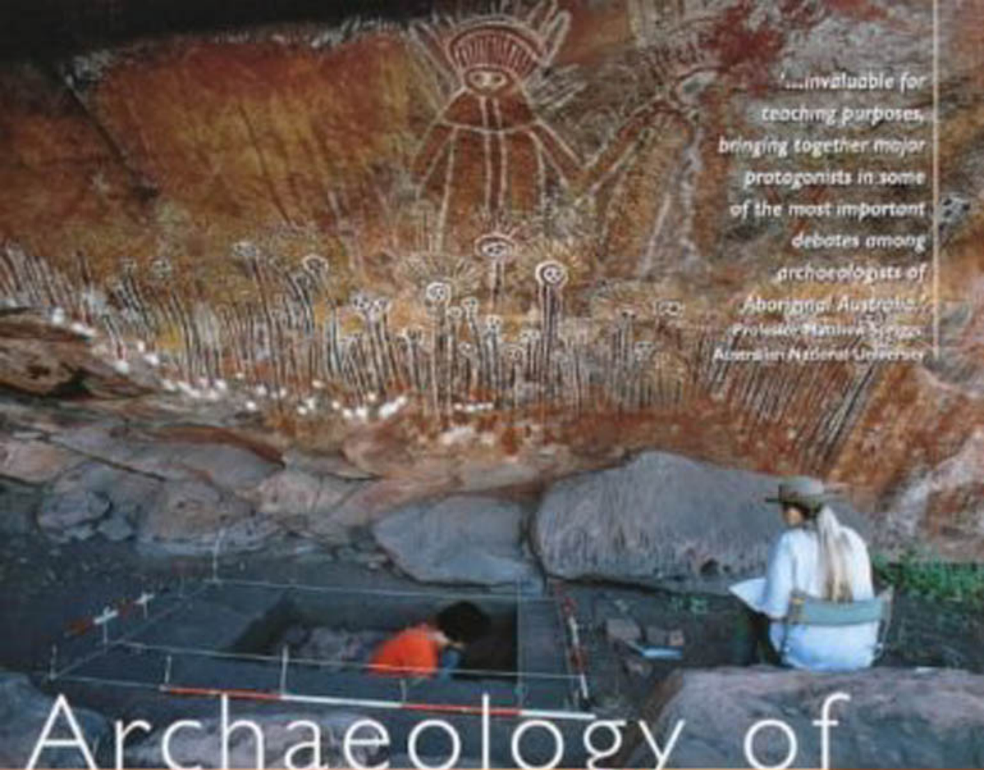

'....invaluable for teaching purposes, bringing together major protogonists in some of the most important debates among archaeologists of Aboriginal Australia.'

Professor Matthew Spriggs
Australian National University

Archaeology of ABORIGINAL AUSTRALIA

A Reader

EDITED BY TIM MURRAY

'When Greater Australian archaeology first got rolling as a serious intellectual pursuit in the mid-1960s, there were two things about it of real (if not universally appreciated) general interest:

- It confronted a record of the colonisation of a large continental landmass by fully modern humans, comparable in time span and spatial scale to the one from Eurasia west of the Ural Mountains (i.e. Europe), and more than three times as long as the one from the Americas. There were some interesting similarities among these three sequences, but the Australian one stood out in a number of important respects, notably the relative lack of variation it seemed to display in comparison, with the other two. In anthropological terms, this made it a potentially important, arguably critical, comparative case. Any general explanation offered for overall pattern and broad similarities in the European and American records also had to account for the very different character of the Australian one. If the explanation couldn't handle that problem, then it wasn't very good.
- Archaeologists who lived and worked in New Guinea and Australia had a significant advantage over their European counterparts. Many of the behaviours responsible for creating patterns in the archaeological record they were attempting to interpret had persisted in one way or another well into historic times, in some cases right to the present. Historic and ethnographic observation of those behaviours made better archaeology possible. It also gave Australianists an unusually good opportunity to evaluate the interpretive conventions commonly used by archaeologists elsewhere, often with salutary (sometimes surprisingly counter-intuitive) effect.

Throughout the late 1960s and 1970s, most archaeological research in Australia was devoted to developing these two themes. From the mid-1980s onward, however, several new elements began to emerge:

- New dating techniques, applied to sites in Arnhem Land and the Kimberleys, unexpectedly extended the record of continental colonisation, possibly to 60 000 years, perhaps to 175 000 years. The contrast with the earlier favored, 40 000 chronology was (and is) extremely important. If the 60 000 year date is accurate, then Australia may have the earliest record of fully modern people in the world outside Africa. If the 175 000 year date is right, then it's either the earliest record for modern humans anywhere in the world, or (given the problem of getting to Australia from SE Asia) a completely unexpected demonstration of the sea-faring (and by extention other technological) capabilities of pre-modern humans.
- Intensive investigation of the record in several different parts of the continent began to reveal previously unappreciated spatial and temporal variation in Pleistocene technology, subsistence economy and settlement patterns. Suddenly, the local record wasn't as static as it once seemed; yet the sharp contrasts between local Australian and contemporary regional sequences elsewhere in the world still remained. The explanatory challenge of dealing with all the newly appreciated variability was multiplied accordingly.
- Aboriginal rock art, long admired primarily for its aesthetic qualities, gradually came to be seen as a potentially important source of information on prehistoric social organisation. Given the sharp contrasts between ethnographically-known Aboriginal social systems and those represented among traditional hunter-gatherers elsewhere in the world, the possibility of learning more about the evolution of the former and comparing it with the latter added still further potential complexity to the game.
- The growing self-confidence and political sophistication of various Aboriginal communities led many of their members to reassert strong interests in the investigation and interpretation of the archaeological record of their own history. What was to be done by way of research, by whom, and to what end were no longer purely scholarly, but now highly-contested, political matters.

Murray's collection gives its readers a good sense of all this recent upheaval in the study of Australian prehistory. *It will be especially good for an undergraduate audience in that it virtually requires comment from both instructor and students on a broad range of issues, from evolution through human interaction with past environments, both natural and social, to the role of archaeology in a modern, post-colonial state.* Exploring it, and all the issues it raises, should be an interesting adventure.

Professor James F. O'Connell, University of Utah

Archaeology of Aboriginal Australia

A reader

Edited by Tim Murray

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1 The changing contexts of the archaeology of Aboriginal Australia

Tim Murray

Over the past thirty years the human history of what Mulvaney called 'the dark continent' (1961) has become the object of intense national and international interest. These have been the 'decades of discovery', featuring fieldwork and analyses which have rewritten our understanding of the history of Australia almost on a yearly basis. The significance of these discoveries has been recognised by State and Federal governments with the passage of comprehensive heritage protection legislation, and by the international community, through the listing of three great archaeological provinces (Kakadu, Lake Mungo, and South West Tasmania) on the World Heritage Register.

But these have also been the decades where a profound shift in the popular understanding and appreciation of Aboriginal Australia has taken place. The discoveries made during the 1960s, 1970s and 1980s (during the period of what Rhys Jones called 'cowboy archaeology') played an important role in that change of consciousness, but they did so against a background of intensifying Aboriginal activism, the rise of Aboriginal history, and a fundamental change in the composition of the population of Australia.

The practice of archaeology is never divorced from the society which pays for it and consumes its product, an argument which is supported by even the most cursory exploration of the history of late twentieth century Australian archaeology.

Forces for change in the archaeology of Aboriginal Australia

The search for an understanding of the history of Australian archaeology from the 1960s (and of its place in Australian society) requires a constant tacking between the empirical discoveries being made, the approaches used to make sense of them, and the perceptions of the indigenous and non-indigenous communities about what it all means. The reality of change is a fundamental theme in all elements of the inquiry.

Change has occurred in virtually every facet of Australian archaeology. For a start the community of practitioners once included many amateurs, whereas since the 1960s it has become almost completely professionalised. On the other hand, the profession used to be dominated by University teachers and researchers, whereas now far more archaeologists are employed by government instrumentalities or as private consultants to a wide range of organisations. Archaeology is no longer a pure academic pursuit designed to contribute to general enlightenment. It is now also very much about power, politics, and money.

The accessibility of the physical remains of the past has also changed from being essentially uncontrolled, to controls first favouring professional archaeologists and, subsequently, Aboriginal people. Since the 1980s access has become very highly regulated with field studies only being possible with the support of indigenous communities and heritage bureaucrats. The first generation of heritage preservation legislation (that passed by State Parliaments during the late 1960s and the 1970s) can now be seen to have mostly reflected the needs and interests of professional archaeologists and to have been drafted with little or no consultation with Aboriginal people. However, legislation passed during the 1980s, and in this decade, has been arrived at after lengthy consultation with the owners of that heritage, and now mostly reflects the paramountcy of the Aboriginal interest.

If the social and cultural contexts of the archaeology of Aboriginal Australia have seen radical change, then the same applies to the practice of archaeology itself. During this period the discipline has been the site of significant (and continuing) debate about its nature and purpose, particularly how archaeological knowledge is gained and about how we relate it to other sources of knowledge about people and their histories. For over thirty years archaeologists have been locked in intense theoretical debate about how best to understand the past, the form of appropriate methodologies, and the most fruitful questions to ask. Some of this has spilled over into the practice of Australian archaeology and has led to disputes about matters such as the role of ethnographic analogy in archaeological interpretation, or the most effective means of describing and accounting for variation in human behaviour in the prehistoric past.

Yet the structure of these disputes in Australia has tended to preserve a separation between the methodological questions of doing archaeology and the broader questions raised by the significance of the knowledge produced, a separation which has not happened in other parts of the Anglo-Saxon world. It is significant that in each of the general prehistories published since the first edition of Mulvaney's *Prehistory of Australia* (1969), which include the second edition of that work (1975), the Bicentennial prehistory (Mulvaney and White 1987), White and O'Connell's *A Prehistory of Australia, New Guinea and Sahul* (1982), and the three editions of *Archaeology of the Dreamtime* (1983, 1989, 1995), there is virtually no discussion of the purposes of such prehistories, nor of the different ways they could be produced. Indeed, prehistories of Australia tend to seek synthesis rather than to represent debate (a notable exception being White and O'Connell 1982).

Part of the reason for this absence is a traditional antipathy in Australian archaeology between doing and reflecting, which is as much a product of an early need to concentrate all efforts on the prosecution of fundamental field research on a continental scale, as it is from the fact that practitioners have had the field of interpretation pretty much to themselves. While it can be readily established that the *discoveries* made by archaeologists have, until recently, influenced Australian life, it is harder to establish whether reflections by archaeologists are of interest outside the narrow confines of the profession (a significant exception being Griffiths 1996).

But if we step back from debates about technical matters such as the antiquity of humanity in Australia or the nature of change between Pleistocene and Holocene, it is possible to identify two great issues which underlie the relationships between archaeologists as professionals, and between archaeologists and the society which sustains them. These are the role of the archaeology of Aboriginal Australia in the development of national identity, and the impact of the development of an archaeological perspective on Aboriginality which may well differ from that springing from anthropology, history, and (more importantly) indigenous people themselves.

In the case of the first, it is inescapable that in the last decade of the twentieth century Australian prehistoric archaeology has become a complex structure marked by increasing perspectival and organisational divergence. It is also clear that our understanding of the nature of Australian prehistoric archaeology is going to directly impact on broader conceptions of what constitutes the heritage of the country. In the case of the second, developments within archaeological theory are likely to raise questions about our ability to write conventional prehistories that do more than rehearse cultural preoccupations or prejudices.

I have written before (1992) about the reluctance among archaeologists to historicise Aboriginality, to thereby create a framework where the tremendous dynamism of prehistoric Aboriginal societies can link with the demonstrated flexibility and resilience of Aboriginal people in the historic period, to restore a cultural context other than the timeless Aboriginal person operating inside timeless Aboriginal social institutions. Part of that reluctance seems to stem from the false belief that historicising Aboriginal people weakens the links between Aboriginal people of the present and those of the deeper prehistoric past. The claim that Aboriginal people are the inheritors of the 'world's oldest continuous culture' raises issues of continuity, of the interpretative primacy of the ethnographic present over the historic past, and the terms under which new images of Aboriginality might yet be created as a result of thirty years of prehistoric archaeology.

It seems clear enough that these two great issues will not only continue to act as lightning rods for forces for change in Australian archaeology, but that they will also make the archaeology of Aboriginal Australia increasingly contested ground between archaeologists, Aboriginal people, and the society which sustains and constrains them both. In this account archaeology and society should be in dynamic interaction and be free to create new pasts as well as new futures.

Given the significance of these issues, why have no discussions of relationships between archaeologists and Aboriginal people been reprinted in this volume?

Why no Aboriginal voice?

The first reason is quite straightforward. There is now a large literature arising from debates between archaeologists, and between archaeologists and Aboriginal people, about ownership and control of the archaeological past. Much of this literature is directly referred to in the final chapter of this book. Given that Aboriginal viewpoints about such important matters are now more accessible than ever before, making clear reference to such literature overcame the real objections I had encountered, that seeking to reprint such views in the context of this book could be construed as an example of appropriation.

The second reason has to do with the fact that a wide variety of views about the relationships between archaeologists and Aboriginal people exist and that choosing just one perspective on such contentious issues (because of the restrictions of space) would be tokenistic and misleading. Other books specifically devoted to an exploration of these issues already exist and are referred to.

The final reason is that in the present circumstances debate about ownership and control of the heritage of Aboriginal Australians is just as likely to be joined by archaeologists as it is by Aboriginal people. The challenge of rethinking notions of cultural continuity and of Aboriginal history has to be met by archaeologists as well as by Aboriginal people. Similarly, the configuration of relationships involving partnership and cooperation between the two groups requires us to consider a wide variety of perspectives on the goals and aspirations of both groups.

The foregoing discussion should make two things obvious. First, the absence of a formal Aboriginal voice in this book is not to be explained by a rejection of the significance of an Aboriginal perspective on the archaeology of Aboriginal Australia, or by a rejection of the validity of a paramount Aboriginal interest in Aboriginal heritage. Second, that Aboriginal voices are to be heard throughout this book. In the post-Mabo period of Australian history the mutuality of indigenous and non-indigenous Australians has been firmly established, but there is also a real sense in which a future based on justice and reconciliation is there for both groups to create. In this sense the arguments made by archaeologists about the archaeology of Aboriginal Australia also become part of a dialogue between indigenous and non-indigenous Australians.

However, at the present time neither group knows what mutuality means and we have little concrete evidence for predicting the ways in which the politics of ownership, control and exclusion will play themselves out. We can state the obvious and observe that it is unlikely that there will be a uniformity of view across Australia about the politics of heritage and of sovereignty. But among fear and uncertainty lies the possibility that mutuality and partnership can provide archaeologists and Aboriginal people with a real chance to begin to tackle the great issues of history and identity mentioned earlier. Perhaps the process of re-imagining the heritage of Australia can only really become effective when serious dialogue actually takes place.

Structure and selection

The main objective of this book is to convey a sense of the excitement and of the significance of the research which has been undertaken since archaeological research became professionalised during the 1960s. Another objective is to convey the fact that there is by no means a party line among practitioners about how to understand over 40 000 years of human action. There is a wide diversity of opinion on matters ranging from quite basic issues such as the antiquity of the human settlement of Australia, through to much more complex questions related to seeking explanations for change and variation in Aboriginal societies, and for the interpretation of the human history of Australia. The papers reprinted in this collection report major discoveries and innovative schemes of analysis, but above all they represent a clear point of view about 'big picture' matters as well.

The organisation of the book reflects these objectives. The first part consists of General Surveys of Pleistocene and Holocene Australia and of two of the central questions of Pleistocene archaeology: how long ago did human beings first enter Australia, and how was the continent settled? The sense of debate (and of the essentially provisional nature of all syntheses of continental prehistory) is reinforced by demonstrating the highly varied and regionally diverse archaeology of Pleistocene Australia. Since the 1960s Australians have almost become accustomed to a regular extension of the antiquity of human occupation in Australia, but there has been less media interest in the history of Aboriginal societies after Pleistocene settlement.

We should not be surprised by this because writing such histories is a difficult business, involving serious debate about the nature of Aboriginal societies, and the terms under which archaeologists reconstruct historical process from the very faint signals of human behaviour which survive from these very early periods. Archaeologists are still coming to terms with the discovery of such unexpectedly high levels of behavioural diversity occurring so long ago.

Part of the reason for the surprise is the deeply ingrained perception that human behaviour becomes increasingly complex as time passes. In this scenario Aboriginal societies should exhibit greater complexity and variability during the Holocene than in the Pleistocene, mirroring historical trajectories which have been identified in the archaeological records of the Old World. In Australia there has been considerable debate about whether the history of Aboriginal society exhibits this kind of linear trajectory, a debate which has tended to focus around the nature of social and economic intensification during the late Holocene. For nearly two decades archaeologists have debated the causes of intensification, seeing population growth, pressure on resources due to rising sea-levels, contact with island South East Asia, or contradictions within semi-sedentary societies as likely candidates. It is perhaps no surprise that many archaeologists still do not accept that intensification did occur in late Holocene Australia, and that there is even debate about what the crucial evidence would be that would decide the matter.

But there is little doubt that we need further research into both the Pleistocene and Holocene of Australia to help us to gain a clearer picture of change and variation in Aboriginal life. Over the last thirty years archaeologists have also gained a better understanding of the types of information they have to deal with, and have sought to influence the structure of the debates through more consistent attention to methodological and theoretical issues. The essays and papers in the second section (Special Studies) reflect this dynamic and present perspectives on stone artefact analysis, rock art, and the reconstruction of palaeoecology, all of which are important elements in any prehistory. Although it is too early to say whether the innovations in practice which the essayists argue for will have a widespread impact on the archaeology of Aboriginal Australia, it seems clear enough that they signal the need for a more intense engagement between the business of making discoveries and the need to think about what these discoveries mean. One of the outcomes of this re-evaluation of the conceptual armoury of Australian archaeology will be a recognition of the virtues of diversity, both in terms of past human action, and in our approaches to understanding it.

Concluding remarks

The best way to comprehend diversity and to form judgements about the value of the interpretations on offer is to use this book as a starting point for further investigation. This is particularly true of Aboriginal viewpoints about the human history of their country, and the practice of archaeology on their cultural heritage. To this end the final chapter presents suggestions for further reading in a number of key areas while identifying institutions, societies, and publishing venues which cover both archaeological and Aboriginal interests in exploring the archaeology of Aboriginal Australia.

In this Introduction I have sought to portray the archaeology of Aboriginal Australia as being the subject of active research and even more active debate. Above all I hope that you will find the archaeology of Aboriginal Australia to be both fascinating and culturally important, and its pursuit to be a vital component in the very difficult business of defining ourselves and our nation. I also hope that you will be persuaded that the archaeology of Australia is a vital and significant undertaking where differences of opinion are strongly held among archaeologists and Aboriginal people, as they should be. It is far too early to say that we have ‘understood’ the human history of the continent. It is far better to argue that here are just a few of the many possible renderings.

Part I
General Surveys

2 Antiquity

Jim Allen

'Hunting the oldest' has been a phrase occasionally used within Australian archaeology to dismiss the particular interest of some researchers in finding the earliest sites of human occupation in Australia and Papua New Guinea, which together formed a single Pleistocene continent when initial colonisation occurred. However, providing deep antiquity for human history remains one of the particular strengths and fascinations of archaeology for both professionals and the general public alike; to be able to attach some sort of 'oldest' tag to a site or artefact or ancient invention is a guarantee of attracting professional and public attention.

Disagreements about chronologies have also sparked many of the largest archaeological debates, not the least being between the nineteenth-century geologists like Cuvier and Dean Buckland, protagonists of Archbishop Ussher's chronology of 4004 BC for the beginning of the world, who sought to argue catastrophic geological explanations for the earth's formation, and those scientists like Lyell, who argued that geological developments of the earth in the past must have been similar to the present, both in processes and speed. This dispute centred around the then growing body of evidence associating stone tools and the remains of humans and extinct animals in various European and British caves, and the implications of these data for accuracy of the Old Testament (Daniel 1978: 33–8).

A myriad of similar disputes in the first half of this century reflect more than anything else the ingeniousness of archaeologists seeking to impose chronologies on prehistoric sites and periods in the absence of direct, absolute dates. Equally, mid-twentieth century inventions of radiometric dating techniques like radiocarbon dating have brought forward many disputes and reformulations of chronologies. Justly famous is Stuart Piggott's comment on two similar radiocarbon determinations he obtained for the construction of the Durrington Walls henge monument: 'This date is archaeologically unacceptable . . .', although eventually it was shown to be accurate. At the same time, rejections of newer chronological techniques in favour of older ones have on occasion been vindicated: discrepancies between radiocarbon ages and historical records for protohistoric Egypt initially led to the rejection of the initial radiometric chronology. It was not until the radiocarbon determinations were calibrated against the dendrochronology of the bristlecone pine that the discrepancy was resolved and the historical dates upheld (Daniel 1978: 357–8).

Also, before radiocarbon in Europe and Asia and North America, the prehistoric record could be fitted into geological periods—glacials, interglacials, stadials, interstadials—integrated with the palaeontological evidence of different extinct animal species occurring in associated strata with human artefacts. In addition, in Europe, Africa, Asia and North America, archaeological tool types allowed other relative dating schemes to be set up. In Australia, on the other hand, amorphous stone tools and other debris forming archaeological sites neither lent themselves to the formulation of archaeological phases dividing space and time, nor were they normally found associated with extinct animal species to impart to them an element of antiquity. While past ice ages were known to have periodically joined and sundered New Guinea, Australia and Tasmania, before 1960 there was no clear indication that Aborigines had even been in Australia as far back as the end of the Pleistocene.

Relative dating frameworks had been tried in Australia. Firstly, there were, and had been since the end of the nineteenth century, scientists who argued for a high antiquity for the earliest colonists into Australia. Howitt (1898) and Edgeworth David (1923) both argued the case for such an antiquity, mustering geological, palaeontological and archaeological evidence, and logical argument to their support, but insufficiently well to persuade Pulleine (1928) for example. Even by 1961 conservative scientists were still doubting whether Aborigines had arrived in Australia before the end of the Pleistocene (comment by Abbie in 'Discussion on the antiquity of Man in Australia' in Stanner and Shiels 1963: 82). Interestingly, the early arguments frequently centred on Tasmania: the material cultural differences between that island and mainland Australia; whether the Tasmanians walked or drifted there; and arguments such as the dingo failing to reach Tasmania as an indication of a long antiquity of humans there, and by extension, on the mainland. Then as now, Tasmania was a touchstone for Australian archaeology.

Secondly, excavations at the Murray River site of Devon Downs (Hale and Tindale 1930) and subsequently at nearby Tartanga, together with research on Kangaroo Island, led Tindale to propose a five-stage cultural sequence which implied Pleistocene antiquity. Subsequently McCarthy provided a similar sequence for New South Wales and the Northern Territory, but neither of these survived as useful chronological schemes for subsequent archaeological development (see Mulvaney 1969: 99–106).

