

## BOOK REVIEWS

LEVINE, DAVID. *At the Dawn of Modernity. Biology, Culture, and Material Life in Europe after the Year 1000*. University of California Press, Berkeley [etc.] 2001. vii, 431 pp. \$45.00 DOI: 10.1017/S0020859002010842

In the “After-words” to his book the author, David Levine, confesses that his intention had been to write a book quite different from the one the reader has just read. What he had in mind was “a snappy text called *Forgetting the Family*”, “a short history of the modern family” that would meet the “appalling lack of depth” in the recent sociological debate on “family values and its regurgitated clichés”. Instead, he became entangled in broad-based research into “the roots of the world we have made” (pp. 423–427). This confession goes a long way to explaining the skewed design of *At the Dawn of Modernity*, which I shall try to explain in a moment. One cannot but admire the ambitious effort Levine has made to analyse a long-standing historical debate on the medieval origins of the “European miracle” from new fresh angles. However, one is also tempted to compare his new synthesis with other recent efforts in the same direction. These include some of the best general studies of medieval society that have appeared in the last couple of decades – books such as Georges Duby’s *Guerriers et paysans*, Jean-Pierre Poly and Eric Bournazel’s *La mutation féodale*, and Robert Bartlett’s *The Making of Europe*, while Levine’s study also calls to mind Perry Anderson’s work.<sup>1</sup> In my opinion Levine’s book does not quite measure up – not because it lacks scholarly depth (on the contrary), but because its composition is rather impenetrable. To test his ideas on Europe’s medieval dawning, Levine chose to use an intricate analytical model that, for all its complexity, is not applied very consistently. In fact it mixes an unequal basic chronological division (the period 1000–1300 and the period 1300–1500, the former dominated by a “positive feedback system”, the latter by a “negative” one) with a principal conceptual dichotomy (feudalism versus modernity), a change of perspective from top-down to bottom-up, and three interwoven approaches (the biology, culture and material life in the book’s subtitle). The latter are described in the “After-words” as forming a “triple helix of historical forces”. Like our genetic code, this helix is in a process of “constant recombination” – i.e. change – both as a consequence of “internal dynamic” and of constant “interaction with the variability of external circumstances”. Such a complex view of social change makes great demands on the historical narrative, and Levine has answered them by “organizing his text” in a “cloisonnist” way – a reference to the technique of such post-Impressionist artists as Van Gogh and Gauguin, “characterized by fields of intense colors, strong figural outlines, and little, if any, modeling in the round” (p. 422). Applying this technique to historical writing is like “problematizing the past by highlighting its contrasting elements” (p. 422, n. 28).

The elaboration of this dazzling design is not without some serious flaws. One is the erroneous definition of the key contrasting concepts of feudalism and modernity. What

1. Georges Duby, *The Early Growth of the European Economy: Warriors and Peasants from the Seventh to the Twelfth Century*, tr. Howard B. Clarke (London, 1974); Jean-Pierre Poly and Eric Bournazel, *The Feudal Transformation, 900–1200*, tr. Caroline Higgitt (New York [etc.], 1991); and Robert Bartlett, *The Making of Europe* (London, 1993).

exactly made medieval society at first feudal and then premodern? Intriguingly, Levine has given his first chapter – which describes “the making of feudal society” – the Andersonian title of “Lineages of Early Modernization”, while the second, contrasting chapter focuses on “the deployment of new techniques of human organization” (p. 423) and is entitled “Shards of Modernity”. However, these headings conjure up images that are difficult to reconcile: the former suggests a child called Modernity who was secretly nursed on feudal society’s lavish breast; the latter suggests that only fragments of Modernity were floating around, like *Fremdkörper* in the murky waters of huge Lake Feudalism. This is not just a question of juggling with metaphors; it corresponds to a strange ambivalence in Levine’s mind about the relationship (the magnetic attraction perhaps) between “feudal” and “modern”.

This ambivalence has resulted in a somewhat awkward choice of composition. For whereas chapters 1 and 3 are used to set feudal society in place – initially from a top-down perspective (ch. 1), then from a bottom-up perspective (ch. 3) – the chapter caught in between, so to speak, serves as a catchall for stowing away the many key aspects of medieval change that do not fit easily into the traditional description of feudal society but which are clearly too important to omit in a book of such a general scope. So while chapter 1 concentrates on the feudal and religious “revolutions” that marked drastic alterations in the way power was exercised at a material and moral level – both in the relationship between (local and banal) lords and peasants, between kings and aristocracies, and between secular and spiritual power – and chapter 3 basically describes serfdom and the manorial system, chapter 2 deals with everything else: population growth, agrarian expansion, urbanization, commercialization, technological diffusion, the march of the written word, the evolution of centralized bureaucratic government, and dietary change. But Levine falls short in indicating to what degree the “positive feedbacks” characterizing the “virtuous circle of growth and development” that predominated between 1000 and 1300 were generated by “modern” features, and to what degree they were dominated by the dynamics of feudal society itself. On the one hand “towns rescued men and women from seigneurial oppression” (p. 138) and money and commerce saved European economic life from stifling autarky (p. 140); on the other hand there is no reason to relate “economic regression [i.e. low productivity in agriculture] to the feudal mode of production” (p. 179). What lessons about medieval society should readers learn from such contradictory statements?

A second flaw in the book’s structure is that it is only when discussing the mechanisms of “feudalism’s” reproduction in chapter 4 – over half way through the book – that Levine brings his central model of the triple helix into action. If we look back at what moved feudalism on, Levine argues, we see that “barbarian demography”, “Christian culture” and “manorialized feudal society” [...] “wrapped themselves around one another”, thus “combining the biological, cultural, and material modes to form a finely adjusted system of social reproduction” (p. 245). This all sounds very exciting, but it is not easy to immediately grasp his argument, unless one is prepared to expose the author for what he basically is: a specialist in the field of family history, now revealing his true face. The need to reread Levine’s book in the light of this revelation is apparent right from the beginning of chapter 4, where the author discusses the origins of the “deferred marriage system”, better known as the European (i.e. northwestern European) marriage pattern (EMP), expounded by John Hajnal in his famous article of 1965. Hajnal argued that the EMP was unique in the world because (1) it allowed for girls an extended gap between age of puberty and age at first marriage; and (2) it accepted that many young people never married at all.

Hajnal maintained strongly that this pattern had not settled before the end of the Middle Ages, but medievalists, such as R.M. Smith and P.J.P. Goldberg in particular, have openly doubted this and proposed relocating the origins of the EMP to the late Middle Ages – despite the “low-pressure” demographic regime established after the Black Death. David Levine is prepared to go even further. In his view, the system of deferred marriage, just like the nuclear family, “probably existed before the year 1000” (p. 244), and together with the nuclear family it developed into a demographic strut for the feudal order.

To underpin this proposition Levine leans on a curious mixture of ideas launched in the 1970s and 1980s by such distinguished scholars as Luc Buchet, William McNeill, Jack Goody, and David Herlihy. Levine’s argument runs more or less as follows. First, in the disease-ridden post-Roman world the most mortal diseases became endemic only in the warmer and more densely populated Mediterranean basin, not north of the Alps. Second, it followed that even at an early stage girls married younger in the south than in the north. Third, among the German barbarians who had invaded the north women also married relatively late, due to the Germanic preference for partible inheritance. (Here we have, in a nutshell, the “barbarian demography”.) Fourth, the Church’s hold on sexual morals and the institution of marriage was stronger in the north than in the south. And fifth, serfdom and the manorial system became more widespread in the north than in the south, and both these institutions promoted the formation of “standardized” – or “commensurable” in the late David Herlihy’s words – nuclear households.

As stimulating and provocative as this exercise in “thinking with demography” might be in many respects, in the end it raises more questions than it can answer. Did not the barbarians also invade the Mediterranean basin? Why should the Church have had a greater moral impact in the north than in the south? Was not the early medieval aristocracy “using” the Church to pursue its own goals rather than the other way round (a critique aired against Jack Goody’s view)? How widespread was the Carolingian manorial system in the north? To what extent were manors organized in a “classical” bipartite manner – the only form that might have invited the creation and establishment of something like “standardized” peasant *mansi*? And did not serfdom and the manorial system, with their inherent impartible inheritance rules and immobility of people (at least initially), foster the formation of stem families rather than “nuclear households”?<sup>2</sup> And how does the supposed “invention” of the nuclear family/household in the Frankish age relate to Peter Laslett’s famous zero hypothesis (“all families must be assumed to be nuclear unless it can be proved otherwise”), which in this context is too provocative to be entirely ignored, as Levine does.

