

REVIEWS

Mary C. Beaudry (ed.), *Documentary Archaeology in the New World, New Directions in Archaeology*, Cambridge University Press, 1988 (hardback) and 1993 (paperback); pp.218, illustrations, \$49.95. ISBN 0521303435 and 0521449995.

This important publication from one of the major participants in historical archaeology today is not new in date: it yet has much that is useful to contribute to current Australian archaeological discourse. One of its primary messages is that artefacts are not enough, nor is simple quantification: the contextual data supplied by perceptive study of contemporary documents is usually essential to ensure a non-simplistic interpretation of archaeological finds, and often able to contribute its own dimension of more innovative interpretation. In demonstrating this, individual case studies present details of investigative techniques within broader theoretical frameworks that in many instances have interesting possibilities for transference to the Australasian context.

Anne Yentsch's detailed study of inherited properties in early American society showed how, when women inherited land or houses, their property was less likely to be accorded the same significance in local legend as the inheritances passed through the male line. Yentsch demonstrates this well in the North American context: its application to the Australian context raises interesting questions. A later paper in the volume by Henry Langhorne and Lawrence Babits recalls this study in its demonstration of how father-daughter land transfers in particular were under-represented in the documentary record.

Julia Curtis presents an integrated investigation into the records of seventeenth-century imported Chinese porcelain in Tidewater, Virginia: her study is a meticulous example of both iconographic and iconological or contextual interpretation, and she integrates into it both documentary and artefactual study of a number of tidewater wrecks. While the date of this material is earlier than the bulk of Chinese porcelain in Australian land sea sites, the integrated methodology is well worth attention.

Peter Schmidt and Stephen Mrozowski look at an unexpectedly varied range of documentary sources for seventeenth- and eighteenth-century smuggling along the New England coast, including an interesting feedback effect as the British intensified their efforts to stop it. Like Anne Yentsch later in this volume the authors point up the need for holistic enquiry, since in this instance each facet of illicit trade is reflected in different areas of the documentary record: once a full cultural context has been established, an archaeological strategy to investigate a complex behavioural phenomenon can be identified.

Mary Beaudry's paper on linguistic analysis addressed some 600 Chesapeake probate inventories in three date brackets (mid-seventeenth century, 1700, mid-eighteenth century), and analysed changes over time in the language used to describe any form of food-associated vessels. She investigated the area of adjectives, i.e. modifiers such as those for function and composition used in relation to food vessels, finding clear reflections of binary oppositions: part of this study involved coming to terms with marked and unmarked entries (i.e. in simple terms 'small pot' or 'large pot' as opposed to 'pot'), a problem familiar to all who have worked with ceramic catalogues. This innovative and interesting study deserves attention by anyone interested in any form of written ceramic sources, for example ceramic advertisements. The next paper by Beaudry and others draws partly upon the results of the previous intensive study of contemporary inventories to develop a ceramic classification for Chesapeake pottery (the POTS classification) based on contextually-based rather than imposed categories.

The three next chapters (Garry Stone, Marley Brown, Kathleen Bragdon) pursue the great potential of probate inventories, a resource sorely missed in New South Wales

(where they are confidential). Stone checks seventeenth- and eighteenth-century inventories in the Chesapeake region for reinterpretations of common misconceptions about ceramic use and ownership. He explores the changeover from earlier communal eating patterns using shared wooden and pewter vessels in the seventeenth century to the flood of fine porcelain visible in Boston's wealthier merchant households during the 1720s. Brown reviews the behavioral context of the inventories themselves - the possibilities of biases in the selection of those for whom inventories were made, in the choice of the appraisers, and in their potentially varying qualifications for the task, and also the need for spatial and functional analysis of the appraisal process. This short paper, like the later one on historic maps, is a model of how such sources need to be analysed for bias: street directories in Sydney could be considered another example of a data set needing an explicit behavioral context. The Bragdon paper, which compares seventeenth- and early eighteenth-century inventories from a tavern site and a homestead site, adds to the growing body of studies stressing the need for caution in assuming a direct relationship between artefact counts and socio-economic status.

Nancy Seasholes' paper, while drawing its numerous examples specifically from urban Boston and rural Massachusetts, is set to become a standard historical archaeology student text, with its demonstration of the need to assess purpose, audience, bias and accuracy when using historic maps. Following this, military records, another category of archaeological evidence often overlooked, are classified by Lawrence Babits in the same way as any other body of archaeological data, for use at either site-specific or at regional level: Babits' case study is based on military impressment records (receipts for food foraged from local communities), demonstrating that these provide evidence for 33% of the male population not visible on any other form of contemporary documentation.

Kathleen Bragdon's next paper, again using probate inventories as key sources, shows the complexity of intercultural processes among the Christian Indians of southern New England c.1650-1775, and contests the view that these communities were the most completely 'transcultured' of the Christian native population during this period. A more helpful map here would add to a paper of particular interest to those involved in contact studies.

Anne Yentsch's second paper is particularly impressive in its scope, pointing up the underlying theme of seasonality and its implications for social organisation and reciprocity, in a study of land and sea resources on eighteenth-century Cape Cod within an ecological framework. She uses tax lists for land, livestock and personal property holdings, census records for population size and structure, probate inventories for farm livestock, spinning, weaving and fishing implements and wealth totals, and ethnohistorical sources for whaling, cod and other fishing. Yentsch's position is that the interrelatedness of the past only emerges from a holistic enquiry of this kind: in it she weaves together the interacting rhythms for local communities of seasonalities deriving from both sea and land resources.

Joanne Bowen follows with a chapter on seasonality, emphasising the need to resist oversimplified interpretations of faunal remains in rural contexts where complex farming exchange relationships were developed to overcome problems of both meat shortages and meat oversupply resulting from seasonality. In particular Bowen argues against too ready an acceptance of socio-economic interpretations. She uses to demonstrate this the detailed resource of eighteenth-century New England farm accounts to document the exchanges and meat-based events within an extended farming community. While stressing the difficulties of this database, Bowen

effectively shows the need for archaeologists to move beyond recognition of simple seasonal butchering patterns — which remain supported by her study — to recognition of the community's choices and activities in overcoming problems caused in a small community by climatic, technological and seasonal constraints in the management of meat provisioning.

Bowen's paper is one of several providing interesting background detail, in this instance to early New England rural food resource practice, that is of relevance to Australia. George Miller's paper is a useful summary of his series of ceramic index papers to date. Miller stands for the ultimate marriage of ceramic artefacts and ceramic records, to yield a tool of use to all historical archaeologists whether directly or indirectly. However his paper in *Historical Archaeology* 25 updates this one with important new data. Stephen Mrozowski, in considering two key aspects of colonial newspapers — advertisements and readers' letters, shows how these sources can provide key detail refining prices of ceramics, as well as changes in how they are marketed. The rest of this chapter develops from an anecdote published in 1733: a tradesman's wife pursues a path of upward mobility by a series of expensive consumer purchases which progressively ensure her transfer from a working wife to a lady of leisure. Her ill-advised visit to a relative gives the husband the opportunity to reverse the whole process and sell off the purchases, thus returning to his wife's earlier more appropriate lifestyle. Mrozowski uses this to discuss the emic dimension, and also to explore issues of both consumerism and gender roles in eighteenth-century New England.