Radiocarbon dating, used initially to provide absolute dates for these sequences, subsequently outmoded them. Firstly it provided a secure temporal framework into which Australian archaeological sites and sequences might be placed. Secondly, as the antiquity of these sites seemingly increased with each new issue of the archaeological journals, the unexpectedly high antiquity of humans in Australia quickly graced Australian archaeology with a scientific importance at a world level. Thirdly, it allowed temporal comparisons to be made with other parts of the world and brought Australia centrally into discussions of human diffusion and modern human behaviour (see for example Gamble 1993: 214–36).

The known antiquity for humans in Australia quadrupled in the 20 years between 1961 and 1981. In 1961 Mulvaney (1961: 64) noted that the oldest secure radiocarbon date for human activities was from Cape Martin in South Australia and was less than 9000 years old (although he also acknowledged the likelihood that Gill's claims of a human origin for charcoal samples from Keilor, radiocarbon dated to *c.* 15 000 and *c.* 18 000 years ago, might be correct). In any case Mulvaney himself found the demanded association of Pleistocene dates and stone tools at Kenniff Cave the following year (Mulvaney and Joyce 1965). By 1981 Pearce and Barbetti (1981) were arguing that artefacts from an open site on the Upper Swan River near Perth were *c.* 38 000 years old. In between these temporal extremes and in all parts of Pleistocene continent of Greater Australia, radiocarbon has now identified more than 170 sites of Late Pleistocene age (Sharp and Smith 1991). Although in the late 1960s one observer would predict that Australian prehistory was unlikely ever to be of more than parochial interest (Anonymous 1968), by the mid-1970s hunter-gatherer archaeologists on both sides of the Atlantic were paying close attention to Australia. In seeking the reasons for this we need not look beyond the early 1970s' announcements and discussions of the Mungo and Kow Swamp human remains, the Mungo archaeology, and the closer definition of the Australian megafaunal extinction debate, all subjects destined to underwrite principal themes in Australian prehistory; and foremost among the reasons for their importance were their respective antiquities, defined by radiocarbon dating.

In the last five years the apparent plateau of *c.* 40 000 years for the earliest dates for human settlement of Australia and Papua New Guinea, established by radiocarbon dating, has been breached by dates for two northern Australian sites of between *c.* 53 000 BP and 60 000 BP, using thermoluminescence and optical luminescence techniques (Roberts *et al.* 1994). Despite some questioning by Australian archaeologists (Bowdler 1990, 1991; Frankel 1990; Hiscock 1990) and responses (Roberts *et al.* 1990a, 1990b), there has been a rapid, and to my mind largely uncritical, acceptance of this large extension in human antiquity in Australia and Papua New Guinea.

A useful scientific precept is that the more critical and revolutionary the claim, the more complete the data and the more stringent its critical evaluation should be. It was with this thought in mind that I accepted the otherwise *infra dig* task of introducing the three papers debating the 'short' radiocarbon chronology and the 'long' luminescence chronology selected by the editor, given that I had authored one of them, and been jointly involved with the third.

An earlier paper written in 1989, preceded the announcement of the older luminescence dates, and as such provides a summary of the development of the radiocarbon chronology and its implications for Australian and Papua New Guinean prehistory. The first paper, written in 1994, summarised the changed situation following the announcement of the older luminescence dates and the initial skirmishes cited above. The second paper is the response/restatement of the position of the scientists producing the luminescence dates. The final piece is an extract from a longer 1995 reply by myself and my colleague, Simon Holdaway. This extract is included to demonstrate one way in which the debate can be widened to examine aspects of the argument in more detail. Excluded from this extract are further disputes about earlier data and how they were presented.

The question of when this continent was first settled is important because it provides the initial framework upon which other arguments of colonisation, settlement, adaptation and human behaviour depend. As I write this, a claim for luminescence dating of an Australian site twice the age of the sites mentioned here is being prepared for publication. Such a claim will bring into sharper focus other claims made by palynologists that increases in firing regimes represented in drilled column samples for the period 120 000 BP to 140 000 BP reflect human rather than natural occurrences (Singh and Geissler 1985; Kershaw *et al.* 1993). Such ages, should they be accepted, will again change our perceptions of the colonisation of this continent quite fundamentally, even to the point of whether the first colonists were modern *Homo sapiens*, an assumption never seriously questioned before. It is for this reason that this second issue of antiquity is also currently being debated (Anderson 1994; Hope 1994; White 1994; reply by Kershaw 1994).

2a Radiocarbon determinations, luminescence dating and Australian archaeology

Jim Allen

A new view of the chronology of the initial colonisation of Australia

In 1989, reviewing the available radiocarbon determinations from Australia (including Tasmania), New Guinea and the nearer Melanesian Islands, I observed that there was no appreciable difference in the oldest radiocarbon ages from this region, from north to south and east to west, with the exception of the centre of Australia where the oldest desert date was then *c.* 22 000 years ago (Allen 1989). A general antiquity, based on radiocarbon, of 35 000–40 000 years for the initial colonisation of Australia had not been extended during the previous decade, and I concluded that current evidence gave little reason to go beyond a date of *c.* 40 000 years. However, within a few months Roberts, Jones and Smith (1990a) had published thermoluminescence (TL) dates for the Northern Territory site of Malakunanja II suggesting that it was first occupied by humans between 50 000 and 60 000 years ago. Nine TL dates in stratigraphic order span the period 200±1300 years (KTL-156) from the very top of the site to 107 000± 21 000 years (KTL-163) at a depth of 4.58 m, about 2 m below the point where artefacts cease. The vital dates towards the bottom of the archaeological deposit are 45 000±9000 years (KTL-164) from a depth of 2.30–2.36 m; 52 000±11 000 years (KTL-158) at 2.41–2.54 m; and 61 000±13 000 years (KTL-162) at 2.54–2.59 m. This latter date corresponds with the lowest artefact recovered during the 1988 re-excavation of the site (Roberts *et al.* 1990a: 154). There is a marked peak in artefact density from 2.3 to 2.5 m and thus the date of 45 000±9000 years is seen as a minimal date for human occupation; a linear least-squares regression of the nine dates suggests that 50 000 years is a conservative age for the sediments surrounding the lowest artefacts (Roberts *et al.* 1990b: 127). Note that the errors are not standard deviations in the usual sense, but rather are 'total uncertainties' (see Roberts *et al.* 1990b: 126; Roberts *et al.* in press). As Roberts *et al.* (1990b: 126) note, while these wide error margins pertain, greater confidence can be given to the central tendencies of luminescence dates if multiple, closely spaced samples disclose a pattern of steadily increasing age with depth. While true, at the same time there is a logical constraint to this general argument of stratigraphic superpositioning; it will still work if there is a constant error in the data, such as an inappropriate dose rate in the equation. By itself depth-age correlation is no demonstration of real age, but merely of consistency between samples.

This increase in the accepted antiquity for humans in Australia of between 25% and 50% not unexpectedly drew questions, comments and criticisms (Bowdler 1990; 1991; Frankel 1990; Hiscock 1990) and responses (Roberts *et al.* 1990b; 1990c). The questions followed two main lines, firstly concerning the technique and especially the large standard deviations associated with the TL dates, and secondly about the association of the human artefacts and the dated sand sediments. The possibility of artefacts moving down into older deposits, raised by both Hiscock and Bowdler, was rejected by Roberts *et al.* on the grounds that the mixing of artefacts and previously deposited sands in a 'kick zone' would re-set the TL 'clock' of these sediments. Other taphonomic processes which might transport artefacts into older sands have not been considered.

The case of Roberts *et al.* has recently been significantly strengthened by their announcement of a similar age for the basal deposits of a second Arnhem Land site, Nauwalabila I, 65–70 km south of Malakunanja II. This site contains 'securely stratified' artefacts in a rubble base below the sand deposits dated by the related but different luminescence technique, optically-stimulated luminescence (OSL) (Jones 1993: 114; Roberts *et al.* 1993; Roberts *et al.* in press). At Nauwalabila I a sequence of five OSL dates are also in stratigraphic order. The three oldest samples are 30 000±2400 years (OxODK 166) from 1.70–1.75 m depth below surface; 53 400±5400 years (OxODK 168) from 2.28–2.40 m; and 60 300±6700 years (OxODK 169) from 2.85–3.01 m. This latter date is below both the rubble layer and the lowest artefacts, while the date of 53 400±5400 years dates the sands immediately above the rubble layer.

Implications

The central issue is whether Malakunanja II and Nauwalabila I are really >15 000 years older than any other known Australian site as these dates imply. Luminescence dates measure calendrical years and for that part of the radiocarbon range for which we can calibrate radiocarbon determinations against other dating techniques, uncalibrated radiocarbon determinations mainly underestimate calendrical years. Stuiver *et al.* (1991: 10) suggest this underestimation is *c.* 2000 at 14 000 years ago. Mazaud *et al.* (1991) propose a maximum underestimation of 3000 years between 18 000 years ago and 40 000 years ago and a negligible difference between 45 000 years ago and 50 000 years ago. Bard *et al.* (1993) indicate that a determination of 18 000 radiocarbon years represents almost 22 000 calendar years. Stuiver and Reimer (1993) use this last date as the oldest in their most recent calibration program. In western NSW, Bell (1991: 48) compared four paired radiocarbon determinations and thermoluminescence dates for separate hearths each *c.* 30 000 years old, where the TL dates were between 3500 and 5100 years older than radiocarbon determinations. However, substantial comparative sequences of radiocarbon determinations and dates produced by alternative radiometric techniques for the crucial period between 20 000 and 40 000 radiocarbon years are not yet available from anywhere in the world.

A further problem with radiocarbon dating is sample contamination either in nature, by the downward percolation of humic acids or colloidal carbon after burial, or during recovery or processing after excavation. Because an organic sample has lost so much of its radioactive carbon component by the time it is 25 000–30 000 years old, contamination by even small amounts of modern carbon will cause the radiocarbon determination of an old sample to underestimate real age. Aitken (1990: 85–6) has calculated that the addition of 1% of modern carbon to a 34 000-year-old sample will result in a radiocarbon determination which is too recent by 4000 years, and after this point the magnitude of the error climbs alarmingly. An infinitely old sample contaminated in this way will yield an apparent age of only 38 000 years.

Roberts *et al.* (1990b: 126–7; 1990c: 95–6) and Jones (1993: 113) have consistently referred to this contamination problem to suggest 'that the oldest-accepted radiocarbon ages for human occupation in Australia of 35 000 to 40 000 should be regarded as only minimum ages with an unknown upper bound' (Roberts *et al.* 1990b: 126; 1990c: 95). These authors prefer a contamination explanation for the differences between Australian radiocarbon and luminescence dates to one involving the underestimation of calendrical years by the radiocarbon method. They point out that calibration as currently estimated is insufficient to convert a radiocarbon determination of 40 000 years into a calendrical date of 50 000 years (Roberts *et al.* 1990c: 95) although, as discussed, the point remains to be demonstrated in practice.

The contamination explanation raises archaeological questions. Determinations of *c.* 34 000–38 000 radiocarbon years are now known from caves, shelters and open sites in various locations from Tasmania to the north coast and offshore islands of New Guinea, and from New South Wales and Queensland to northern and southern Western Australia. These determinations represent bone, shell and charcoal samples taken from diverse environments across 40° of both latitude and longitude, processed at various laboratories in and outside Australia. If these very similar determinations arise from such a general contamination, it implies that all the Australian sites (and those elsewhere?) with radiocarbon determinations in the range 35 000–40 000 radiocarbon years are likely to be 50 000 years old or more, since as little as 0.5% of modern carbon can reduce such a real age to this radiocarbon range.

The problem is that we do not know that various levels of contamination occur so consistently. For example, the contamination proposition predicts that radiocarbon determinations plotted against depth will begin to 'flatten out' at somewhere less than 30 000 radiocarbon years. Even in undisturbed sites, deeper samples will only be older until this threshold is reached, after which deeper samples will apparently not increase in age much or at all. Yet where good sequences of radiocarbon determinations from undisturbed locations have been recovered, as at several recently excavated southwest Tasmanian sites which date to beyond 30 000 radiocarbon years, this pattern has not been encountered. It must be added, however, that sequences with sufficient antiquity and enough radiocarbon determinations to test this proposition are still too rare in Australia to generalise on this point. However, we still need to maintain the possibility that the true radiocarbon ages of some or many of these old sites are approximately as measured, without having any way of determining which are which, short of re-excavating the sites and re-dating them by alternative radiometric techniques.

Although Jones (1993: 113) suggests that radiocarbon determinations of 35 000–40 000 years are close to the method's practical limits, it is generally accepted that accelerator mass spectrometry (AMS) can measure radiocarbon age back to 50 000 years ago. However, up until now the use of the AMS technique has not produced any age determinations associated with human artefacts beyond 38 000 years in Australia or New Guinea. In contrast, John Head (Radiocarbon Laboratory, Australian National University) is producing consistent and reproducible radiocarbon determinations on non-archaeological marine shell from the Great Barrier Reef which approach *c.* 50 000 radiocarbon years, using conventional (not AMS) techniques. His technique reduces the uptake of atmospheric carbon by preparing the samples in a nitrogen-rich environment.

At Matenkupkum, a New Ireland coastal cave site which may represent a very early incursion by humans (Allen *et al.* 1989: 558), four previous basal radiocarbon determinations on the same species of marine shell clustered between 31 350±550 BP (ANU-5469) and 33 300±950 BP (ANU-5070). One of the samples, from the same square and depth as ANU-5469, yielded a determination of 32 500±800 BP (ANU-5065). A new radiocarbon determination on the same shell species, taken from exactly the same depth and in a square adjacent to ANU-5469 and ANU-5065, but prepared using the new technique, yielded a result of 35 410±430 BP (ANU-8179). Statistically this latter determination is significantly older than its two close neighbours (Head *et al.* in prep.) but only extends the younger of these by *c.* 13% and the older by *c.* 9%, using the central tendencies. There are thus good reasons for thinking that contamination has been minimised for ANU-8179, and that this site was first occupied *c.* 35 000 radiocarbon years ago. If Matenkupkum reflects the earliest period of human settlement of New Ireland and we accept the luminescence dates from Arnhem Land at face value, then Malakunanja II and Nauwalabila I were occupied perhaps 20 000 years before New Ireland. Future calibrations of the radiocarbon determinations may well reduce this gap, but at present we can only guess by how much.

Discussion

As much as I find it improbable that the inland and (at times) semi-arid Arnhem Land escarpment was settled so much earlier than the high biomass tropical coast of New

Ireland, arguing the point in our present state of knowledge reduces to matters of opinion and hypothesis rather than fact. While the radiocarbon contamination argument is technically sufficient to reconcile discrepancies between luminescence dates in the Northern Territory and radiocarbon determinations from elsewhere in Pleistocene Greater Australia, it remains intellectually unsatisfactory because it potentially discards the baby with the bath water. Some radiocarbon determinations >30 000 BP may approximate the real ages of deposits. There is a difference between allowing that sample contamination may have occurred in any sites with radiocarbon determinations of >30 000 BP and presuming that it has. So far in Australia we have not demonstrated any instance where a radiocarbon determination has underestimated the age of an archaeological deposit by anything like 10 000 years when dated by a different radiometric technique. This is not to say that this will not occur, it simply says we should not presuppose it, and especially not presuppose it is the norm. Conversely, the absence of AMS or conventional radiocarbon determinations >38 000 BP from archaeological deposits anywhere in Greater Australia remains a problem. Too great a gap presently exists between the dating sets.

Confirming the older luminescence dates by duplicating these results in other Australian sites is important and ongoing (Rhys Jones pers. comm.). Equally important is developing a better understanding of the relationship between radiocarbon determinations and luminescence dates, particularly by extending the calibrations of radiocarbon further into the Pleistocene. Obtaining paired samples of luminescence dates and radiocarbon determinations from diverse sites where reliable samples for both techniques occur, and where human occupation runs through the terminal Pleistocene from *c.* 40 000 radiocarbon years ago to 10 000 years ago, is a major priority. This may be no easy task since deposition rates for the Pleistocene levels of Australian sites are frequently very low and quite different dates may be separated by only a few centimetres of deposit. However, some of the southwest Tasmanian sites will allow for unclustered radiocarbon ages over most of this timespan to be compared with a series of luminescence dates taken down each section. Work has begun on this project and is planned to be expanded in the next southern hemisphere summer.

Archaeologically there is little basis for rejecting the Arnhem Land luminescence dates on present evidence. However, accepting them has fundamental implications not only for ideas about water crossings and the initial colonisation of Greater Australia, but also for understanding the nature of subsequent settlement, the multi-regional model of human evolution, modern human behaviour and the spread of early modern humans, prehistoric art, and the human role in the extinction of the Australian megafauna, to note but a few topics. Lacking extinct faunal successions and precise lithic technologies in Australian late Pleistocene sites, chronology has always provided the primary analytical framework. Thus, currently we are in the middle of a significant dating revolution.

Acknowledgements

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2b Beyond the radiocarbon barrier in Australian prehistory

Richard G. Roberts, Rhys Jones & M.A. Smith

The issue

Systematic application of radiocarbon dating to archaeological sites in Australia and Papua New Guinea during the 1960s revolutionised knowledge concerning the antiquity of human presence in the region, with established values being extended from mid-Holocene times (Clark 1961: 243) to *c*. 33 000 radiocarbon years ago (Jones 1973) in little over a decade. It became apparent during the next 15 years that an apparent 'ceiling' had been reached, whereby radiocarbon dates of between 35 000 years and just short of 40 000 years were obtained from a number of disparate locations across the continent.

Two 1989 papers interpreted these data differently (Allen 1989; Jones 1989). Allen (1989), taking this limit literally, argued that some of the oldest dates came from stratigraphically less secure contexts, such as river terraces and other open deposits. However, one of us (RJ) had been concerned for several years that dates of this order of magnitude were close to the theoretical limits of the method and that contamination by even a tiny amount of modern carbon could change a sample of 'infinite' age into one with an apparent age of 40 000 years or less (Jones 1982: 30).

Geomorphological examples

The same issue has been discussed in geomorphological research, for example by Thom (1973) concerning evidence for old sea-levels.

More generally, as Chappell (1991) points out, if a deposit which extends in age across the range 20 000 to 100 000 years were to be uniformly sampled, then some 70% of the results ought to be infinite in radiocarbon terms. Were only 1% modern carbon to be added, no sample would give an infinite result and 80% would appear to have an age of about 35 000–40 000 years; this Chappell (1991: 378) calls an 'event horizon'.

Deep lacustrine sequences in Australia, at Pulbeena Swamp in northwestern Tasmania (Colhoun *et al*. 1982; Colhoun 1985: 48–9), Lake Terang in western Victoria (D'Costa & Kershaw in press) and Lake Eyre in South Australia (Magee *et al*. in press)

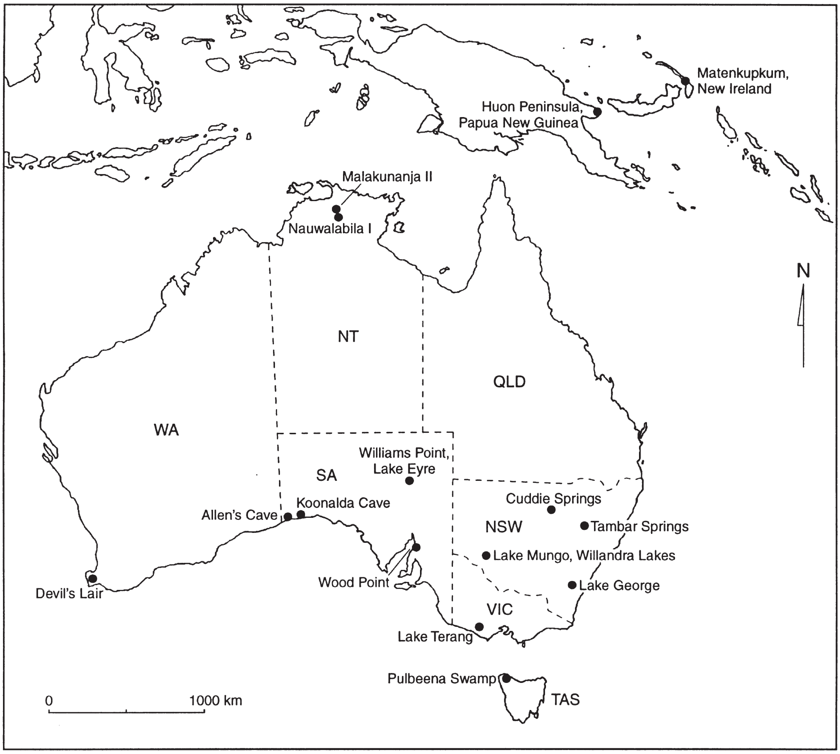


Figure 2b.1: Map of Greater Australia showing sites mentioned in the text.

(Fig. 2b.1), all show this phenomenon: ^{14}C age increases steadily with depth back to about 35 000 years BP, and this apparent finite ^{14}C age then continues into deposits at least 80 000–90 000 years old. At a level in the Lake Eyre sequence at Williams Point, for example, *Genyornis* eggshell was dated by accelerator mass spectrometry (AMS) radiocarbon, by thermal ionisation mass spectrometry (TIMS) uranium series, and by amino-acid racemisation; the surrounding sediment was dated by thermoluminescence (TL). While the three latter methods yielded ages of *c.* 50 000 years, the ^{14}C age was only *c.* 40 000 years BP. AMS radiocarbon values of *c.* 45 000 years BP were also obtained from fine-grained charcoal and pollen collected from deposits that are at least last interglacial in age (Magee *et al.* in press).

The famous pollen sequence from Lake George, on the Southern Tablelands of New South Wales, further illustrates this point. ^{14}C dating of organics yielded ages in correct stratigraphic order back to a value of *c.* 30 000 years BP at a depth of 2 m (Singh & Geissler 1985: 396). ^{14}C ages continued to fluctuate around this value to a depth of 7 m in deposits that on other grounds are believed to date to the last interglacial.

In the geological sequences discussed above, few of the ^{14}C ages are stated to be equivalent to 'background'.

Archaeological examples

Among Australian archaeological sequences, Allen (1994: 341) states he has not seen evidence that, when plotted against depth, ^{14}C ages start to 'flatten out' at slightly less than 30 000 radiocarbon years. However, the Devil's Lair site in southwestern Australia (Dortch 1979; 1984: 40–1) shows just such a pattern, with frequent stratigraphic inversion of radiocarbon ages of 30 000–38 000 years BP in deposits of possibly significantly greater age.

At the Mungo lunette site in southwestern New South Wales, stone artefacts were excavated to a depth of 1.5 m below the Mungo palaeosol which has been ^{14}C dated to *c*. 30 000 years BP. This palaeosol horizon is cemented with calcium carbonate, which reduces the possibility of any contamination from higher levels. A small sample of charcoal (ANU-1263) obtained from a stratigraphic position above the lowest artefact gave a value indistinguishable from background; the result being somewhat coyly reported as being in excess of 40 000 years (Shawcross 1975; Shawcross & Kaye 1980; Mulvaney 1975: 153). The implications of this date, which is still not fully published, were too great for the intellectual milieu of the early 1970s.