For the remaining, larger, part of chapter 4 Levine’s discussion of the “finely adjusted system of social reproduction” is dedicated entirely to the nature of medieval serfdom and the manorial system. It amounts to a series of observations that few insiders would dispute: that the legal status as well as life experience of medieval serfs could vary enormously; that the peasantry was a socially stratified group – with at one extreme a “centripetal” “privileged minority”, governed by patriarchal values, and at the other a “centrifugal” landless underclass, continuously in danger of downward social mobility; that though the medieval manor might not have been a Gulag, nor was it a free world either – the manor lords firmly controlled access to land. Following a short discussion of the land market (the flourishing of which interestingly enough would have been quite independent of the

2. See Wally Seccombe, *A Millennium of Family Change* (London, 1992).

degree of manorialization (pp. 280–281)) and the problem of inheritance traditions and subdivision of peasant holdings, Levine returns to the marriage system and discusses the external (i.e. ecclesiastical, seigneurial, familial) control of marriages, and the consequences of marriage for the intergenerational transfer of property.

Now that the first helix has finally been unwound, it takes only one chapter – chapter 5 – to analyse helix number two as well as the “negative feedbacks” of the late Middle Ages. Once again, Levine starts with the biology string, this time centred on a biomedical description of the Black Death (which according to Levine would have killed off as much as 50 per cent of the European population), its “echo-epidemics” and the establishment of a new “mortality regime” with far-reaching demographic implications (the “bumping upward of fertility” to begin with). “Luxuriant despair” is how Levine has baptized the second, cultural string. It concerns the social-psychological reactions to the Plague, and is heavily indebted to Jean Delumeau’s work on death, sin, and fear of what would happen after death. A central channel for expressing these concerns was lay devotion and its entrenchment in popular religion, and Levine deservedly gives serious attention to devotional practice. But here too his exposé has its weak spots, because many key features of devotional practice and spirituality in the post-Black-Death period – as well as its institutionalized organization and its dark side effects (growing intolerance towards dissenting groups, increasing anti-Semitism) – already had a long history by then. Rather than being conjured up by the Plague epidemics, they were typical products of the religious revolution of the eleventh and twelfth centuries, as Levine himself is the first to admit (pp. 88–92). On the other hand, Levine has picked out with a sure hand the two major exceptions: witchcraft and the rapid increase in anticlerical thinking and feeling. Both of these are highlighted by focusing on key figures; in the case of witchcraft on Joan of Arc, in the case of anticlericalism on Martin Luther. The latter case especially is well presented (in my opinion these pages are among the best in Levine’s book), and one cannot but fully subscribe to the author’s conclusion: “The Black Death [...] modified the Gregorian distinction between the clergy and the laity by insisting on a renewed Augustinianism which emphasized the difference between the saved and the damned” (p. 363). And it was Luther who successfully transformed this new idea on the redundancy of the clergy into a clergyless Church.

Finally, the third, “material” string of the second triple helix is presented as the “social earthquake” that blew away the feudal social order. Introduced by a short reference to the opposition between neo-Malthusian versus neo-Marxist “schools of thought” about this process, Levine’s further discussion of this earthquake boils down to the classic story of the dissolution of the manorial system-cum-serfdom in England, followed by a more general exposition of the economic and social consequences of the radical reversal of the land-labour ratio in the post-Black-Death era, and the divergent responses of various social classes. This creates the wrong impression that what happened in England was happening everywhere, which is wide of the truth. It would be more accurate to say that in many parts of western Europe the greatest shock from the “social earthquake” referred to by Levine had been felt around 1350 – it is the story of the *révolution censive* that Levine hints at several times (e.g. pp. 276–277, and 280), but nowhere explains.

This careless approach to geographical disparities in medieval Europe, paired with a strong English bias when it comes to in-depth analysis of certain key features of economic and social development, is rather typical – and sadly enough not only of Levine’s book. One reason for Levine’s reluctance to take in more – let alone all – of Europe may be his

lack of familiarity with actually “doing” medieval history, which betrays itself in scores of minor mistakes, such as locating Aachen, Brussels, Antwerp, and Louvain in Flanders (p. 134); suggesting that “the Netherlands” were one nation in the fourteenth and fifteenth centuries (p. 334); claiming that the Duchy of Burgundy “traditionally had been a part of the Holy Roman Empire” (p. 353); calling one of the most famous papal bulls issued in the Middle Ages *Unam Sanct[u]m* (p. 358); making the French king “remove” the papacy from Rome to Avignon (p. 359); using “the Sorbonne” as a pars *pro toto* for the University of Paris in the thirteenth century (p. 359); and calling Marsilius of Padua a Spiritual Franciscan (p. 361). None of these errors affects Levine’s main arguments, but they do expose a hermeneutic deficiency that may have made the difference between an interesting book and a brilliant one. Because, although *The Dawn of Modernity* offers valuable insights, as a new and comprehensive attempt at solving one of Europe’s greatest historical riddles, it has not convinced me.

*Peter Hoppenbrouwers*

SCHMIDT, ARIADNE. *Overleven na de dood. Weduwen in Leiden in de Gouden Eeuw*. Uitgeverij Prometheus/Bert Bakker, Amsterdam 2001. 333 pp. Ill. € 22.46; DOI: 10.1017/S0020859002020849

Ariadne Schmidt’s *Overleven na de dood* is a deeply researched and carefully documented study of widows in Leiden during the Golden Age of Dutch history. Based on a rich body of administrative, fiscal, and legal sources kept by the state and city, notably on a collection of appeals (*rekesten*) to the *Gerecht* (Leiden’s governing council) brought by citizens in search of redress, aid, or special favors, and supplemented by more general studies, *Overleven na de dood* makes a valuable contribution to the empirical record. Thanks to this investigation, we now have a much clearer picture of the social experiences of widows in this region during the early modern period, and we are provided material for suggestive comparison with more distant European and North American locales. While the study confirms many of the general conclusions reached by the previous generation of women’s and social historians focusing on similar settings in this period, it offers needed corrections or nuance to some existing research. It also provides decisive evidence about the importance of certain social structures in determining women’s experiences in urban settings like early modern Leiden.

On its own terms, *Overleven na de dood* is an exemplary social history – it assembles and organizes massive amounts of data, presents the material clearly, and offers judicious, if parsimonious, interpretations of it, often based on explorations of secondary literature on similar themes. As I will argue at the close of this review, the book’s limitations – and there are some – arise not from the quality of the work itself but from the inadequacies of the unnecessarily constrained social-history paradigm to which the book is bound. These shortcomings, while hardly diminishing the usefulness of the work, account for some missed opportunities that bear comment.

The book is organized straightforwardly, beginning with a discussion of sources and ending with a conclusion that is in fact a summary of major points made in the empirical chapters (the English summary is a translation of this conclusion). In between, we have a review of the proscriptive literature concerning widows; a discussion of women’s status as legal persons at different life stages and marital states; a careful analysis of marital property,

succession, and inheritance law; a study of women's work for the market (subsistence production, including biological and social reproduction, is largely taken for granted and little described); a description of the incidence of poverty among widows; and, finally, a survey of remarriage patterns. Each chapter contains abundant evidence, much of it statistically rich enough to reveal patterns, but often supplemented by stories taken from the *rekesten* that texture these otherwise abstract analyses.

The author carefully leads us through the intricacies of Leiden's marital property, succession, and inheritance law, showing how it positioned women relative to marital property and to the estates left by their deceased husbands, and the way that local statute law and conventional practices (wills and marriage contracts) subtly modified traditional custom during the period to provide even greater protection for widows. Wives in Leiden were by law contained by the marital household, their property subject to their husbands' managerial decisions and their legal personhood subsumed under his. Nevertheless, they were considered co-owners of the marital estate, and as widows they were full successors to half of its value, guardians of their minor children, and full legal persons. These provisions, while common if not identically configured throughout the north (a point Schmidt acknowledges but does not emphasize), were well articulated and honored in Leiden, with the result that widows formally had the requisite status to assume their assigned roles. But, as Schmidt shows, this status did not guarantee an easy widowhood.

