Reviews


The history of the earlier years of the Australian Agricultural Company has been the subject of various publications and theses over the years. One reason for this has undoubtedly been the copious documentation that has survived, detailing its activities and those of the men and women who were associated with it. Another, as Damaris Bairstow claims at the end of this book (p.366), is that: ‘The A.A. Company’s Port Stephens estate is ... a microcosm of the Australian continent and of its settlement’. In a similar fashion, high hopes and ambitious plans frequently led to disappointments, often because of a lack of understanding of unfamiliar environments, compounded by unsympathetic direction from the other side of the world. Here is a story of both heroic efforts and mean-spirited villainy, of occasional triumphs and frequent disasters; a story in which few of the participants were to survive unscathed. In this new book the author has told that story in an even-handed fashion, concentrating on the 1820s and 1830s, and examining in some detail the lives of people involved as well as the economic and political aspects that shaped the A.A. Company’s fortunes. The grievous consequences of colonial settlement for the Indigenous people of the Port Stephens area are also considered.

Twenty years ago, the author completed a PhD thesis on ‘The Australian Agricultural Company at Port Stephens’, for the University of Sydney. Significantly it was subtitled ‘An archaeological contribution to history’. On the inside of its back cover, the present book is stated to be partly a product of that thesis but mainly the result of further research. Indeed, this does appear to be the case because physical evidence makes a relatively minor contribution to this book; essentially it is concerned with documentary history not with archaeological investigation. So much is this the case that the book is characterized by frequent quotations from the documentary sources, in a manner favoured by some historical writers. The author, it seems, wishes the documents to speak for themselves, as far as possible, but some readers might find themselves wanting more discussion of the information thus presented. Furthermore, such treatment tends to give parts of the book the character of a descriptive chronicle and at times makes it somewhat disjointed and less easy to read than it might have been. The sources of both quotations and of information used elsewhere by the author are meticulously referenced but with a system of superscript numbers and endnotes that are at times difficult to follow. There is also a fairly substantial Select Bibliography, as well as three appendices containing documentary details of livestock and personnel. In addition, the book has an Index of eleven pages.

The result follows the earlier fortunes of the Company in considerable detail, particularly during the periods when Robert Dawson was Commissioner (1824–1828) and Sir Edward Parry was Commissioner (1830–1834), both of whom were specially sent out from Britain. Dawson was sacked from the post, in circumstances that seem to discredit the Company more than him, although Bairstow appears reluctant to take sides in the matter so that the reader is left to wonder about the affair. He does appear to have accomplished a great deal for which he got little credit from the committee of Australian advisers, whose advice was too often tainted with self-interest. However, he also made mistakes and showed poor judgement in some matters but such were to be expected of someone new to Australia who took on the agro-pastoral development of a little-known area. One is left with a strong suspicion that Dawson was treated unjustly. In contrast, Parry was a tougher proposition, a Royal Navy captain who had been knighted for his Arctic explorations and given an honorary doctorate by the University of Oxford, as well as being made a Fellow of the Royal Society. Supported by his fervent belief in God and his quite exceptional wife Isabella, his time in Australia was much more successful, although significantly he did not seek to renew his appointment when it expired. Bairstow’s book is at its best when dealing with the Parrys. The documentation, both official and personal, is extensive and has enabled the author to paint a sensitive and highly detailed picture of the life of these two remarkable people.

The parts of the book dealing with the lives of many of those who worked for the Company, officers, indentured servants, emancipists, convicts, and others, as well as their wives and children, are particularly valuable. In particular, the author takes us into a world of poor communications, loneliness, and fear of the unknown, punctuated with droughts and floods, animal diseases, crop failures, and human sickness. A world of unremitting labour for some, from which many escaped into alcoholism, and where personal animosities thrived. A world where women of all classes were repeatedly pregnant, could expect little medical care, and whose children often died at birth or in infancy. This in spite of the better conditions of those who worked for the Australian Agricultural Company, compared with the situation of some other early settlers. Is it any wonder that many occupants of Company settlements tried to make their homes and gardens look like what they have left in England; is it any wonder that, led by Edward and Isabella Parry, some turned to God for salvation? It should be no surprise that one of the most enduring structures from this period of the Company’s activities is St John’s Chapel in Stroud, built by the Parrys with their own money.

As a production, this book is not without its problems, in spite of an attractive appearance and layout. Published by the author, it presumably escaped the annoying ministrations of publisher’s readers and copy-editors, who render life miserable for those of us who write for major publishers but in the end improve the outcome. The result is a book whose structure is more complex than it need have been and whose rather uneven writing could have been improved. It also seems to have escaped professional proof-reading and there are numerous typographical errors in the text, in one instance a total of six on one page (p.365). Furthermore, in-text references to the maps seem to be confused in some places and, pleasing though the style of the maps is, some parts of the text would have benefited from more cartographic detail.

