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BOOK REVIEWS

New Methods for Social History. Ed. by Larry J. Griffin and Marcel van der Linden. [International Review of Social History, Suppl. 6.] Cambridge University Press, Cambridge [etc.] 1999. ii, 165 pp. £12.95; $22.95; DOI: 10.1017/S0020859002580627

While the 1960s and 1970s are usually regarded as the heyday of social history in all its variants, the end of the twentieth century was definitely *not* a good time to be a social historian. The causes of this change in the "market value" of social history are well known: a whole train of successive "turns" – the "linguistic", the "cultural", and the "interpretative" being the most notable – have swept through the traditional historical landscape, questioning the presuppositions of "traditional" history in general and of social history in particular.

Quantitative social science methods shared the same fate as social history: the high hopes invested in them in the 1960s and 1970s have melted away like ice cream in the summer sun. Lawrence Stone and Emmanuel Le Roy Ladurie were far from being the only social historians in the 1980s who suddenly abandoned the project of "social science history" and called for a "return to narrative". Quite suddenly, these social historians too preferred their ice cream in "new" flavours – a change nicely illustrated by the difference between Le Roy Ladurie's *Les paysans de Languedoc* and his later bestseller *Montaillou*. "Variables" or "factors" and "social structures" were suddenly replaced by "actors" and "culture". "Causal explanation" was almost driven underground by "cultural interpretation". Multivariate analysis, at home in sociology and in political science, gave way to anthropological "thick description". Since those dark days social science history has badly been in need of "new methods", because the "old" methods made very few new converts.

New Methods for Social History is a compilation of articles, each suggesting and illustrating new ways of doing social science history. In their introduction, the editors Marcel van der Linden and Larry Griffin summarize the predicament social historians find themselves in at the turn of the twenty-first century. They are well aware that social historians are fighting an uphill struggle: "Analytical formalism in history seems on the wane", they state – and that is an understatement at best (p. 3). Multivariate statistical techniques – the backbone of "traditional" quantitative social science history – have proven to be of little use to most historians because the presuppositions of this methodology – large and continuous datasets for example – could not be shared outside a limited number of domains in social history, such as demography, social mobility, and collective action. And, even worse, multivariate techniques had proven to be insensitive to two of the "domain assumptions" of history: the relevance of space and time. This volume is meant to cure social science history of those ills by presenting the "methodological advances in the social sciences" and by showing "how those innovations can be fruitfully applied in historical research" (p. 6).

The contributions to the volume can be subdivided into three groups. First, there are the contributions that present hard-nosed statistical methods, adapted to the sensitivity of

historians to time and space. I can only hint at these contributions because they defy a summary for the uninitiated. Chapter 2, "Temporally recursive regression and social historical inquiry: an example of cross-movement militancy spillover", authored by Larry Isaac, Larry Christiansen, Jamie Miller, and Tim Nickel, presents a modified approach – TRR – to the traditional analysis of time series. This article will probably fascinate those who are aware of the pitfalls of plain regression analysis. The same holds for chapter 3 by Holly McCammon, who meticulously unravels the statistical niceties of "event history analysis". In chapter 4, Glenn Deane, E.M. Beck and Stewart Tolnay show how historians can incorporate the factor space in their methodological set-up: weight matrices, like "Moran's I", and other spatial regression models do the job. My favourite graph in this volume is in their article, on p. 77, because it bears a perfect resemblance to a bubble bath. In the last chapter of this type – chapter 7 – Charles Wetherell presents "Historical Social Network Analysis" (HSNA). In future, no historian of networks will be able to do without HSNA.

Chapters 5 and 8 form the second group of articles. Both address "the problem of narrative" in history and suggest ways to methodologically "discipline" unruly narrative. Chapter 5, "Narrative as data: linguistic and statistical tools for the quantitative study of historical events", is authored by Roberto Franzosi. He presents a semantic grammar and a statistical method that make it possible to transform "words into numbers" in narratives about collective action. His basic gambit is to analyse complex narratives in terms of a standard, formal scheme of action. All actions registered in the sources – in Franzosi's case newspaper reports on strikes in Italy between 1919 and 1922 – are dissected into three parts: an actor, an action, and the object of the action: "A semantic grammar is nothing but the simple semantic structure: Subject, Action, Object (SAO) and respective modifiers such as time, space, etc." (p. 82). By reducing all actions reported in the sources to a limited number of types – in his case "conflict" and "violence" for example – SAO makes it possible to chart the "typical" actions and "typical" actors in quantitative terms, including their "typical" interrelationships and their changes over time. Franzosi, for instance, shows how "violent" actions replaced "conflictuous" actions with the rise of fascism between 1919 and 1922. In this way the "undisciplined" narrative sources can be methodologically "disciplined", and according to Franzosi this method offers great advantages when the historian is dealing with a great number of narratives, as is likely when one studies collective action. Nevertheless, he also signalizes that coding actions is not a mechanical procedure – i.e. it requires interpretation from the coders – and that narratives also *lose* part of their content when they are processed through SAO (p. 100).

In chapter 8, Larry Griffin and Robert Korstad present a new technique in "historical inference and event-structure analysis": "Event-Structure Analysis (ESA) is a member of a family of formal analytic techniques designed to analyse and interpret text, in particular the temporal sequences constituting the narrative of a historical event. Its basic purpose is to aid the analyst in 'unpacking' an event – that is, in breaking it into constituent parts – and analytically reconstituting it as a causal interpretation of what happened and why it happened as it did". In this manner ESA bridges "the often damaging methodological chasm separating narrative history and generalizing social science" – "partially", the authors add cautiously (p. 145).

Like SOA, ESA requires a reformulation of narratives in terms of singular descriptive statements about their "atomic" actors and actions, in which the chronology of the action is the ordering principle. These statements are then processed using a computer

programme called ETHNO, which schematizes the statements and their interrelationships, ultimately in the form of a tree. Unfortunately, these schematic trees *only* start "growing" after the social scientist or historian has *imputed* causal relationships between its branches, based on counterfactual reasoning, social theory, etc. The authors are well aware of this: "ESA does not mechanically spit out answers to pressing historical questions, and causality, significance and meaning are not 'discovered' through its use. It assumes that the analyst, not the algorithm, possesses the requisite knowledge to anticipate possibilities of unfolding action, counterfactualize questions and conditions, explain what happened and interpret meaning. Thus the hard work of interpreting causality and extracting meaning from the event falls, as always, to the investigator"(p. 163).

Charles Ragin's "The logic of Qualitative Comparative Analysis" is chapter 6 and represents the third type of contribution. Here Ragin presents his well-known ideas about the comparative method, i.e. not a technique to tackle a specific problem, but a methodology for comparison as such. Steering a middle course between the – complex – "case" and – singular – "variables" approaches, Ragin elaborates a method called "Qualitative Comparative Analysis" (QCA). QCA conceives of historical cases – here, the political mobilization of ethnic minorities – as "configurations" of variables – here, their relative size, fluency, and wealth. By comparing these "configurations" in terms of different "outcomes" – here, the presence or absence of political mobilization among an ethnic minority – their "causal complexity" can be analysed. Essential in this approach is the recognition that "no single causal condition may be either necessary or sufficient for the outcome in question" (p. 115). This recognition certainly makes good sense, as I myself have argued elsewhere.[1]

Like Franzosi, Griffin and Korstad, Ragin is well aware of the limits of the method he proposes: "The real test of any representation of evidence is how well it helps the researcher and his or her audiences understand specific cases or sets of cases. Broad representations of cross-case patterns provide maps that guide and facilitate in-depth investigation; they are not substitutes for this type of investigation" (p. 121).

I could not agree more with Ragin, especially because my favourite ethnic minority – the Quebecois in Canada – seems to contradict his main argument. While he uses QCA in order to argue that there is a positive causal relationship between the political mobilization of ethnic minorities and their relative growth and wealth, the Quebecois are quite politically mobilized, even though their size and wealth relative to the rest of Canada have been going down rather than up, as far as I know.

What is one to make of all these "new" methods for social history? Reading the articles, I was often impressed by their ingenuity, but at the same time I had a constant sense of ambivalence. Although I have much sympathy for attempts to build bridges between historians and social scientists, I sometimes felt that this was a case of "a bridge too far". Let me try to spell out why.

The first reason for my ambivalence is that the "new" quantitative methods seem to share some of the important problems attached to their "old" precursors: many of the "historical" illustrations in the articles – and they are certainly no more than illustrations – are derived from research on collective action and demography. This limitation is probably connected to the type of source material required for statistical processing, but it is surprising nevertheless because the editors explicitly identified this limitation vis-à-vis the "old" methods and suggested "new" methods as a way of overcoming it.