Widows sometimes took up the trade of their husbands, both in crafts formally providing widow's rights, but also in businesses where they might have worked alongside their husbands during marriage. A great many widows were employed, however, as wage laborers, especially in textiles, sometimes carrying on a trade they had practiced as wives, at other times taking up the craft because the estates they had inherited could not support them. Extended families helped destitute widows, but such women only rarely became dependents of their kin and still less often resided with them, not even with grown children; instead, widows maintained their own households, joined with other single women, took in lodgers, etc. Some institutions, in the beginning of the period almost exclusively municipal, provided aid to poor widows, and as the seventeenth century gave way to the eighteenth, some professions (such as preachers and teachers) and a few crafts (the shippers, for example) established pensions for the widows their members had left behind. The incidence of poverty among widows was thus unevenly distributed across the social spectrum. Widows of craftsmen and of those men whose businesses generated capital tended to escape want and maintain economic rank commensurate with the social standing they had enjoyed as wives. In contrast, widows of men who had depended on a wage fared much less well, for the estates they inherited were meager and until the later centuries they had no alternative institutionalized support networks.

Remarriage patterns were similarly uneven. While Protestant Leiden, like Catholic Europe had historically done, expressed ambivalence about remarriage and even instituted rules requiring a minimum period of mourning, it was generally accepted that widows remarry, and they regularly did so. But the very rich and the very poor did so at rates considerably below those of women in the middle. Schmidt does a good job of showing why women of this large "middling class" would have sought marriage, and why they would have made attractive marriage prospects for men of that class: for the men, a woman with a shop, craft rights, or the skills to manage either was an ideal wife; the woman's ideal was a man to take over the responsibilities of head of household and supplement her

income. Rich women, Schmidt implies, did not need these protections, and poor women could not attract the men who could provide them.

Schmidt also provides some suggestive data about trade endogamy, measured by the rate of intra-occupational remarriage. Although her presentation is a bit confused, and the data not sufficiently rich (or sufficiently well detailed) to allow firm conclusions, it appears that widows of craftsmen and of the richest merchant-entrepreneurs were more likely to remarry than most other widows. This is unsurprising information, but the data also hints that widows of men in crafts providing widow's rights remarried less often than did widows of craftsmen whose trades granted no such privileges and, more surprising still, that few occupational sectors (bakers being one) exhibited a high degree of intra-occupational remarriage.<sup>1</sup>

All this information is framed by two principal issues. First, whether women in Leiden – indeed in Dutch cities generally in this period – were “free” as women elsewhere were not, a claim advanced by more than one contemporary observer. Schmidt answers this by emphasizing the second element of her frame – the role of the nuclear household in the political, social, and economic structure of Leiden. Women co-headed a nuclear household that functioned in this commercial and industrial society as the center of property accumulation, management and distribution. As such, they were subsumed by the family – responsible to it throughout their lives and as widows also responsible for it. Their “freedom”, Schmidt rightly insists, was the effect of these responsibilities.

In this Schmidt is corroborating and giving texture to an argument long circulating in the scholarly literature. Although acknowledging many of those who have come before her, she seldom engages this literature, typically using it instead simply as a source for comparative data or calling attention to discrepancies between her findings and those of her predecessors. She does not, by and large, take considered account of the prior arguments (and the often different uses to the data was put) or ask why patterns elsewhere might have been different.

1. Schmidt's data is not robust and is not well enough organized to be entirely convincing. She is able to show, for example, that widows of craftsmen without widows' rights probably remarried more often than their likely representation in the universe of widows would suggest, but the actual numbers are very small (30 widows out of 314 in a sample of 5 years taken over 5 decades). Furthermore, in discussing remarriage within trades she implicitly assumes (but does not provide evidence) that widows had the same range of occupational choices among new spouses that single women did. This need not have been true. Is it not possible, for example, that craftsmen would have preferred first-time brides, while their widows would have attracted a disproportionate number of men *without* craft rights? Or, still a bit more fancifully but not entirely unreasonably, might such widows have attracted only journeymen in the craft who were seeking a mastership, not masters themselves? In that case the intracraft remarriage rate for such widows, although unexceptional in percentage terms, would mask a high degree of a particular kind of trade endogamy. Finally, although she provides a scattered discussion (mostly in her chapter on work) about the precise (and variable) definition of widows' rights from craft to craft and about the hierarchies within occupational sectors, she does not give us specific enough information of this kind when presenting the remarriage data. “Textile workers”, for example, who make up almost 40 per cent of her remarrying widows (Tables 7.6. and 7.7), comprise an extremely diverse group sociologically, as she points out. Why then lump these widows together if one is seeking to reveal patterns governed precisely by sociological difference that would be obscured by such a broad category?

This reluctance to move from the sources to their broader meaning permeates the text. The book begins, for example, with a chapter sketching the contradictory stereotypes of widows drawn by the cultural texts that circulated in early modern Leiden (as throughout the north) – the lusty, lustful, and uncontrolled widow on the one hand, and the pious, dutiful, and loyal one on the other. Other than pointing out the contradictions and the difficulties any widow faced in trying to live the preferred stereotype, Schmidt does little with this material, allowing it to sit, inert. To be sure, the relationship between text and experience is difficult to unravel. But the last generation of cultural historians, many of them Schmidt's own compatriots (Herman Pleij, for example) have provided some useful models, and her book would have been strengthened had she made a more strenuous effort to render these texts part of her analysis rather than simply a pendant to it.

More troubling, however, are the refusals to deploy the rich social theory that gave birth to and still fuels the best social history, from Pirenne to the *Annales* to today. For example, while we are given a clear picture of family structure and widows' relationship to it, we are offered only the most meager discussion of why this social form existed or performed as it did in Leiden – what made it the ideal, with what other kinds of families did it compete, what political structure supported it, whose interests did it serve. Admittedly, it would be unfair to ask Schmidt for the study she did not intend, for this is not a history of the Dutch family. But she might easily have drawn on the rich sociological and historical literature to help readers understand the conditions that made the nuclear family the core of Leiden's community. We would also have benefited from a more thorough exploration of the early modern economy in Leiden. What conflux of events produced an economy so privileging the family structure? What institutional features of the city's *ambachten* and *neringen* made them more or less hospitable to women? What made this period in Leiden's economy different from its past, different from other cities in the region – or is its difference not the point? Again, we cannot fairly ask for an economic history as such, but it is fair to ask Schmidt do more than make only casual references to the structure and health of the economy in her study of women's work. Finally, she might have been more sensitive to gender itself. Schmidt does a fine job of locating the patterns of widows' experiences in family and of mapping the treacherous terrain of widowhood, but she never seeks to expose the tensions thus unleashed, whether in gender relations, in the definition of femininity or masculinity, or in way these ideologies were expressed.

The result of this reluctance is a static history, a portrait of a moment that is not well differentiated from others separated by time or space, and one that seems to have no past, and presage no future. While the virtues of the book are real, they reside almost exclusively in the reliability of the data unearthed and organized for us. So sound an empirical study deserves a sturdier conceptual frame.

Martha Howell

PARTHASARATHI, PRASANNAN. *The Transition to a Colonial Economy. Weavers, Merchants and Kings in South India 1720–1800*. [Cambridge Studies in Indian History and Society, vol. 7.] Cambridge University Press, Cambridge [etc.] 2001. xii, 165 pp. £37.00; \$54.95; DOI: 10.1017/S0020859002030845

This work is an important contribution to the “revisionist” historiography of

eighteenth-century India. Its focus is the position of labour, particularly in the South Indian textile industry. Contrary to earlier assumptions that Indian labour was in ample supply and was always poor and exploited, Parthasarathi shows that Indian weavers were in a better bargaining position in the early eighteenth century, while their options were severely reduced by the growing power of the English East India Company (EICo), which disciplined labour, inhibited its mobility, and strengthened the legal position of merchants in their transactions with the producers. The second part of the book contains convincing accounts of weaver solidarity in protest movements and during production stoppages. Parthasarathi contrasts the limited powers of intervention enjoyed by Indian rulers with the increasing intensity of EICo control. He could have found additional evidence in the work of Sergio Aiolfi (*Calicos und gedrucktes Zeug. Die Entwicklung der englischen Textilveredelung und der Tuchhandel der East India Company, 1650–1750* (Stuttgart, 1987)), who has documented how – even in recruiting European soldiers – the EICo ensured there were weavers and other textile workers among them who would be helpful in giving “advice” to Indian artisans. Aiolfi has also shown how white Indian cotton cloth increasingly became an essential input for the London cotton printers. This caused a massive shift in procurement from South India to Bengal in the first half of the eighteenth century, a phenomenon that seems to have escaped Parthasarathi’s attention.