In conclusion, although this book contains little archaeology, it is of importance to historical archaeologists. As a source book concerning early colonial settlement, it provides valuable information about a major agro-pastoral estate and the people associated with it. Furthermore, it will be of particular use to future researchers who might eventually wish to investigate the archaeology of the Port Stephens Estate.
in greater detail than has yet been attempted. In these respects, whatever its problems, this book has made an important contribution to the discipline and we should be grateful to the author for the considerable amount of work that it necessitated.

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This monograph of 104 pages deals with surveys and excavations carried out by Eleanor Casella in 1992–1997 for her PhD thesis. As I offered her a floor to stay on when she passed through Sydney during the project I confess to perhaps not being entirely detached in my review. Also, being a good post-modernist, I confess right now that my 'objective' view of the past is coloured by lurid tales of the goings-on at the dig, earlier papers heard at conferences, and lengthy discussions about how this work fits, or does not fit, within the traditions of Australian historical archaeology. Therefore you may choose to take my overall conclusion—that this is an important publication and contribution to the discipline—as being heavily qualified and conditional. However, let me try to convince you otherwise.

The Ross Female Factory is situated roughly midway between Hobart and Launceston in the middle of Tasmania. The female factories, found in NSW and Tasmania, were women’s prisons aimed at both segregating women from the societal factors that contributed to their criminality and offering a structured form of industrialised work discipline—hence their name. Rather than stamping out license plates the women were occupied in daily rituals of control and routine, the influence of the scriptures and learning skills they could apply in their own lives or in domestic service.

Female factories feature as one stage in a long history of attempting to treat the condition of criminality in society by doing more than just imprisoning and punishing. Accepting that they achieved this aim was dependent upon accepting that work was redeeming, strict segregation had value in rehabilitation, that expressions of sexuality could be controlled and that the inmates were not innately (or, in modern terms, genetically) bad or criminal. The philosophy that made female factories logical only made sense by accepting specific tenets of western thought about the relationship between the individual and society and the way that society affected people and turned them into criminals capable of redemption. This view had evolved through the nineteenth century and was heavily influenced by the failure of past approaches to reforming known felons or to prevent society from breeding more. Exploring the female factory as an establishment and operating institution also therefore allows Casella to investigate how these relationships between individual and society were implemented, and how they changed through time.

The institution at Ross had a complex evolution. It began in 1833 with quarters for road gangs, and gradually grew until 1847 when it was decided to turn it into the last of the four female factories in Van Diemen’s Land. These were one element in a range of institutions for the imprisonment and control of women convicts in the colony. It was shut in 1854 following the cessation of convict transportation. In the seven years that it operated as a female factory there were a number of changes recorded to the structure. Some of these consist of ‘tweaking’ the design because the women were engaged in outbreaks of disorderly conduct. The changes in space appear to have been aimed at both ensuring better security and also at accommodating some of their protests about the conditions. From the historical account the women were not without power and certainly did not act as if they had none.

The archaeological excavations included exploration of the Crime and Hirings Classes (women were graded into those who were to be kept confined and those who could be engaged for domestic service), Assistant Superintendent’s Quarters and the Solitary Cells. These yielded a range of artefacts including Aboriginal stone artefacts as well as pre-1847 convict material. Casella’s discussion of the excavation strategy, stratigraphy, context and the artefacts is presented clearly and with sufficient detail to allow alternative interpretations of the artefactual, stratigraphic and structural evidence to be made. The illustrations are clear, especially Figure 1, the Dance of the Happy Trowels, which is perhaps all the more disturbing for its clarity.

The artefacts are interpreted to show that there was a large amount of ready trade in indulgences that were, in theory, forbidden. Clay tobacco pipes and ‘black’ bottle glass in a supposedly smoke-free and dry institution require some measure of explanation. Similarly ‘ecofacts’—food remains including animal bones, shell and seeds—support written records of food smuggling. Casella’s conclusions refer to the different view that archaeological evidence can provide of the way designed places were used. The role of contraband in maintaining an illicit link between gaoler and prisoner, and its use as barter currency inside, cut right across the way that the authorities had intended the place to operate. This may also have been brought in as barter for sexual favours, with the solitary cells particularly containing a lot of ecofacts. Prostitution, whether controlled by the women or in cahoots with their gaolers, would have fatally undermined the institution’s reformist intentions. If the implications of the artefact assemblage can be assured by further analysis and excavation, and it must be said that it is a small sample to work from, then the excavation has produced an excellent illustration of just how archaeological evidence can demonstrate the difference between intention and actuality.

