

## Book reviews

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Michael Quinlan, *The Origins of Worker Mobilisation: Australia 1788–1850*, Routledge: London, 2017; 308 pp., ISBN 9781138084087, AUD 221.

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When I was writing *The Southern Tree of Liberty* (2006), developing an argument about the early years of working-class politics in Australia, I relied heavily on articles by Michael Quinlan and his collaborators (Irving, 2006; Quinlan et al., 2003). Before they appeared, historians of early labour had to work with measly information: some incidents of pre-modern convict resistance, a few tiny unions, a few short strikes, a few ‘immature’ workmen’s organisations. The result was, in his words, a set of ‘fractured accounts’ of worker activity. The new approach in Quinlan’s articles knitted the fractures and dazzled us with data. Let’s not focus just on the narrow, formal aspects of worker organisation, he said, let’s see how much informal activity there was. Answer: a hell of a lot. Let’s also use a concept that makes sense of this activity, and that places it in the context of state and capitalist power, the idea of a class in formation – the working class. This is why his book is called *The Origins of Worker Mobilisation* – to put the stress on a class process rather than on the usual fare of labour history, strikes and unions, cultures and ideologies. The title is also a pointer to its significance, which is far greater than its impact on those of us working in the field of early Australian labour history. Throughout the research that went into this book, Quinlan kept in mind the question that underlies all others in labour history namely, how and why do workers come together?

The evidence Quinlan presents in the book is both qualitative and quantitative, but it is the latter that gives the book its distinctive character. For three decades, using a specially designed relational data base, he has been entering information about worker mobilisation, informal as well as formal, making a file for each instance, no matter how small or ephemeral, and not just for New South Wales, but for the other colonies as well. He did this personally to ensure consistency. He recorded evidence of strikes, court actions, go-slows, demonstrations, mutual insurance schemes, petitions, mass absconding, sabotage and political meetings. He gained this evidence through the painstaking reading of a huge range of sources, including convict conduct records, police gazettes, official correspondence, court bench-books and colonial newspapers. By the time he sent off the manuscript to the publisher, he had recorded 6426 instances of worker mobilisation. Remarkably, this is probably only about two-thirds of all the instances of organisation for which evidence can be expected to exist, because, as he explains, his research in the court records is incomplete.

A little over half of this book is given over to presenting and quantifying the evidence collected in the data base from different research perspectives. For example, Chapter 4 analyses the components of organisation, and each of chapters 5–10 is dedicated to a separate industry grouping: transport and maritime, rural and extractive, construction and building, manufacturing, government and community services, and commercial, personal services and retailing. In these chapters, the analysis is conducted in terms of the characteristics of action coded by the database. The most interesting of these are whether the organisation was formal or informal, involved convict or voluntary labour, whether it was a strike or a form of non-strike activity, whether female workers were involved, and the objectives of the organisation. And there are many lesser attributes such as duration, numbers of workers taking part, and location (in or outside a capital city) that are part of the analysis. This produces in these chapters a very fine-grained empirical analysis. Luckily for the reader, the writing is lifted by the inclusion of information about the struggles of actual convicts and free workers.

Quinlan's book is, however, much more than a report on a quantitative research project. Indeed, its importance rests on the wider framework in which the research is placed. Chapter 1 begins with a succinct account of the collective impulse in earlier scholarship, tracing the idea through early institutional studies of unions, the social history of labour under the impact of class analysis from the 1960s, to more recent studies of unfree labour, especially slaves and convicts. In connecting to this unfree labour literature, Quinlan launches his book into the developing field of global labour history, whose two legs are informal labour resistance and precarious labour, whether in the Global South or in the long history of labour relations before the emergence of the 'classical' or standard employment relationship in the 'Western' labour movement (Breman and Van der Linden, 2014). One of the key findings from the data base is the preponderance of informal convict organisation. His table summarising collective action reveals that 56.7% of strikes and 88.1% of non-strike collective actions were conducted by convicts (p. 115).

Another aspect of his framework is the role of the state. Quinlan reminds us that the Australian colonies were prime examples of state-facilitated capitalism and adds that the role of the state provides a further link between the Australian evidence of worker mobilisation and studies of the 17th-century slave societies and more generally of early European capitalism's reliance on state power to subordinate labour. In Chapter 2, 'The Law, the Courts and Inequality at Work' he argues that 'collective action by workers can only be understood when the centrality of the state and regulation is recognised' (p. 35). He details the 'sheer scale of regulatory activity' in the Australian colonies and the widespread resistance by individual workers that can be found by the study of litigation. He suggests that there were at least 330,000 employment-related cases between 1788 and 1850. These cases are windows onto workers' lives and experiences, especially when, as sometimes happened, workers used the courts against their employers. Quinlan insists on the significance of these cases: they show the experiences of exploitation and oppression that workers drew upon to mobilise and develop a class identity. The courts, as Quinlan writes, were an intense area of struggle that researchers often overlook (p. 67). In this, one of two stand-out chapters in the book, the argument is clinched, and the richness of the qualitative material assembled in the data base is revealed.