Mary Praetzellis *et al.* re-introduce the importance of studying the middle-class norm, rather than concentrating in historical archaeology only on ethnic minorities. Understanding of dominant group consumer behaviour is seen as almost a necessary preliminary to studies of minority consumerism. This paper stresses the need to use the extensive literature available on consumer behaviour past and current, and illustrates how late nineteenth-century middle-class material culture can be studied by means of exhibition catalogs and newspaper advertisements.

One statement of Stone's says much about the overall direction of this stimulating book: '*I am appalled to discover that because I dig ceramics as an archeologist, I have tried, as a historian, to excavate them from inventories. The archaeologist's goal, after all, is not to count sherds, but to study culture. We should approach inventories in the same way, as students of behavior, not artefacts.*' The editor has splendidly succeeded in attracting studies which use documentary sources as a resource in their own right from which to search behaviour, not artefacts. One of the strengths of this book for the wider public is its multilayering. The papers can be read at both the simple and particular level of technique-specific information exchange, as well as within a wider context of broad disciplinary study. Despite the firm reminder in its title, its usefulness as well as its stimulus extends well beyond the New World. Of interest especially to those working in New South Wales could be Yentsch's holistic approach to understanding past community lifestyles, Bowen's seasonality study, Mrozowski on the gender implications available from newspapers, the material culture and consumerist values of the middle class — but the list goes on. It is a book absolutely not to miss.

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Y. Desloges, *A Tenant's Town: Québec in the 18th Century*, (Studies in Archaeology, Architecture and History), National Historic Sites, Parks Service, Environment Canada, Ottawa, 1991; pp. 300, soft cover, US\$20.35. ISBN 0-660-14204-X.

This book is in fact, even if not in terms of formal designation, a sequel to R. Chénier's book *Québec: A French Colonial Town*

in America, 1660 to 1690, also published by the Canadian Parks Service in 1991 and reviewed in an earlier issue of this Journal. It is very pleasing indeed to see such solid historical and archaeological research monographs being published by the Service. Unfortunately, however, this volume is not of quite the same high standard as its predecessor.

Both books began life in the French language, but this second volume did not receive as high a standard of translation as the first. This has led to passages that are sometimes merely annoying, occasionally unclear or potentially misleading and, more rarely again, incorrect. While there is no easy way of being sure, I feel that the fault does lie more with the translation than with the original research or writing. I say that because there is a preponderance of very good and very worthwhile material in this book, generally backed by research of a high standard. Having said that, though, there are times when it is hard to see the forest for the trees — so much minute detail is given, especially in the tables and figures, but also in the textual explications of them, that the broad picture is lost.

It is, indeed, an extremely detailed study of a town of only 7200 people in 1755 (despite the sub-title, the study period is limited to the first half of the eighteenth century). This degree of detail has both its positive and negative sides. On the positive side, this study goes a long way toward achieving one of its implied goals, to paint a picture of the lifestyle of the inhabitants. Such detailed and often highly personal records have been available to the researcher that she has been able to give a vivid description of many aspects of daily life. The negative side has already been alluded to in the previous paragraph. The author's obvious enthusiasm for the detail and for the wealth of data available seems to have somehow prevented her from seeing the larger picture and the general trends as clearly as she might have done. The study is thus stranded in the 'neutral' territory between a generalised quantitative study and an unambiguously detailed, qualitative study of individuals or small groups: it is neither one nor the other as effectively as it could be.

Nonetheless, the detail given does provide an in-depth portrayal of the small eighteenth-century French outpost of Québec. The first part deals with the population, with chapters on demography, occupations and what modern writers would refer to as socio-economic groupings. The second, and in many ways most important, part then deals with housing conditions, including property management processes and tenure. It is this section that justifies the book's title, *A Tenant's Town*. Finally, the third section deals with consumption and production, providing some intriguing insights into the types of food, furnishings, interior decoration and home-based economic activities to be found in the town. It is this final section that comes closest to giving the flavour of daily life in eighteenth-century Québec in a vivid fashion.

There is also some very useful material in a series of appendices. The most novel and most fascinating to me is the one that deals with 'succession inventories' drawn up on citizens' deaths. While somewhat akin to our wills, they provide much more detailed information than any modern will of which I am aware. I would now like to know if such inventories had their parallel in the British Isles and, even more specifically, in British colonies such as early New South Wales. Advertisements for deceased estate sales in the *Sydney Gazette*, for example, have something of the same flavour about them. The other major appendix discusses price index fluctuations and is also both useful and interesting, as much for the methodology used as for the results. Other appendices are shorter and deal with occupations of heads of households and with two censuses undertaken during the period covered.

In summary, this book is to be commended for providing in print the results of such thorough research based on an extremely valuable data resource. I may be wrong, but it certainly seems that there is more detailed written evidence for life in earlier eighteenth-century Québec than there is for late

eighteenth-century Sydney. Perhaps I have not looked in the right places yet. Desloges's book is a valuable source for anyone interested in town life in the century before last. Unfortunately the detailed information contained in the book is not as accessible as it might have been.

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Ros Fraser (ed.), *Aboriginal and Torres Strait Islander People in Commonwealth Records: A Guide to Records in the Australian Archives, ACT Regional Office*, Australian Government Publishing Service, Canberra, 1993. \$20.

Australian Archives and Public Record Office of Victoria, *'My heart is breaking': A Joint Guide to Records about Aboriginal People in the Public Record Office of Victoria and the Australian Archives, Victorian Regional Office*, Australian Government Publishing Service, Canberra, 1993; pp.xi, 191, illustrations, \$11.95. ISN 0644324988.

Please would you allow me to have my two daughters with me here [another] one of them died and I have not seen her before she died and I should like the other two, to be with me and comfort me.

Please do not disappoint me for my heart is breaking to have them with me. Please to send them up here as I cannot leave this station ...

(*My heart is breaking*, Frontispiece, n.p.)

One of the recommendations of the Royal Commission into Aboriginal Deaths in Custody was that the Commonwealth and State governments 'provide access to all government archival records pertaining to the family and community histories of Aboriginal people so as to assist the process of enabling Aboriginal people to re-establish community and family links with those people from whom they were separated as a result of past policies of government'. These two books are part of the response to that directive.

