too high a price in terms of national security, unhealthiness, instability, vice and misery. These considerations lay at the heart of the moral dimension which Malthus added to political economy. "When viewed from a perspective dominated by the system of natural liberty, it contained what was potentially at least an interventionist programme based on the idea that the wise legislator might have to take measures to correct an imbalance between agriculture and manufacturing" (p. 267). This view reflects Malthus's approach to looking at institutions from a normative perspective. In contrast to Ricardo, Malthus adheres to the vision that the science of political economy resembles more the field of morals and politics than that of mathematics.

Malthus's support for retaining a measure of protection for domestic agriculture during the debate on the Corn Laws in 1815 is a natural consequence of his agrarian sympathies. He argued that agriculture constitutes an exception to the general principle of free trade, advocated by many political economists who shared a commitment to Adam Smith's system of natural liberty. Here too lies one of the reasons for the fundamental disagreement between Ricardo and Malthus. Another is their divergent applications of the theory of rent to policy. Winch presents a fair account of the classic debate between Malthus and Ricardo on basic issues of economic theory in general and on the protection of agriculture in particular. While Ricardo tended to take a long-term view, Malthus was often more concerned with short-run developments, although his principle of population clearly belongs to the long-run domain. Winch concludes that while Ricardo's popularity peaked at the time of his death in 1823, Malthus's combination of political economy and natural theology became more and more popular around 1830, partly with the application of his population principle to the Poor Law

question. The contribution of Malthus to the debate on poverty led to the Poor Law Amendment Act of 1834.

In closing, let me quote in full the last two sentences of Winch's book. "Although historians are under no obligation to resolve past disputes, there does appear to be a need for a form of intellectual history that combines sympathy with enough distance to ensure that we do not simply perpetuate previous misrepresentations. That, at least, has been one of the articles of faith underlying this book." I can only repeat that Donald Winch has done a fine job in developing his own judgement, based on his extensive knowledge of and deep insights into the historical and economic developments of the time.

Arnold Heertje

PIGUET, MARIE-FRANCE. *Classe. Histoire du mot et genèse du concept des Physiocrates aux Historiens de la Restauration*. Presses universitaires de Lyon, Lyon 1996. 196 pp. F.fr. 160.00.

According to Piguet, the history of modern Western thought is dominated by a limited number of key words. These words originate at certain moments in history, and then develop through the course of time, during which the meaning of these words shifts. This history can, the author claims, be investigated at the linguistic and the social – socio-linguistic – levels, which implies analysing syntactical, syntagmatic, paradigmatic and pragmatic features of these words. The result of such an analysis is a semantic and conceptual "archaeology", along the lines of the now familiar discourse analysis from France. Piguet pays particular attention to the moments and places in which the meanings of such cardinal words shift and fracture. Such an analysis must place the changing meanings of a word in the contexts in which such a meaning and shift in meaning occurred. By "context" we mean here not concrete historical – socio-economic, political, cultural-ideological – circumstances and transformations, but the context of the texts within which the words appear. This, the author claims, enables one to investigate the "how", "where" and "why" of shifts in meaning.

In the vocabulary of our modern culture, the word "class" has become one of the central words dominating our contemporary approach to *penser le monde*. How and when did "class" take over as an indicator of social division (*division sociale*) from "order" (*ordre*), which had served to denote the political-juridical and other social divisions of the Ancien Régime? In her book Piguet demonstrates the effectiveness of this socio-linguistic method, but also its limits, at least in socio-historical terms. Before discussing this further, it will be useful to summarize briefly the principal findings of her book.

Piguet confines her study almost entirely to France. She claims that the transition from the use of the word "order" to that of "class" can be traced in the eighteenth century, and particularly in the second half of the eighteenth century. The author believes the change which saw class take on its modern, twentieth-century meaning was completed in the writings of historians of the Restoration period. In particular, "class" broke through among the Physiocrats (Quesnay and his followers), thus in the formative years of modern economics. Before reaching the Restoration period, Piguet examines revolutionary texts between 1789 and 1794, and then the work of Saint-Simon. The author also uses dictionaries as a source, but these treat words abstractly, divorced from

any context, and, furthermore, new meanings are included only after they have already been around for some time. We do, however, find contexts in the writings of economic, social and political philosophers, and also in the work of historians. The author has examined all this material to see how frequently the word class was used and how its meaning evolved.

“Order” was a signifier denoting the hierarchical social structure of the Ancien Régime, in which vertical ties were the foundation of social harmony. The concept denoted juridical-feudal order too. It was also used to hierarchically classify people with a precise social function. “Class” began to be used to distinguish between people on grounds of inequality, achievement and economic income. Unlike the more concrete “order”, “class” is a more intellectual and abstract concept that serves as a criterion with which to hierarchically classify groups of people. Piguet also presents analogies with the way in which *ordre* and class were used in biologically categorizing plants and animals in the eighteenth century.

In the writings of Quesnay class was already an accepted word, one used in classifying groups of people according to their economic activity: the *classe productive*, the farmers, the only class that is productive, because only agriculture is a source of *produit net*; the *classe propriétaire*, the landowners; and the *classe stérile*, comprising useful, sometimes even essential, people in sectors of industry and services who though not themselves generating *produit net* depended on the agrarian *produit net* for their livelihood and work. In Quesnay’s writings, we find for the first time a classification based on economic functions, Piguet argues.

Although the word class was used in the political discourse of the 1789 Revolution, for example in the Abbé Sieyès’ celebrated pamphlet *Qu’est-ce que le Tiers état?*, there was nothing really innovative in how it was used. A different case can be made for Saint-Simon’s writings. He completely severed the link between class and the traditional division of society according to the juridical-political order, even in his writings on Ancien Régime society. He contrasted *classe industrielle* with *classe féodale*. The first, the most numerous, comprised all groups engaged in useful work, and included the agricultural population. The second comprised the nobility and the clergy.

In the writings of historians of the Restoration period the meaning of class was extended to include the notion of class struggle, a sense it did not have among the Physiocrats but which it did for Saint-Simon. In doing so, class acquired its mature modern meaning, Piguet argues. The Physiocrats believed the *classe productive*, the *classe propriétaire* and the *classe stérile* needed one another.

These are the most essential of Piguet’s findings. We noted earlier that discourse analysis is a powerful tool. This is because the linguistic approach guarantees rigour and precision. It would be very useful if more such historical analyses of words and concepts were available, for example analyses of “labour” or “capital”, to mention just two other absolutely central concepts. However, the method and its application by Piguet prompt a number of questions.