Parthasarathi provides an interesting estimate of the share of British and Dutch exports in the total production of the South Indian textile industry and arrives at 22 per cent for the period 1700 to 1725 (appendix 2.4, pp. 73–77). If this is true, the production under European control was not as large as to affect the overall structure of the industry. Presumably this share increased in the second half of the eighteenth century. However, Parthasarathi provides no further estimates, even though these would have been relevant to his argument concerning the deteriorating position of the weavers due to the growing power of the EICo. Actually this growing power affected Bengal more than South India. As shown by Konrad Specker (*Weber im Wettbewerb. Das Schicksal des südindischen Textilhandwerkes im 19. Jahrhundert* (Stuttgart, 1984)), the South Indian weavers could still compete with textiles imported from Great Britain in the first half of the nineteenth century. Cheap food and cheap cotton enabled them to make both ends meet. Of course, this was also due to the fact that there was rampant deflation at the time because the British drained the silver from India which they had earlier pumped into it when they bought Indian textiles. However, cheap food and cheap cotton had actually provided the Indian textile industry with a comparative advantage in the eighteenth century, as Parthasarathi has pointed out (pp. 43ff.). He even indicates that peasants did not find cotton cultivation very remunerative (p. 75), but that they planted it because they could get credit for it and because it helped them to spread risks.

As an economic history Parthasarathi’s work is somewhat sketchy, but as an innovative contribution to Indian social history it deserves great praise. Although he does not belong to the school of “subaltern studies”, Parthasarathi has written a book that could well rank as one of the best of its type. He has given a voice to the South Indian weavers, who were very articulate in describing their grievances and resourceful in organizing their protests. When analysing the British measures to discipline Indian labour, Parthasarathi also looks at the contemporary British scene and shows how labour regulations and vagrancy laws – which restricted the mobility of labour – could serve as a precedent for the actions of the colonial rulers. The thrust of his argument that the impact of colonial rule smothered

Indian initiatives like a wet blanket deserves attention. The book will surely initiate a lively debate among historians and social scientists with an interest in India.

Dietmar Rothermund

MARGO, ROBERT A. *Wages and Labor Markets in the United States, 1820–1860*. [NBER Series on Long-term Factors in Economic Development.] The University of Chicago Press, Chicago [etc.] 2000. xii, 200 pp. \$28.00; £20.00; DOI: 10.1017/S0020859002040841

In *Wages and Labor Markets in the United States, 1820–1860*, Robert A. Margo aims primarily “to improve the measurement of the numerator of the real wage index, that is, the measurement of nominal wages” (p. 24). That goal may sound dull, or trivial, or worse. I promise it is not. Offering historical time series and interstate cross-sectional evidence on wages from two rich and newly coded data sources, Margo is able to confront directly the competing stories about the performance of antebellum labor markets. Understanding labor, about two thirds of a nation’s annual income, is fundamental to understanding economic history. Margo’s contribution is fundamental.

The slim volume betrays the magnitude and the sophistication of the historical research contained. Though offered with a humility that would make St Francis blush, the book can be seen as a kind of wink-and-nudge at the Webb-like tome; in 181 pages the author deftly dismisses 120 years of evidence and arguments while rewriting with his own and better evidence the quantitative history of antebellum labor markets. (The book also presents, like a publishing-house bonus, a fascinating econometric study of the California Gold Rush, important in its own right (ch. 6).)

The new data concern the wages of “free labor” in the United States, 1820–1860. The sources are the 1850 and 1860 manuscript censuses of social statistics (pp. 30–35) and the payroll records of civilians hired by the United States Army (*Reports of Persons and Articles Hired*) (pp. 25–30). Taken together, the census and army civilian data enable Margo to document and analyze wages for more years, more states, and more occupations than have previous data sources (pp. 29–31). These are the chief advantages.

What does he find? Margo’s evidence suggests, in his words, that “pessimistic” and “optimistic” perspectives contain partial truths, but that on balance the evidence warrants a new interpretation. In Margo’s formulation, the “optimist” believes that “markets or market-like processes were reasonably effective in responding to economic change”, that “growth in antebellum living standards was generally steady and that most in the working class shared the gains” (p. 2). Optimists, in other words, *believe in the market*. “Pessimists assert”, on the contrary, that nominal wages did not rise with the price level in the mid-1830s and early 1850s, “producing declines in real wages and [...] a wave of strikes and labor agitation”. The influx of immigrant workers in the 1840s drove wages down further, say the pessimists, creating what Robert Fogel has called a “hidden depression”. Both pessimists and optimists believe there was a significant change in the relative demand for skilled and unskilled wage labor, creating, though without consensus on magnitudes, an increase in the skilled–unskilled wage differential.

Margo finds that antebellum labor markets were most effective when “facilitating the shift of labor out of agriculture” (p. 153), Deep South to the Middle West. He finds they were “reasonably effective” when economic development required geographic mobility of

workers, but they were “least effective” in keeping the wage structure intact. (Margo does not say why the wage distribution of, say, the 1820s would be preferred; in any case, he does not propose an explicit standard for judging a wage distribution.) The evidence suggests that real wages grew most rapidly for white-collar workers and more rapidly for common laborers than for artisans – a finding that “supports” the conventional notion that the factory system displaced the artisan workforce. In the long run, Margo finds, “the benefits of antebellum economic development were manifested in higher real wages on average”. Gains in output, he says, in fact “trickled down”. Still, he is careful (I do not mean with the cliché) to emphasize that relative gains obtained by white-collar labor in the early nineteenth century were temporary. Returns to white-collar labor were reduced by the surge in supply of “educated labor” in the early twentieth century (p. 159). So far, Margo’s is mostly an “optimistic” story. Yet – more in line with the camp of pessimists – the episodic and sometimes sharp declines in real wages led some workers to seek out poor relief (especially, Margo believes, between 1850 and 1860), while other workers sought retribution or justice.

The book is not flawless. The first half of chapter 2, “The Growth of Wages in Antebellum America: A Review”, is a soporific tour through the slough of historical and contemporary attempts to describe and explain well the growth of wages in antebellum America (pp. 6–23). Nabokov, and a fellow Russian refugee, the economic historian Alexander Gerschenkron, could not agree on how to translate Pushkin (Nicholas Dawidoff, *The Fly Swatter: How My Grandfather Made His Way in the World* (New York, 2002), pp. 190–191). (Nabokov lusted the literal translation.) But Nabokov and Gerschenkron agreed that the quality of any work could be seen in the way a writer handles details. Margo’s review of previous research confuses readers with its plodding arrangement and insufficient attention to milestones. Still, Margo eventually reveals the interpretative importance of the details in wage and price measurements and in a way that far surpasses other attempts. A good example is his discussion of the David–Solar real wage index (p. 22). The story suggested by the David–Solar index, Margo finds, is “considerably” different from others: “According to the David–Solar index, the late 1830s witnessed a spectacular jump in real wages. However, the path followed by real wages in the early 1840s is influenced by the price deflator: if [their] price index is used, real wages fell, but, if the Williamson–Lindert price index is used, they rose.” (p. 22) The other shoe drops when you look at the 1850s: David–Solar prices show “robust” growth (about 14 per cent) between the periods 1846–1850 and 1856–1860 while Williamson–Lindert prices show “stagnation”. Margo shows that this one detail, one of several details causing the different points of view, is a “consequence” of using “Vermont prices” in the David–Solar index to interpolate real wages, prices (from one state only) which show “much greater decline” than the other series. (Margo is able to construct a multistate price deflator.) The case of the Vermont price deflator is one instance among many in which Margo strives nobly for exactness in detail, and gets it.