The nature of the archaeological material dated

At most Australian sites, both geomorphological and archaeological, the ^{14}C chronology is derived largely from samples composed of charcoal and finely comminuted soil organics, which are more susceptible to contamination than shell or eggshell samples.

This problem has been recognised by some for at least a decade. In the Willandra Lakes area of New South Wales, Bowler & Wasson (1984: 191) noted:

> recent evaluation of the reliability of some radiocarbon dates obtained, casts doubt on the validity of organic dates [from charcoal specks; J. Bowler pers. comm. 1994] in the time range greater than 25 000 years

and

> consistent evidence suggests that all organic samples older than 20 000 to 25 000 BP are subject to substantial and irregular patterns of contamination by younger organic complexes. This makes them appear too young. In some cases, the errors involve differences of up to 10 000 apparent

> years between dates on shell and organic samples from the same midden. In all tests of consistency, the unionid [that is, freshwater] shells provide more reliable results in this part of the time scale near the limits of radiocarbon detection.

In discussing contamination, Allen wrongly presumes that we believe that *all* Australian ^{14}C samples older than *c.* 35 000 years BP are contaminated to some degree. We do not. We believe only that ages of 35 000 years BP are either *that age or more*, and that problems of contamination are apt to compress the early part of the ^{14}C chronology for human occupation of the continent. While some such ages may be correct (after the half-life and any calibration corrections), others may be considerably in error due to contamination. Not all samples need be contaminated—in this respect, it is Allen who presupposes ubiquitous contamination. However, because the Libby half-life of 5568 years (rather than the correct half-life of 5730 years) is used to calculate conventional ^{14}C ages, all uncalibrated ^{14}C ages require a correction of *c.* 3%, irrespective of any additional adjustments for changes in the atmospheric production rate of ^{14}C. Conventional ages may differ from calendrical years by as much as a few millennia in the time period up to 40 000 years but could differ negligibly at 45 000–50 000 years (Mazaud *et al.* 1991).

Luminescence ages in relation to radiocarbon ages

During the 1981 excavation of the Lindner Site, Nauwalabila I, in Deaf Adder Gorge in the Kakadu region of the Northern Territory, one of us (RJ) faced what, at that time, seemed an insoluble dilemma. Here was a deep stratified sequence of artefacts in sands, with the quantity of charcoal decreasing exponentially with depth and no charcoal in the basal third of the deposit (Jones & Johnson 1985). On geomorphological grounds, the basal sands were believed to be of considerable antiquity. TL dating of naturally deposited sands presented a potential solution (Jones & Johnson 1985: 183) and became feasible through a geomorphological research program to TL-date sand aprons in the Kakadu region (Roberts *et al.* 1991). Charcoal persisted only a few centimetres below the surface of these natural mantles of unconsolidated sand, so the much older charcoal surviving at archaeological sites offered the best prospect of calibrating TL ages against ^{14}C ages. It is ironic, given the present calls for calibration by Allen (1994), that this has been a keystone of our research methodology from the beginning.

In our work, the best match between ^{14}C (scintillation and AMS) and luminescence (TL and optical) ages is from Allen's Cave. This rock shelter is situated in a shallow, collapsed doline on the Nullarbor Plain, a karst limestone area in South Australia, and receives wind-blown sands from the Great Victoria Desert. In the cave deposit, a hearth with an antiquity of *c.* 10 000 years yielded calibrated radiocarbon ages in excellent agreement with the luminescence ages for the unheated sediments immediately beneath (Roberts *et al.*, in press b). The integrity of this feature precludes any possibility of post-depositional charcoal or sediment translocation. We intend to submit full details of this inter-comparison for publication in *Antiquity*.

Discussion

New Ireland and New Guinean sites

Allen's ^{14}C ages from New Ireland were obtained from marine shell. While they are the oldest yet reported from that island, they are not the oldest in that tropical region. TL and uranium series ages for archaeological deposits on the Huon Peninsula in north-eastern Papua New Guinea indicate an age of 40 000–60 000 years for artefact-bearing deposits (Groube *et al.* 1986). More recent TIMS uranium series determinations for the coral-reef terrace supporting these deposits indicate an age of 52 000–61 000 years (Chappell *et al.* 1994). One of us (RGR) has recently collected further sediment samples from this site for optical dating, to reduce the uncertainties associated with the earlier TL determinations. At the present state of knowledge, the age of the Huon site is consistent with the ages for first occupation of the two northern Australian sites. Allen and his team apparently have not applied luminescence dating techniques to the sands underlying the midden at the New Ireland site, nor did they excavate to the base of the sands. Had they done so, we would be in a better position to know the time-period during which the site was *not* occupied by people, as we have done at Malakunanja II. We do not presuppose that the oldest sites in New Ireland have necessarily been located by archaeologists.

Tasmanian and South Australian sites

Luminescence ages generally have error margins that are too large to 'fine-tune' the ^{14}C time-scale. However, constraints on the magnitude of radiocarbon calibration can be made by selecting samples from contexts in which luminescence dating is considered to yield reliable ages and radiocarbon contamination can be discounted. Because luminescence dating of unheated sediments relies on the dating signal being zeroed by sunlight prior to sediment deposition, limestone cave deposits such as those in southwest Tasmania proposed by Allen are not ideal for age inter-comparisons. The true age of the deposit (that is, the elapsed time since the deposit was last reworked) will be overestimated if the luminescence 'clock' is not reset completely prior to sample burial. Optical dating has been shown to overestimate the age of cave deposits in deep karst systems where, en route to the cave floor, sediment is stored for long periods and remobilised intermittently in the darkness of the cave (e.g. Koonalda Cave in South Australia; Roberts *et al.* in press b). TL dating is apt to fare worse. In addition to concerns about radiocarbon calibration and contamination, incomplete zeroing of the luminescence dating signal is a potential contributor to discrepancies between ^{14}C and luminescence ages.

Northern Territory sites

For Malakunanja II and Nauwalabila I, Allen is concerned about possible systematic luminescence age overestimation and post-depositional movement of artefacts. Our confidence in the luminescence chronology at both sites is based not only on the sequence of multiple, closely-spaced samples in correct stratigraphic order but also on the luminescence chronology being pinned at three points in both profiles. Near-modern TL and optical ages were obtained close to the ground surface; at two deeper sampling locations, luminescence ages accorded closely with the calibrated ^{14}C ages for associated charcoal pieces (Roberts *et al.* 1990a; 1990b; in press a).

Allen appears not to have understood the purpose or significance of the regression between TL age and depth at Malakunanja II (Roberts *et al.* 1990b). He comments that, 'by itself, depth–age correlation is no demonstration of real age, but merely of consistency between samples'. Our TL ages are reported in calendrical years, and the total uncertainties incorporate all known sources of random and systematic error, including those associated with the laboratory and environmental dose rates. The regression, based on these TL ages, indicates not only stratigraphic consistency between samples but also the general relation between the depth of the deposit and its age (in calendar years) at any chosen level.

Allen errs in claiming that we have not considered the role of taphonomic processes in artefact displacement, other than the mixing of sediments and artefacts in the 'kick zone'. In rejecting the possibility of *major* downward displacement of artefacts at Malakunanja II (Roberts *et al.* 1990a; 1990b), we noted no indications of such movement —neither a decline in artefact concentration nor sorting by artefact size or density. The greatest concentration of artefacts was between 2.3 m and 2.5 m depth, and there was no apparent sorting by size or density over this range. Furthermore, sample KTL164 directly overlies a small pit filled with rubble, stone artefacts and haematite. It is highly improbable that such a feature could have been created by post-depositional displacement. This feature gives us confidence that the TL age for KTL164 of 45 000±9000 years is a reliable minimum date for human occupation of Malakunanja II.

To cross-check our results at Malakunanja II, we investigated the Nauwalabila I site (Roberts *et al.* 1993; in press a). There, the earliest artefacts (described by Jones & Johnson 1985), bracketed by optical dates of 53 400±5400 and 60 300±6700 years, were recovered from a layer of sand and interlocked rubble into which the post-depositional movement of artefacts from younger levels can be discounted.

We are therefore confident of the stratigraphic integrity and chronological coherence at the Malakunanja II and Nauwalabila I sites. The individual TL age determinations and the corresponding regression analysis for the Malakunanja II samples, together with the optical ages obtained for the Nauwalabila I samples, strongly supports our view that initial human colonisation of the northern part of Australia took place between 53 000 and 60 000 years ago. We favour a date of *c.* 60 000 years for first landfall (Roberts *et al.* 1993; in press a).

In conclusion

As Jones (1989; 1993) has stated before, and as Allen (1994) reiterates, we are in the midst of a significant dating revolution. With few extinct faunal successions, precise lithic technologies and geomorphic benchmarks to guide us at Australian sites, this process includes a diversification of dating methods used in Australian archaeology. We do not advocate reliance on any single dating method, be it radiocarbon or luminescence. Pleistocene chronologies should be constructed using the widest range of appropriate dating techniques—such as radiocarbon, luminescence, electron-spin resonance, uranium series and amino-acid racemisation. We have adopted this approach in our previous investigations and have a program in place to continue such chronometric comparisons to elucidate the date of initial occupation, and the relationship with megafaunal extinctions, at sites elsewhere around the continent, such as at Devil's Lair in Western Australia, Wood Point in South Australia, Cuddie Springs and Tambar Springs in New South Wales (Dodson *et al.* 1993; Furby *et al.* 1993; R. Wright pers. comm. 1993), and sites in far north Queensland and northwest Western Australia where conventional radiocarbon ages exceed 37 000 years BP (David 1993; S. O'Connor pers. comm. 1994). The phenomenon of the radiocarbon barrier, discerned in the Australian record, is likely to be a general problem and warrants close attention by scholars working in other parts of the world.

2c The contamination of Pleistocene radiocarbon determinations in Australia

Jim Allen & Simon Holdaway

The radiocarbon 'barrier'

Roberts *et al.* (1994), in responding to a paper by one of us (Allen 1994), misrepresent the position taken there and the actual data on a number of occasions. At the same time they add nothing substantive to the main issue raised, that the present gap of *c.* 15 000 years between the dates for earliest human presence in Australia using luminescence and ^{14}C techniques requires better explanation. On this point Roberts *et al.* (1994: 611, 615) reach the same intellectually unsatisfactory explanation as in their previous papers: 'the phenomenon of the radiocarbon barrier' is that determinations older than *c.* 35 000 BP 'are close to the theoretical limits of the method and that contamination by even a tiny amount of modern carbon *could* change a sample of "infinite" age into one with an apparent age of 40 000 years or less' (our emphasis. See also Jones 1982: 30; 1989: 762; 1993: 113; Roberts *et al.* 1990a: 126–7; 1990b: 95–6).

If the age limits in archaeological radiocarbon determinations for Australia arise from a technical contamination 'barrier' then it should also be seen in radiocarbon determinations from natural, non-cultural contexts (hereafter 'geological' contexts). If it is a real phenomenon, corresponding generally to the human settlement of the continent, then we should see different patterns in the determinations from each of the different contexts. We thus assembled ^{14}C determinations from many Australian and New Guinean localities (the two countries being parts of the same Pleistocene landmass). These were divided into two groups, according to whether they derived from undisputed cultural contexts ('archaeological' contexts) or geological contexts.

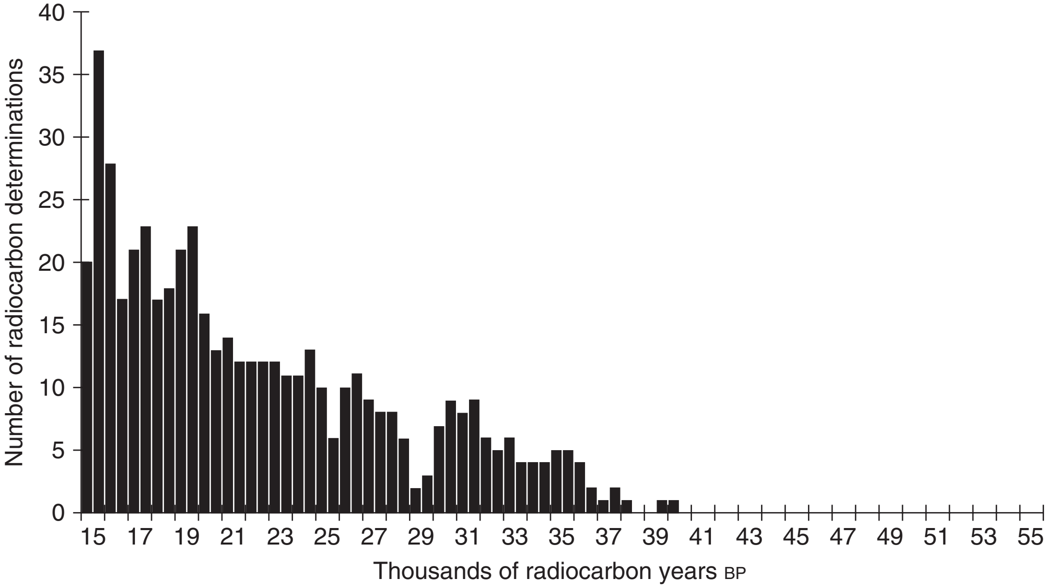
We began by constructing lists of geological and archaeological uncalibrated determinations which exceeded 14 999 radiocarbon years in their mean ages, listed in the Appendix below. Both sets were assembled from either published sources and/or our own research data, or data offered to us by colleagues. We initially included all such determinations without reference to their locations, the types of material used for dating, whether the determinations were finite or infinite (i.e. older than a nominated age), whether or not determinations were considered by their publishers to be anomalous, and without particular reference to the year or place of their publication.

Subsequently, we chose to exclude determinations for which we had no laboratory number (to avoid duplication, and also infinite determinations). This left a set of 217 geological and 254 archaeological determinations. The archaeological determinations used here derive from a search of *Archaeology in Oceania*, *Australian Archaeology*, *Queensland Archaeological Review*, *Antiquity*, *Nature*, books by Flood (1989) and White with O'Connell (1982), and the Terra Australis series and the Occasional Papers in Prehistory series published by the Department of Prehistory, Australian National University. These were augmented by all other published determinations over 14 999 BP known to us, regardless of publisher. Our own unpublished determinations from Tasmania were included, as were unpublished dates given to us by Richard Cosgrove and Peter Veth, whom we thank. Geological determinations were derived from *Quaternary Research*, *Search*, *Australian Journal of Earth Sciences*, *Nature*, *Australian Journal of Science*, *Australian Journal of Botany*, *Palaeogeography*, *Palaeoclimatology*, *Palaeoecology*, Colhoun (1985) and determinations kindly provided to us by Peter Kershaw, Geography and Environmental Science, Monash University. Other published determinations were provided by Nick Porch. Many laboratories are necessarily represented in both sets, and we included in our database the mean, standard deviation, laboratory number and type of dating material. This allowed various combinations of determinations to be examined.

We note that an alternative approach to the one adopted here would have been to select only those determinations that would have passed the criteria proposed in a number of recent chronometric hygiene studies (Anderson 1991; Spriggs & Anderson 1993; Schiffer 1986). We did not follow this line because such studies are always open to the accusation that the particular criteria selected bias the result in one or other direction. Instead, we have accepted all finite determinations on the assumption that large sample numbers will overwhelm individual contamination problems and reveal a relatively robust pattern.

The data-collecting procedure biased the samples against the eventual result by deliberately including all published archaeological determinations >30 000 radiocarbon years known to us, but merely taking all geological determinations >14 999 radiocarbon years, as encountered in the literature search. We are aware that the archaeological set is much more complete than the geological set and that a more extensive search would greatly enlarge the latter set, including that component of it which has determinations >40 000 radiocarbon years. We excluded other unpublished determinations, including the archaeological determination ANU-1263 referred to by Roberts *et al.* (1994: 612) as >40 000 BP 'and not fully published' because of its infinite status (see below). This procedure also excluded some tens of unpublished geological determinations >40 000 BP from Australia and New Guinea (John Head, Radiocarbon Dating Laboratory, Australian National University, pers. comm.). However, we consider the geological set to be sufficient to substantiate our argument.

Figure 2c.1 displays these sets using a method proposed by Rick (1987) which involves calculating a moving sum of the means of the determinations. The total



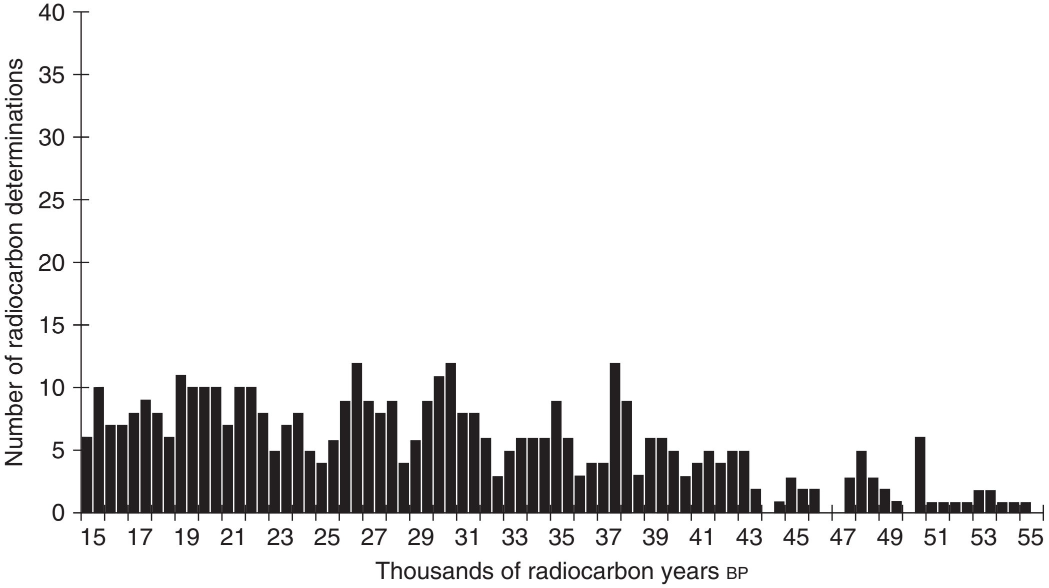


Figure 2c.1: Moving sums of archaeological (top) and geological (bottom) radiocarbon determinations greater than 14 999 radiocarbon years from Australia and New Guinea. The different *x*-axis intercepts and slopes of the graphs suggest that sample contamination alone cannot account for the lack of archaeological determinations older than 40 000 BP

number of determinations that fall within a 1000-year period centred on a multiple of 500 years is plotted on the graph. The centre point is then moved to the next 500-year multiple and a new value is plotted for the 1000-year spread centred on this value. Modern database programs allow automation of the counting process. Rick (1987: 61) argues that the method preserves general trends in the raw data but smooths out short-term erratic variations. It is therefore appropriate and useful for comparing patterns in the distributions of large numbers of ^{14}C determinations.

As stated, Fig. 2c.1 excludes all infinite dates from both sets because the tendency for laboratories to choose particular cut-off points for infinite determinations during the last 30 years results in such determinations forming artificial peaks. This exclusion reduced our original geological sample by 56 and the archaeological sample by two, a point to which we will return.

The two plots differ in two major respects. Firstly, no archaeological determination is older than 40 000 BP, while a large number of geological determinations extend to 50 000 BP and beyond. The barrier proposed by Roberts *et al.*, apparently present in the archaeological samples, does not occur in the geological sample. Secondly, the shapes of the two plots clearly differ. While the plot for geological determinations is essentially flat, but shows fewer determinations earlier than 43 000 BP, the plot for archaeological determinations shows a steady increase from 38 000 BP to 15 000 BP, with a single major fluctuation between 30 000 and 28 500 radiocarbon years ago.

Discussion

The large differences between these distributions suggest that it is highly unlikely that the more limited age range of the radiocarbon determinations is an artefact of the method. For the archaeological sites represented by these ^{14}C determinations, which include all the oldest sites dated by radiocarbon in Australia and New Guinea, the best general explanation is that archaeological material was not deposited in them much before 40 000 uncalibrated radiocarbon years ago. At the same time, finite radiocarbon ages for natural deposits can be measured by the radiocarbon technique considerably beyond this point. We recognise and emphasise that such finite ages may seriously underestimate the real ages of such deposits and that contamination may be a very real problem, but this does not undermine our point: the technique can and does produce finite determinations well beyond the barrier proposed by Roberts *et al.*

As well, the view that the oldest archaeological samples being measured are younger as a group than the older geological samples as a group is supported by the fact that our original data set contained 56 geological samples of infinite age but only two infinite archaeological determinations. While there are probably more, we are aware of only two other infinite archaeological samples in Australia, the Mungo determination referred to by Roberts *et al.* mentioned earlier and one of our Tasmanian determinations which we excluded because we have a finite age for another part of the same sample.

Alternative explanations for the pattern require differential contamination of samples from archaeological and geological contexts. There are two broad types of contamination. Samples may be contaminated by the physical mixing of deposits so that material from higher in a stratigraphic succession is mixed with lower material. Alternatively, contamination may occur where samples are modified pre- and/or post-depositionally or at the times of collection or assay by processes that do not involve physical mixing.

While physical mixing is certainly a problem in natural sites (see Colhoun 1986), a case might be made that it is more likely to occur in archaeological contexts where human activity is an extra agency that moves material—including charcoal—through processes like scuffage. Thus, it might be argued that the difference between the plots of archaeological and geological determinations reflects differences in the ways human and natural sites are formed. However, except in the most severely disturbed or shallow sites, processes like scuffage are unlikely to lead to samples >30 000 BP being contaminated with modern carbon. A more likely scenario is that the material so added to samples will be older or younger by only a few hundred or a few thousand years, as possibly is the case in Devil's Lair (see below). In such cases the effect is much less severe than with a modern carbon contaminant. If the contamination is of the order of 1–2% and the age difference between the sample and the contaminant is no more than 5000 years, the errors introduced will not be more than a few hundred years (Taylor 1987: 118–19). Even if the proportion of contaminant is higher than 1–2%, it is hard to see how differences in the degree of mechanical mixing could account for the plots in Figure 2c.1. We pass over the added consideration that archaeological deposits of any age, not just older deposits, may be affected in this way.

Pre- or post-depositional contamination or contamination introduced during treatment of the sample is harder to control for and is a problem with radiocarbon determinations that we readily acknowledge. However, it is again difficult to see how, on average, such forms of contamination could affect archaeological and geological samples so much and so differently as to account for the patterns apparent in Figure 2c.1. To substantiate the claim that the archaeological radiocarbon distribution is a product of a contamination barrier it would be necessary to argue that Australian and New Guinean archaeological samples as a group are contaminated by a higher percentage of modern carbon than geological samples in the same vast region. It is difficult to justify such an argument across the large number of radiocarbon determinations presented here.