First, there is the question of the book’s temporal and geographical scope. One might doubt whether a historical-linguistic analysis of the concept of class can be restricted to the eighteenth century and, as far as the nineteenth century is concerned, the period up to and including the Restoration, or whether a comparison with other countries can be avoided completely. Did the word class play no role whatsoever in the history of ideas prior to then? Might not the conceptual content, the meaning, have already been apparent, but perhaps expressed using one or more other signifiers? Piguet herself refers

to a number of seventeenth-century authors in passing. A more serious reservation is that her analysis does not extend as far as Marx: surely class and class struggle became key concepts in the work of Marx, having an extraordinary influence that continues to this day. It is odd too that Piguet does not discuss early French and British socialism and that she mentions the major early nineteenth-century economist Jean-Baptiste Say only briefly, and Frédéric Bastiat not at all, despite the fact he was a highly influential popularizing economist around 1850.

With Marx and the early British socialists we encounter the problem of geographical limits. The question of how the concept of class developed, particularly in Britain, seems crucial. An examination of the roles of Adam Smith, David Ricardo and Thomas Malthus would have been illuminating in this context, and they also had important ideas regarding differences between the social classes. Only Adam Smith is given even cursory attention, and then only indirectly through (and perhaps because of) the various successive French translations of the *Wealth of Nations*. That Adam Smith observed there to be fundamental differences between socio-economic classes is not mentioned. Is there really no scope for historical-linguistic analyses of texts on British economic thought of the pre-Adam Smith period, another period merely touched on by Piguet? What interferences were there between British and French ideas regarding the phenomenon of class?

Finally, a few remarks on discourse analysis as a tool to analyse how our way of *penser le monde* originated and developed. What might the relationship between that analysis and “real” social history be? The method apparently excludes anything except the texts being studied. Of course, the history of ideas is in itself important. Social historians of the critical period of the eighteenth and nineteenth centuries might perhaps want to relate this analysis to the real social structures and transformations of the time. Only then can one answer the “why” as well as the “how” and the “where” of certain meanings ascribed to words and of shifts in these meanings. The *penser* is an essential subject of study, but so too is the *monde* corresponding to a certain way of *penser*.

Studies of the relationship between *penser* and *monde* should confront the changes in meaning ascribed to certain words with the historical and social reality in which those changes took place. For example, the Physiocratic triptych *classe productive*, *classe propriétaire* and *classe stérile* is based on a notion by Quesnay and his followers with regard to the social circumstances in which they found themselves, which they observed, and which they also wished to replace by a system of *laissez-faire*. Another example: the ideas of these and later French economists can be compared with those from Britain, like Adam Smith, David Ricardo and Thomas Malthus. Differences between French and British ideas might be reducible to socio-historical differences between the two countries. This would enable one to pose real “why” questions, such as: Was the concept of class discovered earlier in British thought than in French thought? Are there indeed characteristic differences between British and French concepts of class? Can such variations with respect to time and content not then be reduced to differences in socio-economic structure and development between the two countries?

Lout Bots

PAKULSKI, JAN and MALCOLM WATERS. *The Death of Class*. Sage Publications, London [etc.] 1996. viii, 173 pp. £37.50. (Paper: £12.95.)

Debates about social class have dominated much of twentieth-century sociology, in both its Marxist and non-Marxist versions. There has, of course, been an important intellectual split between European and North American sociology in the sense that the European debate was concentrated around the Marxist and Weberian studies of economic versus social class, whereas in North American sociology class analysis was subordinated to the empirical study of status differences, social mobility and ethnic communities. In recent years, there has, however, been mounting dissatisfaction with the problematic of class.

There are essentially two reasons for this sense of ennui among class theorists. First, the condition of post-communism has meant that since 1989 there has been no vitality in either Marxist or neo-Marxist theories of society, and second with fundamental changes in the nature of technical production and cultural consumption, there is little confidence in the concept of class as a way of describing, let alone explaining, social structure. *The Death of Class* is concerned with both theoretical and empirical issues. It is a judicial, if not funereal, summary of the class debate.

At the outset, we should distinguish between the strong and the weak versions of class theory. The strong Marxist version had a robust theory of history and social structure, attempting to provide a general account of the historical development of human societies via the analysis of class struggle. Adjacent theories of ideology, mode of production, state, alienation and false consciousness derived their intellectual existence from the master concept of class. The weak version merely argued that social inequalities (of income, housing, health and personal consumption) were most effectively understood as class inequalities. With the demise of the influence of writers like Nicos Poulantzas and Louis Althusser in the early 1970s, the strong version of class theory was abandoned. Thus the strong version was dead before the demise of communism as a social system, and the so-called class debate in sociology was in fact a dispute about the adequacy of the weak variety of class analysis. In Britain, what we might call the Oxford School (John Goldthorpe and Gordon Marshall) of weak class theory attempted to defend the view that class remains the best concept for explaining income and occupational inequalities. We can read *The Death of Class* as the final, end-of-the-century demolition of this weak version.

Pakulski and Waters set out to terminate this tradition of sociological analysis of social structure because class is “dead” as a viable description of social systems of inequality and occupational structure, because it no longer provides an adequate account of modern society. While they argue that “class” is not viable from a theoretical point of view (for example the failures around problems of classification, boundaries and specification), their study is primarily concerned with empirical problems. “Class” is no longer particularly relevant to understanding identity and consciousness by comparison with gender, nationality and ethnicity. It no longer appears to explain twentieth-century social movements which appear to be driven more by generational and religious conflicts. It is no longer relevant to political choices where “class dealignment” has uncoupled party politics and social class. Government changes in industrial democracies appear to be increasingly unpredictable, because the preferences of floating voters cannot be easily measured in class terms. There is a universal decline in trade union membership throughout the advanced industrial societies, but the notion of “professional dominance” appears to be equally moribund. In the cultural sphere, culture has been “democratized” by technological changes in the mass media from radio, cinema, television, mobile phones and personal computers rather than fragmented around class. Whereas

in the 1950s Richard Hoggart in *The Uses of Literacy* and Raymond Williams in *Culture and Society* were inclined to lament the erosion of the authenticity of class-based cultures, postmodern theory takes cultural hybridization for granted. Finally, social struggles over resources are more likely to be determined by “status blocs” or “status columns” which form political coalitions around welfare distributions than by class.

It is important to recognize that death-of-class theorists do not naively assume that inequality and conflict have disappeared in modern societies; the argument is simply that class inequality and class conflict have disappeared. Pakulski and Waters note for example (p. 157) that, while household wealth has become more widely distributed in this century, income inequality has in recent years sharply increased. However, “class” is not sociologically relevant to a description of these phenomena. By contrast, conflicts in Bosnia, Palestine and Cambodia point to the resilience of ethnicity, religion and generation as explanatory concepts for the analysis of contemporary conflicts. Thus, *The Death of Class* is not only the definitive obituary on a whole tradition of sociological research, it also points forward to new opportunities for analysing societies which are increasingly fragmented, complex and hybridized. Optimistically, the burial of class might result in a renaissance of sociology, or at least a discipline less petrified by the ghosts of the founding fathers.

Bryan S. Turner

COFFIN, JUDITH G. *The Politics of Women’s Work. The Paris Garment Trades 1750–1915*. Princeton University Press, Princeton 1996. xiii, 289 pp. Ill. \$35.00.