The Queen Victoria Museum at Launceston is the repository for the archaeological collection and also the publisher of the monograph. It is a handsome publication, cheap at the price. There are very few archaeological monographs published about Australian historical archaeology, and even fewer that are as analytical and comprehensive as Casella’s. This is not the venue to explore why this is the case, but the Museum should be congratulated for taking on its publication and making a significant contribution to the home-grown literature of convict archaeology.

So, having heard about the volume, we go back to my big claim. Why is this important? Well, leaving aside its demonstration that you can still publish quality historical archaeology monographs in Australia, which shows up nearly all our locally based academics, agencies and consultants, I think there are three things that are important about this book. Firstly, it represents a non-Australian investigation of an Australian archaeological site. While we may lament interference in our own affairs from foreigners it is, I think, critical for the future of Australian historical archaeology that it begins to contribute to the global picture of the past two centuries. In so many ways Australia was an unrepeatable
social experiment and the value of its archaeology in informing international perspectives needs to be encouraged. And it can not simply be Australians going to WAC and SHA and saying what we found; an essential part of the deal has to be in making our archaeology more accessible for others to investigate, even for their own national perspectives and not ours.

The second reason Casella has produced an important work is because it does address female convicts as its subject, but does so, almost uniquely, from an archaeological perspective. There is a solid and expanding historical literature on women in colonial Australia, both free and convict, but little of it takes account of more than the most simplistic interpretation of the physical evidence. Casella’s interpretation of the artefacts, that they show otherwise undocumented but important behaviour of the inmates, reinforces the need to bring the analytical techniques and interpretations of historical archaeology into the historical analysis of female factories and women convicts, as well as showing its potential for convict archaeology all round. While we may predict that versions of the power structures among convicts and the inmates of institutions would inevitably develop and be maintained, Casella offers the first tangible demonstration that these are recognisable in the archaeological record, even though they may not be apparent in the documents.

The third and most important issue that makes this a book worth buying is that it makes an important contribution to demonstrating that these are recognisable in the archaeological record, even though they may not be apparent in the documents.

The monograph betrays its origin as a report to the management agency on the dig, being long on description and simplistic interpretation of the physical evidence. Casella’s work on convict stockades to Susan Piddock’s Factory monograph that strikes me now as a bit naïve. Yes, we do have a good handle on the bricks and mortar, and the implications of the changing use of space in reflecting different theories about how prisons should work as machines for making you better. This has been well-explored—from Karskens’ work on convict stockades to Susan Piddock’s detailed analysis of different types of institutions in colonial Australia. We do not have the next level of analysis down from that, which looks at how the convicts reacted to the changing regimes that were imposed on them. This is essentially an archaeological question because, as Casella shows, the documentary record is remarkably silent or ambiguous on the power structures that developed within the walls, the way that the secret inner world worked. The technical ability to find this is well within our capabilities as archaeologists, and the only tools we need are an ability to understand what we need to look for and the ability to see it once we have found the evidence. For that reason Casella gives you a chance to see what you may not otherwise see.

The monograph betrays its origin as a report to the management agency on the dig, being long on description and much shorter on interpretation. It is a good model excavation report that has all the things these are supposed to have. Read in conjunction with Casella’s other work, and the growing literature on convict archaeology [notably AHA 2001], it will be a rewarding addition to understanding convict institutions and for creating ideas on how to find out even more.


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It is believed that the event can be staged with such colorful and dramatic realism as to provide an arresting stimulus to a better evaluation and appreciation of the principles of freedom and democracy on which this Nation is founded – principles which today hold the hope of the Free World. (Francis F. Wilshire, superintendent of the Manassas National Battlefield Park, 1960)

This silly business would perhaps have served a more useful purpose, however, if re-enactment of the battle itself could have been replaced by realistic scenes of the field after the battle had been fought, when death and suffering and the indescribable tragedy of war had taken command – as they always have and always will. (Editorial, Sunday Star, Washington, D.C., 1961)

I bought this book at the World Archaeological Congress in Washington last year, and took it with me on a tour of historic plantations along the Louisiana stretch of the Mississippi River, seeking interpretive programs that explored the slave experience. But only at the famous Laura Plantation was the role of an exploited underclass acknowledged or celebrated by guides.

This book examines why it is that race and slavery remain conspicuously absent from public and popular discussions of the American Civil War (1861–65)—the bitterly divisive conflict between the northern Union states led by President Abraham Lincoln, and Confederate rebels (the South) who opposed his emancipist agenda. In his introduction Paul Shackel explains how in working as an historical archaeologist on many nationally significant American sites, he became fascinated by the ways that collective memory expresses—and represses—contested issues such as racism. He explores the struggle of African–Americans for inclusion in the public memory of the civil war through four case studies—the John Brown Fort, the Heyward Shepherd Memorial (both in Harpers Ferry, Virginia), the Shaw Memorial in Boston, Massachusetts, and Manassas National Battlefield Park (also Virginia)—all National Parks Service sites.