As a final explanation for the differences in the plots from Figure 2c.1, it might be argued that the differences in the materials submitted for radiocarbon assay are affecting the results. It is true that archaeological determinations have been predominantly obtained from charcoal samples, whereas a wider variety of materials have been used for geological determinations. In our sample, 71% of the archaeological determinations were obtained from charcoal, whereas wood, mud, shell and peat accounted for 56% of the finite geological determinations, and charcoal only 10%. There seems little evidence, however, that charcoal determinations are uniformly more recent than those from other raw materials, although no obvious and conclusive way of testing this suggests itself to us. The means for determinations on charcoal versus other major dating materials for both archaeological and geological sites in our data set are very similar to each other. Only for Tasmanian geological dates older than 30 000 radiocarbon years is the mean for charcoal determinations substantially younger than that for other materials (34 830±4438 for charcoal versus 41 510±7012 for other materials). While this might represent differential contamination of materials, we feel it is more likely to represent the tendency in Tasmania for pre-30 000 BP deposits to be represented by swamps. Wood, peat and mud are selected for dating in these sites simply because they are the most abundant materials, and only nine determinations are on charcoal. In sum, we can find no evidence that selection of material accounts for the pattern apparent in Figure 2c.1.

The differences in the shapes of the plots in Figure 2c.1 apparently reflect the wide range of Quaternary interests that have led geologists to obtain radiocarbon determinations which create a flat distribution. In contrast, the steady increase in the number of archaeological determinations represents an increasing record of human habitation in the Pleistocene continent, possibly reflecting population increase and/or the better survival of younger sites.

Lastly, on an associated point, proponents of the 'long' chronology in Australia argue that a number of Pleistocene sites have cultural deposits at depths where organic remains for radiocarbon dating have disappeared, and thus that the radiocarbon record is an artefact of preservation. Without minimising this problem in archaeological sites, we would observe that the oldest radiocarbon determinations presented here are at or close to the levels of earliest cultural deposition in their respective sites, and that those sites with significant basal cultural deposits lacking datable organics rarely have radiocarbon determinations from higher in their sequences which imply significantly greater basal antiquity than that indicated by the current radiocarbon chronology for Greater Australia.

Corrections

It is necessary in the context of this paper (and the wider debate) to correct some of the assertions of Roberts *et al.* (1994). Of less importance are the personal misrepresentations; readers wishing to compare statements and positions attributed by Roberts *et al.* with what was actually written by Allen (1994) will quickly arrive at their own conclusions.

1 Matenkupkum

Contrary to the statement by Roberts *et al.* (1994: 614), the New Ireland site of Matenkupkum was excavated by Chris Gosden, in 1985 as an independent member of the Lapita Homeland Project and in 1988 as an independent researcher (Allen was present during the second season, but worked at the nearby site of Matenbek, not at Matenkupkum). A detailed account of the Matenkupkum 1985 season has appeared (Gosden & Robertson 1991), and a subsequent report on the 1988 season is in preparation (Gosden pers. comm.). The assertion by Roberts *et al.* that sands underlying cultural deposits in Matenkupkum were not excavated cannot be deduced from the published account and is incorrect. Two squares in the 1985 10-m trench were taken down to concreted beach rock. At the cave entrance there is *c.* 1 m of sterile beach sand; at the inner end of the 1985 trench there are 1–2 cm of beach sand. A third square excavated in 1988 at the rear of the cave was taken down to an unbroken limestone floor. A detailed geomorphological study of the site (Manning 1990) indicated the beach sands were deposited when Matenkupkum, currently 15 m above sea-level in an uplifted coral terrace, was at the level of the sea. By extrapolation with nearby dated terraces at similar heights, John Webb, Department of Geology, La Trobe University, suggests that Matenkupkum was last at sea-level *c.* 190 000 years ago (Manning 1990: 107).

2 Luminescent dating in Tasmania

Roberts *et al.* (1994: 614) feel able to conclude that Tasmanian sites where we have begun to compare luminescence dates with good radiocarbon sequences which go back beyond 30 000 radiocarbon years are inappropriate to the task, despite the fact that they do not know which sites are being sampled, nor, with the exception of Jones' brief visit to Bone Cave in 1988, have any of them seen any of the sites involved. Few of the identified sites in southwest Tasmania are in deep karst systems, so that the comment about zeroing the luminescence clock of sediments in such systems is a red herring in this context, and better applied, perhaps, to sites like Allen's Cave on the Nullarbor and Devil's Lair in southwest Western Australia. The sites we are investigating were chosen because of their good exposure to sunlight. As well, dating burnt artefacts and hearths by luminescence techniques may be a better alternative to dating sediments in these sites, as it may obviate the widespread problem of assuming that sediments carried no residual luminescence at the time of deposition, whatever the site. In any case, as it happens, two of the four Tasmanian sites to be tested are not limestone caves or shelters at all.

3 Data related to the contamination question

a Pulbeena

It is claimed (Roberts *et al.* 1994: 611) that the Pulbeena swamp sequence shows ^{14}C age increasing steadily with depth to *c.* 35 000 BP and samples maintaining this apparent finite age into deposits at least 80 000–90 000 years old. The reference cited (Colhoun

1985: 48–9) lists 22 ^{14}C determinations for Pulbeena, one of which is infinite at >55 000 BP. Of the other 21, six are younger than *c.* 28 000 BP, none fall in the period 30 000–40 000 BP, 12 give finite ages between 40 000 BP and 50 000 BP, and three finite determinations range between 50 000 BP and 55 000 BP. We note that inversions do occur and that all determinations may underestimate real age. Here, however, the technique appears to have no 'barrier' at 40 000 radiocarbon years BP.

b Lake George

It is correctly noted that organic ^{14}C determinations from Lake George continue in stratigraphic order to *c.* 30 000 BP at *c.* 2 m depth and then fluctuate around this date for the next 5 m of deposit (Roberts *et al.* 1994: 611). However, Roberts *et al.* fail to report that three inorganic (carbonate) dates from *c.* 2.25 m, *c.* 4.40 m and *c.* 8.54 m gave infinite (>37 800 BP) results. After a ^{14}C determination from 20–30 cm depth gave a figure of 975±100 BP, a second determination was run on the NaOH insoluble fraction of a specially treated 10-kg sample from the same depth. It produced a determination of 3430±80 BP, and a second similarly treated sample from 40–43 cm gave 5460±170 BP. Thus all organic samples from this site are probably contaminated, not merely the older ones. As Singh and Geissler (1985: 397) note: 'it was quite clear that while organic analyses gave spurious dates a tentative chronology could be established on the basis of results from the inorganic radiocarbon dates'.

c Devil's Lair

Roberts *et al.* (1994: 612) note as evidence that archaeological radiocarbon ages 'flatten out' at *c.* 30 000 BP, that there is 'frequent stratigraphic inversion of radiocarbon ages of 30 000–38 000 years BP in deposits of possibly significant greater age' at Devil's Lair. Dortch (1984: 41) lists 27 ^{14}C determinations from the site divided into those with alkali pre-treatment and those without. Both groups contain inversions. If we follow Roberts *et al.* and treat the determinations as one group, then one determination, 31 400±1500 BP (SUA-457), appears too old. Stratified below this sample are eight ^{14}C determinations of which two are out of stratigraphic order, both in the same layer of the site. More telling, there are more frequent inversions in the metre of deposit between 121 cm depth and 222 cm depth where dates vary between 13 975±450 BP (GX-7249) and 21 820±480 BP (SUA-977). Simple site disturbance throughout the sequence seems a more parsimonious explanation than modern carbon contamination of the older ^{14}C determinations in this site.

d Malakunanja II and Nauwalabila I

On the relationship of ^{14}C and luminescence dates, Roberts *et al.* (1994: 614) note that for both Malakunanja II and Nauwalabila I at two sub-surface locations 'luminescence ages accorded closely with the calibrated ^{14}C ages for associated charcoal pieces'. For Malakunanja II questions of just how closely these accorded were raised by Hiscock

(1990) and Bowdler (1991) and partly answered by Roberts *et al.* (1990a). For Nauwalabila I the same question can be asked. Luminescence sample $Ox_{OD}K166$, from Pit L29, spits 51–2 (depth 170–175 cm) gave a date of 30 000±2400 BP. A charcoal sample, ANU-3182, from L28–51 plus L29–52 (depth 174–179 cm) gave an uncalibrated determination of 12 000±600 BP. This was rejected by Jones and Johnson (1985: 181–2) as being the result of a contaminated small sample. Instead, they accepted earlier ^{14}C determinations from the site (Kamminga & Allen 1973: 95) of 13 195±175 BP (SUA-236) from 125–130 cm and 19 975±365 BP (SUA-237) from 170–190 cm. This was because (a) 80% of this latter sample derived from 170–175 cm depth (and might thus be considered approximately the same depth sample as the rejected sample ANU-3182), (b) because the determinations were in stratigraphic order *vis-à-vis* depth, and (c) because they made sense in terms of sand accumulation rates predicted by the younger ^{14}C determinations in the site (Jones & Johnson 1985: 181). Even if SUA-237 provides an equivalent depth sample to $Ox_{OD}K166$, when calibrations for determinations as old as 20 000 radiocarbon years are published, it is improbable that 19 975±365 BP will calibrate beyond *c.* 24 000 BP. SUA-237 thus provides no close ^{14}C corroboration for the luminescence date of 30 000±2400 BP.

Conclusion

The most parsimonious explanation for the differences between the plots for geological and archaeological radiocarbon determinations is that the oldest archaeological sites represented by these determinations were not occupied by humans before *c.* 40 000 uncalibrated radiocarbon years ago. As previously observed (Allen 1994), we presently do not know precisely what this means in calendar years as it is well beyond the current range of calibration. However, luminescence dating of two northern Australian sites by Roberts *et al.* (1994) suggest they were occupied *c.* 55 000 years ago, and like Roberts *et al.* we do not believe that 40 000 radiocarbon years equal 55 000 calendar years. Again as previously observed, we have no reason to doubt the luminescence dates on archaeological grounds, other than that they are discrepant with the radiocarbon determinations discussed here. Roberts *et al.* are content that the discrepancy is to be explained by a technical barrier to older radiocarbon determinations, whereby contamination problems compel us to consider that determinations >*c.* 30 000 radiocarbon years can either be the given age or older. They offer no method for determining which are which. However, Figure 2c.1 now suggests that while contamination of older samples remains a problem, it is a poor explanation for why no Greater Australian archaeological sites have yet produced a set of acceptable radiocarbon determinations older than *c.* 40 000 BP. Various scenarios might reconcile this discrepancy without discrediting either dating technique: much earlier occupation in northern Australia than elsewhere, which *c.* 40 000 years ago triggered a rapid and massive colonisation of the rest of the continent, or two separate migrations of which the second was eminently more successful, are two such models. However, none have sufficient data yet to address them.

Finally we wish to stress again, in the face of doubts raised by an anonymous reviewer, that we do not doubt that questions concerning laboratories, sample materials, the dates when samples were run, questions of enrichment processes etc., might be raised to disqualify particular determinations on our lists. As we have said, we do not doubt that some determinations may be wildly discrepant with their actual calendar ages. The strength of our approach as we perceive it, is that by using hundreds of determinations individual discrepant results will be minimised in the overall trends. As stated, our aim is to question the contamination explanation of Roberts *et al.*, not to defend the accuracy of radiocarbon determinations >30 000 BP, *per se*. Added to this, as also stated, the characteristics listed above, materials, laboratories etc., apply to both sets of samples indiscriminately. We believe that different tests, using a single dating medium, or the output of a single laboratory, or determinations done only in the last decade—or indeed tests combining these and/or other parameters—would likely duplicate the present results. At the time of writing we are pursuing such further analyses in conjunction with the Australian National University Radiocarbon Dating Laboratory.

Appendix

Laboratory number, mean, standard deviation, and material for archaeological and geological radiocarbon determinations used to construct Figure 2c.1. Site names have been entered for determinations where no laboratory number is available.

Lab. number	*date/s.d.*	*material*	*lab. number*	*date/s.d.*	*material*
archaeological determinations					
Anu-2160	15 000±160	carbonate	Anu-3984	15 490±360	charcoal
Beta-54739	15 040±280	charcoal	Anu-3002	15 550±180	freshwater shell
Anu-880a	15 120±235	freshwater shell	Beta-45813	15 560±200	charcoal
Riddl-531	15 140±160	charcoal	Anu-948b	15 560±240	freshwater shell
Beta-46305	15 160±210	charcoal	Beta-45805	15 570±180	charcoal
Beta-30952	15 200±200	charcoal	Beta-45807	15 620±190	charcoal
Sua-1805	15 210±160	freshwater shell	Anu-2783	15 670±530	charcoal
Anu-5763	15 220±560	bone	Anu-461	15 690±235	freshwater shell
(Cucka-doo 1)	15 270±210	unknown	Beta-45814	15 720±180	charcoal
Beta-37553	15 290±190	charcoal	Beta-45815	15 730±170	charcoal
Beta-26511	15 290±140	charcoal	Nza-160	15 780±430	insolubles
Anu-3983	15 390±380	charcoal	Anu-2509	15 820±190	freshwater shell
Anu-1613	15 400±330	charcoal	Beta-37545	15 840±170	charcoal
Beta-37546	15 440±260	charcoal	Anu-70	15 850±320	charcoal
Beta-37552	15 450±220	charcoal	Beta-40347	15 870±270	charcoal
Anu-880b	15 450±240	freshwater shell	Beta-16654	15 930±240	charcoal
Anu-1567	15 450±1500	charcoal	Beta-42993	15 960±310	charcoal
Anu-2313	15 450±240	freshwater shell	Beta-44076	15 980±140	charcoal
Anu-373b	15 480±210	freshwater shell	Beta-46306	16 010±300	charcoal

Lab. number	date/s.d.	material	lab. number	date/s.d.	material
archaeological determinations (continued)					
Anu-1221	16 100±100	charcoal	Beta-26961	18 290±290	charcoal
Anu-2755	16 100±180	freshwater shell	Sua-456	18 400±540	charcoal
Sua-964	16 120±200	unknown	Beta-27077	18 480±200	charcoal
Beta-27075	16 120±180	charcoal	Sua-687	18 500±1700	unknown
Anu-2508	16 170±260	freshwater shell	Beta-52810	18 610±360	charcoal
Beta-54736	16 200±120	charcoal	Beta-4323	18 680±180	land snail
Beta-27054	16 200±590	charcoal	Anu-7105	18 730±600	marine shell
Wk-1367	16 300±90	bulk soil	(Kenniff)	18 800±480	unknown
Anu-2162	16 350±430	carbonate	Gak-335	18 800±800	charcoal
Beta-44078	16 420±90	charcoal	Anu-2870	18 850±370	charcoal
Anu-2580	16 500±200	calcite flowstone	Anu-1042	18 860+2160–1700	charcoal
Beta-45812	16 500±190	charcoal	Anu-5-10	18 900±1800	marine shell
Beta-74543	16 670±70	charcoal	Sua-101	19 000±250	charcoal
Beta-26512	16 820±110	charcoal	Anu-618a	19 030±1410	bone
Nza-231	16 940±635	insolubles	Beta-27080	19 080±280	charcoal
Sua-1315	16 970±620	charcoal	Anu-3331	19 100±unknown	charcoal
Wk-2934	17 010±260	charcoal	Sua-976	19 160±380	charcoal
Anu-536	17 010±650	charcoal	Sua-33	19 250±900	charcoal
Anu-2782	17 020±310	charcoal	Beta-44374	19 270±230	charcoal
Beta-45808	17 030±430	charcoal	Sua-915	19 300±500	organics
Anu-2502	17 050±340	freshwater shell	V-96	19 300±720	charcoal
Anu-2520	17 050±1470	charcoal & wood	Anu-71	19 300±350	charcoal
Wk-2933	17 100±250	charcoal	Anu-148	19 400±450	charcoal
Anu-3562	17 100±1350	charcoal	Beta-26958	19 460±210	charcoal
Gx-7253	17 100±810	charcoal	Beta-4325	19 520±170	land snail
Beta-62283	17 110±150	charcoal	Anu-1774	19 520±300	charcoal
Anu-2331	17 290±470	freshwater shell	Gak-628	19 600±550	charcoal
Anu-2503	17 300±190	freshwater shell	Beta-26959	19 670±340	charcoal
Sua-1248	17 370±290	charcoal	Wk-2934	19 670±470	charcoal
R-11879/1	17 410±330	marine shell	Sua-1249	19 700±400	charcoal
Anu-6540	17 460±840	charcoal	Anu-7106	19 750±600	emu eggshell
Beta-44075	17 530±430	charcoal	Anu-2785	19 770±850	charcoal
R-11769/4	17 540±220	charcoal	Ti1-11018	19 800±390	diprotodon teeth
Sua-975	17 560±460	charcoal	V-92	19 900±2000	charcoal
Beta-42994	17 570±220	charcoal	Sua-237	19 975±265	charcoal
Beta-25620	17 660±250	charcoal	Sua-2614	20 040±440	marine shell
Anu-1044	17 720±840	charcoal	Beta-26149	20 140±300	marine shell
Anu-3139	17 800±unknown	charcoal	(Allen's Cave)	20 200±1000	unknown
Beta-42066	17 880±135	charcoal	Sua-32	20 400±1000	charcoal
Anu-19	18 000±400	charcoal	Sua-1041	20 440±360	unknown
Beta-44079	18 060±170	charcoal	Anu-3332	20 500±unknown	charcoal
Beta-26508	18 090±510	charcoal	Sua-2341	20 560±250	charcoal
Beta-37557	18 110±210	charcoal	Gak-7081	20 650±1790	charcoal
Anu-456	18 150±340	charcoal	Sua-1041	20 740±345	charcoal
(Lake Victoria)	18 200±200	unknown	Anu-137	20 760±800	charcoal

Lab. number	date/s.d.	material	lab. number	date/s.d.	material
archaeological determinations (continued)					
Anu-138	20 830±810	charcoal	Wk-1098	24 600±1400	charcoal
Anu-1612	20 850±290	charcoal	Anu-3001	24 650±600	freshwater shell
Beta-54740	20 880±390	charcoal	Gx-7251	24 685±1150	charcoal
Beta-16886	21 000±220	charcoal	Anu-618b	24 700±1270	bone
Anu-3510	21 200±290	charcoal & wood	Nza-246	24 745±2400	humic acid
Gx-7252	21 270±620	charcoal	Anu-77a	24 800±1600	charcoal
Anu-5953	21 280±280	marine shell	Nza-230	25 120±1380	humic acid
Beta-27076	21 410±240	charcoal	Sua-2354	25 200±250	marine shell
Anu-51	21 450±380	charcoal	Anu-7107	25 230±480	emu eggshell
Anu-7458	21 700±550	emu eggshell	Beta-44084	25 510±450	charcoal
Sua-977	21 820±480	charcoal	Anu-2314	25 800±500	freshwater shell
Anu-245	21 900±540	charcoal	Anu-2754	25 920±560	freshwater shell
Beta 51435	21 940±620	charcoal	Sua-685	26 000±500	charcoal
Beta 19901	21 950±270	charcoal	Anu-2578	26 140±570	calcite flowstone
Beta 26960	21 980±310	charcoal	Beta-29989	26 190±180	charcoal
Anu-2307	22 050±440	freshwater shell	Anu-375b	26 250±1120	charcoal
Wk-1575	22 100±500	marine shell	Sua-1510	26 300±500	unknown
Beta-62284	22 240±720	charcoal	Lj-204	26 300±1500	charcoal & wood
(Kings Table)	22 300±1190	unknown	Anu-190	26 450±880	charcoal
Beta-26962	22 370±470	charcoal	(Koolan Island 2)	26 500±1050	marine shell
Beta-18884	22 440±1370	charcoal	Beta-42065	26 710±210	charcoal
Beta-42995	22 650±200	charcoal	Beta-42997	26 830±1240	charcoal
Gak-629	22 700±700	charcoal	Anu-3000	26 900±590	freshwater shell
Beta-40349	22 730±180	charcoal	Beta-46872	27 160±250	charcoal
Anu-1498	22 750±420	charcoal	Anu-372b	27 160±900	freshwater shell
Anu-77b	22 900±1000	charcoal	Beta-62285	27 250±530	charcoal
Beta-40348	22 980±560	charcoal	Wk-1365	27 300±1100	marine shell
Beta-29986	23 130±460	charcoal	Beta-30953	27 370±1690	charcoal
Beta-48329	23 220±190	charcoal	Sua-539	27 700±700	charcoal
Beta-37551	23 220±270	charcoal	Beta-25880	27 770±770	charcoal
Beta-29988	23 270±170	charcoal	Beta-61608	27 780±230	charcoal
Anu-3008	23 360±750	freshwater shell	Beta-28323	28 000±720	charcoal
Beta-48330	23 420±340	charcoal	R-11795	28 060±600	marine shell
Beta-37549	23 630±250	charcoal	Beta-46170	28 310±200	charcoal
Beta-25382	23 640±310	charcoal	Beta-37568	28 320±660	marine shell
Anu-244	23 700±850	charcoal	Beta-37554	28 330±630	charcoal
Beta-48331	23 790±200	charcoal	Anu-5990	28 740±280	marine shell
Beta-26150	23 820±290	marine shell	Beta-29987	29 000±520	charcoal
Beta-48332	24 040±230	charcoal	Beta-46171	29 570±280	charcoal
Anu-2371	24 050±500	freshwater shell	Beta-44081	29 800±720	charcoal
Anu-2161	24 100±400	freshwater shell	Wk-1576	30 000±850	marine shell
Beta-25879	24 190±410	charcoal	Beta-47113	30 000±600	charcoal
Sua-1316	24 200±1400	charcoal	Beta-42059	30 210±300	charcoal
Beta-42996	24 210±730	charcoal	Beta-44375	30 280±450	charcoal
Sua-31	24 600±800	charcoal	Beta-25881	30 420±690	charcoal