At times, a book that has been long in the making ends by disappointing those who have been anticipating it; that is most emphatically not the case with Judith Coffin’s thoroughgoing revision of her doctoral dissertation. Despite the fact that three of its chapters contain previously published material, *The Politics of Women’s Work* is a truly fresh and satisfyingly complex reinterpretation of the subject of women’s work in the Parisian garment industry in the nineteenth century, and how this was represented in the economic and cultural policy debates of social scientists, reformers, labor unions and politicians.

Coffin contributes to several current scholarly debates, which she explores in her introduction. These include questions about the uneven character and impact of the Industrial Revolution and the resilience of homework, and the extent to which that industrialization separated home and work. She also discusses how best to achieve balance in historical accounts combining on the one hand, a social constructionist view which accepts that “definitions of ‘male’ and ‘female’ shape the lives of men and women” (p. 12) and, on the other hand, attention to historical specificities; in her own text, she achieves an admirable balance. Some of the socially constitutive features Coffin examines include the extent to which subjects demonstrated agency, the role of technological innovation and other empirically demonstrable factors in changing definitions, and how such discourses may vary according to the class and other social identities of the speaker. Although Coffin notes that her study is not a “uniquely French story”, her thoughtful documentation of several specifically French factors – France’s particular pattern of economic development, nineteenth-century French modes of social science

research, and the importance of French women's labor force participation (and the concomitant lesser impact of the ideal of married women in the home) – is one of the most useful features of her study.

The book starts off on a very strong note by moving back in time to the eighteenth century, asking to what extent later assumptions that sewing clothing at home has been women's task since time immemorial, and that the garment industry was a "traditional" industry are founded in fact. Here Coffin demonstrates that sewing clothing was not done in individual households in most of early modern France. In the second half of the eighteenth century, the guild system (then under attack by the monarchy and undermined by non-guild production) in Paris was concerned not so much with women's encroachment but non-guild ("clandestine", illegal) workers in general. (Many of these workers were as skilled as guild members.) Women and girls helped in the shops of their tailor husbands and fathers, and legally made clothing for women and children. There were ongoing disputes about women's role within the guild context; they were banned from the tailors' guilds, but were permitted to belong to the linen drapers – those who dealt in white goods such as lingerie, household linens, men's shirts and baby clothes. (Hence the drapers were a merchants' not producers' guild.) There was also a guild of seamstresses which was often engaged in boundary disputes with the tailors' guild. However, the conflict between productive labor and family claims on women (which so consumed the later nineteenth century) was remarkable for its absence in this period.

Coffin next turns to new machinery, that aspect of the Industrial Revolution that has caught the attention of contemporaries and historians alike. Following the lead of Maxine Berg, she traces, via the history of the sewing machine, how "women's work" became a social problem, a distinct object of social investigation" (p. 46).<sup>1</sup> The inventors of the first sewing machines and the early nineteenth-century French political economists both saw machines as a progressive force for women's labor, making their work easier and more productive at the same time. By the 1870s, both class and gender issues had become "problems" (even to political economists), as inequalities between workers and their bosses, women and men workers raised demands for legal restraints on capitalists.

The history of the sewing machine's innovative marketing to individual workers, surrounded by anxieties arising from contradictions between and among aggressive marketing, essentialized notions of femininity as incompatible with technology, and concerns about female sexuality and morality are Coffin's next concern. These anxieties were repressed, but not forgotten, by the realization of both commentators and French workers that two wage earners were needed in most working-class households. The resurgence of homework followed. At the end of the century, in the same period in which the Second Industrial Revolution saw increased scale and mechanization of production in new production processes, improved small machine technologies led to a movement of some manufacturing into workers' dwellings. Strong downward economic pressure on households and capitalists' efforts to evade labor laws limiting women's work in large-scale mechanized industry likewise played a role in this resurgence, which occurred not only in Paris (where it was fed as well by immigration), but in regions which had lost their product markets to competition from the French industrialized

1. Maxine Berg, *The Machinery Question and the Making of Political Economy, 1815–1848* (Cambridge, 1980).

sector or other countries. The outcome was that homework or dispersed production became increasingly women's work.

The rest of Coffin's study covers more familiar territory. She first looks at the issue of married women's work, and the several state Office du Travail and private investigations that newly professionalized sociologists conducted at the turn of the century. Some of these studies were based on relatively large-scale but seldom adequately representative surveys, others on the Le Play family monographic method – in-depth case studies of individual households seen as emblematic of a specific type of worker and “his” family (for example, the Parisian carpenter, or the textile worker of the north). Homework was conceived as a way to reconcile household need and the ideology of “women's place”, or definitions of gender as a division of labor or spheres” (p. 154). The myth that married women's wages were merely supplemental was the link supplied by the governmental surveys that made that reconciliation possible.

The three last substantive chapters consider the “Gender Politics of Sweated Labor”, highlighting opportunities for comparisons with American and European state agency studies and private drives for similar types of reform. Here Coffin discusses the engagement of French trade unionists (mostly male), socialist and liberal feminists, social Catholics, and conservative nationalists (whose anti-Semitism was aroused by the often immigrant Jewish small entrepreneurs or subcontractors in the garment industry) around the issue of homework. The strikes which Parisian female garment workers mounted in the first decade of the twentieth century all failed, partly because of lack of organization; male unionists in the Federation of Clothing Workers were frustrated, despite serious efforts, in recruiting women on a continuing basis into their organizations. One outcome of union failure was labor's increased support for legislation that restricted women workers' hours and union – if not always individual workers' – opposition to married women's wage work.

Both feminists and social Catholics entered the fray via the anti-sweating crusade. This cross-national campaign sponsored congresses on the issues in which representatives from European and North American countries met and compared their findings and various legislative attempts to address the problem. In France, the growing concern with sweated labor intersected with larger social and political anxieties about falling birth-rates and flagging industrial strength (as compared with German and American spectacular industrial growth). New studies with a different slant – social economy – investigated conditions in the garment industry at work and off-work, raising the specter of homework as brute exploitation causing disruptive personal and family relations. One organizational innovation in France and elsewhere was the campaign for state intervention of middle-class women as consumers for regulation of the length of the workday and conditions in the ateliers. In France, a minimalist minimum wage law limited to women's wages was passed in 1915; Coffin crisply labels it as motivated by “charity rather than justice” (p. 247). As she points out in her conclusion, passage of this law marked the collapse of “laissez-faire certainties” (p. 251).

Coffin's tracing of two centuries of change in women's work in the garment trades ends at the dawn of an era of new ways of thinking about wages, consumption, and the contingencies of everyday life which over the interwar years and the 1940s–1950s culminated in the welfare states. Coffin leaves the question of the rising consumption side of the social economic picture to others; her elegant interpretation of the economic factors behind Frenchwomen's role in the production of clothing, and the role of the

social scientists, reformers and labor unionists who shaped public representations of women workers is an important contribution to social and cultural history.