Margo may be exaggerating the economic significance of some estimates. In a section called “Real Wages and the Antebellum Welfare Explosion, 1850–1860” (pp. 152f.), Margo attempts to connect his new finding of short-run decline in real wages with new evidence on the rates of pauperism – what Margo calls “welfare usage”, the percentage of the population receiving public assistance. (The pauper data are available in the 1850 and 1860 censuses of social statistics). Summarizing his previous work with Lynne Kiesling, Margo observes: “[a]ccording to the 1850 census, 5.8 per 1,000 persons received public assistance

at some point during the year. By 1860, the rate had jumped to 10.2 per 1,000 persons, an increase”, he says, “of 76 percent” (p. 152; L. Kiesling and R.A. Margo, “Explaining the Rise in Antebellum Pauperism, 1850–1860”, *Quarterly Review of Economics and Finance*, 37 (1997), pp. 405–417). Various and throughout the book, Margo deems the increase in welfare usage “marked”, “a substantial rise”, an “explosion” (pp. 2, 152). But Margo may have better described welfare usage like Mark Twain did in his *Life on the Mississippi* (New York, 1946), speaking of the size of cities: “When I was born [the city of] St. Paul [Minnesota] had a population of three persons; Minneapolis [Twain quipped] had just a third as many” (p. 390). A pauperism rate exploding to 1.2 per cent is tiny in comparison with the 6 to 7 per cent of antebellum New York, or the 4 per cent of 1870s Indianapolis, or the 5 per cent of the 1990s nationwide, (and these tally outdoor relief only).

A bigger problem derives I think from Margo’s interpretation of the correlation between wages and welfare usage. On p. 152 he cites supply side and market reasons for the swelling welfare rolls, reasons such as lack of access to capital markets, and lack of opportunities in self-employment in agriculture “that might have smoothed consumption in response to a short-run economic downturn”. He argues that “fully 30 percent of the rise in [welfare usage] can be attributed to a fall in the average real wage”, more, he says, “than any other single factor” (p. 153). But there are variables omitted from the analysis that when included could easily diminish the observed correlation. To take just one example, the 1850 and 1860 census data do not say what share of the public assistance was “outdoor relief” (relief in food or cash or fuel that was consumed in one’s own household) and what share was “indoor” (primarily, in the United States, that was the fully institutionalized relief to the poor living in an almshouse.) Yet the share of men and women in almshouses that were able-bodied (and therefore able to work) was in 1850 apparently not much more than a third, a number that would fall in trend to 7 per cent by the early twentieth century as the almshouse became a home for the aged, feeble-minded, and physically disabled who lacked friends and family (most, it turns out, were never married; S.T. Ziliak, “Pauper Fiction in Economic Science: Paupers in Almshouses and the Odd Fit of *Oliver Twist*”, *Review of Social Economy*, 60 (2002), pp. 159–181). In other words, to the extent that relief was given to the unmarried, aged, feeble-minded, and physically disabled poor in almshouses, Margo is attributing too much of the demand for pauper relief to the decline in real wages. Other reasons could be cited.

Readers of this journal will look in vain for any attention to rhetoric, power, politics, and the “multiple meanings, ambiguities, and historical ironies” of the very idea of “free labor” that social historians have come to expect through works such as Jonathan Glickstein’s *Concepts of Free Labor in Antebellum America* (New Haven, CT, 1991), p. 1; and David Montgomery’s *Citizen Worker: The Experience of Workers in the United States with Democracy and the Free Market During the Nineteenth Century* (Cambridge, 1993). Likewise, students of the industrial revolution will wonder how to situate Margo’s findings in the larger debates about the institutions of relative backwardness and what Gerschenkron called “the great spurts” in a nation’s growth. The optimistic response is that Margo has contributed data and analysis that will be fundamental to that future research. A pessimistic response is that the National Bureau of Economic Research (the NBER) is still not prepared to publish a book that seriously examines rhetoric and power in the context of an empirical inquiry; in other words, the NBER is not prepared.

Still, an accomplished jazz and classical guitarist will be invited (as Margo has) to play at the “Chet Atkins Appreciation Concert”. This book makes strong claim to the idea that an

accomplished historian of antebellum labor should one day be invited to speak at a “Bob Margo Appreciation Conference”.

*Stephen T. Ziliak*

PERKINS, MAUREEN. *The Reform of Time. Magic and Modernity*. Pluto Press, London [etc.] 2001. ix, 158 pp. Ill. £45.00. (Paper: £14.99); DOI: 10.1017/S0020859002050848

This book discusses the process by which the belief that each individual has the potential to shape his own future replaced the conviction that human fate was preordained. Perkins shows how, in the nineteenth century, under the banner of “progress”, an all-out attack was launched on traditional views by labelling them as superstitious. “Truth” replaced “error”, which meant that a “rationalized” concept of causality, as defined by Max Weber, overcame a popular worldview typified by magic and other non-natural forces. In her analysis of this extremely complex and intricate process, Perkins confines herself to one of its major aspects: the reform – in Britain – of the perception and understanding of time and temporality.

She identifies the global recognition in 1884 of the meridian passing through Greenwich as prime meridian as a good symbol of modernization because it set the clock for the expansion of coordinated time schedules and arithmetic in general. It was consistent with a development that had set in more than a century earlier and which strongly altered the character and use of the calendar. The acceptance of the Gregorian calendar in Britain in 1752 has a comparable symbolic meaning, as it divested time measurement of its religious character. Since then, many traditional feast-days, both ecclesiastical and secular, have lapsed. Almanacs, those lists of important dates, which were bought every year and used extensively by people at all levels of society, underwent a considerable change when it became possible to insert advertisements, which were, as Perkins puts it, “designed to encourage the consumer to buy future security”.

Scientific methods and techniques were presented as the only possible way to predict what was to come. But to claim that one was able to predict the future on a scientific basis could be rather hazardous, as the first head of the new government department of meteorology in Britain found out. He committed suicide in 1865 when too many of his weather forecasts turned out to be disastrously wrong. Fortune tellers and astrologers certainly did not meet the standards set for meteorologists, but they nevertheless continued to draw multitudes of customers, particularly young maidservants who were anxious to know whether they would find suitable husbands. Organizations like the Society for the Suppression of Vice, which was founded in 1802, lodged official complaints against these wizards in order to suppress their practices. The idea that in their dreams people often saw their own future, or that of others, predicted was another aspect of popular culture that sincerely bothered members of the educated urban middle class. Arguing that when people dream they cannot control themselves, and that the thinking capacities of a person who could not distinguish dreams from reality were similar to those of animals, reformers attempted to convince members of other social strata not to attach any credence to what they thought they were experiencing during sleep. They also attacked the use of the widely read chapbooks that offered techniques to understand dreams or forecast the future. The idea that the future was already fixed and pre-ordained was contrary to the typical

middle-class value of individualism. Cultures that had no concept of time and change were seen as backward. In the eyes of the British colonizers, the natives they met in Asia and Africa often lacked insight into the fact that – in time – everything changes. But it was also contended that in Great Britain the Irish and the gypsies were devoid of this concept. Perkins gives a number of similar examples that show how in the nineteenth century a new perception of time was propagated. The cases she presents are fascinating, and reason alone to read her book. It is clear that a serious attempt was made in nineteenth-century Britain to disenchant time.

But is this sufficient basis for Perkins's contention that it was in that period that popular culture and its inherent fatalism were genuinely weakened by the spread of the art of time reckoning? Offensives against popular culture were also launched in the early modern period, especially during the Reformation and the Counter-Reformation. And in the early seventeenth century too arithmetic and mathematics were highly respected crafts in the Dutch Republic. One reason why the witchcraft trials there ceased so early on was the growing awareness that the world was, as it were, computable. The merchants in the city were not the only entrepreneurs; so too were Dutch farmers in the countryside, who had already abandoned the autarchic peasant model a century earlier and opted for specialization and a "rational" way of conducting their business. In the seventeenth century, almanacs, which were just as popular in the Netherlands as they were in Britain, acquired a whole new outlook. Astrology, for instance, lost the paramount importance it had had in former times. It seems that this change, which Perkins locates in the case of Britain in the nineteenth century, was already well under way two centuries before in the Netherlands. This does not mean, however, that Dutch culture stood alone in this. Perkins is familiar with the excellent work of Patrick Curry on the history of British almanacs and the importance of astrology in those chapbooks. Curry has argued that in British almanacs astrology lost much of its credibility in the second half of the seventeenth century.

Another point of critique concerns the lack of definitions. "Magic" is a complex and slippery subject. The belief in "magic" plays a central role in this book, but Perkins has omitted to offer any sort of definition. She repeatedly uses it in conjunction with the word "superstition", and she herself emphasizes (p. 13) that "Wherever those who hold one belief dismiss another as superstition, it is certain that the interconnectedness between knowledge and power may be seen". She is clearly aware of the political content of words such as "superstition", so it is strange that she fails to clarify how she defines "magic". The location of the boundary between magic and religion is always the outcome of a political debate. In early modern Europe Protestants scolded Catholic priests, accusing them of being magicians, not only because the latter claimed that they could change the substance of bread and wine, but also because they were often willing to exorcize demons and heal the sick by applying all sorts of sacramentals.