1. Chris Lorenz, *De constructie van het verleden* (Amsterdam, 2002), pp. 137–163.

The second reason for my ambivalence is more substantial. It concerns the old "ghost of positivism", which still seems to be haunting the "new" methods. There are two reasons for my suspicion. The first is the apparent *lust for formalism*, which is unchecked by any cost-benefit consideration, i.e. without any judgement concerning *efficiency*. If, as a number of the authors in this volume openly admit, it is true that the basic steps involved in using the "new" methods, such as the identification, categorization and aggregation of "data", remain essentially *interpretative*, what are the benefits of this extraordinarily time-consuming *formalism*? Of course, formalizing "data" may enhance the quality of comparison when we are dealing with large sets and may enlighten the structure of explanatory arguments, as the authors argue – and these certainly represent important methodological qualities. But what is the point of formalizing narrative structures, for instance, if we end up with the very same problem we hoped to solve through "objective" formalism, namely "subjective" interpretation? What is the point of "transforming words into numbers" if the problems attached to the latter are lookalikes of those attached to the former?

Let me take Franzosi's article to illustrate this point. When he shows that, and how, one page of Mousnier's book *Peasant Uprisings*[2] can be coded in terms of his SOA grammar (pp. 100–102), and one observes that the "coded" passage takes almost *twice as much* space as the uncoded original, one can be forgiven for asking the obvious question: what is the use of coding it? Admittedly, Franzosi poses the question of the "economy" of SOA himself (p. 85), but I am not convinced by his answer.

Similar objections can be raised against Griffins and Korstad: if all the formal techniques of ESA and ETHNO do nothing to address the problems of causal reasoning and the imputation of meaning, why bother about ESA and ETHNO? If ESA and ETHNO do not improve on Max Weber's analyses of causal reasoning in the human sciences, why not just stick to Max Weber?

The second reason why I suspect a "hidden positivism" has to do with the notion of *narrative* in this volume. In as far as the notion of narrative is reflected upon, it is basically regarded as a complex that can be analysed in terms of its constituent parts. These parts are basically conceived of in terms of "atomic actions", which can be formalized in schemes like SOA and ESA. According to this view, the basic ordering principle of narrative is the chronological sequence and the interconnection of "atomic actions". And the point of most of the "new" methods is, as far as I can see, to formalize the analysis of these "atomic actions" and their interrelationships.

Two important objections can be brought against this notion of narrative. The first is that the ordering principle of narrative is *not* chronology: narrative *constructs* temporal order and *not* the other way around. The second is that the ordering principles of narrative are not found in their "atomic" parts, but in the narrative as a "configuration" or "complex whole". The ordering principle of Marxist narratives of "history as class struggle", for instance, cannot be located in their parts, e.g. in the description of industrial conflicts as a form of class struggle. On the contrary, individual industrial conflicts are described in terms of class struggle *because* they are parts of a *Marxist* narrative that *makes us "see"* industrial conflicts as "symptoms" of class struggle. Therefore, the very concepts needed to describe "atomic" actions in terms of SOA or ESA derive from a "higher" level of organization, so to speak: the level of narrative as a complex whole.

2. Roland Mousnier, *Peasant Uprisings in Seventeenth-Century France, Russia and China*, tr. Brian Pearce (London, 1971), pp. 84–85.

This "holistic" argument has been developed by philosophers of history such as Louis Mink, Hayden White, and Frank Ankersmit. However, in their reaction against the positivistic idea that the only task of the historian is "to get the facts right", they overstated their case. Justifiably rejecting the (positivist) identification of history with "factual" research, they unjustifiably claimed *complete* autonomy for the narrative level vis-à-vis "factual" historical research (resulting in some variant of narrative idealism). In their approaches the interconnection between narrative "synthesis" and factual "analysis" in research dropped out of the picture.

My main objection to the concept of narrative behind *New Methods for Social History* is the complement of my objections to narrative idealism.[3] In this volume "factual" analysis is presented as autonomous, and, consequently the level of narrative "synthesis" drops out of the picture. While narrative idealism conceives of the representation of fact as the exclusive "product" of narrative, the "new methods" conceive of narrative as the exclusive "product" of factual research. In this sense both narrative idealism and the "new methods" are still under the spell of positivism. I think *both* views make the same type of mistake, because in order to "get history right" one should do justice to *both* its analytical and its synthetic level – and that implies doing justice to their *interconnection*. "New" methods for history therefore need to be embedded in a "new" analysis of narrativism.

Chris Lorenz

Beecher, Jonathan. Victor Considerant and the Rise and Fall of French Romantic Socialism. University of California Press, Berkeley [etc.] 2001. xvi, 584 pp. Ill. $65.00; £40.00; DOI: 10.1017/S0020859002590623

For some three decades, Jonathan Beecher has been known among historians of European ideas as the most serious English language student of the French utopian theorist, Charles Fourier (1772–1837). The fine collection of texts he helped translate and introduce in 1971 was followed fifteen years later by his magnum opus, a 600-page biography of Fourier.[1] During the ensuing decade and a half Beecher devoted his considerable talents to a close study of Victor Considerant, Fourier's principal disciple, which was published in 2001.

Beecher's unusually thorough and well-rounded study of this important figure adds much to our knowledge. It discusses Considerant's ideas in depth, his intimate biography (well-constructed from personal correspondence), the Fourierist movement of which he was the leading organizer and theorist, his role in the French Left during the Revolution of 1848 and the Second Republic, his forced exile in Belgium after the failed uprising of June 1849, and his determined, but unsuccessful, effort to create a Fourierist experiment in Texas in the 1850s. Perhaps Beecher's most important insights, from the standpoint of the intersection of social history and social thought, illuminate Considerant's accomplishment in extending the movement in the 1830s and 1840s and his contribution to the Democ-Soc build-up during the Second Republic.

3. I have analysed the problems of White's and Ankersmit's brands of narrativism in "Narrativism, Positivism and the 'Metaphorical Turn'", *History and Theory*, 37 (1998), pp. 309–329.

1. Jonathan Beecher and Richard Bienvenu, *The Utopian Vision of Charles Fourier: Selected Texts on Work, Love, and Passionate Attraction* (Boston, MA, 1971); Jonathan Beecher, *Charles Fourier: The Visionary and His World* (Berkeley, CA, 1986).

This book will therefore be of special interest to students of the cultural and political context of nineteenth-century French romantic socialism as well as to those more generally interested in nineteenth-century social ideas. Certainly, compared with his study of Fourier, Beecher's greater attention to social-political history is mandated by the historical context of the disciple, quite different from that of his master. Fourier, seventeen years old in 1789, may have become an adult in an epoch of unparalleled turmoil. Indeed, like Saint-Simon and Robert Owen, he was in quest of a new harmonious social order to replace the unjust one destroyed by the French Revolution and commercial modernity. Fourier, however, came of age intellectually during the Napoleonic wars, a period congenial to escape into a hypothetical world but not – particularly since it was followed by a decade and a half of Old Regime restoration – to efforts to reignite the flame of revolution. Contrariwise, Considerant was twenty-two years old at the time of the first such effort – the July Revolution of 1830 – and his most creative period spans the turbulent years of the Orleanist Monarchy and the Second Republic.

A student at the French officers' academy, the École Polytechnique, Considerant was similar in age, education, and political conviction to the uniformed *polytechnicien* depicted on the barricades of July in many of the paintings of the era, including Delacroix's "Liberty leading the people". As such, in addition to the revolutionary tradition of the *Carbonari* – powerful among young officers in the 1820s[2] – he was exposed to an education in mathematics and engineering that made him particularly open to the attempt by Fourier to apply eighteenth-century natural science to social philosophy.[3] But the social romantic mood of the age also made him sensitive, as Fourier was not, to a reawakening of the revolutionary spirit in the entire nation, high and low together. Anticipating the romantic socialism Considerant was to epitomize, the armed *peuple* being led by Delacroix's goddess of the Republic included – alongside the military cadet – a ragged artisan, a working-class adolescent, and a top-hatted bourgeois.

The Democ-Soc movement of the Second Republic, in which Considerant was to be prominent, continued and broadened the social coalition Delacroix had depicted two decades earlier, joining "peasants, rural and urban artisans, small tradesmen, and petty entrepreneurs, as well as minor officials and members of the liberal professions".[4] This class amalgam went together with a pre-Marxist socialism that was radically idealistic. Considerant "did not speak of socialism as an expression of the emerging industrial society but of the new society as an emanation of the socialist idea [...] 'socialism has faith [...] in the young society which it carries in its loins' " (p. 235).

Both the ideology and the class mixture were typical of an age in which the French revolutionary model of 1789, characterized by the cooperation of dissident elites and pre-industrial populace, still prevailed. Class cooperation, written into the program of the most radical Democ Socs, was mandated by the persistently pre-industrial character of the

2. Alan B. Spitzer, *Old Hatreds and Young Hopes: The French Carbonari Against the Bourbon Restoration* (Cambridge, MA, 1971), p. 244. Spitzer cites evidence that there was even a *Carbonari vente* in the École Polytechnique in the 1820s.

3. "Students graduated from Polytechnique with a faith in man's ability to utilize the methods of science to harness and control the forces not only of nature but also of society [...] they all shared the belief that problems of social organization could be solved in basically the same way as problems in the building of a road or a bridge." Beecher, *Victor Considerant*, p. 26.

4. Edward Berenson, *Populist Religion and Left-Wing Politics in France, 1830–1852* (Princeton, NJ, 1984), pp. 227f.

"people" that was to carry out the new revolution. In fact, parts of the Democ-Soc program – for example "elimination of the salt tax and [...] restoration of traditional forest rights" (p. 244) – answer to precapitalist peasant demands, an aspect of pre-industrial radicalism that takes us to contemporary historiographical debates around the work of E.P. Thompson, Eric Hobsbawm, William Sewell, and Anton Blok.