Both therefore are addressed to Aboriginal and Islander researchers rather than to academics and, as such, to people with a particularly personal and intense interest in sifting through government documents. As Ros Fraser writes in her introduction, their purpose is to assist those seeking to recover personal and community identities. Containing detailed lists and summaries of the archival files, these books should also do much to end the process by which Aboriginal enquirers, even in the recent past, have been denied access to their own family records.

For all researchers, this process of uncovering the reality of Aboriginal experience will be far from pleasant. Even the brief descriptions of the documents show something of the trauma previously locked away in the dusty maze of the archival record. A random glance for example of the Commonwealth list for 1932-34 reveals headings like 'Death of Cockatoo Frank Northern Territory'; 'Adoption of Children — NT'; 'Apprentices (Half- Castes) Regs — NT — wages to be paid'; 'Request for information re case of G.F. Davies, NT — charged with shooting an aboriginal'; 'Commutation of death Sentence Aboriginal "Longlegs"'; 'Stott, Mounted Constable - Prosecution for Assault on Lubra'; 'Free Conveyance of Aboriginal lepers Broome to Darwin' (pp.154-5).

Despite the fact that these documents in many cases reinforce the stereotypes, myths and distortions of the past, used in conjunction with oral testimony, they will undoubtedly contribute to the current reinterpretation of Australian history by Aboriginal writers, artists, film-makers and singers. If we believe that knowledge, no matter how shocking, is ultimately empowering, then books like these are valuable indeed.

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J.M. Birmingham, *Wybalenna: The Archaeology of Cultural Accommodation in Nineteenth Century Tasmania*, Australian Society for Historical Archaeology, Sydney, 1992; pp. 202, illustrations, maps. ISN 0909797153.

The story of the destruction of traditional Tasmanian Aboriginal society has often been told. It is a story of heroes and villains, of oppressors and oppressed, of interlopers and indigenes, of those with the power to act and those whose major role seemed to be in joining the queue passively waiting for admittance to the kingdom of God. Above all, the story of the destruction of traditional Tasmanian Aboriginal society is a story which changes (unsurprisingly) to suit prevailing social and intellectual moods.

A book review is hardly the place to make a number of obvious points about the Whig interpretation of history, but there are several curious features about this book which bring out the Butterfield in me. Wybalenna has had a very long gestation, with the fieldwork carried out between November 1969 and February 1971, yet the interpretive thrust of the work is (by Australian standards at least) very modern. It is worth recalling that this project lived for over twenty years before publication, and what a twenty years they have been! During this time we have witnessed tremendous changes to the perception and self-perception of Aboriginal Australia, not to mention the rise of the heritage industry and the invention of Aboriginal history. In 1969, only just after the passage of that famous referendum which gave the Commonwealth real powers to act in the interests of Aboriginal advancement, Australia was still locked in the model of assimilation. Since that time we have passed through multiculturalism and on to reconciliation, both models which have been designed to foster a sense of an Aboriginal inclusion in Australian society. Along the way there have been major milestones in this process of redefining Australia — Wattie Creek, the tent embassy, the passage of the Northern Territory land rights act, the Bicentennial, the Royal Commission into Aboriginal Deaths in Custody, and of course, Mabo legislation.

The research reported in this volume spans this entire period and thus has the potential to support a reflection on the pace and scale of change in white scholarship about Aboriginal Australia. That possibility alone should have made Birmingham's Wybalenna study a benchmark in Aboriginal historical archaeology. Unfortunately the potentials and possibilities of the project have not been fully realised. While Wybalenna provides very useful archaeological data about aspects of white Aboriginal interaction on Flinders Island during those crucial years, the fact remains that two fundamental problems with the volume have restricted its impact outside that limited context.

The first problem has to do with the fact that the research design for the fieldwork as reported by Birmingham, with its emphasis on ideology, consumerism, colonisation, and epistemology, bears very little relationship to the analysis of the archaeological contexts reported here. This lack of fit might well be seen as an expression of the well-known inadequacies of middle range theory in archaeology, but it might equally be noted that the interpretive context worked through in the late 1980s and early 1990s is dramatically different from the thinking which prevailed when the site was excavated. In short, the site was dug to solve problems of context and stratigraphy which were quite different from the issues which have surfaced during the phase of analysis which took place during the 1970s and 1980s. Therefore, in the main, evidence was simply not collected which can support a critical evaluation of the interpretations offered here on any other basis than the internal logical of the statements made. This does not mean that very useful data were not collected. What it does mean is that those data have been asked to bear an interpretive burden which they cannot plausibly do.

The second problem stems from the first and is an expression of a lack of critical self-reflection about the implications of these changes in our disciplinary mind-set since the excavations were completed. While it might be readily acknowledged that Birmingham has developed problems of real interest and consequence from her research at Wybalenna, the fact remains that there is insufficient reflection about how these might be approached in the present circumstances. But how could this be otherwise when the interpretive models used here (especially Leone and Potter's dominant ideology 'theory') have only the most tenuous links with empirical archaeological phenomena?

Now this particular shortcoming is hardly Birmingham's fault, nor is she by any means the only archaeologist who has sought meaning from promising but currently underdeveloped sources. But it seems to me that her achievement would have been the greater if she had offered an interpretation of her Wybalenna material which was more directly linked to the evidence that she had amassed, and then gone on to reflect on how the general inquiry into how indigenous Australia 'survived the nineteenth and twentieth centuries, might now be done. In this way there would have been the chance to work through the models of interpretation which she currently favours, to critique them, and then to go beyond them (a task which is already more than overdue).

These strong criticisms should be mitigated by three important considerations. First, Wybalenna is a significant site for both Aboriginal and non-Aboriginal Australians. As such, this publication assists us to establish what archaeological research has been undertaken, and also assists us to a better understanding of what was found there. Thus the *documentation* of the site is a signal achievement which goes some way to foster an open discussion about how all interested parties might find meanings there.

Second, the excavations at Wybalenna provide another example of interaction between Aboriginal people and Europeans in *environments controlled by Europeans*. In the past decade there has been some remarkable work done on Aboriginal communities in Mission stations and government camps (particularly David Rhodes' sadly undervalued work at Lake Condah Mission in Victoria). There is also a developing link between Aboriginal history and Aboriginal historical archaeology in the archaeology of Australian pastoralism. These studies have all recognized that even in the most controlled environments control was never total and that the simple fact of survival often depended on such contexts having (generally unacknowledged) elements of openness. Birmingham has documented another of these cases at Wybalenna in a way which also allows us a vantage point from which to re-read the message of dominance and control which was stated in Robinson's diaries and other 'official' sources.

Third, the material culture (both Aboriginal and European) recovered at Wybalenna provides another link in the chain of the Tasmanian Aboriginal experience which can now be demonstrated at Burghley, Wybalenna, and Oyster Cove. The importance of this link should not be minimised, especially because it is now possible to demonstrate at least some of the contexts wherein Tasmanian Aboriginal society survived the holocaust of the 1820s and 1830s. Current research in Tasmania and in the Furneaux Islands will build on these places to reinforce the notion that an archaeological characterisation of these transformations in Tasmanian society and identity can be sustained.