The processes of class formation are further developed in the other stand-out chapter, the one on peak and political organisation. Here, the key analytical point is the evidence of the politicisation of worker issues such as unfair competition created by state policies of convict transportation and assisted immigration, and the cross-over of radical intellectuals into labour campaigns and worker leaders into the movement for popular democracy. Quinlan's data base contains evidence of 20 political organisations of workers prior to 1851 (p. 267). (An interesting comparison would be with the number of 'liberal' political organisations formed by the business and professional classes at this time. I think the working class would stand up pretty well in this comparison.) Clearly what we are seeing here is a critical moment in the formation of the working class, its acquisition of political knowledge and its development of political practices.

This raises an important issue for Quinlan's project: what contribution does the quantitative analysis of a data base of 6426 instances of worker organisation make to a history of working class formation in Australia? Note that I said quantitative, because in this excellent chapter the evidence from the data base is entirely qualitative. Moreover, this chapter is about formal organisation, whereas the argument of the book emphasises the importance of informal organisation, which was constant but mostly small-scale and always ephemeral. This was a stage of mobilisation that was, to use a term that recurs in the book, volatile. Might it not be said that this volatility was no more significant than white noise, an irritant to the ruling class who knew how to suppress it without provoking an orchestrated response? Might it not be said that these quantitatively analysed actions are valuable pointers to the internal processes of working-class formation (especially to a wide range of different situations and experiences), but not much use for a history of class struggle?

Quinlan understands this issue. On pages 285–287, he writes about class as a relational concept, and his project was never intended as a history of class struggle. I raise it because I think there is room for future scholars to use his quantitatively analysed evidence to do what I'm suggesting. They will ask questions about periods and turning points, and in doing so will want to consider, as historical conjunctures, Quinlan's model of 'layers or transition points' in the development of worker organisation (pp. 294–295). Scholars will note on page 84 the figure showing the instances of formal and informal worker organisation and note the peak of informal activity in 1833, the peak of formal organisation in 1890, and the crossing point of the two modes of organisation as late as 1867. They will note the strike waves of 1826 and 1840 and, roughly speaking, their coincidence with peaks of non-strike collective activity (see p. 112). Then they will find the qualitative evidence about working-class learning and strategizing to make sense of these moments, and the ruling class responses to them. They will situate them in gendered and racial dynamics.

In its own terms, Quinlan's book is a major achievement, not least because it has the capacity to propel the history of class structures here and overseas in new directions. Moreover, it is timely, as he points out, to consider this history in a period when precarious labour and informal methods of struggle return, as the global working class' reality, in the neo-liberal era.

## References

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Alex Millmow, *A History of Australasian Economic Thought*, Routledge: London and New York, 2017; viii +250 pp., ISBN 9781138861008 (hbk), 9781315716162 (ebk).

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This is a very welcome and in some ways also a very ambitious book. The previous histories of economic thought in the Antipodes by Craufurd Goodwin (1966) and by Peter Groenewegen and Bruce McFarlane (1990) restricted their coverage to the western side of the Tasman Sea, but Alex Millmow has chosen also to include New Zealand; I estimate that somewhere between one-third and one-quarter of his book is devoted to Kiwi economics. In one sense, however, his ambitions have been surprisingly restricted. The book begins not in 1788 but in the mid-1920s, and it ends seven decades later, in the late 1990s; the decision to say nothing about developments in the last 20 years is never satisfactorily explained.

After a brief introduction and a short chapter 1, ‘Setting the scene’, Millmow devotes the second chapter to ‘The professionalisation of Australasian economics’, where he describes the principal university departments of economics in both countries in the 1920s and concludes by examining the formation of the Economic Society of Australia and New Zealand in 1924 and the publication of the first issue of the *Economic Record* in the following year. The next eight chapters are devoted to successive decades, from ‘The practical utopia of economics’ in the 1920s (chapter 3) to ‘The age of economic reform’ in the late 1980s and 1990s (chapter 10). The brief concluding chapter 11 offers Millmow’s assessment of ‘Australasian economics at century’s end’. At every stage, a great deal of attention is paid both to institutional developments and to the role played by Australasian economists in the formation of economic and social policy. A very high level of scholarship is displayed throughout, with the ample footnotes and bibliographies that are provided at the end of each chapter accounting for almost 40 pages of text.

I was particularly impressed by chapter 4, ‘Ordeal by fire: Australasian economists and the Great Depression’, and chapter 5, ‘How Keynes came to Australasia’, both of which draw heavily on an earlier book (Millmow, 2010). Here, Keynes is revealed as a supporter of the Premiers’ Plan, and hence as an advocate of money wage cuts for Australia, in flagrant contradiction to the case against them that he would soon make in chapter 19 of the *General Theory*. Millmow notes that Kiwi economists had much less influence over macroeconomic policy in the wake of the Great Depression than their Australian counterparts, with the post-1935 Labour government in New Zealand increasing its expenditure significantly and imposing exchange and import controls