As Beecher shows, Considerant's work illuminates the future as well as the past of nineteenth-century socialism. His critique of work before 1848 as well as his radical democratic vision in the aftermath of the Second Republic strike us as surprisingly contemporary. Indeed, scholars interested in the background of contemporary critiques of privatization and deregulation of public services will also find nuggets in Beecher's work.

Before 1848, when his ideas were the principal translation of Fourier for a broad public, Considerant's Fourierist critique of work was quite similar to the ideas on alienation of his young contemporary, Karl Marx. It is no wonder that Beecher prepared his collection of Fourier in the late 1860s, when critical ideas about work suddenly took on a significance neither Marx nor Fourier could have anticipated. Considerant's criticism of representative democracy also has a modern ring. After 1848, reacting to the impotence of formal democracy during the Second Republic, which many in the radical left attributed to betrayal by elected leaders, Considerant argued for supplementing or even replacing representative institutions by the extensive use of referendum and popular initiative to ensure involvement of citizens in legislation and administration (p. 283).

Another, more politically relevant, aspect of Considerant's polemics in the 1840s also has a decidely modern echo: his opposition to the privatization of the French rail network. At the very onset of mass rail transport, he supported the idea of state ownership and opposed private development as "one of those acts of extreme immorality and transcendent folly that tarnish forever whole periods of history". Although the poet-politician Alphonse de Lamartine led the opponents to privatization, "the Chamber of Deputies adopted a government-sponsored plan that gave private investors just about everything they wanted. The development of a French railroad system was carried out under private management and for private profit but with the government agreeing to provide subsidies, land grants, and low-interest loans and to pay for the construction of bridges, tunnels, and railway stations. For Considerant, all of this was a bitter pill to swallow – the more so in that the government's encouragement of private investment was followed by a speculative 'railroad mania' that lasted two years and culminated in several spectacular bankruptcies. '*Is France for sale*?'[5] he asked in a bitter editorial in *Démocratie pacifique* in June 1844".[6] Considerant thus anticipated by more than a century and a half José Bové's denunciation of neoliberal orthodoxy, "*Le monde n'est pas une marchandise*",[7] not to mention the contemporary European-wide questioning of railroad privatization.

In terms of the sociocultural context of personal biography, Beecher's book is also fascinating. His Considerant comes over as a full-blooded romantic, a familiar of the men and women who shaped the literary culture of the July Monarchy. It is clear from his account that Considerant should be considered a "Benjamin" of the generation of Hugo, Michelet, Balzac, Sand, Delacroix, and Vigny, whose births preceded his by between five

5. My emphasis. (AM)
6. Considerant as quoted by Beecher, *Victor Considerant*, p.118.
7. José Bové et François Dufour, *Le monde n'est pas une merchandise* (Paris, 2000), transl. *The World Is Not For Sale: Farmers Against Junk Food* (London, 2001).

and ten years. Like them, he was influenced by the strange mixture of science and religion that characterized the romantic generation.

A comparable mixture had been present in the work of his master, Fourier, as well, but Fourier's science was that of the seventeenth and eighteenth century. He modelled his notion of passionate attraction on Newton's theory of gravity, and his religion was an Enlightenment deism mixed up with numbers mysticism.[8] From Considerant's romantic contemporaries, however, the disciple imbibed the new paradigm shaped by the biological sciences. Alongside some interesting pre-Darwinist evolutionary theories – like those of Lamarck and Geoffroy Saint-Hilaire – were pseudo-scientific ideas of magnetism and religious notions that moved effortlessly from the early Christian gospel of poverty to beliefs in spirits evoked by moving tables and loved ones recallable from an afterlife. Rare were the intellectuals of the first half of the nineteenth century who were immune to such beliefs. Michelet was not, nor was Victor Hugo, nor were early socialists like Pierre Leroux and Considerant. The latter had no difficulty in "reconciling [Fourierism] with a lively interest in Mesmerist doctrines of animal magnetism", as evidenced by his correspondence with "a professor of natural history at the Jardin des Plantes who was one of Mesmer's leading French followers" (p. 41).

Nonetheless, there were frequent quarrels between Fourier and his young disciple, stemming partly from Fourier's irascible and suspicious personality, partly from Considerant's efforts to make the master's often extravagant notions palatable in a later period with a different mentality. On the creation of the movement, Beecher points out the disciple's pragmatic approach to the popularization of Fourier's ideas, "avoiding extended discussions of Fourier's cosmogony and his theory of universal analogy and drawing a veil over Fourier's sexual fantasies and his radically anti-patriarchal vision of 'a new amourous world' " (p. 169). Indeed, as opposed to the late-enlightenment scepticism and irreverence that shaped Fourier's generation, the age-cohort of romantics that grew up under the restored Bourbons remained permanently afflicted by one particular aspect of the Restoration's conservative morality: its projection of nostalgia for a lost world into reverence for *les pères*, which we often find in the actual closeness of sons to fathers (Hugo and Michelet are exemplary) as well as in the social romantics' adherence to a patriotic version of patriarchal reverence. Considerant, too, is an example of filial piety, embracing his father's embattled liberalism and anticlericalism from his youth, a conformist in his very nonconformity.

There are a few areas where one might have wanted a bit more detail from Beecher. While Beecher's periodization of the period 1846 to 1851 is clear, one misses a sharper delineation of the 1830s and early 1840s. For example, the difficulties of the Fourierist movement in the late thirties correspond to the difficulties of the French Republican Left and the incipient workers' movement as a whole had between 1835 and 1840, a period of conservative consolidation and clerical revival that followed four years of insurrectionary attempts after the July Revolution and ended with a major recession. The improved fortunes of the Fourierists in the 1840s corresponded to the renewal of Republican, labour, and Socialist agitation that accompanied both the end of the recession and Thiers' foreign policy fiasco in 1840. One also wonders if Beecher could not have better fleshed out the personal biography. He does have some interesting comments on his subject's personality, but Considerant's filial loyalty both to his anti-Bourbon father (whose dream of moving to

8. Beecher, *Charles Fourier*, p. 224.

America the son was to fulfill) and to his mother-in-law, who first conveyed Fourier's ideas to him and made important financial contributions to the movement, merited a closer look. Finally, Beecher missed an opportunity to clarify some word play lost in translation when he cited Considerant's criticism of a fellow-Fourierist's proposal for "an orphanage run on Fourierist principles" as "about as childish as they come, no pun intended" (p. 290). The French for "childish" – *enfantin* – was the name of the leader of the rival Saint-Simonian movement, and should have been put between parenthesis.

Apart from these rather minor points, Beecher's *Considerant* is an excellent study of a fascinating political figure. One hopes for a paperback edition that will make it purchasable for students and teachers unable to afford the $65 hardcover price.

Arthur Mitzman

Jennings, Lawrence C. French Anti-Slavery. The Movement for the Abolition of Slavery in France, 1802–1848. Cambridge University Press, Cambridge [etc.] 2000. x, 320 pp. Ill. £35.00; $54.95; DOI: 10.1017/S0020859002600628

Professor Jennings's book, despite what its title indicates, really examines French antislavery under the July Monarchy and the revolutionary emancipation of 1848. It describes the efforts of a group of abolitionists, drawn from the regime's elite and most of whom were in Parliament, as well as the activities of several independent activists. The former repeatedly failed to overcome resistance to emancipation from the colonial lobby, the ministry of the *marine* (which oversaw both the navy and the colonies), and leading members of the French establishment, including Louis-Philippe. Independent abolitionists fared little better, publishing newspapers and tracts but making little progress until the late 1840s. This momentum was overtaken in 1848, when the abolitionist Victor Schoelcher convinced the minister of the *marine* to appoint him as undersecretary charged with securing immediate emancipation. According to Jennings, however, the abolitionist failure under the July Monarchy resulted not just from powerful opposition, but from the movement's very nature. Timid, deferential, overly focused on Parliament to the extent of breaking up during its recesses, and unable to agree among themselves about the best means of achieving emancipation – let alone how to win over the administration – these humanitarians inevitably made little headway, even when one of their own was in cabinet.

This work's significant flaws, however, keep it noticeably short of its stated goal. Jennings does assume a difficult task. After outlining the deficiencies of the few other books in the field, he expresses hope that his "will prove to be a [...] definitive account of the French movement against colonial slavery in the first half of the nineteenth century". Regarding sources, he notes, "Most important, all available French abolitionist papers and writings [...] have been carefully scrutinized." By these high standards, though, this book is wanting.