Lab. number	date/s.d.	material	lab. number	date/s.d.	material
archaeological determinations (continued)					
Gx-7255	30 590+2220–1420	charcoal	Beta-68159	33 260±420	charcoal
Beta-28324	30 750±1340	charcoal	Anu-5070a	33 300±950	marine shell
Anu-680	30 780±520	charcoal	Beta-62323	33 600±670	charcoal
Beta-23404	30 840±480	charcoal	Beta-68158	33 850±450	charcoal
V-82	31 000±1650	charcoal	Anu-3508	34 200±850	freshwater shell
Anu-1262	31 100+2250–1750	unknown	Wk-1513	34 200±1050	marine shell
Anu-5469	31 350±550	marine shell	Anu-3507	34 700±900	freshwater shell
Sua-457	31 400±1500	charcoal	Beta-42122b	34 790±510	charcoal
Anu-65	31 600+1100–1300	charcoal	Sua-1704	35 100+1500–1300	resin
Beta-46873	31 610±370	charcoal	Sua-586	35 160±1800	charcoal
R-16098/1	31 770±390	marine shell	Anu-8179	35 410±430	marine shell
Sua-546	31 800±1400	charcoal	Beta-62319	35 570±480	charcoal
Sua-2870	31 900+700–600	charcoal	Anu-2586	35 600+1800–1500	charcoal
Sua-585	32 480±1250	charcoal	Anu-3509	36 000±1100	freshwater shell
Anu-5065	32 500±800	marine shell	Beta-47849	36 100±800	charcoal
Beta-47046	32 630±400	charcoal	Sua-1665	37 100+1600–1300	resin
Anu-5070b	32 700±1530	marine shell	Sua-698	37 750±2500	charcoal
Anu-331	32 750±1250	charcoal	Sua-1500	39 500+2300–1800	charcoal
geological determinations					
Nz-399	15 000±350	peat	(Cont. Shelf, NSW)	17 900±600	shell
Wk-927	15 100±180	unknown	Anu-2928	17 950±310	carbonate
Gak-7556	15 100±750	peat	Gxo-064	18 140±450	shell
(Lake Corangamite)	15 200±530	charcoal	Sua-1042	18 190±340	charcoal
Beta-13518	15 300±260	unknown	Beta-16156	18 330±260	organic fraction
Anu-3151	15 450±200	carbonate	Anu-2533	18 800±500	wood
Anu-961	15 510±210	unknown	N-634	18 900±430	carbonate
Anu-2929	15 650±260	carbonate	Sua-2856	19 000±170	wood
Sua-376	15 740±700	charcoal	Sua-153	19 180±360	organic sand
Gak-5619	15 900±510	organic sand	Beta-20778	19 180±260	lake mud
Sua-2589	16 030±150	unknown	Nz-34	19 200±500	carbon
Sua-2104	16 200±1200	charcoal	Y-868	19 270±520	wood
Sua-2579	16 460±140	unknown	Anu-2927	19 300±300	carbonate
Grn-7882	16 590±110	peat	(Carpentaria)	19 300±600	shell
(Carpentaria)	16 650±380	shell	Beta-1887	19 410±330	organic soil
Anu-3152	16 650±600	carbonate	(Carpentaria)	19 600±1000	shell
Lj-516	16 910±500	shell	(Lynch's Crater)	19 670±350	unknown
Anu-3153	17 050±300	carbonate	Sua-2155	20 100±470	wood
(Carpentaria)	17 180±380	shell	(Lake Colungulac)	20 100±500	charcoal
Beta-7806	17 350±250	mud	Gxo-063	20 170±750	shell
Anu-2930	17 400±250	carbonate	Nz-435	20 200±165	unknown
Pta-2506	17 670±180	calcite	Sua-152	20 250±360	organic sand
Sua-1359	17 700±400	lake mud	Anu-199	20 340±500	carbonate
Sua-1111	17 820±215	carbonate	Arl-165	20 450±1000	unknown

Lab. number	*date/s.d.*	*material*	*lab. number*	*date/s.d.*	*material*
geological determinations (continued)					
Sua-2521	20 660±280	wood	Gak-5153	26 760±1360	wood
Gak-892	20 900±540	charcoal	Sua-2493	26 800±700	clay
Wk-1537	21 050±140	lake mud	Anu-2932	26 800±600	carbonate
Sua-2154	21 180±370	plant	Anu-3691	26 960±580	shell
Sua-1045	21 250±270	organic soil	Sua-2535	27 300±500	sediment
Gak-891	21 350±850	charcoal	Gak-5588	27 400±2900	charcoal
Anu-3266	21 600±310	carbonate	Sua-2583	27 510±240	unknown
N-567	21 600±650	carbonate	Gak-6324	27 600±1700	peat
Gak-8256	21 620±750	shell	Anu-1679	27 700±2000	shell
Anu-1351	21 800±950	clay	Anu-2480a	27 800±700	wood
Sua-153/2	21 905±440	organic sand	Sua-2390	27 800±800	organic mud
Anu-2016	22 040±690	carbonate	Y-229/2	27 900±2000	shell
Beta-12748	22 130±420	unknown	Anu-2143	28 100±1250	carbonate
Grn-7689	22 130±180	peat	(Lake Colungulac)	28 240±100	shell
Anu-2931	22 150±400	carbonate	Y-230	28 240±1100	shell
Anu-3268	22 460±340	carbonate	Gak-5154	28 930+1970–2220	wood
Gak-652	22 700±1100	shell	Nz-198	29 000±800	wood
Sua-2534	22 790±140	sediment	Sua-154	29 050±830	charcoal
Arl-136	22 860±500	peat	N-566	29 100±1250	carbonate
Beta-16155	23 260±2540	carbonate	Gak-5589	29 340+3080–2220	charcoal
Beta-13776	23 280±830	organic mud	Sua-2532	29 440±60	sediment
Arl-223	23 500±8300	bone	Gak-8922	29 630±3030	peat
Anu-3065	23 590±450	carbonate	Anu-4980	29 740±3030	wood
Gak-5597	23 640±1030	organic sand	Anu-2145	29 750±1300	carbonate
Wk-822	23 650±320	lithified beach	Anu-1276	29 950±950	peat
Gak-5594	23 860±890	wood	Sua-2559	30 000±1000	organic mud
Gak-5155	24 090±1030	wood	Anu-2535	30 050±2000	wood
Anu-1278	24 100±600	clay	Sua-3043	30 120	organic soil
Sua-2434	24 200±2100	organic mud	Beta-5435	30 200±1180	charcoal
Sua-2584	24 580±280	unknown	Nz-194	30 300±800	organic sand
Gak-656	24 950±300	shell	Gak-1163	30 400±2300	charcoal
Anu-738	25 000±800	unknown	Anu-143.5	30 460±1500	wood
Sua-151	25 380±640	organic sand	Cs-549	30 500±650	shell
Gak-5596	25 660±1200	organic sand	Gak-655	30 600±450	shell
(Lynch's Crater)	25 700±800	unknown	Anu-2144	30 700±1500	carbonate
Sua-2223	25 700±1500	organic mud	Gak-5970	30 860+4100–2770	charcoal
Anu-145	25 770±5500	soil	Anu-143	30 920±2030	wood
Cs-468	26 000±400	calcarinite	Nz-596	31 000±2300	wood
Sua-2587	26 240±480	unknown	Anu-1350	31 100±1000	peat
Anu-1318	26 300±650	peat	Anu-1349.6	31 300±1200	peat
Anu-1275	26 450±650	peat	Sua-1046	31 500±900	wood
W-323	26 480±800	wood	Sua-2372	31 500±500	clay
Sua-2494	26 500±700	clay	Anu-3694	31 680±1770	shell
Beta-19704	26 500±500	marine shell	(Lynch's Crater)	31 680±600	unknown
Gak-3014	26 700±1700	wood	Gak-5620	31 960+3400–2380	peat

Lab. number	*date/s.d.*	*material*	*lab. number*	*date/s.d.*	*material*
geological determinations (continued)					
Gak-5690	32 350±3680	charcoal	Nz-193	39 000±3000	charcoal
Beta-6510	32 790±650	wood	W-2393	39 300±800	wood
W-2392	32 800±400	wood	Sua-2929	39 400±900	marl
Gak-5691	33 240+5610–3270	charcoal	Gak-7695	39 600±1000	charcoal
Gak-5625	33 240+3370–2370	wood	Anu-1349	39 700±3900	peat
Anu-2142	33 250±2100	carbonate	Sua-2925	39 900±800	shell
Beta-4045	33 590±510	wood	Sua-347	40 000±unknown	wood
Gak-906	33 600+3400–2400	charcoal	Anu-3908	40 200±2100	wood
Anu-146	33 700±2200	wood	Anu-4017	40 500±2150	wood
Grn-8646	34 100±700	humic	Grn-8636	41 100±800	humic
Gak-893	34 200±4100	charcoal	Grn-7999	41 150+1450–1250	wood
Anu-3419	34 200±1800	charcoal	Grn-9438	41 450±700	wood
Anu-4805	34 510±720	shell	Anu-4016	41 700±3000	wood
N-633	34 600±2700	wood	Sua-2277	41 900±1000	peat
Anu-1349a	34 800±1800	peat	Grn-9458	42 200±800	wood
Anu-1146	35 000±1900	organic mud	Cs-598	42 300±3000	shell
Anu-3699	35 080±1160	shell	Grn-9844	42 500±1100	shell
Anu-1277	35 100±1400	peat	Sua-1785	42 600±3600	coral
Anu-6344	35 200±1700	marine shell	Grn-8589	42 700±900	wood
Anu-3690	35 330±1520	shell	Sua-2278	43 000±1200	wood
Anu-3692	35 440±1050	shell	Sua-2685	43 000±1300	unknown
Sua-1287	36 200±3400	wood	Grn-7690	44 700±1500	wood
Cs-558	36 300±1450	shell	Cs-548	45 100±5100	shell
Grn-8606	36 300±700	wood	Grn-9765	45 200±2600	humic
Anu-3970	36 700±3300	wood	Sua-2834	46 300±2500	marl
Beta-6511	37 010±1040	wood	Grn-9341	46 400±1300	peat
Anu-3418	37 100±1600	wood	Grn-7481	47 500+2700–2000	wood
Sua-1789	37 300±1600	coral	Wk-1778	47 600±1550	organic soil
Nz-349	37 500±1900	wood	Grn-8627	47 600+1900–1500	humic
Sua-2834	37 500±600	shell	Grn-8526	48 200±250	wood
Cs-546	37 600±1700	shell	Grn-7691	48 400+1900–1600	wood
Cs-662	37 700±3000	shell	Sua-2599	48 700±2900	wood
Anu-4015	37 750±1500	charcoal	Grn-8754	49 250±300	wood
Sua-2460	37 800±800	wood	Grn-9767	51 300+4400–2800	humic
Cs-469	37 900±1700	shell	Sua-3041	52 110±6000	organic soil
Anu-1147	37 900±1000	organic mud	Grn-8277	53 400+1700–1400	wood
(Lynch's Crater)	37 940±1870	unknown	Grn-9459	53 400+3700–2500	wood
Sua-2446	38 800±700	unknown	Grn-7322	54 200+11 000–4500	wood
(Great Barrier Reef)	38 900±2000	coral	Grn-9905	55 200±500	peat
Anu-1279	38 900±3400	clay			

3 The pattern of continental occupation: late Pleistocene colonisation of Australia and New Guinea

M.A. Smith

Although Australia and New Guinea were widely settled by people prior to 30 000 years ago the dynamics of this process remain unclear. Several factors constrain our ability to directly reconstruct the way in which this took place. First, the range and quantity of archaeological data are only adequate for looking at questions about the distribution of prehistoric settlement only from about 30 000 BP, a time potentially as far removed from the date of first landfall as it is from the present day. Secondly, the lack of precision of dating methods in this time period (where luminescence and ^{14}C dates often have uncertainties of several thousand and several hundred years respectively) and uncertainty about the real ages of sites with radiocarbon dates older than >25 000 BP (Allen 1994; Allen and Holdaway 1995; Roberts, Jones and Smith 1994) severely limit our ability to chart the spread of settlement across the continent in the crucial first few millennia after initial landfall. In this context, ideas about the colonisation of the continent, often drawing on other sources of data, play a crucial role in interpreting available archaeological evidence.

Background to continental settlement

The first human movements into Australia and New Guinea took place sometime before 35 000 BP (Allen 1989; Groube *et al.* 1986; Pearce and Barbetti 1981), probably between 50 000–60 000 years ago (Roberts, Jones and Smith 1990; Roberts *et al.* 1994). Sea passages of a few days would have been enough to reach the Australian and New Guinea landmass from adjacent parts of the Indo-Malaysian archipelago, suggesting that the process is just as likely to have been a steady trickle of people across the frontier as a discrete migration. These people presumably were already adapted to tropical coastal and island environments with watercraft and coastal voyaging as part of their repertoire (Irwin 1992).

The continuity of marine ecosystems and useful plant species across Wallacea (Golson 1971) would have assisted settlement of coastal, estuarine and riverine environments in northern Australia and New Guinea, although movements into this region also brought a change to a distinctively Australian terrestrial fauna. Eastward migration along the

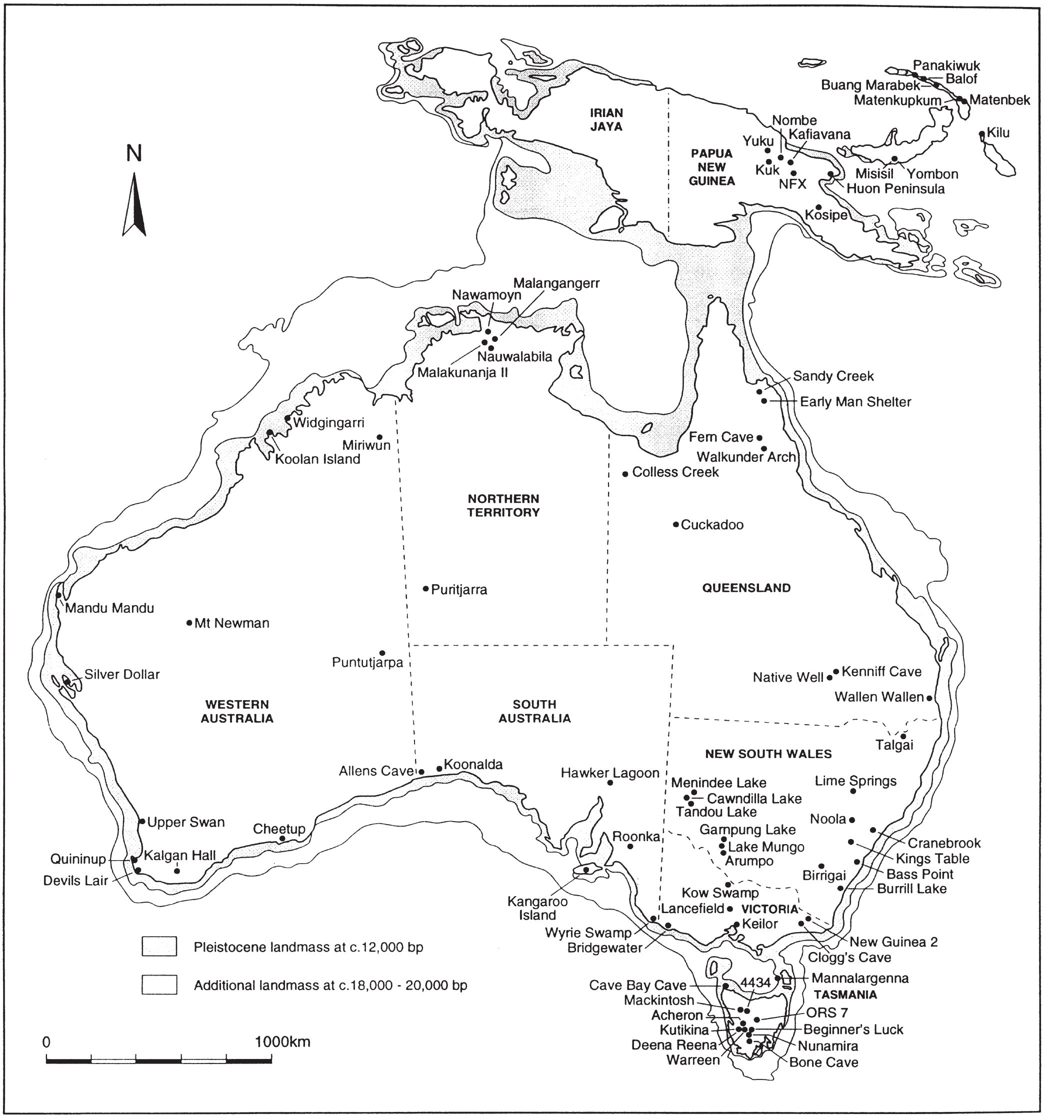


Figure 3.1: Pleistocene sites in Greater Australia

tropical belt brought people to the island archipelagoes of the Bismarck and Solomon Islands, areas with rich marine resources but with terrestrial environments that are depauperate in useful plant species and fauna. Movements south or east into the interior of the continent would increasingly have brought people into contact with radically different environments to those in island South East Asia, in particular the major montane regions of the New Guinea cordillera, the arid interior of the Australian continent, and the high latitude temperate environments of Tasmania. However, by 30 000 years ago these diverse environments had all been occupied in some fashion or another (Smith and Sharp 1993).

The initial dispersal of humans across Australia and New Guinea took place in the context of lowered sea-levels (–60 to –80 m for much of the critical time before 30 000 years ago) (Chappell 1994) and a continent somewhat larger than the three large islands (New Guinea, Australia and Tasmania) which make up the region today. Much of the additional land area was made up of exposed sections of the northwestern (Sahul) continental shelf. New Guinea was joined by a land bridge to Australia (with –12 m as the critical depth determining whether a land bridge or a shallow strait separated these areas). To the south, land access to the temperate mountainous island of Tasmania was contingent upon sea-level dropping to at least –55 m to expose the Bassian Rise. Recently revised sea-level estimates (Chappell 1994) indicate that people could have walked to Tasmania at any time between *c*. 60 000 and 10 000 years ago.

Ideas about the pattern and process of dispersal

Scenarios for initial settlement of the continent fall into two broad groups: those which postulate rapid early dispersal across the continent; and those that suggest the process of settlement was slower and more patchy, with significant time lags (often tens of millennia) between the earliest movements into the continent and first occupation of some regions.

Arguments for rapid early settlement rest on several assumptions. First, that the colonists had a highly flexible response to new ecological conditions and a high intrinsic rate of population growth, leading to rapid filling of the continent. Some workers suggest that dispersal would have followed the rivers and drainage systems of the interior (Birdsell 1957; Mulvaney 1961), especially those in the vast internal Lake Eyre and Murray/Darling basins. The floral gradients that exist between northern and central Australia would have assisted this process, as people could have acquired crucial ecological knowledge of useful plant species prior to moving into the interior (Golson 1971). Birdsell (1957) quantitatively modelled the process of continental settlement showing that Australia could be filled to saturation within a few millennia of initial landfall. He estimated that it would take between 845 and 4124 years to reach a population of 300 000 people (the estimated pre-contact population of the continent—now thought to have been closer to 750 000 people [White and Mulvaney 1987]). The key assumptions in these arguments are that dispersal is driven by rapid demographic growth, that limits of carrying capacity are approached before groups bud off and that social units are replicated during the process.

All of these assumptions are open to question. First, the human population may have grown only slowly during the first few millennia of settlement (White and O'Connell 1982: 46–54). Small founder populations are subject to stochastic fluctuations in numbers (McArthur, Saunders and Tweedie 1976) and in the northern third of the landmass endemic malaria may have checked the rate of population growth in coastal lowlands (Groube 1993). These factors may have been offset somewhat if northern Australia continued to receive small groups of immigrants for several millennia after first landfall, or if several discrete founding populations were established along the coastlines of north-western Australia and western New Guinea. Beaton (1990) usefully reminds us that even populations with a near stationary growth rate will grow substantially over 5–10 millennia. Secondly, empty territory appears to change selective parameters in favour of rapid dispersal (Kitching, 1986; Stodart and Parer 1988; Rindos and Webb 1992). With new territory, and a rich terrestrial fauna, readily available human groups may well have budded off before reaching carrying capacity and without replicating existing social formations. Just how thinly social and demographic networks can be stretched and still be viable remains to be explored, though some desert groups historically managed to maintain viable social and demographic units at population densities of the order of one person per 200 km^2 (e.g. Long 1971).

The alternative view is that human groups had minimal capacity to adapt to new ecological conditions and preferentially occupied certain habitats, either coastal and riverine zones (Bowdler 1977), or woodland (Hallam 1987; Horton 1981). The most influential of these models is by Bowdler who argued that there would have been a strong focus on aquatic resources in the initial colonising phase, leading to a pattern of early settlement tethered to littoral, riverine and lacustral habitats. She suggested that people would have been excluded from montane and desert regions until after 10 000–12 000 years ago. A key part of her argument is that the failure of lakes and rivers in semi-arid parts of southeastern Australia (especially in the Willandra region) after 15 000 BP forced people to turn to local terrestrial resources, including the grass and acacia seeds prominent in the ethnography for inland Australia. These changes made possible widespread settlement of the arid interior of the continent in the post-glacial period. Other models also single out the arid zone as posing special difficulties for settlement (Horton 1981; Veth 1989). Both Hallam (1987) and Horton (1981) suggest that early settlement would have focussed on well-watered woodland rather than aquatic or desert environments, with a more flexible response towards the exploitation of terrestrial resources than Bowdler allows. Horton in particular emphasises the availability of potable water as a crucial determinant of early settlement and uses the distribution of large megafauna species as a guide to the late Pleistocene distribution of well-watered woodland (though this assumes that large herbivorous megafauna were still extant when humans entered the continent). Archaeological work since 1977 has radically changed the context in which all of these models were put forward (Allen *et al.* 1988; Allen 1989; Bowdler 1990a; Kiernan *et al.* 1983; Smith 1987; Cosgrove 1989), but somewhat similar arguments have been put forward for late (*c.* 5000 BP) settlement of the major continental dune fields (Veth 1989, 1995). These hinge on the special difficulties posed by regions with uncoordinated drainage, hummock grassland and dune fields and postulate a need for technological and social adaptations in adjacent regions before these regions can be occupied.

A major difficulty with *all* these arguments is the time frame. It is very unlikely that it took tens of millennia for people to adapt to unfamiliar or difficult ecological conditions and there is now archaeological evidence showing that montane (Gillieson and Mountain 1983; Mountain 1993; White, Crook and Ruxton 1970) and arid habitats (Maynard 1980; Smith 1987, 1989) were exploited well before 12 000 years ago. It is also now apparent that economies based on terrestrial resources were in place before this time in several parts of the continent (e.g. Bowdler 1990b; Cosgrove 1989; Kiernan, Jones and Ranson 1983).

Another problem with these arguments is that they risk confusing dispersal and optimisation strategies (see Rindos and Webb 1992). Initial settlement need not involve optimal adaptation to local conditions and population densities comparable to ethnohistoric levels. A case in point concerns ideas about initial human movements into the major continental dune fields. The argument that these regions were occupied for the first time during the Holocene (Veth 1989) seems to overstate the need for economic and social adaptations to settle these regions (Smith 1993). This model has recently been recast (Veth 1995) placing greater emphasis on areas with uncoordinated drainage networks but is clearly at odds with archaeological evidence here (Cane 1995; Gould 1977; Martin 1973; Wright 1971; Smith 1987, 1989). New evidence for Pleistocene use of the sandy deserts is rapidly altering our picture of desert settlement, suggesting that at the very least the chronological framework for these models needs recalibrating (Smith *et al.* 1991; Veth *et al.* 1990; Veth 1995; Veth and O'Connor 1996).

Several other factors suggest that major time lags between the colonisation of coastal regions and the interior of the continent are unlikely. First, the linear configuration of the coastal zone means that it is difficult to model population growth without concluding that some movements into the interior would have taken place long before the coastal perimeter of the continent was fully settled (White and O'Connell 1982, Fig. 3.7). Secondly, the western coast of the continent is arid and westward movement around the perimeter of the continent would probably have required an early adaptation to arid conditions and a reduced emphasis on littoral resources (Nicholson and Cane 1994; White and O'Connell 1982: 52). Thirdly, populations moving inland may have been free to grow and disperse at much faster rates than those occupying northern coastal or swamp habitats if Groube (1993) is correct about the presence of the malaria parasite in these regions. Birdsell (1957) also suggests that the reduced carrying capacity of arid environments would necessitate larger territories and increase the rate of dispersal of people in the interior. Studies of exotic animals confirm this. For instance, rabbits are recorded historically as spreading throughout arid and semi-arid areas at 100 k/yr compared to 10–15 k/yr in forest and coastal formations (Stodart and Parer 1988).