Turning to the specifics of the volume, Birmingham's discussion begins with a background to the project and then breaks into a standard division between 'The Archaeological Record' and 'The Documentary Context'. The last section 'Recovering Meaning: Answers and Questions' is an attempt to glue Record and Context together. As such the volume, despite some rhetorical flourishes, follows a conventional Australian pattern. Indeed, where some might regard the archaeology as

both record and context, and the same for the written documents, Birmingham clearly sees them as being distinct. This is quite mistaken in my view, a mistake which leads her to *de facto* argue that the meanings of things can be *recovered* rather than simply *ascribed* (which is what she really was doing). These are just two examples of an uncomfortable tension between theory and the empirical which pervades the volume, a tension which really comes to a head with an analysis of the core orientation of her research.

Birmingham clearly states that her original goal was to ascertain whether archaeology could add something to the official and unofficial documents of events at Wybalenna. But this inquiry is hardly a disinterested one as Birmingham is promoting an interactive model of cultural resistance, rather than reading an archaeological lesson over the graveside of traditional Tasmanian society. I applaud the view that Aboriginal people should not be seen as passively occupying God's waiting room at Wybalenna, and I concur that we should attempt to build this more active notion of agency into our interpretations. Nonetheless this is miles away from the assertion that she has evidence of an 'episode of indigenous cultural resistance to immigrant colonialist ideology'. It takes a while to unpack the implications of this view, but for me the initial question has to be 'what constitutes evidence of resistance, and could this evidence be explained another way?'

It is worth noting that this discussion, which is a core methodological issue of archaeological studies of identity and change, was never undertaken. As such we then are forced to conclude that archaeological evidence of resistance is *by definition* evidence of resistance. The four assumptions which Birmingham used to support the definition suffer from the same weakness in that there is no necessary link between the presence or absence of Aboriginal or European material culture and a collaboration with or resistance to European domination. Indeed, the evidence might be read as a failed attempt by Aboriginal people to conform to European standards, or evidence of a lack of will (or even interest) in enforcing those standards by the officials, or even a palimpsest of (over a decade of) changing views among Aboriginal people themselves about how to respond to their situations. Again, because of the absence of the link which Birmingham clearly prefers, her argument for archaeological evidence of resistance rests on assertion.

I do not dispute that the Tasmanian Aboriginal people deported to Flinders Island had every reason to resist European domination. Indeed Robinson's journals and official documents cited in Plomley's exhaustively documented *Weep in Silence* make it very clear that they did not passively accept their fate. I also agree that not every inhabitant of Wybalenna behaved in the same way and that there was a considerable variety of experience and attitude among the Aboriginal and European populations. Finally, I also accept that much of the behaviour associated with disobedience (a code word for traditional Aboriginal social and cultural activities) took place *away from the settlement*. But these, when taken with the methodological problems outlined above, surely lead us to the inescapable conclusion that the archaeology supports the documents, but we are not much further enlightened about how resistance might have expressed itself in terms of the material culture excavated from the cottages.

In this review I have focused discussion on some of the methodological problems which spring from the choices Birmingham made in the analysis of Wybalenna. Nonetheless it is worth pointing out that these criticisms stem from the fact that Aboriginal historical archaeology is still in its infancy and that, as a result, so many of its concepts are open to debate. Wybalenna is a significant step in building the historical archaeology of Aboriginal Australia, and it would be most unwise to claim that the field has a firm shape or direction. Birmingham has done us all a signal service by bringing Wybalenna to publication because in so doing she has afforded

us the opportunity to react against a major site and a 'no holds barred' statement of some of its many possible meanings.

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Dean E. Arnold, *Ecology and Ceramic Production in an Andean Community*, Cambridge University Press, Cambridge, 1993, xxi, 278. ISBN 0 521 43289 8.

Philip J. Arnold III, *Domestic Ceramic Production and Spatial Organisation. A Mexican Case Study in Ethnoarchaeology*, Cambridge University Press, Cambridge, 1991; pp.xvi, 177. ISBN 0 521 391997.

These two 'New Studies in Archaeology' volumes, coincidentally by authors with the same surname, deal with related issues of ceramic production. They also share aspects of a common approach to ethnographic observation and its application to archaeological explanation, especially a self-conscious concern for explicit theory in presenting their data and arguments.

Dean Arnold has become a familiar name to archaeologists working on pottery. In this book he presents a detailed ethnographic case study on ceramic production in the Andes, drawing largely on his original fieldwork in the area in the 1960s. This is the full treatment of the material which served as the starting point for his broader survey, *Ceramic Theory and Cultural Process* (1985), in which he developed and illustrated concepts of 'ceramic ecology'.

The primary approach is therefore by way of cultural ecology (as initially defined by Julian Stewart) in which ceramic production can be viewed as an adaptation to local conditions. A main aim is to elucidate the relationships between environment, technology and social organisation and to use these to develop more general principles — or cross-cultural generalisations — which can be applied to archaeological material. Arnold addresses many of these issues by way of a systems approach, arguing that it is the most appropriate way of examining linkages between different factors and allows the identification of formal models, suitable for cross-cultural comparison. At the heart of the analysis are the potters. Ceramic analysis must be undertaken through an appreciation of potters' knowledge of local conditions and resources and their place within the broader social as well as the natural environment. An important principle in analysis is the concept of choices: seeing pottery production as a sequence of decision-making stages, each limiting and structuring the range of further options as the potter selects clay, decides on form and finally on decoration.

Dean Arnold's case-study is set in the village of Quinua, one of the major pottery-producing villages in the Peruvian Andes, adjacent to the major pre-Inca city of Huari, in an area also significant in Inca and Spanish periods. He presents a view of the society, settlement pattern and subsistence economy as structured by topography and climate, before briefly reviewing local culture history. The ecological context of pottery production is then presented against this background. Pottery is largely produced by men on a part-time or occasional, seasonal, basis, when weather and the competing demands of agriculture permit. It is produced both for local distribution (generally everyday domestic use) and also for sale to tourists and others.

Arnold presents information on the availability of clays, and how they are selected and prepared for different vessel types. This is one step in the sequence of decision-making, then traced through the forming, decorating and firing processes. He goes on to consider how these products fit into local behavioural patterns: types of food produced, methods of preparation, ways of carrying vessels, and so on. It is here, perhaps, that the formal language of discourse can become a little trying ('a second way in which Quinua vessels are related

to the flow of matter and energy consists of enhancing nutrient flow by removing toxins that produce an adverse physiological response for humans', p. 117) but the discussion is important in establishing how pottery products relate to other technologies and social practices.