As noted above, the book studies the July Monarchy, Jennings's field of expertise, rather than the first half of the nineteenth century. It devotes to 1802–1830, 60 per cent of its stated scope, one short survey chapter based on secondary works and one already well-mined primary source – appropriate to introduce to a work about the Orléanist regime, but insufficient for a main topic. Indeed, the conclusion increases suspicions that these early years were tacked onto an original manuscript about the July Monarchy. One might defend such cursory coverage by noting that Jennings's topic is emancipationism, not slave trade

abolition – the battleground of those earlier decades. It is always difficult for an historian trying maintain focus to keep the two apart. In this case, however, the effort is unhelpful. The movement fighting the trade in the 1820s instantly became the antislavery one of the 1830s. Parallels abound and comparisons would have been extremely enlightening. Both used similar tactics to overcome: foot-dragging bureaucracies; public indifference; atomized humanitarian movements; state restrictions on publications and public meetings; difficulties over the right of search; and a problematic relationship, in the light of French nationalism, with British activists (who provided great resources but whose assistance tarred antislavery with an English brush which proved highly detrimental during the era's frequent bouts of intense, popular French Anglophobia). The two French movements are best approached as a connected whole. Even when Jennings integrates slave trade issues, though, worrying lapses in detail appear: for example, he has the most important British seizure of an alleged French slaver, the *Marabout*, a *cause célèbre* with great political ramifications, occur off the wrong continent. In short, this work should either have made clear that it was about the July Monarchy or more fully integrated all abolitionist activity.

A fuller investigation might have mitigated the reverse snobbery which sometimes mars this work. A never-repeated antislavery petition by newspaper employees becomes "the noble efforts by the workers of Lyon". Cyrille Bissette, a black but frequently bankrupt newspaper publisher, whose print runs never exceeded 200 and whom abolitionists of every opinion shunned for hot-headedness which impeded the cause, becomes "heroic". The Duc de Broglie, on the other hand, who supported the cause for years, if sometimes equivocating on immediate emancipation, is "haughty, aloof and condescending". Moreover, he became head of the "highly elitist, legalistic, hierarchical" French antislavery society not because of a decade of combating the trade but because "of his high status within the [...] Orleanist ruling clique". Louis-Philippe is treated more harshly. We are constantly told of his opposition to emancipation, but get little hard evidence, the best being the opinion of a Lyon-based abolitionist. Jennings may be right about the king's views, but it is conjecture to insist "Only the determined behind-the-scenes [and therefore undocumentable?] opposition of the monarch to [...] emancipation" explains the lack of government action during his reign. Public opinion certainly suffices for the 1840s, without royal manipulation. Louis-Philippe repeatedly appears as a shadowy figure who must have been doing something because that was his way. This is argument by assertion.

Such excessive anti-elitism and the failure to examine seriously the 1820s are particularly regrettable in that they prevent reappraisal of the decades-old interpretation (usually expressed with at least a hint of condemnation) of the abolitionists as a timid, ineffective, Parliamentary elite. From 1815 onwards, French abolitionists, often modelling themselves on and with the help of their British colleagues, were as active as possible without provoking a ban on their organization and their own arrests – as Jennings notes for the July Monarchy. They consciously sought to change French public opinion and tried to form provincial auxiliary societies. Moreover, their Parliamentary tactics were successful in obtaining the first serious French slave-trade abolition law in 1827 and, as Jennings indicates, regularly had the government contemplating concessions throughout the 1830s. Indeed, except that the abolitionists had fewer resources and did not engage in corruption, their tactics frequently mirrored those of their opponents, whose efforts Jennings labels "brilliant". It is plausible that the difference between the French and more robust British abolitionist campaigns had more to do with greater political restrictions in France than with any lack of nerve.

This assessment is impossible to weigh, however, because of another major problem

with the book: it lacks evidence from the abolitionists. Despite his claims of completeness, Jennings consulted the papers of only two French abolitionists – Schoelcher and François Isambert. The former archive included none of Schoelcher's letters, consisting entirely of correspondence between colonial officials and colonists which the campaigner had collected while in office. Of Isambert's letters, only a handful appear in the footnotes. Correspondence between the latter and British abolitionists from a British collection help make up the lack. For the most part, though, the book deduces abolitionist thinking from (frequent) conjecture or from contemporary publications, in particular pro-abolition newspapers. Newspapers, on which Jennings cut his historical teeth, have their place, but cannot replace the abolitionists' own voices in a history of the movement. Indeed, the first private letter from Isambert which Jennings cites indicates that the opinions which abolitionists advanced in documents for public consumption were far more optimistic than their real views. It is unfortunate that Jennings used no further private papers: the best parts of the book are those which integrate them or which consider the colonists or government, for whom his documentation is more complete. If these are "all available French abolitionist papers", then the question arises whether enough evidence exists to write a proper history. It is very surprising, however, if the papers of Broglie, Grégoire, Lamartine, and Tocqueville, to name a few, are silent on abolition. Furthermore, Jennings mentions "precious insights" obtained from Anglo-French correspondence in the British Anti-Slavery Society Papers. Such international communication, however, also exists in the Thomas Buxton, William Wilberforce, and Zachary Macaulay papers and other British archives (Jennings mentions the importance of Macaulay to the French movement). It is all too easy, of course, to advise a conscientious, Ottawa-based academic to consult archives an ocean away, but some of the above are available in the same rooms as papers which Jennings checked, and others are on microfilm.

Finally, besides the book's substantive problems, its text needed a major revision before publication. Frequent, unnecessary repetition of points – whether the reiteration of arguments several times within a few pages or innumerable reminders of certain points (how many times need we hear of the abolitionists' elitism?) – jars the reader. Moreover, when Jennings does have documentary evidence, he tends to include far more information than necessary: detailed accounts of antislavery meetings, parliamentary debates, or troop strengths in the colonies add little to his argument, while a membership list of the French antislavery society would have been more useful as an appendix. Errors and idiosyncracies in vocabulary usage and grammar (particularly in quotations translated from French) often mar the text. Redundancies are a particular problem, some quite egregious, such as "continued to remain", "to cooperate and work with", and "indicative in the way it demonstrates". Finally, slang can, where it improves the vigour and clarity of standard English, be welcome, but Jennings's modernisms, such as the slavery or emancipation "scene", rarely do so, and "monster reform banquet" is an image more comic than informative.

In summary, this book is a useful, although flawed, addition to our understanding of French antislavery under the July Monarchy. The definitive history of nineteenth-century French antislavery, a field too little explored, remains to be written.

*Paul Kielstra**

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Papathanassiou, Maria. Zwischen Arbeit, Spiel und Schule. Die ökonomische Funktion der Kinder ärmerer Schichten in Österreich 1880–1939. [Sozial- und wirtschaftshistorische Studien, Band 24.] Verlag für Geschichte und Politik, Wien; R. Oldenbourg Verlag, München 1999. S 680.00; DM 79.00; DOI: 10.1017/S0020859002610624

Most historical work on child labour and the economic function of children focuses exclusively on labour in the context of the Industrial Revolution. This reinforces the idea that pre-industrial child labour was not nearly as bad as child labour during the Industrial Age, and that before industrialization children were working pleasantly side by side with their parents engaged in only light tasks. Relatively few studies are available on child labour within the pre-industrial context. Until World War II, children in European families were large net contributors to the family budget, and no historian will want to question this proposition. However, there is little direct empirical material that would enable us to gauge more precisely the extent of their economic role in the family and the way this was organized within the family economy and within the context of familial survival strategies. This book fills gaps in both these areas. However, the author's main aim lies in another direction. In this book – a revised version of her doctoral thesis written at the University of Vienna – Papathanassiou explicitly aims to write a "subject-related problematization of child labour", where child labour is analysed in the context of the everyday history of childhood. What she means to say is that she wants to place the children at the centre of her analysis rather than offer a top-down type of analysis which looks mainly at child labour laws or aggregate statistics of the number of employed children. Her book is thus announced as being primarily a study into the history of childhood, while the issue of the family economy and children's economic role therein seems to occupy second place.

Papathanassiou is able to provide this "subject-related" perspective because her book is based on unique material, namely the large collection of biographical material collected at the Institut für Wirtschafts- und Sozialgeschichte at the University of Vienna. This database contains 800 life histories – autobiographical or biographical texts in various shapes and forms relating to individuals from the poorer strata of society – of which the author has used 150 cases for her study. The texts consist of oral histories, with childhood memories written down by the children of the autobiographers, and written histories by children about their parents or grandparents. In a small number of cases the material was supplemented with additional questionnaires and interviews. Most of the collection pertains to the interwar period. The material was selected to reflect social position (poorer strata) and background (urban versus rural). The author concedes that there are methodological problems with this type of material, but she lightly dismisses most of these by stating that the dangers of "conscious distortion" in relation to childhood memories are relatively small. This is being very optimistic to say the least, and it leaves the issue of unconscious distortions of childhood memories completely undiscussed.

The study is structured in a very thorough and lucid way. It begins by examining child labour legislation and debates in Austria from an international perspective. This is followed by a description of the economic and social position of the poorer strata in Austria between 1880 and 1940. Next, the author presents five chapters that, at first glance, seem to be structured around the type of family economy in which the children functioned: agriculture, domestic work, industry, gathering activities, and the moral economy (including begging, theft, and the acceptance of gifts). However, this is not

entirely the case, as the chapters are actually structured around the type of work the children were involved in, and this does not necessarily reflect the economic basis of the family as a whole. For each of these sectors she carefully discusses the organization of work, the type of activities – and how these related to gender and age – payment systems, and working time. In five further chapters, Papathanassiou goes on to discuss entry into occupational life, the role and position of the parents, schooling and school life, play, and the attitudes of children towards work and their perception of self. A concluding chapter, in which she returns to the theoretical issues, completes the study. Amongst the poorer strata Papathanassiou includes not only all those belonging to working-class and day labourers' families, which constituted about 40 to 50 per cent of the total Austrian population during this period, but also many of the peasant smallholders and agricultural workers. All these groups were in a most precarious social and economic position before the 1940s, with the consequences of World War I, the collapse of the dual monarchy, and the world economic crisis of the 1930s contributing heavily to continuous crisis and impoverishment. Papathanassiou focuses exclusively upon the younger children – those up to and including the age of fourteen.