*Louise Tilly*

KULCZYCKI, JOHN J. *The Polish Coal Miners' Union and the German Labor Movement in the Ruhr, 1902–1934. National and Social Solidarity.* Berg, Oxford [etc.] 1997. xv, 283 pp. £34.95.

Confronted by multiple and fractured identities, some labour historians have lost confidence in their project. For reassurance, they should read the opening chapters of this superb case study of the “ethno-class consciousness” of the *Zjednoczenie Zawodowe Polskie* (ZZP). As Kulczycki shows, linguistic, cultural and ethnic divisions did not preclude class solidarity and industrial militancy in the Ruhr: while reaching deep into the Polish migrant community, the ZZP integrated itself into the wider labour movement – at least in the years before the First World War.

It is a remarkable story. Uprooted from a rural homeland, Polish migrants – a group characterized by high levels of social isolation and occupational homogeneity – sought mutual protection by borrowing and adapting the associational culture of the German miners. (Although Kulczycki refrains from comparative analysis, there is an important contrast to be drawn here with other migrant groups such as the Irish who implanted their own ethnic forms of collective mutuality.) Once established, these mutual aid societies acquired a class dynamic which was to distance and emancipate Polish workers from conservative ethnic interests: Church leadership and the influence of the middle-class intelligentsia at the head of Polish nationalist organizations. Thenceforth, it was but a short step to institute a formal trade union specifically for Polish-speaking migrants in the Ruhr, a quarter of the workforce in the region's mines, workers disparaged by the Germans and their trade unions.

A beneficiary of tension and division between the social-democratic *Alter Verband* and the Christian *Gewerkverein*, the ZZP soon established its niche within the wider labour movement, boosted by its prominent role in the 1905 strike. Avowedly Christian itself, the militant ZZP rejected the industrial conciliation of the *Gewerkverein*, while distinguishing itself from the international socialist image promoted by the *Alter Verband*. Union officials remained low-paid and close to their roots, aiding the ZZP in its dual mission to express and support the class grievances of the miners while identifying with the cultural characteristics that differentiated Polish-speaking miners from native workers. Kulczycki draws an intriguing parallel with more recent developments, as the ZZP grew into a Solidarity-style social movement, the largest and strongest Polish organization in the Ruhr, boosted rather than hindered by government persecution. Indeed, from its centre of gravity in the Ruhr, the ZZP, an organization for and of the Polish working class, was able to absorb labour organizations in the homeland and to dictate the form and character of the consolidated Polish labour organization in Germany. In this respect, the ZZP commands the attention of all students of migration. As Kulczycki asks (p. 73): “is there another example of an organization formed in emigration that absorbed and dominated organizations that affected such a significant aspect of public life in the homeland?”

Before the First World War, the ZZP not only integrated itself into the German

labour movement but also succeeded in equating the interests of the Polish nation with those of the Polish working class. In arguing the case so convincingly, Kulczycki adds an important contribution to the new awareness of a positive or symbiotic relationship between ethnicity and class. Until recently, labour historians tended to regard ethnicity as divisive and dysfunctional, a hindrance to working-class collectivism. With the de-centring of class and the deconstruction of essentialist categories, historians have come to appreciate the salience of ethnic affiliation. Ethnicity is recognized either as a functional and inclusive alternative to exclusive class-based forms of collective behaviour, or as a proactive force, an essential preliminary to the construction of wider class-based attitudes and structures. For Kulczycki, class and ethnicity are perceived as complementary and mutually reinforcing.

After the war, it was to be a different story. The first full-length study of the ZZP, 1902–1934, this is a book of two halves. Having charted the structural and cultural foundations for collective “ethno-class” action in heroic fashion, Kulczycki provides a depressing analysis of the “self-liquidation” of the ZZP in the post-war years. Priority was accorded to the interests of the new Polish state, and to the promotion of return migration. Industrial militancy was eschewed, as the ZZP, in a process of disengagement from the German labour movement, subordinated itself to German-led unions, alas just at the point where German-Polish relations were to deteriorate, and when the impracticality of early return migration to the homeland was becoming apparent. The welfare needs and long-term interests of members in their Ruhr “home” were often overlooked as the ZZP endorsed the instrumental attitudes of Polish officials towards emigration from the Ruhr. The post-war history of the ZZP, Kulczycki concedes, confirms the assumption of a necessary conflict between national and social solidarity.

This is a meticulously researched study with a fine sense of balance. Clear summaries are provided to help the reader through the complex web of detail. What it lacks, however, is a wider sense of perspective and assessment. Such criticism is perhaps unfair: as a detailed monograph, there is no place for comparative analysis and reflection. However, Kulczycki’s case study raises far more questions than it can answer on its own. Final assessment of the ZZP must wait until scholars with interests in other ethnic and migrant groups have assimilated this important study.

*John Belchem*

RADCLIFF, PAMELA BETH. *From mobilization to civil war. The politics of polarization in the Spanish city of Gijón, 1900–1937.* Cambridge University Press, Cambridge [etc.] 1996. xviii, 354 pp. Maps. £40.00; \$59.95.

This work is, without doubt, an important and, in some respects, path-breaking study of Spanish social and cultural history. In the first place, its sheer scope is enormously impressive, focusing on the city of Gijón from the turn of the century right through to the outbreak of civil war in 1936. Moreover, events in Gijón are seen very much as a snapshot of the wider picture of social conflict and political mobilization which convulsed early twentieth-century Spain. Second, it is methodologically challenging, introducing a number of concepts ultimately borrowed from the domain of cultural studies into the terrain of what used to be called labour history.

The picture Radcliff paints of growing social polarization is, I think, a convincing

one, and serves as a corrective to those historians who have apportioned blame for the Civil War on the “extremism” of socialists and anarchists (while paying insufficient attention to its cultural and social conditionings), and/or who have sought its roots in short-term miscalculations of the major political actors. Gijón, a medium-sized city which had undergone a significant process of industrialization, emerges as an urban environment in which labour militancy and popular protest could not be effectively channelled into parliamentary institutions. Suffice it to add that if this was the case in a place like Gijón, there was even less likely to be room for negotiation and compromise in hotbeds of labour activism like Barcelona, Seville and Zaragoza.

The methodological renewal is, I think, stimulating, but in some respects debatable. The import of cultural theory has had a number of consequences. In the first place, though implicitly rejecting purely linguistic readings of protest movements, Radcliff brings the cultural into sharp relief, stressing the battle to establish hegemony (in its Gramscian sense) between the city’s “social and business elites”, and its “popular classes”, and also the struggle taking place within the opposition movement itself, between republicans and anarcho-syndicalists, to establish their own cultural agenda as dominant. Second, there is a de-centring of more traditional concerns of labour and social history with the labour process and workplace struggles, to focus on a wider range of protest movements within the city’s poorer neighbourhoods, ranging from mobilizations over the rising price of bread and scarcity of coal supplies, to conflicts over rents. Radcliff, indeed, specifically argues that Marxist-inspired analyses have tended to give primacy to male-dominated, class-based conflicts within the sphere of production, thereby underplaying the range of populist struggles that took in workers, consumers and producers, and which cut across class lines to encompass the broader community.