The Civil War remains deeply significant to the United States, some suggest because it was the only war it has ever fought on its own soil. Although slavery was abolished at the end of the Civil War, Shackel shows how the racial ideology that underpinned it only strengthened, as ideas about biological human difference developed in the wake of social Darwinism. He traces the exclusion of African–Americans as (1830–1883), Australasian Historical Archaeology 19:84–96.
participants in the Civil war from public memory, starting with Reconstruction (1866–76), through Jim Crow (legislative segregation and the disfranchisement of African–Americans) up to the 1960s Civil Rights movement—a turning-point that prompted the designation of 75 National Historic Landmarks by 1977, including the church from which Martin Luther King Jr. preached during the Montgomery bus boycott. (For non-American readers, some basic background to the Civil War would have been welcome: a simple account of the chief issues, events and players would have helped place the more detailed analyses in context. A map showing the places discussed would also have been very useful.)

As bereavement turned to nostalgia, the south began to defend slavery and secession, and to represent the war as a glorious and heroic ‘Lost Cause’. Powerful southern heritage groups such as the United Daughters of the Confederacy (UDC) vetted school textbooks, commissioned histories, and defended the Ku Klux Klan and its activities, as a new Confederate tradition emerged. By the 1950s race had become a violently divisive issue within American society, and the Civil War Centennial sought to promote unity, bringing an economic tourist boom to the South (organizer Karl Betts is reputed to have said that the South may have lost the war but it was going to win the centennial (45).

The concrete experiences offered by public commemoration celebrate values which have become central to American culture—although, as my two epigraphs suggest, these have always been profoundly contested. As the former President of the Association for the Preservation of Civil War Sites explained in 1998: ‘Battlefields are the heart and soul of who we are…On the site where these men fought, people can touch history. The battlefields are some of the most effective classrooms in America…it teaches moral courage, the value of commitment to a cause and the ultimate sacrifice’ (49). As Shackel notes, this process de-politicises the meaning of war and evades its fundamental causes and effects.

The first case study addresses John Brown Fort, Harpers Ferry (Virginia, fifty miles northwest of Washington). Ardent and martial abolitionist John Brown became known during the 1850s for his attacks on pro-slavery forces, especially his raid of the federal arsenal at Harpers Ferry in October 1859, although his plan to arm slaves with the weapons he and his men seized failed, and he was convicted of treason. His actions are often seen as sparking the Civil War, and the ‘fort’ (actually a brick engine-house) became a powerful abolitionist symbol. It was re-located in 1891 to Chicago for the Columbian Exposition and then in 1895 returned to Harpers Ferry. It became important to African–American activists, who rebuilt it at Storer College (for freed slaves) in 1910. In 1968 during the Civil Rights era it was moved again to the Harpers Ferry Historical Park, as an official strategy to reinterpret to include the issue of slavery, such references are being removed from this landscape in favour of reconstructing troop movements.

Shackel’s second example is the Heyward Shepherd Memorial located in Lower Town Harpers Ferry. Shepherd was a free African–American man who, ironically, was the first casualty of John Brown’s raid on the federal arsenal. White newspapers represented him as a faithful, ‘industrious, inoffensive’ martyr, and he was used to demonize Brown. When the UDC was created in 1895, it decided to erect a faithful-slave monument to him (as well as a ‘national mammy memorial’ to ‘remember the best friend of their childhood’ [86]), to counter northern narratives of slavery. For decades, debate about the proposal continued, while the Ku Klux Klan’s strength grew in the south, with a strong presence in Harpers Ferry; many feared that an inscription that explicitly justified slavery and claimed that African–Americans believed in the institution would worsen racial conflict. Finally the monument was erected in 1931 opposite the John Brown Fort Obelisk, amidst controversy. The intense emotions aroused by this memorial – its wording, location, and especially what it symbolized about race relations during the Jim Crow era – re-awakened during the 1980s when the monument was stored and covered during renovations: African–Americans sought to have it destroyed while the UDC tried to have it reinstated, Mrs Dewey Wood remarked in 1989, ‘Why should the NAACP be opposed to this? It is a monument to one of their people…I really don’t know what they find offensive about it’ (106). In 1994 the Maryland Division of the Sons of the Confederate Veterans initiated a campaign for the reinstatement of the monument, and in 1995 it was uncovered again, accompanied by a ‘wayside’ panel that recounts some of the controversy surrounding it – which itself sparked fresh debate!

Shackel’s third study stands in Boston, Massachusetts: the Robert Gould Shaw Memorial to the man who led the first northern-raised African–American troops (the 54th Massachusetts) into battle at Fort Wagner in 1863. They suffered major losses but their courage and strength proved a moral victory for abolitionists. Unveiled in 1897 the sculpted frieze (by Augustus Saint-Gaudens) represents the white soldier on horseback dominating his black subordinates, but the African–American community has re-worked its significance as one of the few commemorations of their involvement in the Civil War.