Archaeological evidence of continental colonisation

The critical time period for initial continental colonisation is so remote from us that problems of archaeological visibility and of chronological resolution will prevent us from investigating the pattern and rate of early settlement except in the very broadest terms. Time lags of tens of millennia in the settlement of marginal regions would be detected of course, but we are unlikely to pick up variations of a few thousand years. Despite these problems, a more detailed examination of site distribution can now be attempted than was possible when Bowdler (1977) and Horton (1981) reviewed the evidence. Current data suggest the continent was fully occupied by 10 000–15 000 years ago, though some regions such as the New Guinea highlands, southwestern Tasmania and parts of the arid interior may not have a record of continuous occupation. The distribution of sites dated to ~30 000 years ago or earlier (see Smith and Sharp 1993: Fig. 7) suggests that people had reached the extremities of the continent by this time. This picture will of course change as luminescence dating techniques are more widely deployed in archaeology, but the probability is that this will strengthen rather than weaken the picture of widespread early settlement.

Three areas where our picture of the nature and extent of late Pleistocene settlement changed dramatically during the 1980s are Tasmania, central Australia and island Melanesia. The papers in this section are chosen to reflect this new dimension to our understanding of early continental settlement. Each of these regions is briefly reviewed below.

High latitude regions

The inhospitable, densely forested southwest region of Tasmania appears to have been substantially unoccupied in the nineteenth century (Jones 1971). However, a pattern of discoveries initiated by finds at Beginners Luck Cave (Murray *et al.* 1980) and most dramatically at Kutikina (Fraser Cave) (Kiernan *et al.* 1983), and later at Nunamira (Bluff Cave) (Cosgrove 1989), showed that the region has one of the richest records of late Pleistocene occupation in Australia, dating to a time when the area was exposed alpine grasslands on the extreme southern margin of the continent. The earliest date for occupation of Tasmania currently is ~35 000 BP from Warreen in the southwest of the island (Holdaway and Porch 1995). These finds had immediate ramifications for models of continental occupation. First, they indicated human exploitation of highland and extreme high-latitude habitats at a much earlier date than previously allowed. This use appears to have continued throughout the last glacial when the region carried the largest Tasmanian ice sheet. Secondly, these sites have rich faunal assemblages, dominated by small macropods. This prompted Bowdler (1990a) to reassess her coastal colonisation model, conceding that there was evidence for an earlier adaptation to terrestrial resources than she had thought.

Tasmania also presents one of the few opportunities to test ideas about the speed of continental colonisation. Recent work at Parmerpar Meethaner shows the presence of a sterile layer dated to ~40 000 BP beneath the earliest occupation layer at ~34 000 BP (Cosgrove 1995). If as the excavator suggests this pinpoints the arrival of people in Tasmania, it suggests that either there were substantial time lags (of the order of

20 millennia) between the colonisation of northern and southeastern Australia, or that the chronology for northern Australia is incorrect. On the other hand, it is not known how long the sterile layer at Parmerpar Meethaner took to accumulate. If the layer turns out to span a relatively short period of time, this evidence will weaken the argument that first occupation of the region has been effectively bracketed.

The desert

In the Australian arid zone the nature of late Pleistocene occupation became an issue early on. Evidence from sites in the Pilbara—such as the Newman site (21 000 BP) (Maynard 1980)—and on the Nullarbor Plain—such as Koonalda Cave (22 000 BP) (Wright 1971) and Allens Cave (N145) (>20 000 BP) (Martin 1973)—was interpreted as opportunistic use of the upper reaches of coastal catchments or of the hinterland of otherwise coastal territories (Bowdler 1977). Decisive evidence for early occupation of the arid centre of the continent came from Puritjarra rock shelter (Smith 1987, 1989; Smith *et al.* in press) dating back to ~35 000 years ago. This has since been supplemented by dates for other sites across the arid zone: JSN at 14 400 BP (Smith *et al.* 1991); Cuckadoo 1 rock shelter at 15 000 BP (Davidson *et al.* 1993); Katumpul at ~22 000 BP in the Laverton region (Veth 1995: 36); and at Serpent's Glen at ~24 000 BP (Veth and O'Connor 1996). The balance of evidence, despite being rather sparse, indicates widespread exploitation of the arid interior of the continent in the late Pleistocene, from at least 30 000 years ago. Recent dates from the Little Sandy Desert (Veth and O'Connor 1996) indicate that the pattern of Pleistocene occupation took in some of the most arid parts of the continent, including the major continental dune fields. It remains to be determined whether these sites represent some form of opportunistic use of the arid zone during the late Pleistocene (Bowdler 1990a; Veth 1995) rather than fully operating systems (cf. Smith 1989). With the accumulation of new data the former seems unlikely.

One of the key issues concerns the economic orientation of the people using these sites. Initial settlement of the region was once thought to be primarily controlled by the ability of people to adapt to new plant resources, especially grass and acacia seeds. However, on present evidence identifiable seed grinding implements do not appear in archaeological assemblages until about 3000–4000[1] years ago, suggesting that initial settlement of the desert was not closely linked to intensive use of these resources (Smith 1986). Direct evidence relating to prehistoric subsistence is rare. The predominance of macropods and emu eggshell in faunal assemblages at the Silver Dollar site 25 000 years ago suggests an early adjustment to inland resources (Bowdler 1990b). Similarly, on the Nullarbor Plain, Allens Cave has a rich faunal assemblage dominated by small macropods (Cane 1995) with occupation extending back to ~39 000 years ago. The Nullarbor sites represent an early adaptation to a particularly arid region, one that is all the more impressive given that at Allens Cave this appears to have continued throughout the glacial maximum when the site was situated amid saltbush steppe on a vast arid inland plain (Cane 1995).

Reports that rock engravings near Karolta in the south eastern sector of the arid zone date to 30 000 years ago (based on cation ratio assays and AMS ^{14}C dating of charcoal embedded in rock varnish) (Dorn *et al.* 1988; Dorn and Nobbs 1992), if substantiated would strengthen the argument for the early establishment of fully operating regional systems in arid Australia.

Islands

Until 1988 most discussions of initial late Pleistocene colonisation of the Australian region were restricted to lands that were linked at one time or another by land bridges, to form the prehistoric continent that scholars sometimes call Sahul (Ballard 1993). One of the big surprises of the 1980s was the discovery of a major Pleistocene presence on large islands in the Bismarck and Solomon island archipelagoes to the east of Sahul (Allen *et al.* 1988; Allen *et al.* 1989; Wickler and Spriggs 1988). The key dates are from Panakiwuk (~15 000 BP), Matenkupkum (~33 000 BP) (Allen and Gosden 1991), Matenbek (~20 000 BP) (Allen *et al.* 1989) and Kilu Cave (28 000 BP) (Wickler and Spriggs 1988). Subsequent fieldwork has confirmed but not substantially extended this chronology. These dates suggest that the large islands of northern Melanesia were first occupied as part of the initial colonisation of Australia and New Guinea, although the discrepancy with the luminescence chronology for northern Australia continues to pose problems for this view.

Evidence of late Pleistocene occupation at Pamwak rock shelter (>12 000 BP) on Manus Island indicates the capacity to colonise across ocean barriers of >200 km, with a blind crossing of at least 60 km required to reach Manus (Fredericksen *et al.* 1993). Together with other evidence, for long distance transport of obsidian and human introduction of the phalanger to supplement the depauperate fauna of these islands, the Manus evidence indicates that late Pleistocene maritime capabilities went well beyond accidental drift voyaging. This leaves open the possibility that the initial colonisation of these regions was part of a deliberate strategy.

Concluding comments

The most likely pattern of continental settlement is probably one in which the coastal and riverine habitats of northern Sahul are preferentially settled in the first few millennia of settlement. These initial movements may have carried people out into the large islands of northern Melanesia, and may have involved deliberate rather than accidental voyaging. However, well before these coastal environments were fully occupied people may have widely occupied the northern savannas of Australia and the Sahul shelf. Settlement probably proceeded slowly at first and more rapidly in subsequent centuries, with rapid dispersal promoted by the better health of populations in the drier climates of the inland, and the larger territories required to support each group in the interior. Similar factors may have been involved in the early occupation of the central cordillera of New Guinea, where the richest terrestrial fauna were to be found in montane forest and alpine habitats, and where the cooler climate provided some relief from malaria.

Selective occupation of rich coastal habitats probably promoted rapid settlement of the northern and eastern coastlines of the continent. In contrast, the arid littoral of the west and northwest coast of the Australian landmass is likely to have promoted an early move to terrestrial resources as part of a broad spectrum mixed economy. The arid interior of the continent may well have been settled within a few millennia of first landfall. However, desert populations are likely to have been low and dispersed and settlement is always likely to have been subject to an ebb and flow as a concomitant of high environmental variability in these regions. The major Australian deserts—including sand ridge deserts and stony deserts—may have been among the last parts of the continent to have been settled by people, or the last areas where fully operating systems were established, but it is debatable whether their settlement was delayed tens of millennia after colonisation of the northern savanna.

Early exploitation of high latitude temperate habitats is exemplified by the Tasmanian data. On present evidence it appears that the region was first occupied by ~35 000 BP but earlier sites may remain to be found. It is fair to say that the Tasmanian and New Ireland dates for initial settlement at ~35 000 BP are difficult to reconcile with present indications that people first arrived in northern Australia between 50 000–60 000 years ago. Lags of tens of millennia in the first arrival of people in these three regions are unlikely. Existing chronologies for continental settlement will almost certainly need to be recalibrated and the picture is bound to change considerably as luminescence dating of archaeological sediments is more widely deployed and tested in a variety of circumstances.

3a When did humans first colonise Australia?

Jim Allen

The early history of our own species, *Homo sapiens*, has recently undergone drastic revision. New fossil, genetic and archaeological data indicate the appearance of anatomically modern humans at least 90 000 years ago in both sub-Saharan Africa and Israel (Lewin 1988; Stringer 1988; Stringer & Andrews 1988; Valladas *et al.* 1988), whereas earlier models derived from Europe (e.g. Trinkaus 1984: 286) had suggested this event was much later, around 40 000 years ago. The recent revision puts a new perspective on several old debates in Australian archaeology, particularly the timing and manner of the original colonisation.

Until very recently textbooks using the Western European model have identified modern humans from their predecessors by a combination of physical modernity, a greater range of specialised stone tools reflecting more elaborate manufacturing techniques and sophisticated artistic achievements. This package, however, never completely accommodated the Australian data: while it has always been accepted that Australia was originally settled by anatomically modern humans, and while Australian Pleistocene age art has been reasonably claimed, Pleistocene stone industries in this country show none of the sophistication of contemporaneous industries in Europe and many other parts of the world. As White (1977) has noted, this led an earlier generation of prehistorians to conclude that since Australian stone tools were crude, the related human histories must be 'unenterprising' and 'monotonous' (Clark 1968: 21–2; Chard 1975: 161). More recent appraisals (e.g. Foley 1987; Gowlett 1987: 215) recognise that there is no valid correlation between physical modernity and the sophistication of tools as previously believed; more importantly, the diverse array of modern archaeological analyses has relegated stone tool technology to being only one of many measures of past cultural vitality.

A second problem of accommodating the Australian data within the European model, much less discussed, has been the demonstrated antiquity of humans in Australia, which is the same 40 000 year period recognised for anatomically modern humans in Western Europe. This allowed no dispersal time to the other side of the world, whatever the direction.

The new data have extended modern human antiquity and thus offer a solution to this problem, but raise others. Paradoxically one of these is now why it took so long for *H. sapiens* populations to reach Europe, South East Asia and Australia. Research into the human fossil evidence in Australia is a history of conflicting interpretations, especially those concerned with anthropometric analyses (e.g. White & O'Connell 1982: 75; Thorne 1983; Brown 1987: 41, 55). Currently the view that Australia was colonised

Figure 3a.1: Bone Cave, today in the heart of the forest in south central Tasmania, was first occupied by humans about 29 000 years ago. For most of the late Pleistocene the inhabitants of this small cave, seen here to the right of the picture, lived in sight of glacial ice.

by two separate populations, a gracile group from Asia and a robust group descended from *Homo erectus* forms in Indonesia (Thorne 1977; Wolpoff *et al.* 1984: 411), is rejected by Stringer and Andrews (1988: 1267) and Brown (1987: 62), who conclude that the data do not indicate the presence of two biologically distinct groups in Australia in the terminal Pleistocene.

If we cannot determine whether there was one or more human groups in Pleistocene Australia from a set of remains which probably exceeds in number those from Europe in the same period (Gowlett 1987: 215), it follows that these data can shed no light on the nature or dynamics of initial colonisation. As well, if we cannot specify a reasonably close ancestry for them outside Australia, then these fossils also offer little indication of when initial colonisation occurred. More reliable are dates derived from archaeological remains.

Over the past three decades perceptions of when humans first colonised Australia have altered radically. In 1961 Mulvaney observed that the oldest secure radiocarbon date in undoubted association with human activities was from a site at Cape Martin in South Australia, dated to less than 9000 years ago (Mulvaney 1961: 64). A mere five years later Golson was able to present a lecture titled ‘Fifty Thousand Years of New Guinea History’ (Golson 1970: 192), an estimate attendant upon the needs for lowered sea-levels to facilitate human access into Greater Australia (the single landmass which at various periods in the Pleistocene included present day New Guinea and Tasmania) but especially upon a demonstrated Pleistocene age for that first colonisation. Fittingly, Mulvaney himself had broken the Pleistocene barrier with dates for Kenniff Cave in Queensland in 1962 (Mulvaney & Joyce 1965: 169) although, by 1972, Mulvaney's initial claim of 16 000 years had elsewhere been doubled by a series of nine dates for occupation sites round Lake Mungo in western NSW which ranged from *c.* 24 000 to *c.* 32 000 years ago (Barbetti & Allen 1972). McBryde obtained a date of *c.* 35 000 years ago at another western NSW lake site in 1975 (Flood 1983: 50) and the following year Bowler (1976: 59) offered further radiocarbon dating support for this extended antiquity. At the same time Bowler (1976: 64) nominated an age range for the artefact-bearing D Clay at Keilor, near Melbourne, of between 36 000 and 45 000 years ago on the basis of rates of soil formation and silt deposition. So far this antiquity has not been confirmed by radiocarbon dating.

Elsewhere, dates approaching 35 000 years ago were published for the southwest Western Australian site of Devil's Lair (Balme *et al.* 1978: 38), although the human associations may be younger (Dortch 1984: 46), and in 1981 an antiquity of 38 000 years was claimed for artefact-bearing layers at Upper Swan River near Perth (Pearce & Barbetti 1981). It is difficult to assess this claim from the preliminary report which is the only publication so far to appear. If authentic, this is the oldest reasonable association of radiocarbon determinations and obvious human artefacts in the present Australian continent. By 1983, in the highlands of present-day Papua New Guinea, dates for human activity between 25 000 and 30 000 years ago had been claimed for Kosipe (White *et al.* 1970), Kuk (Golson & Hughes 1977) and Nombe (Gillieson & Mountain 1983). In the same year Flood (1983: 251–2) listed some 20 Australian sites with deposits dated to more than 20 000 years ago.

Whereas the known antiquity for humans in Australia quadrupled between 1960 and 1980, this increase in antiquity has levelled off in the present decade. While archaeological research has not diminished, and while sites older than 20 000 years ago have continued to be discovered on a regular basis, claims in the past ten years or so for human sites older than *c.* 35 000 years ago have been few. In noting several of these claims, it becomes obvious that there is a qualitative break in the data before and after this watershed date. There has been no serious questioning of the human origins and associated dating of most of the key sites younger than 35 000 years ago, the majority of which are clearly habitation sites. However, earlier claims include no obvious habitation sites and fall into two main categories: (i) stone tools in alluvial river valley deposits where the artefacts cannot be directly dated or where the dating itself is subject to interpretation; or (ii) particular geomorphological phenomena which have no immediately perceived natural explanations and for which a human agency is invoked.

In the first category we can include Keilor and, provisionally, Upper Swan, both previously discussed. To these we might add sites along the Greenough and Murchison Rivers in Western Australia discussed by Wyrwoll and Dortch (1978) and Bordes *et al.* (1980) where stone tools occur and the remains of extinct giant fauna may be associated.

On the other side of the continent Nanson *et al.* (1987) claim between two and seven stone tools in gravels in the Cranebrook Terrace near Sydney. These gravels are suggested by the authors to date to between 40 000 and 47 000 years ago. Allowing that these are human tools and that they were *in situ* when collected (or that their ascription to the gravel layer is correct), it is important to note that the suggested date range is an interpretation which makes it not immediately comparable with the dating of the other sites discussed here. ^{14}C dates from the terrace have been contaminated with younger carbon in the groundwater. A series of five Australian National University Radiocarbon Dating Laboratory determinations, given rigorous pretreatment to remove younger carbon, offer at two standard deviations a range of dates for the basal gravels from 30 100 to 47 700 years ago. Undescribed adjustments to these dates based on Barbetti and Flude's (1979) suggestion that geomagnetic variations cause the radiocarbon timescale to underestimate real age in the late Pleistocene, are said to put them in agreement with two thermoluminescence dates for the gravels of 47 000±5200 and 43 100±4700 years ago (Nanson *et al.* 1987: 73).

Although derived from a tectonically uplifting marine terrace rather than a river valley, a similar example involves the thermoluminescence dating of tephras found sealing several undoubted stone artefacts on the Huon Peninsula in eastern Papua New Guinea (Groube *et al.* 1986). Here ages of at least 40 000 years ago are claimed. Allowing that the tephras themselves are *in situ* and that the thermoluminescence dating is acceptable, these are the oldest human artefacts in present-day Papua New Guinea.

In the second category, where an undemonstrated human agency is invoked, two recent cases can be noted. Singh *et al.* (1981: 45–7) have suggested that humans were present in Australia some time between 128 000 and 75 000 years ago on the basis of a sudden increase in burning, represented as an increase in the amount of charcoal in the sedimentary profile from Lake George near Canberra. Higher rates of charcoal continue from that time onwards in the profile. Elsewhere claims have been made for human origins for a riverine shell bed and also marine shells cemented into the top of an eroding cliff line near Warrnambool in Victoria. A date of 80 000 or more years has been claimed for both these deposits (Goede 1989). From an archaeological perspective, the lack of any clear human associations with data such as these do not permit them to be taken as strong indications of human presence in Australia at ages double those derived from undisputed archaeological evidence. Beyond the watershed date of 35 000 years ago the significance of claims for greater antiquity is such that they require concomitantly more rigorous demonstration. At present the opposite is largely the case.

This brief review of the data offers us two alternatives. The first is that the available evidence *is* representing the original human colonisation of Australia not much before 40 000 years ago. The second is that there is a phase of 'invisible colonisation' of unknown length, not yet represented in the archaeological record.

Intuitive reconstructions of initial colonisation (White & O'Connell 1982: 46–8) assume, given what is known of human cultural development in other parts of the world at this time, that the settlement of the island world of Wallacea, to the northwest of Australia, was a gradual process, requiring the survival of small groups of accidental voyagers on a number of island crossings. Eventually this movement culminated in a successful landing in Greater Australia. This model implies, given the successful but accidental crossing of at least 90 km of open sea on one of the legs, that this initial settlement was probably by small numbers of people and unlikely to have been supplemented by frequent later voyages.

Models of dispersal within Greater Australia and reconstructions of population increase have suffered from being largely untestable against available data. Thus people may have dispersed rapidly to all parts of the continent, reaching a population level as high as that at European contact in as little as 2000 years after initial landfall (Birdsell 1957) or may have skirted round the coastal perimeter and up major river systems, not inhabiting the interior until the very end of the Pleistocene (Bowdler 1977). Criticisms and modifications of both these positions are also available (e.g. Horton 1981; White & O'Connell 1982). Demographic models ranging from Birdsell's initial growth model above, through steady growth models, to a rapid increase in the mid-Holocene have also been proposed and debated (Beaton 1983, 1985; Lourandos 1980, 1984; Hughes & Lampert 1982).

The data necessary to further question these various alternatives now appears to have been provided by a series of recent archaeological discoveries which considerably advance our understanding of the behaviour of humans in Pleistocene Greater Australia. A review of these findings also reveals much about the wider issues.

Excavation and analysis of deposits in four limestone caves in New Ireland, Papua New Guinea, have indicated that this island was occupied at least *c.* 32 000 years ago (Allen *et al.* 1988). By 28 000 years ago humans had also reached the Kilu site in the Solomon Islands to the south (Wickler & Spriggs 1988). The earliest settlers on the New Ireland coast exploited the marine reef resources including shellfish, crustacea and fish—the latter representing the earliest known occurrence of marine fishing anywhere in the world. They also took bats, reptiles, birds and rats. By 20 000 years ago they were regularly transferring obsidian from west New Britain, a straight-line distance of 350 km and involving a water crossing of about 30 km, and had also introduced phalangers to New Ireland. Subsequently they also introduced other wild mammals to the island. Other evidence from New Ireland indicates the occupation of new sites inland from the coast 14 000–15 000 years ago to which New Britain obsidian was carried soon after 8000 years ago. The picture emerging is one of broad spectrum hunter-gatherers who by 20 000 years ago had become part of a system which involved long-distance transfer of raw materials and which expanded throughout the Pleistocene (Allen *et al.* 1989).