To take the question of hegemony first, there is no doubt that the field of cultural politics is an exciting one, which is offering new insights. My qualms refer to the concept’s application within a specifically Spanish context. Spain was, between 1875 and 1923, ruled by a corrupt oligarchic regime which, it seems to me, did very little to attain or preserve cultural hegemony over much of urban Spain. Rather than struggle on the terrain of popular cultural politics, it stayed in power through electoral manipulation (carefully twinning urban with rural areas within parliamentary seats in order to be able to manipulate rural constituencies to drown the urban voice), and recourse to force. Radcliff focuses in particular on the left-wing offensive against the Catholic Church and its values, but it is well known that throughout Spain the Church largely ignored lower-class urban areas, siting the bulk of its parishes and convents in the well-to-do parts of town, focusing on the education of the middle and upper classes, while setting up Catholic unions, which were largely unsuccessful because of the key role social elites and business interests played in them. Indeed, much of this becomes clear from Radcliff’s own empirical research. Matters began to change after 1923, and especially 1931. Then a far more strident Right, fearful of reform and revolution, began to mobilize the population behind “traditional” Catholic values. However, attention was still focused on the upper and middle classes and the peasantry. More working-class areas tended largely to be seen as irretrievably lost.

This brings out another problem in the interpretation. The dichotomy between “elites” and the “popular classes” tends, I think, to underestimate the circle of support for more conservative ideologies within cities like Gijón. Certainly, Radcliff can point to instances in which the “little men/women” fought side by side (for example, farmers’ support for protests at taxes on milk and the backing of some workers at least for a

campaign by tavern owners to stay open all day Sunday). More broadly, it is very clear that the freethinking, secular tradition of nineteenth-century republicanism provided the base of a “democratic culture” which could take in elements from the lower and middle classes. However, other conflicts Radcliff analyses tend to point to the possibility of sharp divisions between these social strata, and a move to the Right by quite broad sections of middle-class opinion in the polarized atmosphere of the 1930s. For example, Radcliff stresses the intransigence of Gijón’s employers, but, as she notes in chapter two, the industrial structure of the city was rather dispersed, and one would have expected not only the larger businesses but also many of the smaller employers to have been affiliated to the hard-line employers’ federation. Similarly, Radcliff discusses the importance of rent strikes, but if Gijón was anything like Barcelona then many of the landlords will have been small men and women renting a property or two to boost their income. Indeed, in studies of Barcelona in the 1930s Nick Rider and Christopher Ealham have stressed the importance of rent strikes in polarizing politics between the republican *Esquerra* party (which to a large extent came down in favour of its petty-bourgeois base) and the anarcho-syndicalists. Similar observations could be made on protests and riots over the rising price of food. Indeed, at a political level Radcliff notes the ambiguity of the Gijón republicans when workers questioned the free market, and notes that the most moderate of the groupings to emerge from the republican tradition, the Reformists, moved to the Right in the Second Republic, and entered into alliance with the Catholics in the general elections of 1933 and 1936. But this did not lead to their marginalization because they were seen as the representatives of a rich plutocracy. On the contrary, the party was to govern Gijón municipality between 1931 and 1934 (after 1933 with Catholic Right support).

Radcliff’s theoretical schema also raises the question of the relationship between the concepts of “working class” versus “popular classes”, and between the place of work and neighbourhood within social/cultural history. On the one hand, I would argue, the general European trend away from concentration on the labour process and unionization is, in general, a positive one. The labour process was the focus of much of the debate in the 1970s and discussion of the wider public sphere and its diverse but inter-related “clusters of communication” can only stimulate debate and provide new perspectives. On the other hand, especially in the case of a country such as Spain in which in many areas little social history has been carried out, there are also costs. Thus it seems likely that questions such as changes in the labour process, patterns of union formation, shop-floor and local bargaining, are important in understanding union militancy and employer intransigence. Yet in Radcliff’s study these two variables are to an important degree taken as given.

By focusing on the neighbourhood Radcliff is able to give us a rounded and nuanced picture of protest movements. In particular, her study is of great importance on the question of labour and gender. Building on the stimulating thesis developed by Temma Kaplan that women’s protest primarily grew out of threats to their perceived role as family providers (though implicitly rejecting Kaplan’s claim that female networks focused on the Church), Radcliff offers us a richly textured analysis of women’s consumer-based agitation. However, both at a local and general Spanish level, it still seems to me that in the early twentieth century the primary destabilizing and polarizing element was the growth of labour unions, employer counter federations and industrial strife and militancy. And on the back of this mobilization the CNT was to emerge in the 1930s, in a number of areas, as the focus of discontent. In this respect, I think the

concept of “popular classes”, though on occasion helpful, can be too monolithic, not taking sufficient account of, on the one hand, the strains and tears visible outside elite society, and, on the other, especially from 1914, the centrality of the working class (whether the men or their wives and families, whether as producers or consumers) in protest action. Again, as in the case of the discussion over the elite/popular hegemonic struggle, there seems to be some tension here between the overarching theoretical model and more detailed discussion of cultural politics within the city. As Radcliff notes, there was both overlap and tension between republican and anarchist cultural milieux. And, she goes on to argue, one of the weaknesses of the Left was that neither grouping was able to establish cultural hegemony over the anti-Catholic forces. This is of course true, and it is important to see these struggles in relation to cultural politics. However, in stating that the result was “a hegemonic stand-off that [because it weakened the Left] led to civil war” one does, I think, need to be careful not to replace economic and social determinants by a new cultural primacy. Bound up with these divisions could also be, as we have noted, divergent economic interests, and prejudices rooted in not only the cultural sphere, but also, I would argue, in social and economic criteria (on the side of the middle and lower-middle classes, disdain at workers’ lack of education and inferior employ, on the part of workers a critique of the petty bourgeoisie’s desire to imitate elites). And, of course, the establishment of cultural hegemony over the Left could in itself to have done nothing to block an attempted coup launched by sections of the military and backed by Catholic-conservative Spain.

To conclude, therefore, this is a stimulating and polemical study, which, I would hope, will have an important role in enriching debate on labour and popular protest in Spain, and more broadly the origins of the Spanish Civil War.

*Angel Smith*

BUKHOVETS, OLEG GRIGOR'EVICH. *Sotsial'nye konflikty i krest'ianskaia mental'nost' v rossiiskoi imperii nachala XX veka. Novye materialy, metody, rezul'taty.* [Desiat' novykh uchebnikov po istoricheskim distsiplinam, 6.] Mosgoarkhiv, Moskva 1996. 398 pp.