The fourth case study explores the battlefield landscape of Manassas, in Virginia, where the sites of two southern victories, the First and Second Battles of Manassas (known in the north as the First and Second Battles of Bull Run), 25 miles southwest of Washington, have hosted re-enactments and celebrations that reinforced the myth of the faithful slave. Interpretation in the park focuses on the battles at the expense of the diverse community that lived there, and even now, despite a congressional mandate that requires park interpretation to include the issue of slavery, such references are being removed from this landscape in favour of reconstructing troop movements.

Books like this cannot evoke the fear, hate, and passion of racial conflict in the same way as films such as Mississippi Burning, the novels of Richard Powers or Harper Lee, or even narrative histories such as Taylor Branch’s Parting the Waters. But importantly, they show how these intense human conflicts arise and flourish around tangible symbols and places; Shackel demonstrates how the landscape around us expresses present-day struggles, and that re-enactments and memorials are profoundly emotive, symbolizing values central to many Americans’ identity. For Australian readers, this critical and scholarly analysis serves as a reminder that we have too few accounts of the ways that inherited relationships and attitudes are given expression within our own landscape.

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Explorations in slumland: a reprise

Our purpose here is to respond to some of the issues raised by Mary Casey in her review of The Archaeology of Urban Landscapes (Casey 2003). As editors and contributors to this collection, we begin by taking issue with her general comments about the main premise of the book. We then focus on the specific comments on chapters by Karskens and Murray and Mayne, and conclude with some general observations of our own.

General matters

Casey states that the main premise of the collection is ‘the interaction in archaeology and history’, or ‘the need to integrate archaeology and history’ (2003: 85). On this basis she dismisses the papers by Ross, Solari and Malan and van Heyningen as not ‘achieving a successful incorporation of archaeology and history as proposed in the introduction’ (2003: 85). We did indeed discuss the importance of an integration of archaeological and historical evidence as a necessary component of new historical archaeologies of the modern city. However, the main premise of our book was the need to understand how slum stereotypes had obscured the richness and variety of life in the poorer quarters of such places. We argued that understanding how slum narratives are generated, or indeed how places ‘become’ slums to those who do not live in them, is crucial to revising the historical of such places and to the process of ‘unleashing our imaginations, and the historical narratives we can construct as a result’ (Mayne and Murray 2001b: 4).

The chapters on London, Oakland and Cape Town were placed in Part I (Setting, scope and approaches) precisely because they were about these issues, and were not reports on particular archaeological cases. Therefore they were not exemplars of integration, but discussions about perceptions and identities, both of which will play a role in achieving integration (which is itself an ongoing process). Casey’s evaluation of their ‘success’ betrays a more general misunderstanding of our project and of the contribution each of the chapters made to exploring some (but by no means all) of its elements. The Archaeology of Urban Landscapes presents a very diverse group of archaeological and historical contributions that gain a unity through their consideration of ‘slums’ and through the exploration of a range of archaeological and historical issues. Some of the contributions (especially those on Sydney’s Rocks, Lowell, New York and Minneapolis) are the product of either long-standing or completed research projects that are both well published and sufficiently mature to foster quite penetrating analyses of core categories and concepts such as class, gender, ethnicity and consumption. Others either had more specific goals (Belford’s discussion of the Sheffield Crofts, and the account of changes to the Quebec waterfront –, or reported preliminary results of work in progress (‘Little Lon’). All contributions played their part as grist to the imagination (Mayne and Murray 2001b: 5).

Casey’s misunderstanding of our purpose is compounded by her discussion first of theory and secondly of difference. In her view the contributions that presented ‘complex, thoughtful and convincing analyses of their slums’ were ‘those that presented a deeply theorised understanding of the archaeological evidence within the context of recent material culture studies’ (2003: 86), Casey’s focus on archaeology and material culture studies, instead of embracing the broader program of the hermeneutic circle, is indicative of her views on theory in urban historical archaeology. For instance, Casey believes that Yamin’s paper on the Five Points site should ‘inspire many archaeologists to undertake similar approaches once they have completed the analysis and synthesis of the archaeological program’ (2003: 86). However this is precisely contrary to what Yamin (and others) have advised, in favour of a process which is firmly based on recursive reasoning between archaeological and other data sets throughout the entire process, from research design to publication. This also raises the question about what is meant by ‘deeply theorised’ — is it simply the application of categories such as class and ethnicity as a framework within which to interpret patterned distributions of archaeological phenomena? Given the constitution of archaeological theory, it is frequently beside the point (especially in material culture studies) to conceive of ‘archaeological evidence’ as being meaningfully distinguishable from the wide range of theories that have created it and other forms of evidence about the past. In short, archaeological data do not constitute themselves and while the methodologies (such as minimum vessel counts) and interpretative frameworks advocated by Casey, are, when the conditions are right, valid and integral parts of successful historical archaeological investigations, the sum of their parts does not constitute deep and meaningful archaeological theory.