Not surprisingly, during this period children's work in Austria primarily took the form of agricultural labour: about two-thirds of all employed children worked in this sector of the economy. Agricultural work was undertaken not only by children living in rural communities, but also by children from the urban working classes, who, together with their mothers, might spend the entire summer away on the land, living with relatives. Not only were divisions between urban and rural fluid and flexible therefore, those between the nuclear family and extended relatives were too. Papathanassiou is able to show that entire families might undergo longer periods of "restructuring" to accommodate the allocation of different family members to different economic activities. Children's work typically consisted of tasks that were non-complex and time-intensive, such as looking after small cattle, and activities were strictly segregated by sex. This resulted in gender patterns all too familiar but striking nonetheless: young girls and their mothers were mostly involved in small-scale subsistence agriculture, while boys and fathers were more often found on the larger farms, which would also produce for the market sector. In most families mothers were economically active, and so children – girls in particular – were required to help out with domestic work as well. In fact it is here that children put in the longest working hours: nearly all children worked at least thirty weeks per year, or even more, doing a great variety of domestic chores. Factory work by children under fourteen declined in this period and became almost negligible as a result of technological developments and restrictive labour laws, which caused the greater part of child labour to switch to proto-industrial production within the household, in particular in textiles, where very young children could effectively be set to work.

Papathanassiou's description of the incorporation of children into factory work around 1900 reminds one strongly of the situation in early industrial England: young children were recruited to the factory by parents as their helpers and co-workers; this enabled parents to keep an eye on them while at work, but it also reflected the fact that the parents simply could not survive without their children's economic contribution. Given the economic necessity of child labour, it is surprising that in the discussion of the extent to which school attendance created pressures or relief for the family economy, no reference is made to the most obvious pressure – namely the opportunity costs of the time children spent away from the labour market. Children's work was essential to working-class

families and Papathanassiou clearly describes how work and diligence constituted central values in the socialization of children. The social and economic situation of these families left little room for other more child-orientated types of socialization. In practice therefore children had to combine work and schooling, which contributed to increased demands on children's time. However, time spent in school was also used to recover from the hard work in the early hours of the morning. Moreover, the biographies testify to the pleasure children had in going to school. Finally, the economic contribution of children to the household also included activities, such as ritualized begging, which were greatly tabooed for healthy adults.

The compelling conclusion implied in every chapter is that between the 1880s and the 1940s family economies were very flexible, and that an enormous pluriformity of economic activity was often carried out within one and the same family – ranging from protoindustrial or industrial work to artisanal work, but also including agricultural activities, gathering or begging, running errands, or other miscellaneous jobs, the combination of which might differ from one season to the next. The dividing lines between urban and rural, between economic sectors, and between households and families were extremely fluid and were constructed and reconstructed in such a way as to benefit the most optimal combination of opportunities and resources at that particular moment, while observing clear gender and age lines. It is in this area – the concept of the family economy – that Papathanassiou's main conclusions, and therefore the main contribution of this study, are to be found. She argues – rightly – that family economies were of a strongly mixed character well into the twentieth century; families were tapping as many economic resources as they could find, based on strong interconnections between production, reproduction, and consumption. These working-class family economies were limited to nuclear family members, but they might temporarily also include extended relatives. In fact, the kin system often determined the particular character of the family economy.

Any historian researching "survival strategies" in working-class families will find this study a valuable collection and a storehouse of wonderful data. Although in the first chapter Papathanassiou claims it is one of the main aims of her study, in her conclusions she completely neglects the issue of what her study contributes to the history of childhood. This issue seems to form a separate strand in her book, and one that is poorly integrated in theoretical terms. The reader might therefore be forgiven for thinking that a second book is lurking inside the covers of this very informative study on child labour in Austria.

Angélique Janssens

McCreery, David J. The Sweat of Their Brow: A History of Work in Latin America. [Latin American Realities.] M.E. Sharpe, Armonk (New York) [etc.] 2000. ix, 211 pp. £55.50; DOI: 10.1017/S0020859002620620

David J. McCreery's *The Sweat of Their Brow: A History of Work in Latin America* is a courageous and innovative book. Historical research on the issue has undergone a huge expansion over the last two decades, but most of it is in the form of monographic studies. Taken together, the myriad works regarding specific areas, time periods and categories of workers have contributed to major changes in theoretical and methodological approaches and enriched empirical knowledge, engendering new themes and questions.

However, so far, apart from some bibliographical reviews, few attempts have been made to provide an articulated and comprehensive view of the findings generated by this historiography. Important initiatives in comparative work have been undertaken, but notwithstanding their value even the best known example, Charles W. Bergquist's *Labor in Latin America: Comparative Essays on Chile, Argentina, Venezuela and Colombia* (Stanford, CA, 1986), offers only a partial view, excluding two of the region's three major countries from its case studies.

Thus a synthetic single volume, written in clear and objective language, ranging from precolonial times to present-day challenges, and, moreover, incorporating gender and race issues, is an outstanding achievement in itself. For a nonspecialist public, it provides a first approach to the historical peculiarities assumed by work and labour relations in Latin America. For scholars, on the other hand, it makes available a competent historiographical rendering, even if more up-to-date in some aspects than in others.

Besides the practical uses of such a panoramic view, more obviously for teaching and as a reference work, it also stimulates the revision of traditional issues by placing them in a new light. That is possible because McCreery has not just summarized the established conclusions of academic research. He has assembled the puzzling and uneven pieces produced by many different scholars and, guided by a well-defined theoretical perspective, focused on the development of "forms of labor mobilization and control".

The author shows how the changes that world capitalism witnessed from the fifteenth to the twentieth centuries affected the ways Latin America has been integrated within it, but at the same time he demonstrates that, if the mechanisms for making work "available" sometimes suffered transformations following systemic changes, they continued to be guided by the imperative of maximizing the generation and concentration of wealth. For most of Latin American history, this meant an array of coerced labour forms.

It is also to McCreery's credit that he opts for a wider understanding of his subject, rejecting traditional views excessively focused on urban workers and the institutional structures that define their place in Latin American social systems. On the contrary, the author stands for a "work history" which, he argues, expresses the experience of the majority of Latin American people in their struggle to survive and make a living, as opposed to a "labour history" focused on minorities and their political activism. Work, in this perspective, may encompass such diverse activities as warfare, crime, prostitution, domestic chores, bureaucracy, and begging.

If this broadening approach has its salutary aspects and is in tune with recent research trends, it also poses some problems and traps. Work is a cultural notion, as is well illustrated by McCreery's discussion of the different perceptions colonists and Native Americans had about it. The latter, for example, were not unfamiliar with the experience of physical and intellectual effort aimed at reaching some concrete result, but they did not share the values related to acquisition and accumulation implied by the former's definition of work.

Sensitive to this cultural problematic, the author also shows how the meanings of work in specific historical contexts are established in large measure by contrast with what is defined as its opposite, as in the "vagrancy laws", whose decisive importance is well demonstrated in the book. This leads to a paradox in that many of the "ways of making a living" included by McCreery in his comprehension of a "work history" were defined, even by those who practiced them, as not work.

Besides this methodological difficulty, one could point out that even if the subject were

defined in a more restricted way it would be hard to accomplish, in a short book, a comprehensive continental narrative over five centuries. Widening the scope causes the author necessarily to focus on some kinds of social activity only in specific times and areas. McCreery shows, for instance, that warfare had been a major way of life in Castile and that this explains important features of Spanish imperialism. But, contrary to what could be expected from the book's introduction, it does not provide a general view of warfare as work through Latin American history.

Regarding McCreery's option for a "work" history as opposed to a "labour" history, it would be worth adding that the distinction makes no sense in Spanish or Portuguese – from the perspective, that is, of the linguistic structures that shaped the cultural universe in which most Latin American peoples have been integrated to the Western notion of work, or labour. Of course, in spite of this limit in our conceptual tools Latin Americans are able to understand the distinction between, on the one hand, a widespread social experience of surviving under exploitative conditions and, on the other, the institutional structures involving representative organizations, politics, the state, and the law.

McCreery is clearly aware of the interplay between both aspects of the work/labour problematic. Particularly in his analysis of the colonial period, as much attention is given to the ways labour in Latin America was regulated by the Spanish and Portuguese empires as to strategies set up by different kinds of workers in order to resist oppression and to bargain for better working and living conditions. However, the author displays a clear inclination to understand political participation and the building of formalized organizations as if they were external to working-class experience (labour, not work).