The chapter on design is substantial. This is a detailed analysis of a sample of painted vessels, considering surface treatment, structure of design fields and principles of layout (such as symmetry). Arnold relates these to vessel forms, and is especially concerned to establish the degree of variability in different classes of pottery or design. Design structure, rather than designs or elements, is seen as of greatest significance and a legitimate way of defining the products of this community of potters. This is pursued further in a discussion of 'Design and Society' relating the potters' choices of design to the social behaviour of the community. Although there is great variability in decoration, not all designs are acceptable, and Arnold presents an interesting analysis of which aesthetic principles were violated by those pots regarded as 'ugly'.

Having described and analysed all these aspects of modern pottery production in Quinua, Arnold considers the archaeological implications of his observations, both specific and methodological. Can the ecological approach be useful for explaining the archaeology and history of the Ayachuco Valley? This forms, in some sense, a test of the value of the whole approach, as an example of the critical interface between culture-historical and systems-based, ecological explanations. The region around Quinua was a focal point for a series of developments in the pre-Inca Middle Horizon (about 600–800 CE), when the adjacent city of Huari supported full-time pottery specialists. This contrasts with the seasonal and part-time nature of modern pottery production. Arnold ascribes this largely to the environmental context — the ancient city insulating potters from the constraints on full-time production (rainfall and availability of fuel). He proposes that a complex interplay between the conflicting demands of farming and potting, seen in a context of desiccation and land degradation associated with increasing population, led to a shift from part-time to full-time potting for those potter-farmers living in particular areas of the valley. Availability of resources and the increases in population provided an economic underpinning for full-time specialists. But an equally, if not more, important factor, Arnold suggests, was the demand for ceramics as a vehicle of religious expression, creating an especially great demand. Overall, however, the message is a simple one: the structure of the pottery industry is conditioned by the ecology, cultural and natural.

The question of identifying craft specialisation is the major theme of the less wide-ranging, but still valuable, study by Philip Arnold. He examines pottery production in several villages in the Los Tuxtlas region of Mexico with a special interest in the layout of workshops and patterns and processes of discard, with a view to establishing general principles linking behaviour and the archaeological record. He frames his argument explicitly around the concept of middle range theory as defined by Binford, and draws also on Dean Arnold's ceramic ecology approach.

The archaeological identification of degrees of craft specialisation is of critical importance in assessing socio-economic complexity. A number of different approaches to defining the modes or contexts of pottery production have been developed. Here an attempt is made to find correlates of pottery production systems — primarily spatial patterning — which might assist in developing principles of organisation of industrial arrangements.

The first part of the book deals with the ethnographic case study of potters in one region of Mexico. Ecological conditions which, here as in the Andes, affect seasonality of production are described together with the availability of raw materials and the general social context. Unlike that of Quinua, pottery in the

Los Tuxlas region is made by women, one seasonal task among many. Attention is therefore given to the size and composition of the households of potters and the implications family and other structures have on production.

These potters produce a fairly limited array of domestic vessels, compared with the range of complex models and other items produced at Quinua. Details are provided of the process of pottery manufacture, with a particular emphasis on the range and diversity of vessels in relation to techniques and work practices of different potters or households of potters. This is followed by a consideration of ceramic consumption and vessel use life. This deals both with questions of ceramic assemblage size in relation to household population: a stronger than normal correlation in this area is attributable to the availability of replacements so that stockpiling is less necessary than in non-pottery producing situations. These chapters, and others, are presented with numerous graphs, tables and statistical assessment of relationships. Too often these are hard to follow, and could have been more fully and clearly labelled and captioned.

The second part of the book deals with ceramic production and spatial organisation, with the aim of developing middle range theory to link archaeological observation with past behaviour. Philip Arnold initially notes three types of data used to identify pottery production locations: tools and facilities; manufacturing discard (primarily kiln wasters); and the form of the finished products. He then relates these to the basic modes of production (as defined by, for example, Rice, Peacock and van der Leeuw). He then considers the use of space as an indicator of the organisation of ceramic production. Different types of technology (for example kilns or bonfire firing) will have different patterns of discard as one (kilns) will repeatedly use the same area, while the other (bonfires) is more flexible and different locations may be used on successive occasions.

The availability of space — the size of the household and its surrounding compound or courtyard — affects disposal patterns of waste products. Arnold presents data on disposal patterns within potters' houselots. Smaller houselots exhibit larger discrete middens as a result of more intensive maintenance regimes. It is not obvious how this can immediately be related to ceramic production. More relevant are the observations that potters with smaller houselots are more likely to use fixed facilities, such as kilns, than are those who have larger compounds, although whether it is compound size that determines firing techniques or vice versa is not obvious.

The third part of this book considers the application and implications of the study, applying the general principles (not analogies) to the Middle Classic Period (300-800 CE) urban complex at Matapan. A series of surface collections and excavations of firing facilities (kilns) was undertaken in what the excavators regarded as one of the production areas of the site. Arnold assesses the discard patterns of wasters and other material at these areas and compares those of different phases, but skates over major problems of sampling and the conflation of material from different periods and episodes. The presence of several kilns for each major midden dump is seen as indicative of a more intensive mode of production than that represented in the ethnographic context and Arnold also suggests that the structure of discard shows an increase in intensity of production over time.

Extrinsic factors (location of the area on the outskirts of Matapan and 'adjacent to a potential transportation artery') are used to bolster the argument. My impression of Arnold's ethnographic material from Los Tuxlas is that site layout, discard and site maintenance activities are conditioned not by intensity of production *per se*, but by availability of space; different or new configurations need not signal different industrial systems, but other, unrelated factors. This case study is not a convincing example of the application of Arnold's middle range theory: has it really added a significantly better

basis for explanation?

Both of these books make important contributions to ceramic ethnography and ethnoarchaeology, a field dominated by Americanist scholars (it is a pity that they seldom look beyond the New World at other archaeological and ethnographic research). Philip Arnold's discussion is perhaps too closely tied to his underlying polemic and the desire for establishing middle range theory relating spatial patterning to ceramic production. The array of variables affecting discard make it difficult to derive general principles of the use of space with regard to any one aspect of behaviour. Commendable as this might be, this detracts from the more general issues implicit in his data. Dean Arnold's study of the potters of Quinua is far broader in scope, with greater depth and value: a stimulus to consider the multiplicity of factors which condition what potters do and how they do it, and how material culture can be construed as mediating society and environment.

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Charles D. Trombold (ed.), *Ancient Road Networks and Settlement Hierarchies in the New World*, Cambridge University Press, 1991; pp 277, illustrated, \$156. ISBN 0 521 38337 4.

This volume results from a simple conundrum. Why should so much effort be expended on roads and causeways in pre-Hispanic America when there are no draft animals, wheeled transport or natural impediments to foot traffic? The result of various attempts to come to terms with answering that question and the implications from the answers proposed is another in the series of 'New Directions in Archaeology' from Cambridge University Press. Published in 1991 it represents the core of the papers presented at the 81st annual meeting of the American Anthropological Association. This meeting, entitled 'Prehispanic Transport Networks in the New World', had been divided into three overall sections based upon geographical area, southwest United States, Meso-America and South America with several papers providing some larger scale views by concentrating on methodological and theoretical issues.