No other subject has generated so much inquiry and discussion in Soviet and Russian historiography as the peasant movement and its “mysterious” soul. This book is intended as a contribution to the methodology of the analysis of social conflicts in the Russian countryside. Bukhovets announces that the aim of his research is to call into question the old “dogmas” of Marxist theory on social conflicts in the countryside. His central point is the observation that many statements of Soviet historiography that are considered to be “axioms” are in fact theorems which need to be proven on the basis of an analysis of concrete historical evidence.

This intention determines the structure of the book. In chapter 1, “About the crisis of historical knowledge”, Bukhovets summarizes the literature (both western and Russian) on modern theories of social conflicts and the approaches to and methods of analysing such conflicts. He stresses that the history of “class struggle”, which used to be such a popular subject in Marxist literature, became totally neglected in post-Soviet historiography.

The next two chapters are central to the book. Bukhovets investigates such complex

phenomena as peasant mentality, which, like that of any other social group, reflects their needs, concerns, social beliefs and dispositions, and expresses their attitudes to the current regime and other social groups, as well as to forms of property ownership. As sources for his investigation the author used petitions and letters (*prigovory*) of the peasantry: requests, appeals, resolutions and telegrams, etc., related to political developments and sent by peasant meetings to the tsar, various governmental bodies, the state Duma (the Russian parliament), political parties, to editorial bodies of newspapers, and so on. It should be noted that not only Russia, but many European countries as well, saw the frequent appearance of such documents in great numbers in the nineteenth and twentieth centuries: for instance, on the eve of the French Revolution and during the 1848–1849 revolution in Germany. In chapter 2, “Riot and conscious revolutionary character”, Bukhovets studies the political mentality and activity of members of the “petition campaign” (*prigovornoe dvizhenie*) in the Samara and Voronezh provinces (*guberniia’s*) in Central Russia in the years of revolution, 1905–1907. In chapter 3, “Symptoms of future cataclysms: social conflicts of a peaceful period”, the peasant movement in five Belorussian provinces in 1907–1917 is considered. The book also provides thirty-one appendices, with a wide range of tables and a select bibliography.

Does the author offer anything new about social conflicts and peasant mentality in Imperial Russia in this book? First, Bukhovets claims that the mentality of the peasants can be analysed only in relation to peasant behaviour and the peasant economy. He has therefore attempted to examine whether the social, economic and political position of the most active members of the petition campaign differed in comparison with that of other peasants. By grouping nineteen districts (*uezd*) he identified two regions in the province of Samara and three in Voronezh in which the social and economic environment of the peasant economy differed. In his view, “the fact of participation in social conflicts did not depend on a better or worse economic position of the peasantry. However, the degree of participation quite definitely depended on these conditions.” Most of those outside the peasant movements were better off than other peasant households in these regions (p. 319). Through a study of the social “passport” of members of the petition movement (*prigovornoe dvizhenie*) he stresses that, as a rule, they lived in the larger villages (settlements), especially in 1905–1907. Most of them belonged to the poor or middle group of peasantry. Moreover, the settlements analysed were not only radical in their political outlook, they were the most radical in their actions as well.

The author discovered that the correlation between the economic position of the peasantry and forms of social conflict was even stronger. The peasants who wrote or signed the appeals, requests and other forms of petitions to the state Duma and other political bodies were the most active in all forms of social conflict in the countryside. However, in his view the agitation of many political parties in the countryside did not result in an increase in the number of social conflicts, peasant uprisings or any other campaigns of civil disobedience. Following the tradition of Soviet historiography, Bukhovets analysed “economic” (mainly protests against landlord ownership, demands for more favourable lease terms, etc.) and “political” forms of peasant movements (whose campaigns were directed against the state). It is quite remarkable that the victims of many anti-landlords campaigns (more than 16 per cent) were often farm labourers (*batraki*) who worked on the estates (and even their wives and children) rather than landlords themselves (p. 324).

Second, one of the strongest features of Bukhovets’s book is his careful and scrupulous analysis of documents. Over one thousand appeals and requests have now become

available to historians. Bukhovets has considerably enriched this corpus of documents. On the one hand he has discovered and brought into circulation many new petitions. For instance, over the course of the many years he has worked in various archives in St Petersburg, Vilnius, Kiev and other cities he has discovered 673 new peasant petitions from Belorussian provinces (previously, only sixteen such petitions were known to exist). On the other hand, he has used petitions in combination with other types of sources, mainly statistical publications such as the land register of 1905, lists of settlements (*Spiski naseleennykh mest*), and others that give a new dimension to these historical sources.

Third, the spirit of the necessity to use quantitative methods and mathematical modelling in historical research runs through the entire book. Bukhovets stresses the fact that hitherto researchers have tended to use the closing (conclusive) clause of the *prigovory*; the greater part of the text of these petitions, which present views, requests, greetings, warnings, promises, etc., have not been analysed before, or if they were it was for illustrative purposes only. Dealing with a thousand documents, it is almost impossible, Bukhovets argues, to overcome analytical difficulties without the application of more advanced methods. Bukhovets makes use of content analysis for this purpose, which enables him to find structure in the “hidden” evidence scattered about in the mass of material which is “imperceptible” in an individual document. Bukhovets compiled a list of statements derived from the text of the petitions and then tried to identify internal links between them, by measuring the “closeness” in relations between statements using mutual correlation ratios (Tschuprow’s *T*). However, he does not explain clearly how he chose these statements. His interpretation of links between them in many cases also seems to be rather arbitrary.

One further innovation is that Bukhovets introduces a new unit of measurement of social conflict. Most researchers who have studied social conflicts in the countryside used one of two indicators: numbers of uprisings or other actions, or the number of villages in which social conflicts took place. Bukhovets stresses the limitations of both approaches and instead proposes an index summarizing both indicators.

The models developed by Bukhovets show the complex character of the peasant political philosophy in that period, in particular the contradictions between their striving for revolution and their conservative attitudes. He identified and analysed two groups of closely connected indicators which reflect “traditionalist” and “revolutionary” systems of political views and ideas. Both of them were present in the minds of the same peasant groups. The political philosophy of the Russian peasantry at the beginning of the twentieth century was characterized by its contradictory nature: revolutionary ideas combined with patriarchal views in the most bizarre ways, even among the most radical group of peasantry.

Bukhovets’ book is the result of more than twenty years of research. This explains the occasional inconsistency in structure and conclusions. Fighting against the old dogmas of the Marxist theory of class struggle, the author has not always been able to avoid using the same terminology and ideas. For instance, he attributes too great a significance to the so-called “two social wars in the countryside”: social conflicts between the peasantry and landlords on the one hand, and actions targeting the well-to-do peasantry (*kulak*) on the other. Nevertheless, this book remains an informative and original investigation of peasant mentality.

*Tatyana Moisseenko*

ONSELEN, CHARLES VAN. *The Seed is Mine: The Life of Kas Maine, a South African Sharecropper, 1894–1985*. James Currey, Oxford 1997. xvi, 649 pp. £14.95.