As the narrative approaches the twentieth century, this dissociation causes the reproduction of some basic stereotypes typical of classic accounts in which workers play a passive role in national and regional histories. Internal migrants, McCreery tells us, brought to Latin American metropolises "traditional rural ideas of deference, hierarchy and paternalism" (p. 147), which have been exploited on a mass scale by populist charismatic leaders. It would be naive to deny that submission, manipulation, and accommodation are all, to some degree, part of Latin American workers' experience. But the results of the region's amazing industrialization and urbanization processes and the incorporation of masses in modern politics cannot simply be reduced to that, as the now academically demoralized theory of populism attempted to do.

One of the most powerful effects of McCreery's book is to offer a panoramic view of the catastrophic dimensions assumed by the economic and political oppression directly associated with the systems of labour control imposed over Latin American peoples. It does not provide to the same extent a general vision of how, through their struggles, workers – even when defeated in their ultimate goals – have managed to dispute the meanings of work within their societies and to construct collective identities based on the values and signs they have chosen to ascribe to their social and political experiences.

It is true that often this "class consciousness" has not fit leftist models, and sometimes even the doctrines whose hopes of structural change rest upon workers' political action have had difficulties in establishing a dialogue with them. In any case, to maintain that anarchism, at any moment, has been "as effective as police spies in dividing and weakening workers solidarity" (p. 133), or that communists "in most Latin American countries had little significant role in organizing workers" (p. 141) contradicts facts well demonstrated by much research. It is also absolutely unfair to thousands of activists who have dedicated, and sometimes sacrificed, theirs lives defending workers' rights.

There is a great gap between the experience of work as the biblical curse referred to by the book's title and the revolutionary hopes with which McCreery concludes. Transcending it is a major task for Latin American workers and their organizations. Writing a labour history able to grasp all the complexities of that process poses a challenge to many scholars. The stimulus provided by this book to the further development of research and debate on the issue makes it indispensable for anyone who nourishes one or both concerns.

Alexandre Fortes

Robinson, David. Paths of Accommodation. Muslim Societies and French Colonial Authorities in Senegal and Mauritania, 1880–1920. [Western African Studies.] Ohio University Press, Athens; James Currey, Oxford 2000. xvi, 361 pp. Ill. Maps. £40.00. (Paper: £16.95.); DOI: 10.1017/S0020859002630627

The common assumption in the study of colonialism in Africa is that colonial rule established a total domination and subjugation of colonized people. Little scholarship has been devoted to understanding the role of African agents (brokers, intermediaries, and bureaucrats working directly or indirectly for the colonial state) and how they influenced the discourses, functions, and efficiency (or inefficiency) of the colonial apparatus. However, both the process of colonial territorial conquest and the nature of colonial rule have provoked considerable debate and controversy. Since the 1960s, historians such as Ronald Robinson have argued that colonial conquest and occupation could not have taken place without the "collaboration of local elites". In exploring "indirect rule" and the nature, structures and operations of traditional chieftaincies, many others – including A.E. Afigbo, J.F.A. Ajayi and A. Boahen – have very convincingly identified ways in which local social and political institutions were incorporated into the colonial states, and they documented the reasons, motives, benefits and losses underlying the reactions of African chiefs to colonial demands. The main focus of such inquiries has been the reach of the colonial state, the resources (imperial, metropolitan and/or indigenous) mobilized to effectively dominate African societies, and the place and role of local processes in colonial governance.

Nonhistorians, in particular political scientists, have joined in the debate – "laterally", as part of the assessment of the nature and the legacies of colonial rule in contemporary Africa. Four scholars are worth citing in this context: Karen Fields, Crawford Young, Mahmood Mamdani, and Jeffrey Herbst. Of course, their very theoretical and less time-bound understanding of colonialism and its trajectories in various imperial settings has found little favor with historians. It has, however, forced them to revisit the colonial state and reconsider the actions, motives, theories and ideologies, and practices of colonial actors.

Three approaches dominate the historical literature. The first focuses on the European discourses, representations, and racial assumptions of colonial domination at the expense of local processes, and the reactions of colonial subjects to Europeans policies, ideologies, and discourses. The second approach interprets the colonial institutions as vehicles that have had radical transformative effects on colonial societies. According to this approach, the colonial condition is a moment of closure and "conversion" (V.Y. Mudimbe). The third

approach argues that the colonial state was a fragile and hybrid construction rooted in a very contested terrain. In this approach, much attention is devoted to the process by which colonial brokers – and the paths they constructed to status, wealth, and influence – shaped the colonial state.

David Robinson's *Paths of Accommodation* challenges the assumptions of the first and second approaches and expands the epistemological, theoretical, and empirical gains of the third. He views colonial rule as a dynamic process in which more attention should be paid to the agency of colonized actors, who have always been engaged in the struggle for power, status, and prestige. The emphasis of his study of Senegal and Mauritania is on the collaborative nature of the relationships between African leaders and French administrators. The book is divided into three sections. The first, "The Framework", deals with the transition from African autonomous polities to the establishment of colonial rule. The second section deals with "Bases of accommodation" and the production of a specific knowledge to rule Senegal and Mauritania, in association with Sufi clerics and the Francophile Muslim community in Saint Louis. The third section deals with the various Muslim clerics who used different paths and knowledge to help colonial officials govern (in the sense defined by Gramsci and Foucault) Senegal (Malick Sy and Amadu Bamba Mbacke) and Mauritania (Saad Buh and Siddyya Baba).

Robinson argues that the process of negotiation and contestation between local actors, Muslim Sufi leaders in particular and French officials – a process he calls "patterns of accommodation" – was of central importance in the construction and operations of the colonial state. In presenting this argument he guides his readers through a careful and critical discussion of his sources and methodology. His book makes two very important contributions. Firstly, there is the very careful and thoughtful examination of the making of France as a Muslim power in West Africa, of colonial practices as a contingent response to local situations rather than as the result of any long-term policy, of the effects of such policy interventions, and of the construction of communities in both Senegal and Mauritania. Secondly, the book unveils the colonial taxonomy by which Muslim communities were classified as radical or moderate within an administrative framework based on three features: customary law, Muslim courts, and universal/metropolitan citizenship, which defined how African communities were to be managed.

Paths of Accommodation is a useful and creative addition to the historical literature on the nature of the colonial state and the role of intermediaries in shaping colonial institutions and policies. However, a wider and more flexible use of oral testimonies and indigenous sources might have broadened the center/periphery approach that privileged the French construction and documentation of Senegal and Mauritania as colonial states at the expense of the voices of the intermediaries and their influence in the networks constructed as the result of their interaction with colonial officials. Revealing their voices and choices more systematically would have helped to contextualize the strategies of the colonial brokers and permitted a reconsideration of the discussion of colonial history in the wider global social and political historical debate about empire, citizenship, rights, and domination.

Mamadou Diouf

Peck, Gunther. Reinventing Free Labor. Padrones and Immigrant Workers in the North American West, 1880–1930. Cambridge University Press, Cambridge [etc.] 2000. xiii, 293 pp. Ill. Maps. £35.00; $54.95. (Paper: £11.95; $19.95.); DOI: 10.1017/S0020859002640623

One of the most sinister characters in the Progressive pantheon of evil was the *padrone*. This malicious migrant had supposedly brought old-world feudal relations to the new world of free labor where his nefarious doings kept immigrant workers in ignorance and servitude. Gunther Peck dissects and demolishes this myth in his award-winning *Reinventing Free Labor*. But Peck uses his initial study of the *padrone* as a basis to launch inquiries into immigrant labor, the North-American west, gender, racial, and class constructions and the many uses of "free labor".

Peck studies Ramon Gonzalez, Leon Skliris, and Antonio Cordasco, respectively a Mexican, a Greek and an Italian *padrone* operating in the US and Canadian west at the beginning of the twentieth century. Setting himself against the myth of the free and open west, he uncovers a region built in part by a vast system of coercive labor. He also writes against the labor history tradition of studying stable workers and their communities by emphasizing the spatial rather than temporal dimensions of class.

Peck is in the best tradition of modern immigration history as he traces groups of workers from Italy, the Peloponnese and Crete, and central Mexico from their home villages through their *padrone*-controlled recruitment and transit to their numerous and often mobile jobsites. Whether as track workers, or miners, or a host of other low-paid jobs, these migrants provided the supplementary labor force which made western development possible.

Certainly his treatment of the spatial dimension in and of itself would be a major contribution. For it was the *padrones* who found that they could commodify space. Through their contacts in the Greek isles or central Mexico they brought workers, legally and illegally, into the US and Canada, delivering them to a railhead or a mine for a price. Far from being a premodern holdover, these *padrones* understood the most modern needs of capitalism and dealt with some of the largest capitalists and corporations like John D. Rockefeller or the Canadian Pacific Railroad (CPR).

Here is a different west from that celebrated by Frederick Jackson Turner or Robert Service. Conditions for common laborers were often intolerable and the native-born would only accept such work under strict limitations. Italian migrant laborers might technically be free to depart, but when they were at an isolated railroad worksite in British Columbia their possibility of successfully leaving was slim. Similarly the Greek migrant who got a job in a Utah copper mine through Leon Skliris would find that quitting his job did him no good, since Skliris controlled mine labor contracting throughout the area. And at least for a while Cordasco or Skliris could point to a labor contract signed by the migrant to further bind him to the job. Moreover the *padrone* or his agents might have trained the migrant in how to evade the US or Canadian immigration laws and thereby claimed a moral right to profit from his labor. Therefore, these "free" workers were in practice very far from "free". They chafed constantly at these restrictions and all the petty tyrannies of weekly employment fees or being forced to use a *padrone*-supplied commissariat.