The present volume attempts to set up the larger framework at the outset and relegates more geographically focussed papers to the end of the volume. To this end it is divided into three sections: theoretical considerations, methodological considerations and regional studies.

The first section consists of four papers which address, in turn, a general approach to the study of communication routes and New World roads (Trombold), an evolutionary approach to roads and the communities that use them (Earle), the use of central place theory, modelling and more concrete concepts such as the needs of construction, road size and movement capabilities of bodies of humans (Hassig) and finally more specific application of some approaches to South America (Hyslop). This final paper is definitely misplaced and should have formed part of the next section on methodological approaches, suggesting as it does new methods of dating, recovering and reporting on ancient road systems.

The second section on methodology reviews the use of techniques which include remote sensing of roads (Sever and Wagner, Sheets and Sever), photo-interpretation (Obenauf), relative dating of roads (Beck) and network analysis (Gorenflo and Bell).

The final section brings together some of the material from the two previous sections in applications ranging from the southwest United States through Mexico to Bolivia. Papers on 'The Structure of the Aztec Transport Network' (Stanley) and 'The Association between Roads and Politics: Evidence for Wari Roads in Peru' (Schreiber) are especially good in providing examples of the application of network analysis to the study of roads, in the case of Stanley, and the association of

the roads with particular polities through a study of physical remains and relative dating, in the case of Schreiber.

The volume is well produced, as we might expect from Cambridge University Press, in the more compact form that is far easier to handle and use but it is unfortunately well out of the price range of those with only a general interest in roads and routes in the Americas. Despite the volume's emphasis on the pre-European Americas I believe that the theory and methodology outlined and discussed have relevance to both archaeologists in Australia and to archaeologists practicing historical archaeology.

The particular relevance lies in one of the stated aims of the volume. This was to illustrate the variation in the location of routes and 'the social value that was placed upon them at any given time'. Further, 'the route system used by a particular culture or group should reflect something of its internal composition, value system (in terms of choosing to connect certain points and not others), and mode of adaptation to the cultural and natural environment'. It might be said that these aims, explicating internal composition, value system and modes of adaptation to the natural environment, would happily be appropriated by many historical archaeologists in Australia to describe their work.

Taking heed of the theoretical and methodological approaches outlined in this volume, and applying them to the development of European occupation in Australia would repay many times over the work involved, by, at the very least, widening the scope of study to the regional and continental levels. Previous approaches to studying the early roads and communications in Australia have concentrated upon the technological achievements, methods of construction and the social impacts of the processes involved in construction. The focus has hitherto been on the particular rather than broad scope. In addition, little attention has been paid to Aboriginal people's involvement in the development of regional communication routes through their influence on early exploration because of their knowledge of the land and routes across the country. It is perhaps time to broaden our outlook, investigating the effects that communications systems (including here railways and perhaps coastal shipping) have had on the development of this country, the range of things this may indicate about the society that built them and the approach that society took to the landscape that confronted it.

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T.P. Hutchinson, *Version 2 (History and Archaeology) of Statistical Methods*, Rumsby Scientific Publishing, Adelaide, 1993; pp.xii, 152, \$17. ISBN 0 646 15653 5.

The University of Sydney classifies general books on statistical methods under Dewey 519.9. There are now more than 1200 such books lined up in the stacks. They nearly all deal with classical statistical methods of parametric and non parametric hypothesis testing. It is obviously hard to choose from such a profusion but, as an archaeologist, I have found Zar's *Biostatistical Analysis* closely mimics the models that I need.

Hutchinson has written another book on general statistics but aims it at historians and archaeologists. I appreciate the gesture, but in many ways the book is disappointing for me as an archaeologist analysing data in the 1990s. Above all, the book is anachronistic. Hutchinson writes as though archaeologists still compute with pen and paper. That's fine for the tiny examples he presents. However, he reasonably asks what we can do if we have 'a few tens or hundreds' of cases to compute? His answer (curiously matched to a different question) is that 'if we have many thousands or millions of observations, we get a computer'.

Why a computer? Surely a more useful recommendation, for the innumerate archaeologist, is to go to the photographic

counter of a neighbourhood pharmacist and buy a calculator with hot keys for automatic computation of basic statistics. It will cost \$25. For an extra \$10, the archaeologist can get a programmable calculator. These cheap and simple gadgets circumvent Hutchinson's perceived problem of the mean absolute deviation being 'awkward to compute' and the calculation of the sum of squared deviations from the mean being 'messy and error-prone'. True, Hutchinson makes parenthetical mention of standard deviation buttons on calculators, but in 1993 he should have assumed their use, not dwelt on the tricks of manual calculation.

What philosophical view of statistics does Hutchinson have? Statistical analysis for him is essentially hypothesis testing (for example his exposition of a test for significance of difference between mean shell size at two Aboriginal shell middens). Testing is his forte, but he does not entirely ignore the inductive usefulness of data description. There is for example a helpful discussion of the scatter plot in terms of what the best straight line means (p. 30). Yet here a critical opportunity is lost to introduce the reader informally to the fundamental simplicity of principal components, whereby for most metric data the 'best straight line' represents the component of general size and a second best straight line (at right angles) represents the component of shape. Principal components are penetrating everyday archaeological practice because most artefacts are best described by many variables.

Even if we accept that archaeologists need another book on statistical hypothesis testing, we might also require that it introduces us to current methods — not just classical parametric and non-parametric tests. Yet we look in vain for approximate randomisation methods. They are particularly valuable for the small sample case and are now well embedded in the popular statistical literature: Noreen's *Computer Intensive Methods for Testing Hypotheses* is perhaps the most accessible introduction. And where are multivariate methods? Hutchinson opens his book with an example of the univariate analysis of tin from fifteen artefacts. What of the other metals and trace elements? In this day and age I cannot imagine many editors being satisfied with a research paper that approached compositional analysis univariately. As I said before, most artefacts are best described by several variables.

In brief, Hutchinson's book is driven by the expository tradition of classical statistics. It largely ignores the day-to-day needs of archaeologists, who are now released by machines both from a wretched computational grind and from the need to make unrealistic assumptions about the parameters of their data. For me, what symbolises Hutchinson's motivation is that nowhere does he mention Tukey and exploratory data analysis (EDA). Tukey has probably done more than any other statistician to make statistics useful to non-statisticians wanting to discover patterns in data. Tukey was driven more by what analysts need to do and less by what statisticians like to do.