Charles van Onselen's latest publication: *The Seed is Mine: The Life of Kas Maine, a South African Sharecropper, 1894–1985*, is a brilliant piece of research, an exciting narrative and advances oral history studies considerably. I am unlikely to be the only reader who devoured the 649 pages eagerly, the result of fifteen years of painstaking research. Although *The Seed is Mine* concentrates on the life of a large family, kin and friendship networks, this biography reaches deep into South Africa's agrarian economic history over a period of ninety years. Adopting this wider focus, pioneered by historians of slavery in America's south, reflects van Onselen's view that biography is "history without boundaries", also a useful definition of social history. Fundamentally this is an in-depth study of agrarian African labour, the very core of South African economic history. All the tensions and bitter conflicts of institutionalized racism are mirrored in the life of Kas Maine, a Sotho sharecropper-farmer and paterfamilias of a large kin network.

Kas Maine's life is anchored in the rural areas and small towns of the northern Orange Free State (from which masses of black families were evicted), and southwestern Transvaal, an area of "hot dusty plains", hence economically depressed. Van Onselen does not see this, as many would, as the most important factor overwhelming those struggling on this barren land. Rather we learn of the drive, and what seems infinite hope driven by determination, of how Kas Maine's initiatives consistently buttressed economic adversity (and political disenfranchisement) such as frequent land evictions. When his efforts were frustrated and his aspirations hit the buffers, Kas recovered (with the help of family, some Afrikaner farmers, shopkeepers and a Jewish trader) each time a little better prepared but still struggling either as a sharecropper or self-employed craftsman and entrepreneur. If Kas ever moaned or drifted into a depression, we are not made aware of this. But what is always clear: racism consistently wielded its uncompromising power, although the complex interaction of colour and class occasionally opened doors.

We are taken through the economic and political history of the Orange Free State and southwest Transvaal; the rise and fall of African sharecroppers; the often precarious position of white settlers rich in land but poor in capital; the mutually advantageous arrangements made by them with African subsistence producers rich in labour, and often also cattle, but lacking land; and the transition of blacks from subsistence to commercial farming despite lacking in capital and the opportunity of obtaining credit. The absence of these vital infrastructures undermined their efforts to diversify. Kas Maine's labours were a constant search for survival, to meet domestic responsibility, and how to reduce risks and optimize times of opportunity. Detail of daily life and the cyclical economic changes, and their impact on disasters or opportunities, are locked into the memory of Kas and his family.

Skilfully, van Onselen takes us along the evolution of South Africa's dispossessed black peasantry. The Native Land Act of 1913, which decreed that Africans could not hold land outside the native reserves, resulted in many evictions and relocations on unproductive land. Sharecroppers made tenancy contracts with poor white farmers who allowed Africans to graze their stock in return for labour or a share of crops. But evictions in the midst of the agricultural cycle continued, wasted planted crops, terminated expansion, and forced Kas, as others, to sell nurtured stock; worse, it created debts forcing Kas to launch new

income initiatives such as shoe repairing, saddle making and “transport riding”. But luck and determination created a niche even within the oppressive structures of Afrikanerdom made perhaps easier for Kas who had a reputation for integrity; Afrikaners and traders gave him loans knowing they would be repaid on time.

Of particular interest is the evolution of class in the South African agrarian formation. We meet “poor whites” whose contact with “prosperous” African sharecroppers become “Africanized”, while more successful Africans become “Afrikanerized”. These developments, more codified with agrarian mechanization and urbanization, become obvious in the last twenty years of Kas’s life. Until the late 1930s Kas’s contacts with Boers were not merely contractual but often surprisingly social. Kas went into partnership with Hendrick Swanepoel operating a transport enterprise. The men shared sleeping under the wagon, eating together and being drunk. A Jewish trader preferred the hospitality of Kas to those of whites. Afrikaners sought Kas’s help in various ways, and Africans knew him as a clever *ngaka* (herbalist). African-white contacts were embedded in the economic formation of rural life and above all determined by the demands for African labour.

Kas achieved considerable success. He bought better wagons, ploughs, tractors (five in all) and even a car, although both of the latter turned out to be questionable purchases increasing pressure on his limited capital. Because of these investments, Kas expressed bitterness when he was “evicted once [land] had been cultivated and the soil proved fertile”. There was nothing African sharecroppers could do, lacking, as they did, political representation. They could only silently watch as “a landlord plough a man’s seedlings into the ground before his eyes”. But we also learn that it was not only Boers “as tough as their *sjamboks*” who were rich but “rich blacks were also shits”.

Repeatedly Kas moved in search of a fresh start, but found it increasingly difficult to activate family labour as children drifted away, or became rebellious, and conjugal relations became strained; he deplored the “implosion of morals in the Maine family”. Family labour was not enough – a steady input of capital and earnings was essential. Greater security generated capital in stock and crops, and sales supplemented with off-season self-employment increased income. Judicious management of cash flow allowed for new investments. Kas’s skills as an *ngaka* – “he dug out several roots with a stick and then disappeared to prepare the herb” – added to the family purse. Economic impoverishment intensified when Africans were confined to labour reserves. Nevertheless, even then, Kas could look back on the occasional good season: “For anyone to have reaped a large harvest on depleted soils of a labour reserve in the 1980s was an achievement. For an octogenarian with faltering eyesight and declining physical powers to have done so well without the assistance of male offspring was extraordinary”. At age eighty-six Kas acquired his fifth tractor! As the years passed, Kas’s labour history was transformed from unpaid labourer to the occasional employer of labour, and from sharecropper to being self-employed.

Woven into Kas’s life are the ups and downs of his family; the upbringing of his children (and their attitudes to him); his marital history; and how he maintained his family and moulded it into an income-generating force. But there were hostilities. “My father”, one of his sons recorded, “was cruel. To him any infringement warranted punishment. He did not work on the farm – he used the labour of wives and children.” He flogged his daughter Matlakala who had been employed as a “*kaffermeid*” by a white mother who abused her because Matlakala hit back. If true, but perhaps exaggerated, this is a harsh judgement of Kas. Kas was prepared to lose the support of his children,

as he did of his son Mmusetsi who left home. Van Onselen records that “Kas found it hard to keep his wives and children from peeling off the family enterprise and entering the market on their own terms.” Precisely the reasons for this, other than intra-family tensions, are not always obvious. But what is more documented is that “The seductive rhythms of weekly wages and urban living had already done much to undermine the uncertainties of rural life”; furthermore “to extract surplus labour for an aging family machine, neither the old nor the young were spared”, demanded increasingly an authoritarian attitude and practice.