Perkins's book is without doubt a very interesting attempt to survey the impact that scientific innovation can have on the culture of specific groups. But its conceptual basis is rather unclear, and its temporal and geographical limits are too narrow. It would have benefited greatly from a comparison with developments at other times and places.

*Hans de Waardt*

KELLY, BRIAN. *Race, Class, and Power in the Alabama Coalfields, 1908–21*. [The Working Class in American History.] University of Illinois Press, Urbana [etc.] 2001. ix, 265 pp. Ill. \$49.95. (Paper: \$19.95); DOI: 10.1017/S0020859002060844

Brian Kelly argues that those primarily responsible for the promulgation and exploitation of racial antagonism in the labor relations of the Alabama coal fields during the first two decades of the twentieth century were the mine operators and other business interests, along with civic and political elites, both white and black. White miners and trade unionists, he readily acknowledges, embraced white supremacy and segregation. But when miners sought through the agency of the United Mine Workers (UMW) to improve their material conditions and free themselves from arbitrary social control in the coal camps, common class interests trumped racial antagonism. While neither the white rank-and-file nor the national UMW leadership abandoned white supremacy, Kelly believes that “nowhere in the early twentieth-century South were the traditions of racial protocol challenged more forcefully than in the Alabama coalfields” (p. 188).

Based on resourceful and exhaustive research in corporate, governmental, and labor materials, Kelly’s account of race and labor in the Alabama coalfields joins Daniel Letwin’s *The Challenge of Interracial Unionism: Alabama Coal Miners, 1878–1921* (Chapel Hill, NC, 1998) in chronicling the struggle for union in the modern South. Letwin’s book, however, concentrates on the period culminating in the UMW’s 1908 strike defeat, whereas Kelly focuses attention on the post-strike efforts of employers to create a stable, union-free environment. *Race, Class, and Power* provides rich detail on the efforts of leading Birmingham-area firms, notably Tennessee Coal and Iron, to create and maintain a labor force that was at once skilled, efficient, and docile. While black mine workers were considered inferior in intelligence and productivity, racial discrimination and the repression ubiquitous in the turn-of-the-century South enabled operators to keep wage rates low, a crucial consideration in a region whose coal veins required labor-intensive exploitation. Even as they recruited black workers and exploited racial antagonisms to keep miners in line and their wages low, TCI and other leading operators attempted to stabilize the coal camps through programs of welfare capitalism, sponsoring churches, schools, and medical services designed to provide a better deal for black miners than was available in the cotton fields or in the urban trades. These programs, in which members of the Birmingham area’s small black middle class were prominent, invariably encouraged dependence on the coal operators and discouraged dissent, union sentiment, or overly ambitious hopes of individual advance. Both the chronic repression and the welfare programs rested upon and sought to sustain the prevailing white supremacist racial order.

The culmination of Kelly’s book is the revival of the UMW during World War I and the great 1921 Alabama coalfields strike, another brutal defeat for the union. Fully cognizant of the limitations in the union’s conception of interracial organization, Kelly nonetheless stresses the uniqueness of the UMW’s efforts. The very logic of race-based exploitation impelled at least a minority of white workers to attempt to revive the South’s sporadic, but by no means insubstantial, traditions of class-based, interracial social protest. To him, it seems perverse to single out a beleaguered labor organization for reflecting attitudes and adopting policies that often short-changed black members. What other agency or institution in the progressive era South even contemplated interracial activism – the churches? The business community? The press? Academia? Moreover, in Kelly’s rendering, white miners and UMW organizers were capable on occasion of transcending

narrow calculations of self-interest in their recruitment of and collaboration with black miners.

*Race, Class, and Power* joins in the debate over the relationship between organized labor and African Americans in modern America. In his extensive notes in particular, Kelly takes issue with the “wage of whiteness” school of analysis, as well as with the related perspective of union critic Herbert Hill. For all the UMW’s limitations, Kelly believes, the miners’ union helped keep alive a sense of class-based interracial struggle that, despite the defeats in 1908 and 1921, bore fruit in the CIO upheaval of the 1930s. In his stress on employers’ responsibility for promoting and sustaining a subordinative social order he also challenges those who see in “free-market” capitalism a profit-seeking logic that benefitted black workers. Progressive-era unions, as Kelly’s scathing account of the racial politics of Birmingham’s craft unions unflinchingly demonstrates, all too often reflected and endorsed the era’s racism. But in the end, at least in the Alabama coalfields, it was so-called “enlightened” employers who both defended and exploited the structures of racial subordination, and the UMW alone that provided an outlet for black activism and a glimpse, however fleeting, of genuine biracial progress.

Robert H. Zieger

BOLLAND, O. NIGEL. *The Politics of Labour in the British Caribbean: The Social Origins of Authoritarianism and Democracy in the Labour Movement*. Ian Randle Publishers, Kingston; James Curry, Oxford; Markus Wiener Publishers, Princeton 2001. xxii, 696 pp. £25.00; DOI: 10.1017/S0020859002070840

*The Politics of Labour* is an example of classic encyclopaedic history-writing that has now become rare. Its size, about 700 pages, and thoroughness remind us of the good old nineteenth-century historians who wrote for a public with plenty of leisure. It is a monumental narrative covering a dozen tiny islands, the largest being Jamaica, and Guyana and Belize on the mainland. They shared one colonial master, Great Britain, and share the burden of a violent and oppressive history of slavery and indentured labour. Though the West Indian nations were able to establish more or less democratic institutions, the culture of oppression has left its authoritarian stamp on these societies. It is a ghost that has been haunting the Caribbean since the “Black Jacobins” of St Domingue, present-day Haiti, liberated themselves from slavery only to create one of the most pitiful and authoritarian places on earth.

Caribbean scholarship produced the now-classic concept of “plantation society”, marked by monocrop production and a social stratification that is highly correlated with racial inequality. Over the past two decades the gradualness of the transition from slavery to post-abolition societies in the Caribbean has been underscored, notably in the work by Rebecca Scott and Thomas Holt. Synthesizing old and new research, Bolland takes a long-term perspective to demonstrate how the class and racial structures of the eighteenth and nineteenth centuries are still relevant categories in the twenty-first century. The institution of slavery, abolished in the 1830s, continued seamlessly in systems of indentured labour. Former slaves who often had no access to land lived on in servitude for decades, as the premises on the plantations at least offered some shelter. In an increasingly monopolistic environment, peasantization (i.e. giving small plots of land to proletarian labour) and

extensive supplies of indentured labour became the prime instruments to depress wages. This made the liberal meaning of freedom irrelevant for Caribbean former slaves: freedom meant merely a shift in the locus of authority, from the slave master to the state, and later on to the hegemony of the United States.

Yet it is at this point that uneasiness about Bolland's narrative starts to creep in. It is true that falling sugar prices reinforced the basic structures of the plantation economy in the Caribbean. But this very process of concentration in plantation capitalism brought about a differentiation of social trajectories. Trinidad, Guyana and Barbados survived as sugar producers, and here unfree labour conditions (indentured labour) continued to exist until 1921. However, Jamaica – where one-third of the British Caribbean population lived – lost most of its sugar industry in the 1840s, long before the relentless rationalization and takeover by metropolitan capitalism of the last fifteen years of the nineteenth century. And even where King Sugar reigned, it did not forge a single-type plantation society with predictable labour relations. Nearby Cuba, for example, followed a trajectory in which the race–class correlation was less sharp than in the British Caribbean.

Whereas Bolland underscores the unity of labour history in the West Indies, one could also argue that these trajectories are markedly different. Bolland begins to explain how labour *and* peasant resistance developed into a West Indian identity in the years 1934–1939, but it is a fragmented narrative. Labour rebellions in Trinidad brought about a united front of Indian sugar workers and urban African workers. Elsewhere, notably in Jamaica, the heritage of Marcus Garvey and Pan-Africanism put its stamp on class formation. The politics of identity were predicated upon the question whether or not a society had important Indian or other minorities, and this in turn was dependent upon the existence of a developed sugar industry. Whereas in Jamaica heavy taxation on land was a source of resistance, in Guyana and St Kitts the revolts were a response to the slump in sugar prices of the depression years. Precisely because race and class-consciousness became the basis for movements of the working poor in the 1930s, I cannot see how these would converge into a politics of identity or into an idea of a “West Indian identity”. This “identity” is probably more an aspiration of the elites trying to escape from their insular parochialism rather than a popular sentiment. It has given the islands a university and an airline company but not a viable federal structure. Therefore Bolland's belief in the unity of the 1934–1939 revolts has produced a somewhat artificial synthesis. He includes for example his own country Belize – not a sugar colony, and situated on the mainland of Central America – in his account of the 1934–1939 rebellions. It seems, however, that he is not entirely convinced himself, as he feels the need to underscore the fact that Great Britain too considered Belize to be part of the West Indies. It is a quaint argument from a labour perspective.