Nor could the law offer them protection. Immigrants arrived at US borders supposedly protected by the 1885 Foran Act which banned imported contract labor. This law forced them to deny that they had an agreed upon job waiting for them. But at the same time they had to prove under other immigration laws that they were not likely to become a public

charge. That is, at Ellis Island or any of the other ports of entry and border crossings, immigrant workers had to learn the fine art of deception. Here again, the *padrone* was initially essential, teaching them what to say and how to say it. Cordasco at one point was successfully breaking immigration laws in Italy, Canada and the US.

And yet, the reign of these *padrones* lasted only a few years. Regardless of temporary structural constraints, these immigrant workers actively challenged how they were controlled. In part this was due to their specific cultural baggage. The young Italian migrants might at first have accepted Cordasco as another father figure. But as they gained experience they began to look for more fraternal than paternal relations. Ever malleable, he could even portray himself as a big brother. But eventually the Italians wanted to be their own men and used their own mobility to thwart the *padrone*. Similarly, the young Greek miners recreated their own culture, complete with male dancing, ceremonial Cretan men's skirts, and an amazing number of guns. The stronger these communities became the more they resented and resisted the *padrone* and the owners.

Here it is worthwhile to say a word about the excellent illustrations that are such an integral part of the book. From the tabloid press presentation of the *padrone* to the photo of Greeks with bottles and guns, Peck has chosen a stunning group of pictures. And kudos to Cambridge University Press for including a full bibliography as well.

The photographs also help Peck's fascinating discussion of "whiteness" among these different groups. He shows that, "whiteness was contingent upon what occupation immigrant workers assumed", as well as a host of other factors (p. 166). So, Italians might be nonwhite in western Canada where they formed a large number of common laborers, while they were white in Texas where their small numbers contrasted with the many Mexicans. But were Mexicans truly nonwhite Mexicans (?) – not if they could successfully claim to be Spanish and thus white. Immigrants in turn created and mobilized different notions of race. Greeks might be the subject of Klan attacks and legal discrimination. But by their united action they won the approval of the Western Federation of Miners which finally welcomed them as members while at the same time banning the Greek workers' close allies – Japanese workers.

At last, the *padrone* system began to erode. There were competitors ever looking to win the coveted labor contract. A warm spring on the Canadian prairies might change the CPR's labor needs and disrupt the many workers already flowing out of Italy. Most of all, workers themselves grew more knowledgeable and better organized. No longer did they need the *padrone* to tell them how to get to the US, a cousin or brother could be more helpful. As they grew more used to the system the exploited turned the table on the exploiters, accepting the *padrone*'s railroad trip to a contracted jobsite, but then leaving on the way for a better job, making their own use of spatial mobility.

What is the final value of Peck's work? I believe that the *padrone* provides the scaffolding which allows him to make his many constructions of work and space, class and community, race and gender. Remove the scaffolding and the construction will remain. For, in fact, these three *padrones* exercised power for a very few years. Furthermore, a bit of family history. My great-grandfather, Louis Till, came to New York in the late 1890s. Somehow or other he became owner of a dockside saloon (to the scandal of his family and any future in-laws). Family legend has him also running an employment bureau on the second story. Was he a *padrone*? I doubt it. But I believe there were thousands of similarly placed businessmen, immigrant and native-born, who did some labor contracting as one sideline among many. Peck's three subjects may simply be at one end of a very long

continuum. But this does not at all detract from the many valuable contributions that he makes.

The story of new groups of immigrant workers paying middlemen to get to the North-American labor market, finding willing employers, avoiding hostile immigration officials, and then amassing knowledge, building community, and reworking gender and race, is one of the great constants of labor history. Today's "coyotes" and "snakeheads" make those long ago *padrones* seem almost benign in comparison.

Seth Wigderson

Green, Anna. British Capital, Antipodean Labour. Working the New Zealand Waterfront, 1915–1951. University of Otago Press, Dunedin 2001. 202 pp. Ill. NZ$39.95; DOI: 10.1017/S002085900265062X

Anna Green has filled an important gap not only in the history of New Zealand but also in our understanding of waterfront work in that country. It is somewhat surprising that this is the first research based study on the history of dock workers in New Zealand. Overseas trade, particularly with Britain (the "home" country), was vital to the development of the economy. Major exports were meat and wool and imports included manufactured goods, steel, timber, coal, guano, and phosphates. The transhipment of goods depended not only on the shipping lines but also the waterfront workers who loaded and discharged the cargoes. In charting the experience of dock workers from 1915 to 1951 she has reassessed the role played by the employers and the state in the turbulent industrial relations that characterized dock work. The climax of this study is the major dispute of 1915 which up to now has been seen as a conflict between an obdurate dockers' union and the government. Anna Green has, however, thrown fresh light on the role played by the employers who were wrongly perceived to be impotent bystanders.

This study focuses on three ports: Auckland, Wellington, and Lyttelton, and is enhanced by thirty-five oral interviews. Of these, twenty-four are of waterfront workers with work experience from the 1930s, and eleven from shipping and stevedoring companies, the Waterfront Commission, and others. Such material permits a deeper understanding and a vividness on the labour process at the waterfront.

Dock workers throughout the world have been subject to insecure employment under the casual system, harsh working conditions, low pay, and long hours. New Zealand workers were no different. Relationships between employers and their workers were hostile and the press generally antipathetic towards the aspirations of the dockers when seeking to redress grievances. In one respect, however, New Zealand was different from many ports in that the government played an active role. As early as 1894 the Industrial and Conciliation Act created an arbitration court and from 1940 a Waterfront Commission was established. The active role of these government agencies added a complexity to industrial relations.

In her opening chapter the author concentrates on the role of the employers. There were four groups of employers on the waterfront, of which the two most important were the Union Steamship Shipping Company handling coastal and trans-Tasman trade, and the British Conference lines (Benmacow) consisting of the Shaw Savill and Albion, the New Zealand Shipping Company, Port Line, and the Blue Star Line. This latter group, with headquarters based in London, exercised a monopoly of trade between Britain and New

Zealand which was jealously guarded. Although it may never be known in detail, the high freight rates Benmacow was able to negotiate with the government or produce boards made this trade a "rich harvest".

Subsequent chapters examine in detail the loading and unloading of ships, the labour process, and the development of trade unionism at the waterfront. As in all ports, the work was dangerous, exhausting, and unhealthy, made more so by the importance of the export of perishable goods that had to be undertaken in refrigerated holds. Trade unions originated on a port basis from which developed a federated union in 1906. Following its collapse, a second federation was established in 1915 that lasted until 1936 when it was renamed the New Zealand Waterside Workers' Union. Supplementing the activities of the union to control the hours of work and improve pay and conditions were the informal resistance strategies of the rank-and-file dockers: spelling (taking rests or absence from the job), gliding away (leaving the job early), go slows, and theft. These were all ways by which dockworkers exercised some degree of control over the work process. Although employers complained of these practices they were unable to eliminate them while the union leadership was ambivalent. They were supported by the union at times of negotiation by putting pressure on employers but they undermined its efforts to gain and maintain better conditions and inhibited the fundamental aim of workers' control.

What makes this book of particular interest is the role of the state. Following the election of a Labour government in 1935 and the breakdown of negotiations between the shipping companies and the union, major reforms were imposed by the new administration. Waterfront employers were forced to accept a labour-bureau system of engaging and allocating work among the dockers. This partially decasualized the work, considerably reducing the ability of employers in hiring and firing. A second reform was implemented in 1940 in the context of the war effort. Following yet another breakdown in negotiations and faced with unrest among the dockers, the government commissioned a report on the waterfront which led to control over the wharves through the Waterfront Control Commission.

The penultimate chapter of the book examines the rash of postwar conflicts that reached a climax in 1951. Concern over the safety of hatches, rates of pay for particularly noxious cargoes, and issues over a minimum guaranteed wage were the basic reasons for the dispute. Employers were becoming increasingly exasperated by constant disruption and sought a showdown with the union. They were assisted by the election of a conservative government in 1949, less sympathetic to waterside workers. A Royal Commission was established to investigate the problem of the waterfront. All interested parties were invited to give evidence but the union boycotted the proceedings because the government refused to include an examination of the profits of the shipping companies in the terms of reference. This proved to be a fatal mistake. Employers made the most of the public hearings and daily newspapers vilified the waterfront workers as lazy, overweight thugs entirely responsible for the waterfront problems. A five-month strike and lockout ensued over the union ban on overtime. The government responded by emergency legislation that drastically curtailed civil liberties, including the seizure of the assets of the union. The shipping companies virtually disappeared from public view. However, the collapse of the strike proved to be a notable victory for the employers. The government set the terms for the return to work which reflected employers' wishes. There was to be no national union of dockworkers. Port unions were established but men were screened before being taken

on. The shipping firms were now able to reassert managerial prerogatives that they had sought for years.