Of course this does not mean that we think that archaeologists should not take the archaeological record seriously – far from it. However we remain committed (as does Yamin) to the recursive reasoning of the hermeneutic circle, where a diversity of data sources can be integrated in ways that do not diminish the integrity of the constituent elements. This, and the approach Mayne and Murray describe as ‘ethnographies of place’ (2001b: 4), comprise the theory of urban historical archaeology advanced in The Archaeology of Urban Landscapes. Therefore the book contained contributions that specifically focused on issues related to the archaeology of the working classes, and others that only peripherally touched on these matters. ‘Rigorous’ and ‘systematic’ analysis (2003: 86) of the kind advocated by Casey was appropriate to some contributions and not others.

Therefore we also disagree with the discussion of difference which closes Casey’s review, and the linear approach to archaeological reasoning that it implies. Her logic appears to go something like this: many of the contributions accepted that the residents of ‘slums’ were different from outsiders; categories such as class are problematic; the archaeological evidence ‘when interpreted from economic perspectives’ (2003: 87) – undefined by Casey– indicates that there is little difference between working and middle classes; yet we know that people thought they were different; material culture studies are the way to explore the meaning of these differences; finally these studies will allow us to understand the ‘degree to which there were “real” differences, as opposed to cultural perceptions of difference’ (2003: 87).

Sorting out this confusion requires us to restate the premise of The Archaeology of Urban Landscapes. These places (and the people who inhabited them) were seen as being ‘different’ by outsiders, although the nature of that ‘difference’ changed over time. Such places were frequently demonised as ‘slums’ (a category that had moral and physical as well as economic aspects) and their residents were considered to all behave in the same fashion – to have the same pattern to their lives (itinerary, poverty, unemployement, criminality, drunkenness etc.). The recognition that such places were much more diverse than previously thought was the direct result of documentary and archaeological evidence—not just archaeological evidence of difference, but also evidence of diversity. Documentray research has been crucial in exposing the mythology of the ‘slum’, but both documents and material culture have helped demonstrate the diversity of lives in such places. Indeed Casey’s own challenge for material culture studies reiterates one of the fundamental propositions made by Mayne and Murray. There
is currently a divergence between the different stories we are now obtaining from documentary evidence and the great variety of evidence stemming from urban archaeology, and this divergence poses exciting challenges to us, as we seek to revise histories of the modern city. Therefore while material culture studies are a very useful component of overall research into this issue, they are hardly likely to contribute to a discussion of meaning without being fully integrated into the historical archaeologies of such people and places. Indeed we would not be discussing the potential disparities between ‘reality’ and ‘perception’ without the documentary record!

Specific issues – The Rocks

We now move on to consider Casey’s criticisms of the chapters by Karskens and by Murray and Mayne. The review is particularly critical of Karskens’ chapter on Sydney’s Rocks district and the Cumberland and Gloucester Street (CUGL) archaeological assemblage. She challenges the validity of the work of a large number of specialist archaeologists who spent some years reviewing, researching and writing about the site and its artefacts. However, this material has been described in a recent review as ‘arguably the most substantial reporting of artefact assemblages undertaken by consultants in Australian historical archaeology’ (Crock et al. 2003:23). Casey seems to be arguing that nothing can be suggested or written about such assemblages before every possible analysis is done; but this is impractical and not how research works.

In any case, this chapter has a broad purpose: to trace out the lineaments of what could be seen at that stage, to engage archaeology with larger issues in Sydney’s urban past, and to engage an audience beyond archaeologists. So it uses appropriate language and takes a broad cultural focus: what does archaeology tell us about the culture of nineteenth century Rocks people?

To assess the significance of the archaeological perspective at this level, existing widely accepted ideas about the nature of nineteenth century working class culture must first be established. Hence the long history of academic, heritage and popular narratives and assumptions about working people and their places were set out in the first part of the chapter. The broad archaeological insights offered in the second part, demonstrate how flawed such ‘standard’ and historically based assumptions are. But the chapter seeks to go further by sketching out the culture that lies beyond the slum myths and their deconstruction. The approach taken was to try to read the main features of the CUGL assemblage from a particular period—stepping back, taking a bird’s eye view. Hence this was not the place for detailed discussions of particular artefact groups and numbers, distribution, and stratigraphy. While questions about where people acquired their goods and how much they cost are interesting, they are different areas of inquiry, and not relevant to the argument.