There are moments of pathos when Kas meets up again with Mmusetsi, now paralysed and incontinent as the result of a riding accident. Kas had to adjust to family demands, and his personal affections, to the economic and political world of apartheid. Over time children and wives were alienated and old age made further intrusions. Yet as husband (to a number of wives) and father, Kas reveals warmth and consideration, seeking to avoid confrontation. But Kas never reneged on his responsibilities. (His family was so large that the Post Office complained “they were unable to deliver mail correctly”.) There were times when all that Kas could rely on was his determination to keep going, to activate friendships, both African and white. The fact that Kas was already eighty-five when interviews began had mellowed and matured his life and softened his verdicts.

Despite being politically disenfranchised and economically deprived, Kas’s relations with white farmers and landlords were pragmatic. Walter Moormeisler, “a Nazi-sympathizing killer”, was “like a brother”, and Koos Meyer who made Kas “rich”, a relationship which did not last when Meyer demanded too much labour and Kas told him one day, “I will hire you, and overwork you just as you are doing to me.” Cas Greyling, a Nationalist Party MP and openly racist, hated kaffirs and Jews alike yet bought oxen from Kas. But a Jewish trader, Hersch Gabbe, gladly accepted Kas’s hospitality. Kas’s characterization of Willem Nieman as “bloody rotten” because he was “unwilling to take a kaffir into consideration”, are outbursts of anger and frustration which are more off-the-record rather than central to his life of labour. Real as these episodes are, Kas “remained largely oblivious to the historical ironies surrounding him”. I rather suspect that this is an overstatement as van Onselen tells us that Kas believed, “There was something inherently unfair in the allocation of capital and labour”; as he put it: “We ploughed day and night, and he [the landlord] sat and did nothing.”

Because Kas was fluent in Afrikaans, cross-racial economic cooperation was possible. Kas farmed on the land of Willem Nieman, a racist to the core and equally objectionable about Jews and “the English”; Kas’s description of informing Nieman of the birth of his first son is worth recording:

When a child was born you went to the landlord and said: “We have a baby boy.” The landlord would be pleased and say: “Oh, you have a little monster, have you cut off its tail?” Then we would say: “Yes, Master, I have cut off the tail, it is a person now no longer a baboon.” That was how the white farmers used to put it to us.

Repeatedly we learn how that complex interface of race-class-gender and labour dominated the daily life of the sharecroppers. To seek confrontation, to show active resistance, could lead to reprisals, and often did. Informal protest was the better part of valour. Kas, we are told, “never fought anybody, not even whites”.

A surprising revelation is that Kas showed little interest in unions or political move-

ments such as the South African Communist Party. He never joined the Industrial and Commercial Workers' Union or the CP although the former "spoke the language of progress to sharecroppers" and pressed for the need for education of their children. He was, he declared, "not persuaded by ideas alone". Kas objected that the ICU's "wild talk about 'whites' and 'strikes'" was bound to end in tears. "The Boers", Kas maintained, "would skin you"; he was "more interested in my farming"; industrial militancy was inappropriate; his efforts were directed to "consolidate his career as a farmer". Van Onselen describes this as the "contracyclical process of social and economic renewal". Gradually Kas and his family moved into a "quasi-urban environment" dominated by diversified commerce, by the diamond "diggers" whose labour differed sharply, as did their political outlook, from that of the sharecroppers-farmers. Kas now devoted much time to leather work and cobbling. The importance of being self-employed, and to have a pass which he obtained, conferred a status of small town life.

Readers will note van Onselen's comment in the Preface that colleagues will no doubt be "interested in the theoretical issues". Sensibly he has not used this publication for such an elevated purpose. (Those who want to know are asked to read previous articles by the author – all cited in the references.) Nowhere are even the briefest references to, let us say, Marxist analytical categories or models; there is no discussion of proletarianization, or models of race, class and gender (although we cannot fail but to note their interface); no models of paternalism or violence; none of patriarchy and kinship structure. To touch on any of these would add little and fundamentally drain the narrative of its easy flow, the way personalities and family are centre stage. But as in all good writing, theory as conceptualization is revealed in the range of this biography and family study. All the ingredients, all the angles, all the data are there, essential for a holistically designed theory of an agrarian economy and how it dovetails into South Africa's economic history. What we seek to know we are given: the larger focus, the greater depth – a rich broader picture. We note what is symbiotic and dysfunctional; what is continuity and what is systemic or radical change; what is free labour and what is not; what are nuclear units, socially and economically, and how wider networks are formed; how social and economic boundaries are sealed, and where and when they are porous; why and when family ties are rock solid and what pulls them asunder. So, quietly we can drink at a fountain rich in arguments about what South African labour history has been, is now, or should be: liberal or revisionist; structural or from "the bottom up"; late capitalism or early globalization (the gold and diamond economy); as a significant contribution to comparative history, or as a distinctive single case. It is all there but we are given it in a most delectable and easily digestible manner. This disguise is theory at its best. If that is heresy, let it be so.

This is research into oral history with a difference. We are given extraordinary detail of Kas's, and his families', daily life. At times there is an almost lyrical quality to how episodes and conditions are revealed. Some examples will suffice: "Kas wrapped himself in a coat, got on his horse and, with the shrill wind [. . .] slowly nudged the animals to town"; or, "the landlord turned on his heels and left"; or, Kas had "animated exchanges that rose above the whine of the car engine". At times we are treated to a vivid description of the local setting: "There half-hidden behind a kindly bluegum – which understood more about camaraderie of farming life than it would ever be willing to reveal in public – an Executive Member [. . .] of the National Party partook of a meal and sipped brandy"; and this brilliant observation of the scenery: "the black-shouldered kite that sat perched on the telephone line for hours". These softly-presented piquant

observations are the hallmark of *The Seed is Mine*. A final example is a revelation of pathos. One is drawn close to Kas in this passage on the occasion of the death of a child.

The depth of the night, as if sensing grief that lay huddled within the confines of the shack, refused to yield to the approaching light of a highveld dawn. Kas stood in the doorway and looked out as a dark steel-grey illumination slowly picked out the outline of the ridge [. . .]. Within the shack not even Bodule [his son] stirred, and Kas did not have the heart to set the daily routine in motion.

Is there anything more poignant to sum up what labour history should be all about but these last few words?

This marvellous book has achieved what few can but many aim for: to draw us into the lives of working people. And in the case of South Africa we not only suffer their pain but also rejoice in their conquests. Here we have agrarian history presented with a literary imagination and a humanistic framework. This was possible because, as van Onselen generously records, all the Maines “opened their doors and allowed strangers to wander round their homes at will”. For this every reader will be grateful. For what we have here is not just oral history but a lasting memorial to Kas and his family.

*Peter C.W. Gutkind*

#### *Editorial Note*

At our request Jürgen Rojahn reviewed Ursula Ratz, *Zwischen Arbeitsgemeinschaft und Koalition. Bürgerliche Sozialreformer und Gewerkschaften im Ersten Weltkrieg*. His review has been published in *IRSH*, this volume, pp. 152–155. Due to an organizational error, the draft version of the translation, which contains a number of faults, was sent to the publisher instead of the version corrected and authorized by the author.