Be prepared for a racy didactic style. Hutchinson quotes an analysis of ice cap thickness and then comments: 'This is complete rubbish, and if you write something like this in an exam, your examiner is likely to give up in despair.' (p.127). I began to feel very much like a recalcitrant pupil while reading the book and therefore took a pupil's sneaking delight to read the sentence: 'So if you get an answer that is supposed to be a probability but is negative or is greater than 1, YOU'VE MADE A MISKATE [sic — and Hutchinson's emphasis]' (p.41).

In spite of my reservations about its scope, I enjoyed reading Hutchinson's book. I expect that I shall sometimes reach for it in preference to Zar, if only because it is about one tenth of the weight: that's not unfair comment - the publishers boast about its lightness.

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P. Hiscock and S. Mitchell, *Stone Artefact Quarries and Reduction Sites in Australia: Towards a Type Profile*, Australian Heritage Commission Technical Publication Series No. 4, Australian Government Publishing Service, Canberra, 1993; pp.viii, 94, plates, figs, tables, \$14.95. ISBN 0 644 249195.

This monograph is more than just a technical guide to classifying Australian Aboriginal stone quarries. It raises general questions about the nature of the archaeological record, archaeological significance assessment, and the value of the concepts archaeologists use to discuss stone tools.

Hiscock and Mitchell were contracted by the Australian Heritage Commission (AHC) to develop 'Type Profiles' for Aboriginal stone quarries. The 'Type Profiles' concept has been developed by the AHC to improve knowledge about places listed on the Register of the National Estate, and to refine the criteria used for assessing places nominated to the Register. Type Profiles are systematic descriptions of a type of place (in this case 'quarries' and 'reduction sites') documenting their general characteristics, range of attributes, variation, geographical distribution and abundance (p. 2). So far so good. But, as the authors themselves soon comment, creating Type Profiles is not as easy as it first appears (p. 11).

Hiscock and Mitchell first provide a detailed description of common techniques and strategies used to make stone tools. Complications are immediately obvious. Even adopting the authors' purely functionalist approach, the range of techniques and strategies available for solving problems of stone tool manufacture have the potential to produce a bewildering array of variation in the archaeological record. And, as they discuss, there is no simple correlation between stone tool manufacture and its archaeological imprint. In many cases it would be difficult if not impossible to understand the processes of stone tool manufacture and use from a single 'archaeological site'. Studies of interconnected sites across landscapes would usually be more appropriate. Individual sites have usually been classified by archaeologists and heritage managers on the basis of implement typology, or estimated age, size or condition. On closer examination, these classifications are problematic. The authors argue for a classification based on raw material procurement, nature of reduction and stage of reduction.

Following this theme, Chapter 3 reviews how the terms 'quarry' and 'reduction site' have been used in Australian and overseas literature. The authors suggest new and very specific definitions for these terms, related to the techniques and strategies of tool manufacture they describe. In order to review the current state of knowledge about quarries and reduction sites in eastern Australia, and as a basis for constructing Type Profiles, the authors examined data from the New South Wales, Victoria and Queensland state site registers. The terms 'quarry' and 'reduction site' mean different things to different archaeologists, and much of the volume is devoted to untangling some of the resulting confusion. Indeed the term 'reduction site' was not actually used in any of the site registers they examined, making simple retrieval of information about them impossible. The authors therefore conducted a more detailed examination of the Queensland site register to see if any 'reduction sites' were in fact recorded, but described as something else. They concluded that, with few exceptions, most recorders did not understand what a 'reduction site' was, or were discouraged by the format and prompting of the site record card from recording sites as such.

Despite these difficulties, and others which the authors acknowledge and discuss, they provide summary information about quarries and reduction sites from the three state site registers and suggest some preliminary Type Profiles. The Type Profiles are only preliminary because most archaeologists have not recorded the kinds of information about these sites which Hiscock and Mitchell regard as most useful. For example 'while site classification should properly concentrate

on information about the nature and stage of activity, this cannot be done because we currently lack such information' (p. 57). Detailed research in a few regions of Australia has so far indicated that there is potentially a huge array of quarry and reduction site types. However, it is impossible to know whether these studies are representative of Australia as a whole. They conclude (p. 57) that it is premature to develop a comprehensive set of Type Profiles for quarries and reduction sites and that far more research is needed. Their preliminary Type Profiles are therefore deliberately open-ended, so that they can be modified and added to as further research is done. They then apply their preliminary Type Profiles to some sites already listed on the Register of the National Estate and provide useful bibliographic references to work already carried out. The final chapter is a well-referenced summary of the research questions which archaeologists have addressed in their studies of quarry and reduction sites in Australia and overseas.

Is this book useful? The answer is both yes and no. It is certainly a well-produced, thoughtful piece of work (apart from some missing paragraphs on p. 32). Hiscock and Mitchell's discussion of definitions and approaches to the study of stone tool working are clear and well-written. The summaries of published work and the extensive bibliography make the book a useful reference volume. The Australian Heritage Commission deserves congratulations for funding the publication of research data on an area of Australian archaeology which has received less attention than it deserves (as noted by Isabel McBryde in her Foreword).

The value of the Type Profiles is less clear. The authors spend much of the volume struggling to devise something which they ultimately conclude is very difficult, if not impossible. The AHC commissioned the development of Type Profiles for their own management purposes, which are not clearly defined in the volume. Hiscock and Mitchell's Type Profiles are based on an ahistorical, functionalist view of stone tool manufacture. Other archaeologists might adopt entirely different approaches to the study of quarries and reduction sites, which could produce very different looking Type Profiles. Hiscock and Mitchell only consider the research value of these places from their own particular research perspective, while the AHC must take other cultural values into account in its management decisions. Only the AHC can judge whether the results presented here are useful to them. It remains to be seen whether the Type Profiles are adopted by the wider archaeological community. This seems somewhat unlikely, given the fate of many other 'recommended' archaeological classification systems.

The volume raises questions about the relationship between research, significance assessment and heritage management which merit re-examination and further discussion as Sandra Bowdler has pointed out (in Bowdler and Sullivan eds, *Site Surveys and Significance Assessment in Australian Archaeology* 1984:1-9). The publication of Hiscock and Mitchell's work at least brings some of these issues to a wider academic audience, rather than confining them to the 'grey literature' of government and consultants' reports, which can only be a good thing.

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Brian Cotterell and Johan Kamminga, *Mechanics of Pre-Industrial Technology. an Introduction to the Mechanics of Ancient and Traditional Material Culture*, Cambridge University Press, Cambridge, 1992; pp xv, 325, 159 figures, 22 tables, appendices, glossary, \$29.95. ISN 0521341949.

The introduction establishes how and why a mechanical engineer and an anthropologist have collaborated to produce a

book on some aspects of early technology. It makes it clear that the framework is the different types of sources of mechanical power and force for which illustrations can be drawn from any area or period of history. It accepts the nineteenth-century idea that, for technological purposes at least, modern ethnographic artefacts can be used to infer the properties of prehistoric ones. The object of the work is to provide some introduction to mechanical ideas to those without an engineering background, whose interests are primarily cultural but whose appreciation of the problems the artefact presents is hampered by technical ignorance. The importance of this should be self-evident.