In the arid centre of Australia the notion that this region was only successfully occupied by humans 10 000–12 000 years ago has recently been revised by a date of *c.* 22 000 years for the initial occupation of Puritjarra shelter west of the MacDonald ranges (Smith 1987). This antiquity helps to substantiate Pleistocene occupation claims based on younger evidence from this zone (reviewed in Jones 1987). More startling are recent suggestions that rock art in the arid zone near Karolta in South Australia may

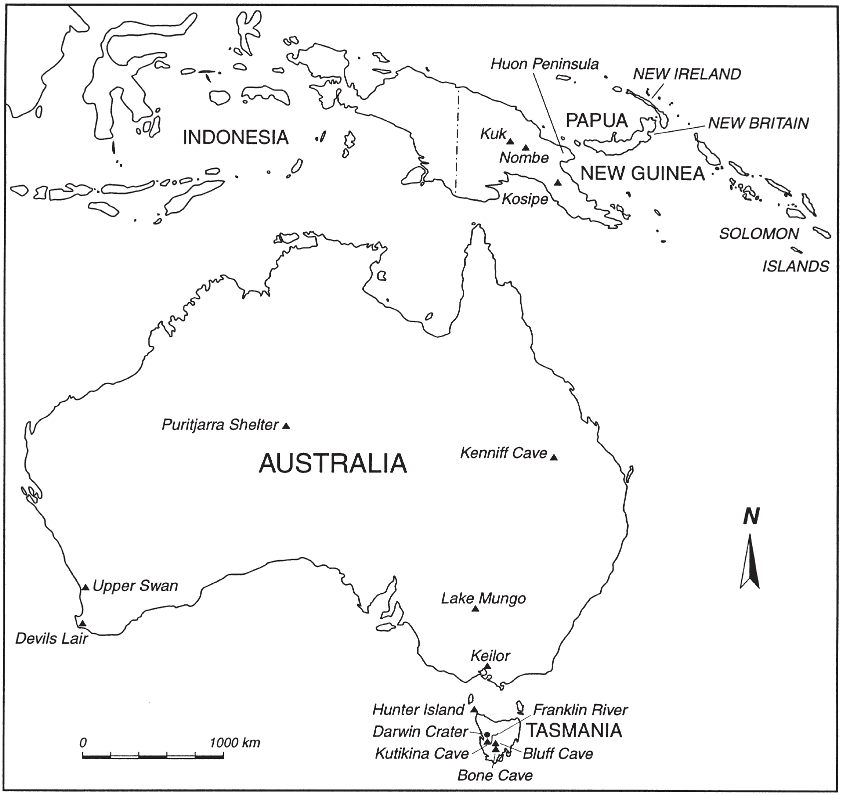


Figure 3a.2: Map showing sites and localities discussed in text.

have a minimum antiquity of 30 000 years. By measuring cation ratios (in this case potassium plus calcium divided by titanium) in the characteristic patina or 'rock varnish' which forms over engravings in this and other regions, Dorn *et al.* (1988) have calculated that 17 of their 24 samples are of likely Pleistocene age and eight are probably more than 20 000 years old. The oldest determination is between 22 500 and 39 800 years ago (2 sigma extreme error range). Allowing that the accuracy of these dates is substantiated, not only is the antiquity of this art remarkable, but also the evidence of cultural as well as subsistence activities suggests that the Pleistocene use of this environment was not haphazard. On first principles and working from known ethnographic cases we may now envisage 'desert-adapted' behaviour, involving small mobile groups, developing as early as adaptations to equally marginal environments elsewhere in Australia. Although the rates of deposition in shelters like Puritjarra (Smith 1987) were low, this probably reflects both the general behavioural characteristics of such a system and the likelihood that most camping occurred away from shelters and caves.

Human absence had also previously been assumed for the uplands of southeastern Australia and Tasmania in the late Pleistocene (Bowler 1976: 74–5; Calaby 1976:

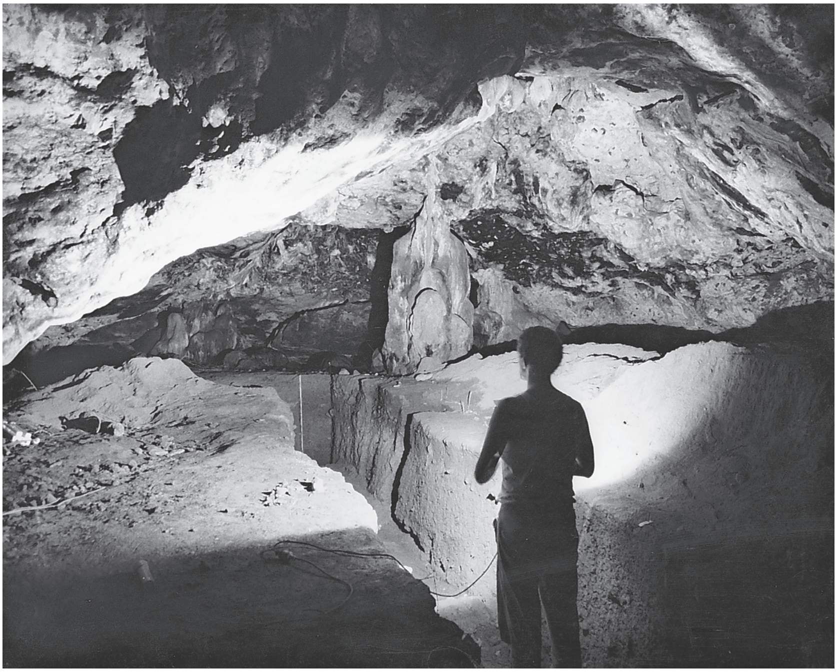


Figure 3a.3: Matenkupkum Cave on the east coast of New Ireland was first occupied about 32 000 years ago by people adapted to tropical coastal living. Deposits from this trench yielded the earliest evidence of salt water fishing yet discovered anywhere in the world.

25). The antiquity of humans in Tasmania, formerly identified at *c.* 22 000 years ago on Hunter Island (Bowdler 1975) and *c.* 20 000 years ago in the Franklin River and Florentine River valleys (Murray *et al.* 1980; Kiernan *et al.* 1983), has recently been significantly extended. Three cave sites in south central Tasmania, all between 350 and 500 m above sea-level, are now dated to *c.* 30 000 years ago in their earliest cultural levels (Allen 1989; Cosgrove 1989). Recent geomorphological research in Bass Strait indicates that the landbridge had intermittently joined Tasmania to Australia for *c.* 7000 years before this date (Blom 1988), so there is no reason to doubt that humans occupied Tasmania as early as other parts of the continent.

Two of these new sites, Bluff Cave in the Florentine Valley and Bone Cave about 20 km south in the Weld Valley, have yielded detailed evidence which links them to a system of human occupation that exploited upland food sources across glacial Tasmania throughout the late Pleistocene. Both sites contain stone tools made from an impactite called Darwin glass, which travelled a straight-line distance of about 100 km from its source to the west and together with other sites in the region, both sites reflect an elaborate hunting strategy carried on for 20 000 years, often within sight of glacial ice, when temperatures were up to about 6°C colder than at present (Cosgrove *et al.* 1989).

Each of these data sets confronts the widely held view that Pleistocene Australians were small groups of dispersed hunter-gatherers occupying limited activity sites and reflecting little regional variation in their archaeological remains (White & O'Connell 1982: 65, 72; Lourandos 1985: 399; Cosgrove 1989). Both the Tasmanian and New Ireland data point to the presence and development of distinctive regional economic systems in place and expanding between 30 000 years ago and the end of the Pleistocene. A distinctively different archaeological signature from either of these systems can be seen in the arid zone of Central Australia. Thus we can argue a greater level of adaptability among Pleistocene Australians than was previously allowed.

The development of this view has direct implications for our understanding of both the nature and timing of the initial colonisation of Greater Australia. A common notion and reasonable assumption is that the first human arrivals in Australia possessed an economy adapted to tropical coasts and that similarities between coastal environments and edible tropical plants in the new and departed homelands would have facilitated this colonisation (Golson 1971: 209; White & O'Connell 1982: 51). This argument was extended by Bowdler (1977: 221) to promote the idea of coastal colonisation of the whole continent on the complementary grounds that the existing modes of subsistence and technology which the first arrivals brought with them would require few changes in order to exploit Australian coastal resources, but also that the capture of unfamiliar terrestrial animals would have required significant changes in the behaviour of the colonists.

If the southern New Ireland sites, as has been argued (Allen *et al.* 1989), reflect the initial colonisation of this island at 32 000 years ago, we must either accept that New Ireland was not colonised particularly early in the wider history of Greater Australia, or that current dates from various parts of the Pleistocene continent at 35 000–40 000 years ago are approximately the dates of initial colonisation.

If we accept the former possibility, that New Ireland was not colonised early, then we reduce the importance of coastal, tropical pre-adaptation as a significant factor in successful colonisation, for a likely corollary must be that the most similar environmental niches would be first occupied. Although New Ireland is distant from the likely initial landfall, under this model we would nevertheless expect New Ireland dates to be earlier than in the southern extremes of Greater Australia, which they are not. There are also two additional water crossings but both new landfalls are clearly visible and the crossings pose fewer problems than those overcome in reaching the western shore of the continent.

If we accept the latter possibility, that current dates do approximate the time when humans reached Australia, we must accept not only a rapid colonisation of the entire continent but one by very adaptive humans who, in only a few thousand years, managed to occupy environments as diverse as the tropical lowlands, the arid zone and the periglacial uplands of southern Tasmania. Neither view supports the model of colonisation around the coasts. Such a rapid colonisation may also demand a larger founding population than previously envisaged.

One measure of the skills of Greater Australia's first settlers might be their ability to make crossings between islands, whether deliberate or accidental. Previous views have opted mainly for accidental voyages, with the technology and personnel ranging from the possibility of a single pregnant woman caught on an upturned tree trunk and swept out to sea in a flood (Calaby 1976: 24) or the use of simple rafts (White & O'Connell 1982: 44) to buoyant and sturdy watercraft (Flood 1983: 36). Also, although a single pregnant female is theoretically a possible founding population, computer simulations of demographic growth (McArthur *et al.* 1976) suggest that small groups of perhaps 5–8 males and females had a much greater chance of successfully establishing themselves on a new landmass (see White & O'Connell 1982: 46–8 for discussion). Such a model implies the use of watercraft of some sort.

The Melanesian data discussed above contribute to this debate in two important ways. While the crossings from Greater Australia to New Britain and New Ireland were merely two further steps along an intervisible corridor of islands reaching back to Island South East Asia (Irwin 1989: 168), the step to the Solomons was qualitatively different. Buka Island cannot be seen from New Ireland. This 170 km crossing requires that one island is vacated before the other is sighted, although both landmasses are for a time simultaneously in view. Although no choice between accidental or deliberate travel can be suggested because under normal conditions a small boat could drift between these islands in four days (G. Irwin unpubl. data), the fact that it was achieved adds strength to the view that viable watercraft were in use by at least 28 000 years ago.

Intentional movement across the 30 km gap between New Britain and New Ireland is, however, demonstrated by *c.* 20 000 years ago with the regular transfer of New Britain obsidian into southern New Ireland after this date. The presence or absence of New Britain obsidian in the Solomons may provide a useful gauge of Pleistocene marine capabilities, but until a larger sample of Pleistocene sites is found there, the absence of obsidian from the Kilu site is of unknown significance.

It appears, then, that a case for the capability for inter-island travel at 40 000 years ago, whether deliberate or accidental, is strengthening. As Irwin (1989: 168–9) points out, this corridor of islands linking South East Asia and Greater Australia provided a 'perfect nursery' for developing maritime skills, having a chain of large, intervisible islands lying safely between belts of tropical cyclones to the north and south. If the capability to travel this corridor is greater than previously suspected, then models concerning the frequency of landings and founding population sizes, which are the basis for many reconstructions of the peopling of Greater Australia, may also vary in the future.

Finally I return to the notion of an invisible phase of human occupation of whatever length before *c.* 40 000 years ago. Almost universally, subscribers to this explanation need to overcome in their own models structural problems of population growth, the geographical spread of people and their adaptations to differing environments. Most have thus invoked the view that the earliest sites were on coasts now submerged by the post-glacial marine transgression 18 000 years ago. However, our first view of coastal sites older than this event, which are claimed to represent the initial colonisation of

Figure 3a.4: La Trobe graduate student Richard Cosgrove excavates into the deposits of Bone Cave. Sites such as this contain a richness of artefacts seldom seen in Pleistocene age sites anywhere in the world.

the east coast of southern New Ireland, suggests that occupation in this zone is no older than elsewhere in Greater Australia.

This is not to dismiss the invisible phase out of hand; indeed, until now, our best guess models of small founding populations have strongly implied that their archaeological correlates will seldom, if ever, be recognised. However, my own suspicion is that, as new evidence appears, this phase will not prove to be particularly long. The most recent evidence allows us to subscribe an inventiveness and adaptability to the Pleistocene colonists of Greater Australia which required no long period of acclimatisation to develop; one might argue that these traits were prerequisites to successful colonisation in the first place. The nearly simultaneous appearance of people throughout the diverse environments of Greater Australia between 30 000 and 35 000 years ago, rather than the sequential occupation of these different regions, argues strongly for this view.

Thus, to return to the beginning: whatever the biological history and antiquity of modern humans, we can point to a curious coincidence. Modern humans seem to have arrived in Western Europe, Greater Australia and the Americas at about the same time. The antiquity of humans in the New World has been the subject of decades of debate

(Owen 1984) but, from recently published evidence, it is difficult to reject claims for *c.* 32 000 years for the site of Pedra Furada in Brazil (Guidon & Delebrias 1986) and even earlier dates of *c.* 40 000 years ago are now claimed for the same site (Bednarik 1989: 103). In Western Europe, while the appearance of fully modern humans may indeed be later than the Middle/Upper Palaeolithic transition there (Gowlett 1987: 216–17), the appearance of anatomically modern humans and the cultural developments that take place subsequent to their appearance are highly significant in human history (Trinkaus 1986: 199–212). In this sense there is a fundamental change which is also a *sudden* change in archaeological terms.

In Western Europe we cannot sensibly postulate an invisible phase of modern human adjustments, merely rapid replacement (Stringer *et al.* 1984: 115–23). At present there is also no need to argue such an extended phase for Australia either. The theory of rapid colonisation proposed by Birdsell (1957) may yet prove closest to the truth. On present evidence, a diverse range of environments was sufficiently well-occupied to become archaeologically visible before 30 000 years ago, and there is little reason to go beyond 40 000 years ago to account for the initial landfall which led to this colonisation.

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3b New evidence from Fraser Cave for glacial age man in southwest Tasmania

Kevin Kiernan, Rhys Jones & Don Ranson

According to ethnographic sources of the early nineteenth century, the densely forested inland region of southwest Tasmania was not then occupied by Aborigines (Jones 1974). G.A. Robinson, who between 1829 and 1834 combed the island to meet most of the surviving Aborigines and persuade them to enter Government settlements, said of the region from a vantage point on the Arthur Range to the south on 13 March 1830 that 'there was not the least sign or appearance of natives or of any white man ever being in this part of the country. The natives that accompanied me assured me there was [sic] no natives ever went inland' (Plomley 1966). It was because of this very inaccessibility that a notorious maximum punishment convict station was established on a small island in Macquarie Harbour in 1825. However, comments from Goodwin, one of the few convicts to escape across the mountains from here, and other explorers suggest that there was at least seasonal Aboriginal use of tongues of grassy country on the Upper Gordon River to the east and around Frenchman's Cap to the north (Binks 1980: 124). A radiocarbon date of 300±150 yr BP (ANU 2787) from an open river bank site on the confluence of the Gordon and Denison rivers (Fig. 3b.1) discovered by an exploratory expedition led by two of us (D.R. and R.J.) in January 1981 shows that some fleeting visits were being made into parts of the region in immediate pre-European times.

Late Pleistocene human occupation east of Tasmania's principal longitudinal watershed has been demonstrated from the Beginners Luck limestone cave site in the Florentine Valley (Fig. 3b.1), an area which was densely forested at the time of European settlement. There, some 20 stone artefacts in a secondary depositional context were initially dated to ~12 kyr BP (Goede and Murray 1977: 2) but more precisely isolated material has since been radiocarbon dated to 20 650±1790 yr BP (Murray, Goede and Bada 1980: 142). Fossils of the extinct megafaunal species *Sthenurus occidentalis* from a nearby site gave an aspartic acid racemisation date considerably in excess of this (possibly of the order of 80 kyr), but they indicate radically different environmental conditions in the region some time during the Upper Pleistocene (Murray 1978: 126). The discovery of surface stone tools on industrially denuded ridges and moraines on the West Coast Range in 1979 (Corbett 1980: 191), and subsequent unpublished finds by one of us (K.K.) and J. Stockton, further demonstrated the possibility of prehistoric

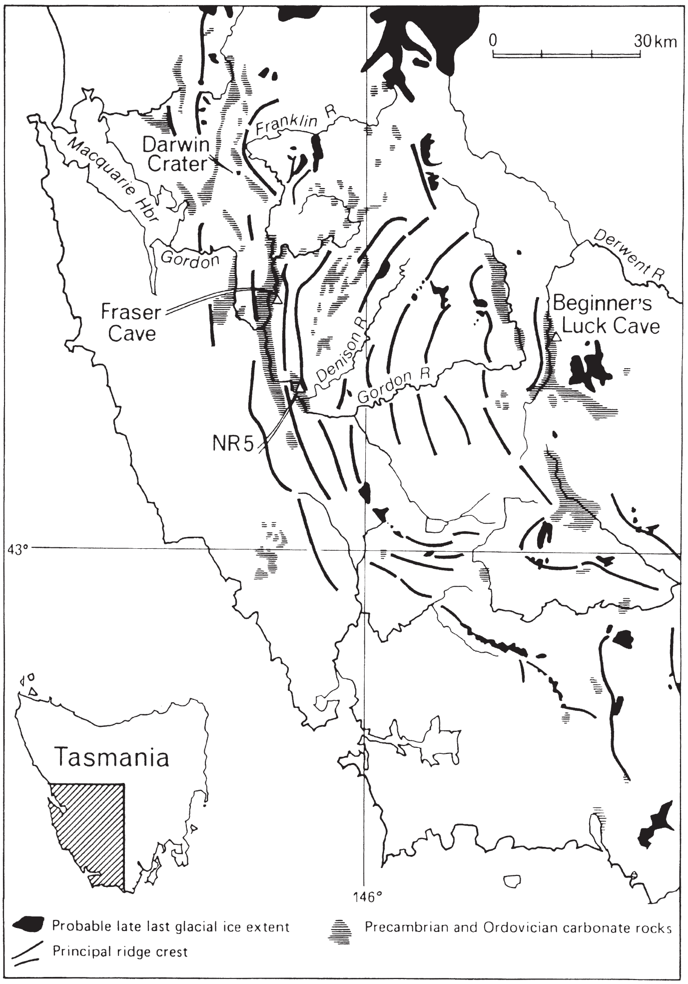

Figure 3b.1: Map of southwest Tasmania showing main structural features and outcrops of carbonate rocks; also location of glaciers at ~18 kyr ago.

remains existing elsewhere in southwest Tasmania, including the region of the Gordon and Franklin Rivers.

This region offers probably the least archaeological visibility of any in Australia, due to the dense vegetation, rapid peat growth and lack of exposure, but the two rivers run through extensive outcrops of Ordovician limestone where karst landforms are well developed (Fig. 3b.1). Since 1974 a series of pioneering speleological expeditions has documented numerous caves (Middleton 1979a), including in 1977 a large one called Fraser Cave (F 34), on the east bank of the Franklin River where original reports noted the existence of an extensive bone deposit (Middleton 1979b: 51). During a later visit in February 1981, Kiernan recognised stone tools and charred bones which indicated this deposit to be of human origin. Accordingly in March 1981 we went up the river to visit the site and conduct a pilot investigation.

Fraser Cave (F 34)

Fraser Cave is a relict outflow stream cave which lies at the head of a resurgence valley 35 m distant from and 10 m above the present level of the Franklin River. It comprises about 200 m of large passages with at least eight surface openings. The largest entrance, which faces north, is 12 m wide and up to 3 m high and gives easy level access to a spacious chamber (Fig. 3b.2). This chamber is 5 m high, and extends inward for 18 m with a width of between 5 and 12 m. The passages are developed along the strike of the limestone beds which dip to the east at a moderate angle. After having been initiated in phreatic conditions, the cave was further elaborated by the running waters of two small streams which are now intermittent. While some inner sections of Fraser Cave are richly decorated by calcium carbonate speleothems, notably rimstone pools, the downstream areas are dominated by clastic deposits comprising 20–40 cm of moderately rounded fine gravels. These are at present immobile and are subject to manganese encrustation. The gravels are externally derived and consist primarily of Siluro–Devonian metasediments washed from the hillslopes to the east. They are overlain by poorly exposed fine sands which contain at least some stone tools. The sand is in turn overlain by flowstone calcite. This sequence is interpreted as reflecting successive loss of competance by the cave streams. The reduction in clastic load and stream flow permitted precipitation of the flowstone. Piping has since removed some of the sands from beneath the flowstone and a few roof-fall blocks lie scattered on its surface beneath a high level entrance.

Excavation stratigraphy and chronology

Within the main entrance chamber, and forming a talus at its mouth, is a poorly sorted clastic deposit between 1 and 3 m thick. At a distance of 12 m in from the entrance on the western side of the chamber, this deposit forms a flowstone-capped bank some 0.7 m high. Numerous stone tools, burnt bones and charcoal protrude from the slightly eroded edge of this bank. A small pilot excavation was undertaken into this face to ascertain the stratigraphical and chronological context of the archaeological remains and to obtain samples for palaeoenvironmental analysis. The excavation pit was 1 m wide and reached the rock floor at a maximum depth of 1.30 m. It revealed a complex stratigraphy as shown in Figure 3b.3. All of the excavated material was wet-sieved using a 3-mm mesh. The charcoal was floated off and stone artefacts and bone material were removed for analysis.

The stratigraphy of these entrance facies may be broadly differentiated into three main complexes. The basal alluvial deposits are similar to those which occur elsewhere in the cave. Gravels are interbedded with lenses of fine sand, while cut and fill structures are common. The lowermost gravel is encrusted by manganese, which indicates

Figure 3b.2: The entrance chamber of Fraser Cave.

the presence of a surface vegetation sufficient to mobilise the mineral before its burial by 10–15 cm of fine laminated sands (unit 2). The most stratigraphically recent gravel (unit 3) has a mean calibre of ~2 cm and contains numerous bone fragments, stone tools and charcoal. A charcoal sample which was obtained from this unit was radiocarbon assayed at 19 770±850 yr BP (ANU-2785) (Table 3b.1).

Alluvial sedimentation was subsequently overwhelmed by a major influx (units 6–10) of angular limestone rubble in a loamy matrix, the rubble stones having a maximum calibre of 10–15 cm. We believe that these units are the product of mechanical weathering in cold climatic conditions. Interspersed between the rubble layers, which are rich in bones and stone tools, are lenses of fine laminated sands (units 5, 7 and 9). These do not contain any cultural debris, and indicate episodic wetness. A radiocarbon assay of charcoal from the upper horizon of this rubble in unit 10 gave a date of 15 670±530 yr BP (ANU-2783).

The subsequent black hearth and rubble complex retains much of its general character. The matrix in which the rubble occurs is heavier and richer in clay, which seems to indicate generally damper conditions. Stone tools and bone are extremely abundant and the clay forming the lower boundary of the complex is baked red by prehistoric fires. A radiocarbon assay of 17 020±310 yr BP (ANU-2782) was obtained from a hearth unit (unit 13) in the base of this complex. This inversion with the stratigraphically lower sample ANU-2783 cannot presently be explained but further samples will be submitted from the top of this limestone rubble to gain greater precision as to its age.