Bolland's comprehensive account of the 1934–1939 rebellions is nonetheless an important scholarly contribution. It is a sometimes passionate reconstruction of how more or less spontaneous labour resistance developed into movements for economic empowerment and political independence. It was during this period that C.L.R. James wrote his *The Black Jacobins* (London, 1938), and Sir Arthur Lewis submitted his report “The Labour Movement Is on the March” to the Fabian Society. A West Indies Federation had already been proposed in the nineteenth century, but during the late 1930s the dream of a unified democratic West Indies became the aspiration of a mass movement. The ideal was carried on by the Caribbean Labour Congress of 1945–1952, though it became diluted as its leadership reached out to Dutch and French Caribbean territories and participated

prominently in Pan-Africanist meetings in the metropole. In the latter part of his book Bolland narrows his narrative to institutions and leading personalities, who were caught in the middle of three often conflicting objectives: of strengthening their popular basis, of wrestling political power from the metropole, and of manoeuvring themselves into a bargaining position good enough to gain independence as the West Indian Federation.

In the 1950s the labour unions acted as organized bases for nationalist parties. But most of these machineries were autocratically managed. Bolland claims that the social structure of these societies fostered authoritarian culture, but one might object that the West Indies was not the only place where political leaders tended to make labour unionism subservient to nation-building. His conclusion that decolonization in the West Indies was a transition from one authoritarian system to another applies to almost every decolonized country in the world. What is far more interesting is that in the West Indies a new culture of radicalism developed outside political structures, as these had become solidly male, middle-class, and authoritarian in the 1960s and 1970s. Women had played a visible role in the 1934–1939 rebellions, but they became marginalized in the institutionalized labour movement. Bolland devotes just a few lines to their exclusion. After the Thompsonian moment of working-class formation, in the 1920s, his book concentrates on the politics of *organized* labour.

Having highlighted the book's limitations, I would still conclude that there is nothing wrong with a solid political history of the role of organized labour and its leadership during the process of decolonization.

*Ulbe Bosma*

WOODHAMS, STEPHEN. *History in the Making*. Raymond Williams, Edward Thompson and Radical Intellectuals 1936–1956. Merlin Press, London; Fernwood Publishing, Halifax; Pluto Press Australia, Sydney [etc.] 2001. viii, 221 pp. £30.00; \$49.95. (Paper: £14.95; \$21.50); DOI: 10.1017/S0020859002080847

This is a collective biography which seeks to fuse contextual history with the lives of its central characters – Raymond Williams and E.P. Thompson in particular. Woodhams's contention is that this generation of British Marxists inherited a "moral socialism" which was formed during the nineteenth century. He examines the interwar formative experiences of his subjects and their experiences of the Second World War. The narrative and analysis then turn to such media as the adult-education movement, the Communist Party, and the Campaign for Nuclear Disarmament. Woodhams's emphasis throughout is on culture and cultural milieu, but it is a very restricted focus even in its own terms. In this account the Communist Party is an inheritor of the puritan tradition. Its members – a sort of elect – bear witness, give leadership, and set an example. They are given to hard work, self-improvement, and respectability – citizens who maintain civility and even decorum in their dealings with one another. The "Celtic fringe" makes a significant contribution to this Party culture, particularly Wales in Woodhams's telling of the story (though there were many more Scots in the Party than Welsh). The purpose of all of this is to situate and explain the intellectual interests and driving forces of Woodhams's principal characters, and there is much to be said for stressing these ethical themes. In the process, however, much of what the well-scrubbed, disciplined, ascetic in shirt and tie did and stood for is

forgotten. Lenin was above all determined on power and ready to use it. So were his followers. Moral outrage is only part of the story. But even the impressive array of cultural struggles actually waged by the Left in everything from theatre, music, and film to literature, historiography, and social science (in local branches, trade unions, trades councils, etc.) rarely receive so much as a mention.

The same limitations can be said to affect Woodhams's discussion of "1956", the *Reasoner*, the crisis in the Communist Party, and the Suez Affair. Though there is nothing new in the history disclosed by this book, the author labours hard to see these events through the lens of his "moral" thesis. Sometimes it seems plausible. The Christian, pacifist, "moral outrage" origins of CND makes this task relatively easy, for example. But there is a tendentiousness and arbitrariness about much of what Woodhams chooses to address. His own intellectual preferences become more useful when he is able to focus on aspects of the New Left, as it unfolded after 1956. A useful discussion of its critique of contemporary capitalism is part of this account as is an analysis of the tensions and arguments which divided Left opinion on this question, particularly in relation to its cultural dimensions. These concerns often comfortably fit into the book's central theme and Woodhams's commentary is at its most perceptive in these sections of the book. Unfortunately, most of this has already been treated more exhaustively in several recent specialist studies of the New Left such as that by Michael Kenny, *The First New Left: British Intellectuals after Stalin* (London, 1995).

The novelty of this book rests on insubstantial foundations. Unlike Gary Werskey's collective biography of communist scientists – *The Visible College* (London, 1978) – which is mentioned in chapter two, Woodhams has no clear biographical focus. His attempted collective biography simply fails to materialize for all but Raymond Williams and (to a lesser extent) E.P. Thompson, though there are numerous walk-on parts for others. While Werskey made coherent connections between science and socialism in the context of his subjects' lives and their historical context, Woodhams can only invoke a nebulous ethical tradition. If this had been as rooted in a past "structure of feeling" as Woodhams would have us believe, the real problem would be explaining why so few socialists emerged out of it. His conclusion is also of doubtful utility; that is, that the moral socialism which fired the Marxist leaders of the first New Left exhausted itself in the second half of the twentieth century, in other words not long after the New Left came into existence. This may well contain an element of truth but we are unlikely to explain it by an exclusive focus on Britain. Woodhams maintains that the conditions which created the Williams–Thompson generation of socialists no longer exist. But the same could be said for their continental European counterparts. We must look for explanations for this "coincidence". In this book the decline is blamed in part on the failure of the British Left – especially the Labour Party. It is seen as having failed to take cultural politics seriously. But Woodhams himself is not entirely persuaded by this answer, which in any case is only presented as an assertion. He also has recourse to what he identifies as a post-1945 "process of secularization", suggesting that the decay of doctrinal certainty is part of the same equation. But the reader is left asking what this latest truism actually explains. Cultural changes of the sort Woodhams focuses on are, at best, only part of the story.

John Callaghan

FERNÁNDEZ, FRANK. *Cuban Anarchism. The History of a Movement*. Transl. by Charles Bufe. See Sharp Press, Tucson (Arizona) 2001. v, 154 pp. Ill. \$10.95; DOI: 10.1017/S0020859002090843

Frank Fernández's *Cuban Anarchism* offers a general approach to the history of the Cuban anarchist movement from the nineteenth century to the present day. It is a translated and slightly expanded version of *El Anarquismo en Cuba*, which was published by Fundación Anselmo Lorenzo in Madrid in 2000. Both the See Sharp Press and the Fundación Anselmo Lorenzo specialize in publishing anarchist books and pamphlets. For the English edition Fernández has revised the initial (weaker but less ideological) chapters, and included explanatory footnotes. Charles Bufe, the translator, has also included several footnotes.

This is a book by a Cuban exile living in Miami, who has also been an anarchist militant since the 1960s. Frank Fernández, a prominent member of the Cuban Anarchist Movement in Exile (founded in 1961 in New York and Miami, and better known by its Spanish acronym MLCE), was editor of *Guámgara Libertaria*, a Spanish-language periodical published between 1980 and 1992 in Miami. It achieved a significant circulation in the US. After *Guámgara* folded, Fernández started to write short books. In 1994 his book, *La sangre de Santa Águeda: Angiolillo, Betances y Canovas* (Miami, 1994) on the assassination of the Spanish prime minister Canovas in 1897, appeared and he began work on the book reviewed here.