Perplexingly, Casey seems to have skipped the first part of this chapter and gone straight to the second, with the result that she failed to grasp the argument. She claims that Karskens is naively ‘surprised’ by the rich material life of these people. But it is not Karskens who is surprised; the point is that the material is surprising in the context of that long and implacable history of slum demonisation (set out in the first section). It may well be common knowledge among urban archaeologists, who deal so intimately with so much of this material. But this is not something to say ‘so what?’ about, because it is not at all well-known to scholars and others outside the field.

Casey also misreads the argument about class and material culture (2003: 87). Karskens expressly argues against (not for) the ‘trickle-down’ or ‘emulation’ model of culture, which is far too simplistic. She argues that genteel culture is like a kind of language which is adopted and adapted by different classes, often for different reasons. Thus certain strands can appear similar in working and middle class cultures, and certain beliefs and aspirations may be shared, but this does not mean they are the same, or that one is mindlessly mimicking the other.

Specific issues – ‘Little Lon’

Since 1996 Murray and Mayne have published several discussions of their work at ‘Little Lon’ all of which have concentrated on the preliminary analysis of the assemblages retrieved from several house sites in Casselden Place, and the reconstructions of patterns of ownership and occupancy of those houses which were derived from a wide variety of relevant documents (Mayne and Murray 1999, 2001b; Mayne, Murray and Lawrence 2000a; Murray and Mayne 2001, 2002a, 2002b, 2003). They have been frank about the limitations of these analyses and explained why more sensitive statistical manipulations than frequency determinations and sherd counts have not as yet been applied to these data (see for example Murray and Mayne 2001: 92). But as it stands they have been able to describe a counterintuitive pattern found among the assemblages of Casselden Place arguing that (at a gross level) there was very little difference between houses that had been continuously occupied by the same family for decades, and those that had been occupied by many tenants over the same period. This has allowed them to begin to critically examine the empirical basis of one of the most important assumptions about slums – that they were predominantly populated by itinerants with little or nothing by way of material possessions – and to begin to reflect about what such assemblages might tell us about the lives of people who lived in Casselden Place. Significantly Murray and Mayne have also been able to focus on a related question – how similar or different are the assemblages from these houses in ‘Little Lon’ to those occupied by the urban poor at the same time in other cities, such as Sydney and London.’

This is still a very long way from writing the archaeology of the Melbourne working class, which is what McConville (2000) and now Casey have upbraided Murray and Mayne for not doing. Murray, Mayne and Lawrence (2000b) have already responded to McConville’s disappointment, explaining that their focus was on establishing the particulars of life at ‘Little Lon’ in order to give some empirical content to generalizations such as ‘working class’. We consider it worthwhile to explore some aspects of Casey’s critique of Murray and Mayne’s work because it bears on the question of what kind of histories should be written about ‘Little Lon’.

Notwithstanding Casey’s criticism of a ‘lack’ of explicit theory in the chapter, her objections to it are based on three issues. First, the fact that Murray and Mayne worked with ‘dated’ analytical routines such as sherd counts and functional categories, rather than MVCs and more sensitive ‘cognitive’ categories. Second, that because Murray and Mayne had no explicit theory, no meaningful narrative of the lives of the inhabitants of such places resulted from the analyses. Third, that Murray and Mayne could not effectively integrate archaeological and historical data because they were seeking to discuss the particularities of people’s lives while using a methodology that focused on process and generalization, rather than on interpretation.

Murray and Mayne have made it very clear why they chose the sherd count approach (it was to do with our misgivings over data quality and our inability to get clear chronological control over the data) (see Murray and Mayne 2001: 92, 94, 98, 101, 103). We were also explicit concerning our severe misgivings about whether our archaeological data
could mesh at all with the lives of individuals in Casselden Place (see for example Murray and Mayne 2001: 102). These misgivings were real, and there is more than enough evidence to suggest that in such circumstances calculating minimum vessel numbers gives only the most spurious appearance of fine-grained control over what is in reality a very coarse data set. Thus all analyses (whatever their goals) should be appropriate to the data under review; one size does not fit all. Indeed in all of the analyses that have been undertaken under the auspices of the Exploring the Archaeology of the Modern City project began by Murray in 2000, a great deal of time has been devoted to evaluating the various excavated assemblages for their potential to be analysed using techniques such as minimum vessel counts (Crook et al. 2003 a,b,c,d,e,f,g in press). Furthermore Casey’s claim that Murray and Mayne relied on gross functional categories for the analysis of the assemblage is simply not the case (nor is her assertion that they used a category called ‘residential’) (2003: 86).