The book starts with a survey of some basic mechanical concepts such as mass, weight, equilibrium and stability and the behaviour of different materials from gases and fluids to solids. It then seeks to apply these to various types of tools, machines and structures. There is no doubt that the result is a useful reference book for archaeologists and historians who need to find information quickly about the application of and effectiveness of various types of engineering practices. It applies modern understanding of the mechanics involved to estimate the skill and power involved in the solutions found by our predecessors to the problems of building, transport, feeding and fighting by land and by sea. It is not uniformly readable. Occasionally it degenerates into a handbook of formulae, important for specific problems but perhaps unnecessary in the body of the text.

It is important to realise that the authors have made no attempt fully to set their findings in an historical context although they have read and offer an interesting interpretation of some of the works of critical figures such as Galileo. Individual developments are dated for convenience, not as part of a worked-out analysis of patterns of discovery and diffusion. Their primary interest is in prehistoric developments rather than in the more narrowly defined historical pre-industrial period of the Middle Ages and early modern period. They make no real attempt at a comparative study of developments in a particular mechanical field.

Historically this is often disconcerting. The authors choose interesting examples of problems with little sense of chronology and no intention of providing comprehensive coverage. There is a long, illuminating and effective discussion of the production of flaked stone tools followed by a chapter on projectiles which is essentially confined to spears, boomerangs and bows and arrows. The weakness of the mechanical approach is well illustrated by the fact that they take no interest in and offer no explanation for the fact that despite the mechanical limitations of the longbow it was more widely used in warfare than the crossbow and held its own against the handgun for over a century.

The authors' personal and professional interests have governed what is and what is not included in the study. The chapter on musical instruments — again effectively covering only the prehistoric period — is intelligible only in terms of anthropological preoccupations. The ethnological bias of the authors virtually ensures that, on any particular subject, there are gaps and aspects dealt with only in the sketchiest fashion. Mechanical calculations and detailed case studies are interspersed with heroic generalisations drawn from very diverse and sometimes suspect sources. In some ways this gives us the worst of both worlds. The case study is interesting, the mechanical explanation valuable, but they do not provide insights into the way in which our ancestors handled similar problems in different contexts. We end up with scattered information on a broad range of topics, rather than a systematic analysis of any one problem.

The discussion of water transport is a perfect illustration of this frustrating combination. While the discussion of stability is interesting, it is limited to a specific type of mono-hull and to Polynesian double-hulls. There is virtually no discussion of the strength or of the deformation produced by the many

different shipbuilding techniques, not even the simplest division between shell or skeleton-built hulls. The discussion of oarpower is valuable but incomplete as the effect of the hull shape and the overall weight of the vessel on the speed produced is set aside. Much more could probably have been deduced from a consideration of the historical evidence which is available, for example, from the Venetian Arsenal experiments in the later Middle Ages. The discussion of sail is similarly incomplete and, although this does not really matter for the mechanical discussion, historically inaccurate. The discussion of wheeled transport and the harnessing of animal power is similarly very largely theoretical, offering little guidance to those who might wish to explain why certain forms nonetheless persist.

This book is a pioneering venture into an area that has been understandably but unfortunately neglected. It is possible to list areas which the authors might have included but chose to ignore such as brewing and distilling and the production of flour. Eventually, one hopes, they or others will start to fill the gaps and extend our understanding of the development of mechanical understanding and practice.

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Alan Roberts (ed.), *Grass Roots History*, Federation of Australian Historical Societies, Canberra, 1991; pp.92, illustrations. ISBN 0 9595714 2 6.

This little book is the proceedings of the Joint Conference of the Federation of Australian Historical Societies and the Royal Historical Society of Victoria in Melbourne, held in 1989. It is useful as a guide to the diverse locations of history making in Australia, though according to the contributors to the symposium on the future of historical societies, all is not well at the grass roots, or at best the jury is still out.

Outlining various community groups in what he calls 'the Australian History movement' — Oz history in universities, the National Trust, genealogy, collecting our movable culture (Australiana) — Roberts observes that 'historical societies come last by a long way today in terms of the number of their members and the amount of income they are able to generate' (p. 69). He cites needless hostility between academic and professional historians on the one hand and amateur historians on the other (elsewhere, Crozier mentions hostility between academics and professionals). There is also the 'absurd' waste of resources caused by the duplication of too many small groups. Gail Le Bransky would seem equally despondent, citing a survey of societies in New South Wales which identified poor attendance, ageing society membership and lack of any clear focus.

On the other hand, Tom Griffiths claimed to know of a society which enjoyed 'lively debate', a youthful membership and had a president with a sense of humour, and Joan Hunt reported enthusiastically on the 320-member Woody Yaloak Historical Society, one of several societies in the Shire of Grenville, near Ballarat, Victoria. With a population of 6,000, there are 440 historical society members — not a bad average at all.

Which just goes to show that with the right approach there is a potentially huge interest in local history out there in the community. Susan Marsden, then the State Historian for South Australia, spoke with enthusiasm about the growth of grass roots organizations in her state, but cautioned that it is the local focus which makes for local commitment. The local focus may operate at a regional or even a state level, however. The South Australian Historical Society formed in 1926 only lasted a few years and, according to Marsden, its demise was due to the input of academic historians who failed to understand the need for a local focus. Professor Keith Hancock was president

and he and the committee decided that 'any topic of historical interest' was appropriate. But that is the realm of historians, not of community: a lecture on Joan of Arc attracted about ten people, while eighty came to hear about Edward Gibbon Wakefield. 'Focus' and what Diane Foster calls 'a clear mission statement' are essentials for success.

If the evidence on the health of local historical societies is shaky, the blossoming of history writing in many and various other places is clearly in evidence. Crozier's discussion of the growth of professional historians' work and organizations should be read by the all-too-many academic historians who have just not kept abreast of recent trends. A developing community interest in history is going on, without a doubt. It is fuelled by an increasingly mature public and governmental attitude towards heritage issues and, indirectly, towards local history courses which are becoming (just) acceptable within the universities and colleges.

The opening lecture — the Augustus Wolskel Memorial

Lecture, no less — of the conference, 'Reflections on the Flesh and Bones of Local History', should remind readers that the stuff of community history has come a long way in the last few decades. Richard Bromme's comments on the elusive meaning of 'community' and the need for the local historian to understand far more than just the local scene are set around a discussion of a history of Coburg, Victoria: 'the important matter is to concentrate on people in action, to strive to read those actions and their meanings, and to represent them to our readers in all the richness and complexity that we can comprehend.' Just how rich local history can be depends not merely on the 'history' but on the 'historian'.

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