*Cuban Anarchism* lacks notes on sources, and the author has used only some of the published material on the subject. Nonetheless, it provides a broad view of the history of Cuban anarchism. It is especially valuable for the post-1925 period, since most books on Cuban labour history omit any reference to the far from negligible role of anarchist unionism and militancy on the island prior to the 1959 revolution.

Cuban politics – both within and outside the country – and left-wing international support for the Cuban Revolution made it difficult to publish much on the history of Cuban anarchism in the twentieth century. After 1959, the already weak Cuban anarchist movement was crushed (and partially absorbed) by the revolutionary process. Most Cuban anarchists in exile settled in the United States, where anarchism was already a very tiny movement – without a periodical in Spanish since the closure of the New York weekly *Cultura Proletaria* (1927–1953). The author alludes to some of these circumstances, but instead of analysing them systematically he vents his bitterness at the lack of the support offered to the MLCE by the international anarchist movement. The publication of Sam Dolgoff's *The Cuban Revolution: a Critical Perspective* (Montreal, 1976; a Spanish version appeared in 1978) helped change many anarchists' views on the Cuban revolution, and since then the MLCE has improved its relationship with other anarchist groups around the world. One result has been to enable the publication of *Cuban Anarchism* by two anarchist presses.

For the period ranging from the mid-nineteenth century until the 1920s the author has relied exclusively on secondary sources. But for the 1940s, 1950s and 1960s Fernández has incorporated much more information from periodicals and even anarchist activists, though no interviews are quoted in a formal sense. The book has six chapters, arranged to reflect the different stages experienced by anarchist organizations in Cuba. They are “Colonialism and Separatism (1865–1898)”, “Intervention and Republic (1899–1933)”, “Constitution and Revolution (1934–1958)”, “Castroism and Confrontation (1959–1961)”, “Exile and Eclipse (1962–2001)”, and “Reality and Reflection”.

The last four chapters are undoubtedly the most interesting. Since the publication in 1976 of San Dolgoff's study on the Cuban revolution, no book has explored the presence of the anarchist and anarcho-syndicalist movement in Cuba after communism became the main current within the Cuban labour movement and an important political force in Cuban politics. Fernández rescues from oblivion the many anarchist activists, organizations, and unions seldom mentioned in the existing literature on the Cuban labour movement.

Despite writing from a militant point of view, Fernández raises several issues that need to be carefully researched: the crisis of the anarchist movement under Machado, its participation in the political crises of the 1930s, the participation of Cuban communists in the Batista government in the 1940s, the renewed influence of anarchism in several trades during that same decade, and the continuation of anarchist unions and political participation in the 1950s. Particularly interesting is Fernández's chapter on the end of the anarchist movement in Cuba during the first few years of the revolution. Fernández acknowledges the massive popular support enjoyed by the 26 July Movement (M26J) at the start of the revolution (p. 75), but, like Efrén Córdova in *Castro and the Cuban Labor Movement* (Lanham, MD, 1987), he suggests that Castro rose to power exclusively as the result of conspiratorial activities. As Marifeli Pérez-Stable has shown in *The Cuban Revolution: Origins, Course, and Legacy* (2nd edn, Oxford, 1999), other causes were at play.

We need more historical research if we are to grasp fully the evolution and impact of a radical movement that, at the time of the Cuban revolution, continued to play a relevant role in several Latin American countries. Extensive use of archival collections, periodicals, printed sources and even oral histories is necessary if we are to achieve a broader picture of Cuban labour history in the twentieth century. Fernández's book has broken new ground towards that goal.

Joan Casanovas

PARODI, JORGE. *To Be a Worker. Identity and Politics in Peru*. Ed., with an Introd., by Catherine Conaghan. Transl. by James Alstrum [and] Catherine Conaghan. [Latin America in Translation/en Traducción/em Tradução.] The University of North Carolina Press, Chapel Hill [etc.] 2000. xx, 177 pp. £29.95. (Paper: £15.50); DOI: 10.1017/S0020859002100848

Jorge Parodi's book was originally published in 1986 as *Ser obrero es algo relativo*. The original title (translated literally it means "To be a worker is something relative") captured the findings and the tone of his study better than the English one. It is unfortunate that the editor of the English version, Catherine Conaghan, who, together with James Alstrum, has otherwise provided a fine translation, did not stick to the original. As Parodi reminds us in this case study of the complex and complicated lives of workers and their union in a metallurgical company in Lima between the late 1960s and the mid-1980s, people have more than one identity. For the men working in Metal Empresa, the company he had chosen for his study, being a worker was indeed something relative – notwithstanding the endeavours on the part of left-wing politicians and union leaders, who attempted to foster their class consciousness and prepare them for their role as agents of revolutionary change. For these men, who were engaged in a struggle for the survival of their urban households

and who normally had to take on more than one job to guarantee a basic income for themselves and their families, no single occupation and therefore no single identity sufficed. While, with the re-emergence of nationalism in Europe and elsewhere, this conclusion is taken for granted today, it was certainly controversial in the Peru of the 1980s.

Parodi's book, which is based on extensive interviews with workers carried out in 1984, is divided into three parts, plus a brief epilogue. Part 1 comprises a study of the company, its workers, and their union. Part 2 is based on long transcripts of an interview with one of his main informants. Part 3 comprises a conclusion and another transcript of an interview with his main informant, who recounts his life during the 1990s and his view of that period. In the first part, which covers about two-thirds of the book, Parodi, making excellent use of his interviews, provides a vivid and incisive account of the life of workers in Metal Empresa, demonstrating how it changed in many important ways between the late 1960s and the mid-1980s, a period of military rule. Inspired by *clasismo*, an ideology that saw class struggle as the means through which workers in particular and the popular sector in general could secure their rights and gain their long-denied dignity, the union played a crucial role in this process. Parodi shows how the establishment of the union fundamentally transformed the workers, how they "went from being wage earners, who could be treated like nonentities, to workers" (p. 137).

At the same time, Parodi does not fail to underline the difficult and controversial relationships between the union and workers. He argues that *clasista* union leaders did not see workers as equal partners; they established paternalistic and authoritarian relations that resembled those between traditional *caudillos* and their followers. Those who did not agree with them were simply silenced. Even more important for the long-term relations between the two partners was the ideology of *clasismo* itself however. It was a radical ideology that regarded compromises and negotiations as signs of weaknesses. According to Parodi, *clasismo*, with its "demand-centric conception of trade unionism in which problems were solved by requesting, then demanding, a response from the firm", "limited workers' consciousness and imagination in adjusting to new and more complex circumstances such as those posed by the economic crisis and democracy in the 1980s" (p. 141). The union did not adapt its strategy – which worked during the relative economic prosperity of the 1970s – to the changing circumstances. And "as unions lost their capacity to win concrete material gains for workers, [the latter] turned away from unionism and began to look for individual ways to improve their lot" (p. 142).

The main shortcoming of the book is that it insufficiently links the micro level (workers), meso level (unions and parties) and the macro level (national politics). While for specialists and Peruvians living through this period brief references to the overall political developments might be sufficient, for those not familiar with Peruvian politics in general and Peruvian leftist politics in particular during the period under consideration the situation is altogether different. It would have been desirable if more had been said about the different policies of the military regimes of General Juan Velasco Alvarado (1968–1975) and his successor General Morales Bermúdez (1975–1980), and what impact they had on workers. After all, while Velasco Alvarado pursued nationalist policies and attempted to integrate marginal urban and rural masses into national society, albeit under the tutelage of the state, Morales Bermúdez reversed them under pressure from the International Monetary Fund and imposed an economic austerity programme. Catherine Conaghan's introduction, although providing a broader historical perspective, is simply

too brief adequately to address this fundamental problem. The extensive references to further reading in the endnotes provide little consolation.

Yet, despite these objections, Parodi's study is recommendable. The book is well written and argued and pays attention to the different expectations and experiences of workers with various ethnic and regional backgrounds. Parodi notes, for instance, that migrants who became factory workers did so as part of a strategy of accumulating capital and acquiring knowledge. Their ultimate aim was an independent business. Men from Lima, on the other hand, saw the job in the factory as an end in itself. Moreover, Parodi's study provides a lively ethnographic account of what it meant to be a worker in Peru. With its extensive and very illustrative quotes, the book certainly leaves a lasting impression on the reader. Workers are not treated as mere subjects; they are given a voice, and Parodi deserves credit for giving them that voice.

*Marcus Klein*