Apart from observing that Casey seems to have overlooked explicit statements that these publications were reports of work in progress (unlike completed projects such as Five Points or the Boot Mills), one can only agree that Mayne and Murray have not provided a theorised narrative of people’s lives at ‘Little Lon’. However, it’s worth reiterating that this was not their intention in those publications. We can only agree that such a narrative would be worth doing and indeed Murray and Mayne will do so when the analyses at ‘Little Lon’, and the adjacent site of Casselden Place are completed in 2004 (see for example Murray and Mayne 2002b). It is also worth remembering that their overall goal has not been to banish theory from urban archaeology, rather to more clearly ground it in the data we have to hand. Given their misgivings about the data from the original ‘Little Lon’ excavation (and of the ‘theories’ that are in vogue in urban archaeology) their capacity to launch into a well-supported grand narrative about the site has, until the excavation of the adjacent site of Casselden Place, been somewhat limited. But what they have done and what they were in fact writing about, is to dramatically change the traditional narrative of ‘Little Lon’, to create a new story of the place that became so much a feature of public outreach during the 2002 excavations there (Murray in press).

Finally, there are Casey’s comments about the particular and the general, which read as a misunderstanding of the differences between processual and post-processual archaeology. It would take more space than we have available to deal with all aspects of this misunderstanding, but suffice to say that the purpose of the diagram representing the model for a research strategy into global material culture in urban settings 1830–1950 (Murray and Mayne 2001: 104) was to seek a practical way of overcoming what Murray and Mayne considered to be a spurious and damaging cleavage (in both history and archaeology) between the particular and the general. Murray and Mayne were very well aware of the difficulty of what they proposed, and the necessity of following this course of action.

Acknowledging that there is an inevitable loss of detail and focus as one moves around the circle of increasing generalisation, does not mean that one cannot return to the specificities of place with new perspectives drawn from the wider world. In the case of ‘Little Lon’ we are only too aware that these important specificities of place, people and time are themselves the products of concepts, categories and units of analysis which are built from acts of comparison and generalisation. Indeed to argue otherwise would be to accept that, at all levels of discussion, every historical or archaeological observable must be unique. This is an argument that would effectively spell the end of our search for understanding (2001: 103). There is nothing positivist nor processual about this search, and it is a sheer nonsense to assume that because they used sherd counts in this instance then the whole of their approach to global material culture would replicate this. Indeed in the analysis of the adjacent site of Casselden Place, which is shortly to be completed, MVCs are used wherever appropriate, mirroring the approach that was taken in the Modern City project from 2000. Perhaps more to the point, their stated preference for the hermeneutic circle is a specifically post-positivist epistemology, and an interest in generalisation and process does not make them either processual or post-processual!

Finally, we note that we are certainly not alone in this search, especially among those who have adopted a more thoughtful approach to building archaeologies of the working class:

If the archaeological study of capitalism is framed as a series of narrow case studies with no movement beyond concrete particularities, and no analysis of the encompassing and structural conditions that give rise to these particularities it cannot be expected to provide an understanding of these subjects as capitalist (Wylie 1999:26).

We are gratified that our work has contributed to much-needed debate among urban archaeologists and historians, and we embrace the notion of fair and open critique as an essential element of that debate. We have sought here to redress the fact that statements of intent, context and limitation, clearly presented in our work, have been misrepresented. No one owns the field of urban archaeology and our collective search for progress requires, above all else, tolerance and openness to different agendas.

References


CROOK, P., L. ELLMOOS and T. MURRAY 2003a Assessment of Historical and Archaeological Resources of the Paddy’s Market site, Sydney. Historic Houses Trust of NSW.

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Notes

1. Reviews Editor's Note: I have taken the unusual step in this journal of publishing a response to a review by Mary Casey of Tim Murray and Alan Mayne’s Explorations in Slumland: The Archaeology of Urban Landscapes. The review involved the exploration of some current debates in the field of urban archaeology. The level of activity in this field, and indeed the pace at which it is carried out in the world of development-driven, compliance archaeology, means that this kind of debate rarely makes it to the printed page in Australia! It is my view that the publication of such debates can not only provide valuable case study material for teaching archaeology, but also contribute to constructive intellectual self-critique in this exciting field of historical archaeology.

2. The EAMC project is generally described by Murray (2001). The London project is discussed in Murray et al. (2003).

3. There is a vast literature on this topic which has strong resonances in social theory and the philosophy of history. Archaeologists have made disparate contributions but they have tended to be derivative of positions adopted elsewhere. A recent discussion of aspects of this debate in historical archaeology can be found in Wilkie and Bartoy (2000). Useful discussions of both sides of the argument in other disciplines may be found in: Baert (1998), Bunzl (1997), Callinicos (1995), Hollis (1994), Jenkins (1999), Lloyd (1993), Lyotard (1984), McCullagh (1998) and White (1973).

Tim Murray, Grace Karskens and Alan Mayne