In 1951, the New Zealand public perceived the conflict as between the government and a militant union which would not accept the national system of industrial relations. For fifty years waterside workers have been the villains of New Zealand history. By taking the long view, however, Anna Green challenges this perception. The iniquities of the labour process over three decades, poor employment practices, the indifference of employers to the safety and welfare of their workers, and inadequate management were the major causes of industrial conflict. Every advance made by the workers had to be wrested from recalcitrant employers either through industrial action or the intervention of the state. The conflict of 1951 was the culmination of decades of hostility between the two contestants rather than a confrontation between the workers and the government.

This book has many merits. It is meticulously researched and well-written, with a variety of photographs illustrating differing features of port work. Above all, it throws light on the experience of port workers in New Zealand which up to now has been neglected. It lacks, however, any reference to the role of women. The waterfront was a man's world yet they had mothers, wives, and children. What role did women play in support of their men? How did they manage to make ends meet faced with the erratic earnings under the casual system? How did they cope with poverty and deprivation? These are questions that the author has failed to address and which would have provided a more complete picture of the working lives of dockers and their families.

It is to be hoped that Anna Green will continue her research. There have been major technological changes in the last fifty years that have transformed dock work throughout the world. The waterfront is now a capital-intensive industry and the workforce has been drastically reduced. Has the managerial freedom secured by the shipping companies in 1951 persisted and what impact has this had upon the labour process in the twenty-first century?

Eric Taplin

Osella, Filippo and Caroline Osella. Social Mobility in Kerala. Modernity and Identity in Conflict. [Anthropology, Culture and Society.] Pluto Press, London [etc.] 2000. xi, 320 pp. Maps. £55.00. (Paper: £19.99.); DOI: 10.1017/S0020859002660626

This comprehensive ethnographic study by two social anthropologists of a community in the southern Indian state of Kerala adds a welcome perspective to the abundant literature on the "Kerala Model" (or "Kerala Experience", as some prefer). Kerala is known mainly for its radical policies, and for the paradox of having higher social indicators – but considerably lower GNP per capita – than the rest of India. Its citizens are educated and widely reputed to have a high degree of political awareness.

Going beyond the "Kerala Model", or any mere quantitative study of welfare and development, the Osellas treat an issue often studied in the context of Kerala and progress, viz. social mobilization. They have penetrated Keralite society to gain a deep understanding of its members, the Malayalees. The authors' interest in Kerala goes back more than a decade, during which time they lived in a village among the indigenous people, and even managed to learn the Malayalam language – a difficult feat for Westerners.

The focus of their study is the Izhavas, an ex-"untouchable" caste who have mobilized

themselves and succeeded in raising their status over the course of the twentieth century. Drawing mainly upon a theoretical framework established by the late Pierre Bourdieu, and utilizing such concepts as economic, social, cultural, and symbolic capital, the authors reject essentialist categories of caste and class. By denying essential identities and highlighting the constructed nature of categories, the Osellas' book differs from most other analyses of Keralite society. They frame their study within the complex relationships of colonialism, tradition, modernity, and globalization. When designating various forms of capital, the authors extend Bourdieu's analysis and distinguish between positive and negative capital, characterizing the latter as "a sort of negative balance to be erased, or negative equity which needs to be compensated".

At the turn of the twentieth century, the Izhavas were stigmatized by association with the pejorative connotations of manual labour, drunkenness, "toddy tappers", "devil dancers", untouchability, and even unapproachability. Under their leader, Sri Narayana Guru, they were organized into the Sri Narayana Dharma Paripalana Yogam (SNDP), with the stated aim of modernizing, uplifting, and advancing the whole Izhava community. This 100-year journey is traced as the community changed identity along the road to "modernity".

Izhava men have long struggled to escape from being manual labourers associated with coconut trees and their products (including toddy and drunkenness). Many of them now have more prestigious occupations, a number having gone off to Gulf countries, where they work and send back substantial sums of money. Izhava women, on the other hand, have embraced the role of housewives – at least at the ideological level. By emulating the dominant communities of their neighbours, the Nairs and the Christians, the Izhavas have grown more bourgeois. Accordingly, being a breadwinner has become an indication of success and masculinity for a man. What the authors do not mention is that femininity has simultaneously become associated with dependency, seclusion, and "homeliness", and this has had a negative impact on gender relations and issues of women's empowerment.

This book singles out efforts to pursue mobility through strategic marriages. Older customs, such as the matrilineal system, cross-cousin marriages, marriages without dowries, marriage to a local person, polyandry, and the union of Nair men with Izhava women (in a polygamous fashion) are considered nonprestigious and to be avoided. Whereas in times past, higher-caste men had alliances with lower-caste women, this era is now derided as a time when "Izhava women were prostitutes for Nair men". The abandonment of polygamy and of the matrilineal system holds true not only for the Izhavas, but for other southern Indian communities as well. Modernity and progress have promoted caste endogamy and monogamy as the only socially acceptable solutions to the organization of reproduction.

The fact that in this process excessive dowries have come to be paid to the bridegroom's family, a practice unknown in the first half of the twentieth century, has proved devastating. This new "custom" has made women more vulnerable, and they are now seen as burdens to support. Men, on the other hand, have grown conscious of their heightened value in the marriage market. This has led to a change of power relations in men's favour. Surprisingly, this development, which is very negative from a gender perspective, is not discussed by the authors.

Another area where it has been possible for the Izhavas to achieve increased status is in consumption patterns. By making money in Kerala or, more often, by means of migratory labour, middle-class Izhavas have shifted to more future-oriented consumption. They now

invest in land, a house, or education for their children. Just as marrying outsiders is considered more socially prestigious, so imported goods are preferred when it comes to such items as refrigerators or televisions. However, land in one's native place and local produce are seen as qualitatively superior. Izhavas prefer either traditional buildings favoured by high-caste Nairs, or newly constructed, modern houses, usually associated with Christians. This is a reflection of the ambivalent stance many Izhavas have regarding which of the two "higher" communities to emulate.

The process of Sanskritization, i.e. imitating the cultural and religious patterns of higher castes (mainly Brahmins), was utilized by Sri Narayana Guru. Many Izhavas today try to shake off their "Izhava-ness" by detaching themselves from the SNDP and emulate higher-caste behaviour (mainly that of Nairs). Nevertheless, the authors reject the essentialist dualism of Sanskrit vs. non-Sanskrit, pointing out that people may make eclectic choices according to their own desire for status. Here again, a critical gender perspective might have been profitably applied by examining how newly adopted religious customs have affected men and women in different ways.

The authors refer to power as a kind of hidden capital. Such power has been achieved through the SNDP, as well as through party politics. Izhava males engage in a patron–client system, hoping to become "big-men" who handle village problems, often assuming the role of representatives of political parties. What Sri Narayana Guru had initiated as caste mobility has become a class struggle. Because Izhavas seek to escape from their caste identity, there has been a failure to create an All-Izhava political party. The Osellas underscore this complexity of class and caste, rejecting simplified analyses of modernity that characterize development in terms of a trajectory "from caste to class". They argue that caste cannot be separated from class; rather, that it "is the modality in which class is lived".

This book shows that caste has not lost its significance for a people's identity, although it may currently be articulated by other means than in times past. Today, people may have friends in various castes. They join in collective demonstrations or come together in public places and in this way occasionally overcome caste barriers. Nevertheless, the same people seldom enter each other's houses or accept food from a lower-caste person. Ultimately, caste is reproduced through marriage, an institution that rarely transgresses caste barriers.

The authors contest modernity as an analytical concept, arguing that it is always contextual. Thus, one may find many "modernities" apart from the Western interpretation of the concept. Modernity can even include a category that we would call tradition. In spite of having achieved "absolute progress", the Izhavas have not changed their position in the social hierarchy. "Modernity" and the "Kerala Model" have led neither to the dissolution of caste, nor to the crumbling of class hierarchies. Christians are often stereotyped as rational and modern, while high-caste Nairs are viewed as carriers of an honourable tradition. Caught between these two communities, the Izhavas have simultaneously embraced tradition *and* modernity in an effort to raise their status. Despite their struggle to advance, however, they continue to lag behind the other two communities, who have already acquired new habits, higher-status jobs, and a better overall economic position. As a result, progress for the Izhavas has been coupled with relative immobility. Referring once more to Bourdieu, the authors point out that a deeply hierarchical structure can only be dissolved if its component categories are changed. Caste continues to remain a highly important part of a people's identity – no matter how full of contradictions and complexities that identity may be.

The Osellas frequently consider gender issues, making their approach a welcome addition to the extant literature on Kerala. Still, one wishes they had drawn even more on their own experiences and analysed changed gender relations in terms of power.

It is questionable whether readers not familiar with this part of the world will sustain interest throughout a book packed with so many details (a reflection of the authors' impressive knowledge), including some 500 indigenous concepts. As these are explained or translated only the first time they appear, the reader must constantly resort to the glossary – often several times on a page. For professionals in the field, however, what the Osellas have presented will probably attain classic status. It provides one with a deep understanding of Keralite society, setting it within a complex analytical framework that goes far beyond most previous literature on the area.

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