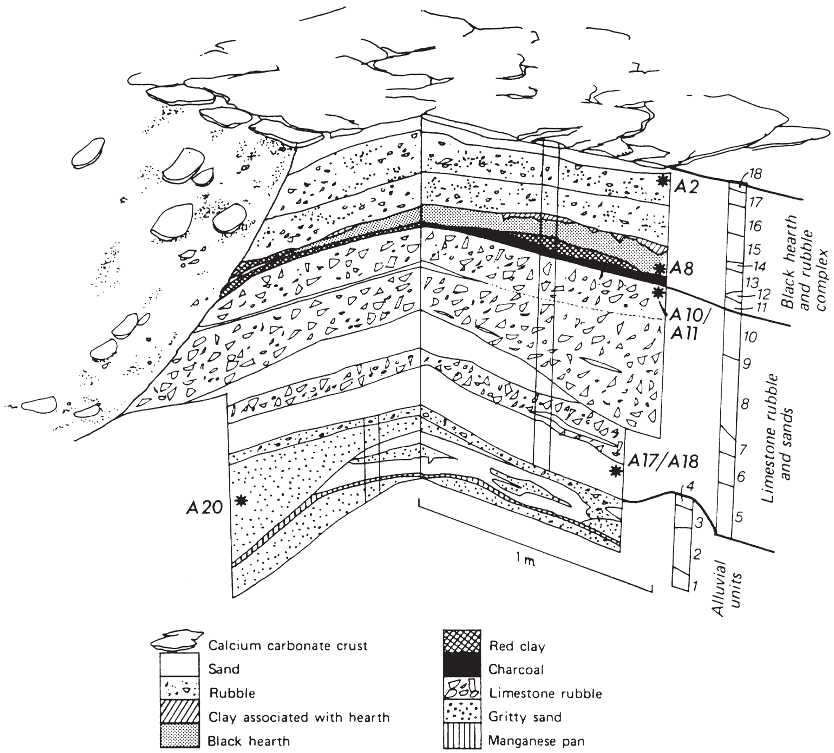

Figure 3b.3: Stratigraphical section of excavated archaeological deposits at Fraser Cave, showing main units and location of radiocarbon samples.

Table 3b.1: Radiocarbon assays of various excavation and stratigraphical units

^{14}C code no. (ANU)	*Excavation unit*	*Depth below surface (cm)*	*Stratigraphical unit*	*Results (yr BP)*
2781	A2	3–5	16 top	14 840±930
2782	A8	23–26	13	17 020±310
2783	A10/11	32–39	10	15 670±530
2784	A17/18	79–87	5	Samples too mineralised
2785	A20	94–96	3	19 770±850

The top date for the hearth and rubble complex was obtained only 3–5 cm below the surface and gave a result of 14 840±930 yr BP (ANU-2781). This dates the uppermost of the horizons which contain tools. Capping this complex is a layer of sterile sand which is 1–3 cm thick, the surface of which is cemented by a crust of calcium carbonate.

The radiocarbon dates indicate the presence of man in Fraser Cave from ~20 kyr BP until ~15 kyr BP. These dates are consistent with the sedimentological evidence which suggests that this occupation corresponded in time to the Last Glacial Maximum. Although it is difficult to separate climatic and anthropogenic effects, the abundance of charcoal in the basal alluvial sequence beneath the entrance facies of Fraser Cave begs the question as to the role that humanly induced fires may have played in destabilising the slope mantle in the vicinity of the site.

The onset of conditions of extreme cold in western Tasmania is reflected by slope instability at Henty Bridge on the coast-facing margin of the West Coast Range, dated at 23 860±890 yr BP (Gak-5594) (Colhoun *et al.* 1979). In the central part of the West Coast Range, 55 km north of Fraser Cave conditions of maximum cold occurred about 5 kyr later. Drifted wood in proglacial silts which immediately predate the arrival of glacial outwash gravels from the Dante Rivulet Valley has been radiocarbon assayed at 18 800±500 yr BP (ANU-2533). These dated silts overlie intact flowering specimens of the alpine cushion plant bolster *Donatia novae zelandiae* which imply depression of the treeline to below 230 m (ref. 11). Here a temperate depression of 5.8°C is indicated by reconstruction of glacial snowline altitudes. A slightly greater figure of 6.3°C is indicated 35 km further inland to the east in the headwaters of the Franklin River, where the glacial snowline was depressed to ~1040 m. In this latter area, diffluent ice from the glacier system in the Derwent Valley on the margin of the central plateau ice cap breached the Derwent–Franklin divide and spilled into the Franklin headwaters (Fig. 3b.1). Here it merged with local ice which accumulated in the lee of a snowfence aligned north–south to form a valley glacier which flowed 12 km down the upper Franklin River valley (Kiernan 1980). Minor cirque and valley glaciers also arose further downstream in the Franklin catchment, notably within the Frenchmans Cap massif 25 km north of Fraser Cave (Fig. 3b.1). Deglaciation appears to have been complete in Tasmania by ~10 kyr BP. The angular limestone rubble in Fraser Cave was produced in cold climatic conditions during this glacial stage and the radiocarbon dates which bracket the rubble and occupation probably broadly delinate the period of maximum cold.

Archaeological remains

Although the total excavation measured ~0.67 m^3 in volume, some 75 000 stone flakes and tools were recovered as shown in Table 3b.2. This sample is estimated to be <<1% of the total artefact-bearing deposit. Because only ~100 stone artefacts were recovered *in situ* from the only other directly dated Pleistocene sites in Tasmania, namely Beginner's Luck Cave on the Florentine River (Figs 3b.1 and 3b.4) and Cave Bay Cave (Bowdler 1974: 697; Bowdler 1977: 205) on present-day Hunter Island (Fig. 3b.4), it can be appreciated that the Fraser Cave assemblage has the potential to transform our present knowledge of Tasmanian late Pleistocene stone technology. The oldest artefact

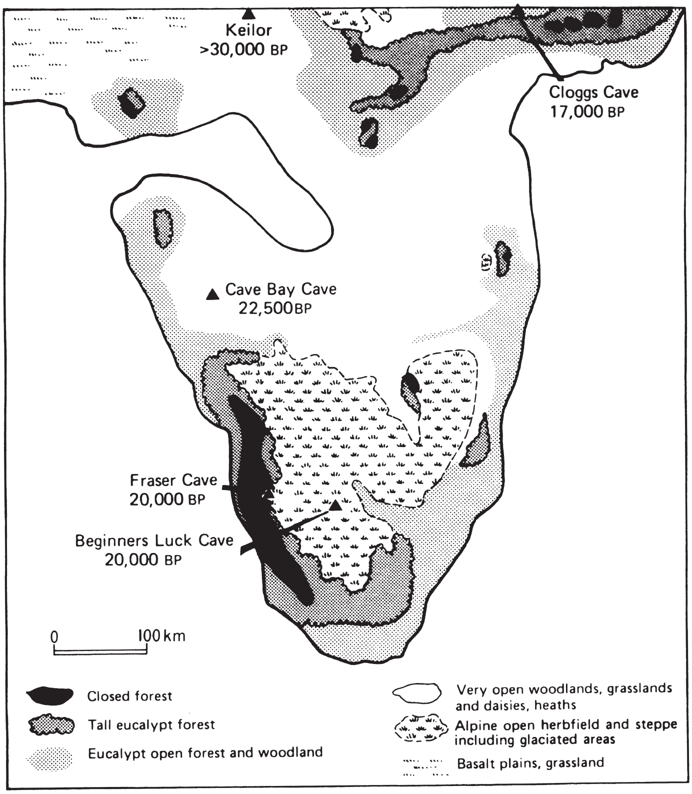

Figure 3b.4: General reconstruction of major vegetation types and coastline of the Bassian-Tasmanian peninsula at *c.* 18 kyr ago, adapted from ref. 27. Location of prehistoric sites shown with their basal radiocarbon dates.

was a single flake recovered from within the basal stratigraphical unit 1, which indicated some occupation probably before 20 kyr BP. However, several hundred flakes and worked tools together with much charcoal were found within the succeeding sands and gravels of unit 3. In almost all the succeeding units, especially the limestone rubble layers, there are numerous stone tools, the only sterile layers being the sand lenses (units 5, 7, 9). In the uppermost complex, there are superimposed hearths with lenses of red-baked clay and abundant charcoal flecks. Occupation ceased suddenly after unit 16 which is dated to ~15 kyr ago. Then the deposit was covered by a thin flow stone, and there is no indication of subsequent human occupation or use of the cave.

The raw materials used for making the stone tools were mostly cobbles of fine-grained siliceous rocks, which could be obtained easily from glacial outwash gravels in the Franklin riverbed nearby. An exception to this lies in a small number of stone tools made from Darwin Glass, which is an impactite associated with a large meteorite crater in the tributary Andrew River Valley 25 km to the northwest (Fig. 3b.1). A westward splash pattern away from the Franklin River has been demonstrated for this material (Fudali and Ford 1979: 283), which suggests that these pieces of glass are manuports. Darwin Glass artefacts appear in the sequence from the unit 8 rubble upwards, indicating that a considerable mineralogical knowledge of this geologically complex region of southwest Tasmania had been achieved by this time.

Typologically, the tools consist mostly of steep-edge scrapers and domed core-scrapers with steep edges that are often at right angles and show extensive stepped flaking. There are also small round 'thumbnail' scrapers and many retouched flakes. In general this assemblage bears a close resemblance to the tools from the lowest levels of the South Cave, Rocky Cape, on the north coast of Tasmania which have been dated to between 8 and 6 kyr ago (Jones 1971). The Fraser Cave sequence thus fills a large part of a crucial gap between the base of Rocky Cape and the brief palimpsest of the 20–22-kyr-old occupation of Cave Bay Cave (Bowdler 1974: 697; Bowdler 1977: 205; Bowdler 1979). From these sites we now have an almost continuous sequence extending from the stone tool technology of the ethnographically recorded Aborigines of the early part of the last century back to just before the Last Glacial Maximum in Tasmania. The Fraser Cave assemblage is also typologically similar to near contemporary industries on the Australian mainland, such as the Lake Mungo assemblage which is dated to ~25 kyr BP (Bowler, Jones, Allen and Thorne 1970: 39; Bowler, Thorne and Polach 1972: 48). All these tools belong to what has been termed the 'Australian core tool and scraper tradition'. It has long been assumed that Tasmanian stone industries were derived from this technological tradition at a time when Tasmania formed part of the single landmass of Greater Australia (Jones 1973: 278). The Fraser Cave assemblage confirms this. After the post-glacial inundation of Bass Strait, this tradition continued in Tasmania with only slow internal evolutionary changes towards a reduction of average size of tools. On the mainland, however, there were transformational changes associated with the introduction or invention in mid-recent times and later of a variety of small gum-hafted tools such as backed microliths, points and adzes (Mulvaney 1975: 210; Lorblanchet and Jones 1979: 463).

Ochre fragments were found in almost all units above the limestone rubble (unit 6) showing that this pigment was being carried into the cave at this time. Despite an intensive search, no signs of rock art were seen on the walls of the cave.

Bone fragments were found in all of the units which contained stone tools (Table 3b.2), and were absent in culturally sterile layers. From a preliminary analysis based on mandible and maxilla counts, ~90% of the bones are of the large wallaby *Macropus rufogriseus* and about 8% are of the wombat *Vombatus ursinus*. The remaining 2% consist of Tasmanian Devil *Sarcophilus harrisii* and various small mammals. Both Bowdler (Bowdler 1979) and Balme (Balme 1980: 81) have independently proposed criteria for distinguishing cave bone accumulations which have resulted from non-human predation such as that by owls and Tasmanian Devils, as opposed to the middens of human hunters. For the latter they suggest an overwhelming preponderance of one or two large game species; a substantial number of bones showing calcination or other evidence of fire; and bones, especially long bones, which have been smashed to obtain the marrow. The fulfilment of these criteria, together with the easy access into and out of Fraser Cave, demonstrate that this bone assemblage is a human midden.

Table 3b.2: Occurrence and size of stone flakes, tools and bone fragments

			Stone flakes and tools					Bone fragments	
			Weight		No.			Weight	
Stratigraphical complex	*Stratigraphical units*	*Vol. (m^3)*	*(kg)*	*(kg m^{-3})*	*>1 cm in length*	*<1 cm in length*	*Total*	*(kg)*	*(kg m^{-3})*
Black hearths and rubble	16–11	0.133	10.9	82	900	15 200	16 100	6.0	45
Limestone rubble	10–5	0.318	58.0	182	6800	51 200	58 000	28.5	90
Alluvial units	4–3	0.089	1.6	18	250	450	700	0.07	8
		0.54	70.5		7950	66 850	74 800	35.2	

From our preliminary analysis, there do not seem to be any representatives of extinct megafaunal species. This is consistent with the revised analysis of the Beginner's Luck site where the 20-kyr-old stone artefacts are associated with bones of modern fauna, mostly *Macropus rufogriseus*, with only one cuboid of a large macropod, cf. *M. titan* (Murray, Goede and Bada 1980: 142). The Fraser Cave assemblage supports the view that by the time of the Last Glacial Maximum, most or all elements of Tasmania's megafauna were already extinct, and that the animals living in these glacial valleys were modern. Such evidence, which contradicts a previously held view (Goede, Murray and Harmon 1978: 139), has important implications in the debate concerning a climatic or human causation for the extinction of the 'giant' marsupials. The faunal evidence from the cave also dispels a previous theory that the Last Glacial hunter-gatherers of the Tasmanian peninsula were incapable of systematic hunting of medium-sized land game but were effectively restricted to the width of one band's territory from the coast (Bowdler 1977: 205; Jones 1977: 317; Bowdler 1981: 6). At the time of its occupation, Fraser Cave would have been ~60 km in a direct line across rugged, snow-bound mountains or 100 km down the most practicable river route from the contemporary coastline (Figs 3b.1 and 3b.4).

The inhabitants of Fraser Cave hunted game with a tight targetting strategy which concentrated on wallabies. These would have been obtained from the open valley slopes which were perhaps cloaked by sub-Antarctic herbfield vegetation. The evidence from the cave suggests a high biomass of these animals having existed in the region during glacial times. The rainforest may have existed within gallery refugia along the main rivers and possibly in areas closer to the lower coastline including the exposed trough of what is now Macquarie Harbour. This is indicated schematically in Figure 3b.4 which we derive from a general reconstruction of the full Last Glacial vegetation by Hope (Hope 1982). We argue that as the climate ameliorated, and conditions became wetter, these forests emerged from their glacial refugia and, despite any effects of man and his firesticks, re-occupied the higher valley slopes of the Franklin and Gordon rivers. This dense rainforest habitat was not conducive to hunters, and so Fraser Cave was abandoned, not to be occupied for the next 15 kyr.

Hunters close to southern ice

In addition to the great richness of this site, its significance must be seen in the context of the human colonisation of the western rim of the Pacific. The colonisation of Australia–New Guinea across the water barriers of Wallacea occurred some time before 40 kyr BP (refs 19, 28). Between at least 50 kyr BP and ~24 kyr BP, the sea was lower than its present level (Chappel and Thom 1977: 275), but it may not have been low enough to expose the entire Bassian bridge, hence the final southward movement of the Australian colonists was stemmed (Jones 1977: 317; Bowdler 1981: 6). Then with the onset of the Last Glacial Maximum, the sea dropped below the crucial 60-m isobath, and exposed a dry route to the Tasmanian peninsula. That man immediately seized this opportunity to expand his range is shown by the near contemporary basal occupation of 22.5 kyr BP at Cave Bay Cave (Bowdler 1977: 205; Bowdler 1979), and from possibly slightly earlier than 20 kyr BP at Fraser Cave and Beginner's Luck Cave (Murray, Goede and Bada 1980: 142). In southern Tasmania, these hunters were then the most southerly human beings on Earth. It would not be until 10 kyr BP that equivalent areas in Fuego–Patagonia were to be occupied by man (Bird 1938: 250; Borrero 1977: 81). At 18 kyr ago, the Tasmanians alone were as close to the great Antarctic ice sheet, then only some 1000 km further to the south (Hays, Lozano, Shackleton and Irving 1976: 337), as some Upper Palaeolithic hunters of Europe were to the northern ice sheets. The Tasmanian hunters probably lived in tundra environmental conditions similar to those which existed in parts of northern Europe. The specific targetting onto reindeer by the European hunters bears comparison with the similar emphasis on wallabies by the sub-Antarctic Palaeo-Tasmanians.

Southwest Tasmania therefore offers a fascinating southern analogue for the study of hunters of the Last Glacial Maximum. However, the future of the Franklin–Gordon archaeological sites is threatened by plans now being implemented of the Tasmanian Hydro-Electric Commission to inundate these valleys. This would flood Fraser Cave together with other unstudied sites which have been located within recent months in the wake of the original discoveries.

Acknowledgements

Our exploratory work in the field in 1981 was supported by the Australian National University, the Tasmanian National Parks and Wildlife Service and the Tasmanian Wilderness Society. We thank our colleagues during the excavation; Barry Blain, Bob Burton, Steve Harris and Greg Middleton; and Hendrick Gout for free helicopter transport of our finds. John Head paid special attention to our dating needs and Winifred Mumford drew the figures.

3c Pleistocene dates for the human occupation of New Ireland, northern Melanesia

Jim Allen, Chris Gosden, Rhys Jones & J. Peter White

The oldest secure date for human occupation in Greater Australia is now 40 kyr BP (Groube, Chappell, Muke and Price 1986: 453–5) for several stone artefacts considered to be in a primary depositional situation between tephras on the uplifted coral terraces of the Huon Peninsula in eastern New Guinea (Fig. 3c.1). Marginally younger dates are claimed from both the southeast and southwest of Australia (White and O'Connell 1982; Pearce and Barbetti 1981: 173–8), while dates in the New Guinea highlands approach 30 kyr BP (Gillieson and Mountain 1983: 53–62).

East of New Guinea, the large islands of New Britain and New Ireland were kept separate during the Pleistocene by narrow but deep water barriers. Until now a date of 11 kyr BP from Misisil Cave in southwest New Britain (Specht, Lilley and Normu 1981: 13–15) was the only indication that humans had crossed the first of these barriers by the terminal Pleistocene.

In 1985, as part of the Lapita Homeland Project (Allen 1984: 186–201), excavations were carried out in a series of caves in uplifted coral limestone in northern and central New Ireland. The three sites reported here all have 1–2 m of stratified deposits, containing stone tools, bone and marine shell food debris, hearths and pits. Radiocarbon assays from these sequences (Table 3c.1) indicate human occupation extending back to *c.* 14 kyr BP at Balof 2, *c.* 15 kyr at Panakiwuk and *c.* 33 kyr at Matenkupkum.

Matenkupkum is a cave 50 m from the present shore and 15 m above it, containing 1.4 m of cultural deposits overlying sterile beach sand and bedrock. Of the seven cultural layers distinguished, the earliest (Layer 7) is dense marine shell midden (Fig. 3c.2). More than 200 flaked stone tools were also recovered from this layer as well as terrestrial animal bones. Its sandy brown matrix was easily distinguished from the sterile beach sand below, leaving no doubt that the earliest deposit above the beach sand is an intact occupation of human derivation. The shells which provided the dating material were part of this human event.

All Matenkupkum shell dates used a single species of gastropod, *Turbo argyrostoma*. From Layer 7, ANU-5070 was divided into degraded (possibly burnt) and undegraded fractions which produced very similar results; two other determinations (ANU-5065

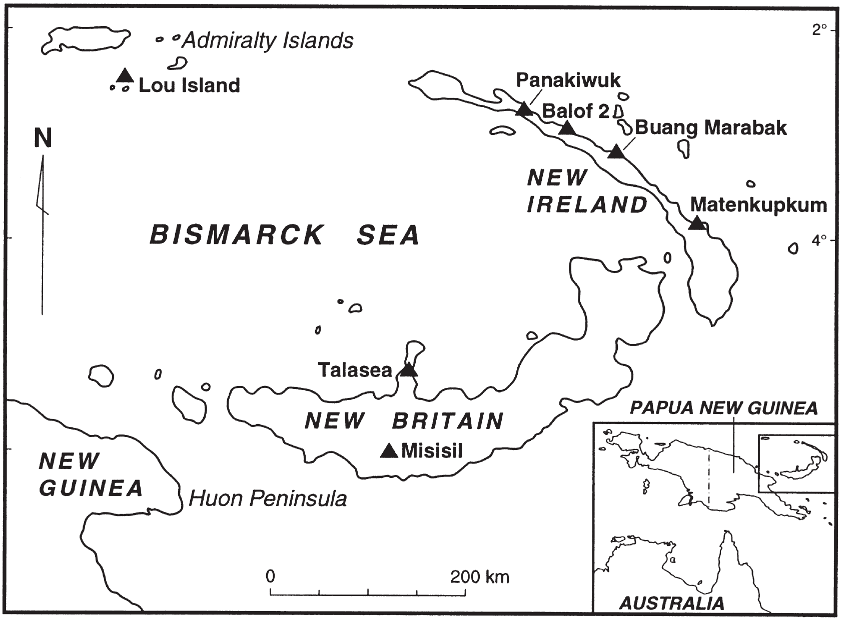

Figure 3c.1: Bismarck Archipelago showing archaeological sites and other principal locations mentioned in the text.

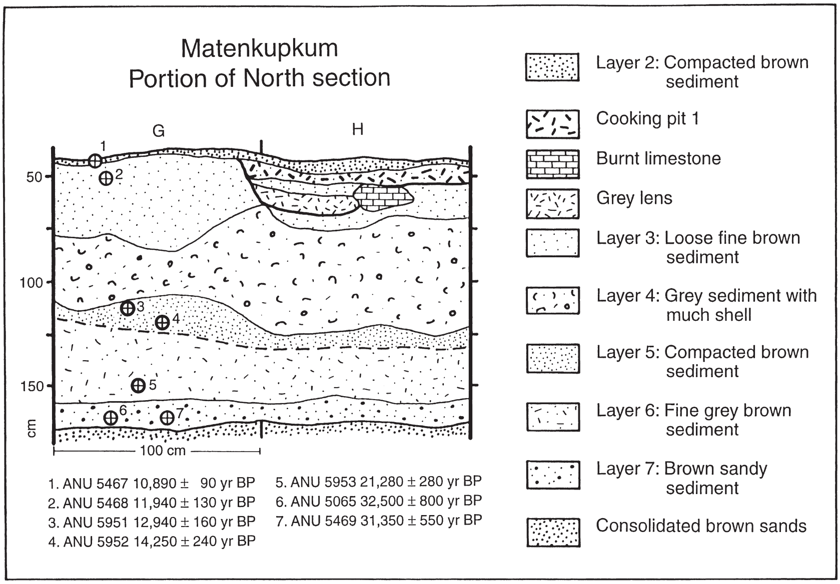

Figure 3c.2: Stratigraphic section of Matenkupkum, squares G and H, showing the locations of seven of the radiocarbon dates reported in this paper.

Table 3c.1: Radiocarbon dates for Matenkupkum, Balof 2 and Panakiwuk

Matenkupkum *Layer*	*Material*	*Date* BP	*Laboratory number*
1	Charcoal	Modern	ANU-5066
1	Charcoal	Modern	ANU-5067
2	Shell	10 890±90	ANU-5467
3	Charcoal	Modern	ANU-5068
4	Charcoal	Modern	ANU-5069
4	Shell	11 940±130	ANU-5468
5	Shell	12 940±160	ANU-5951
5	Shell	14 250±240	ANU-5952
6	Shell	21 280±280	ANU-5953
7	Shell	31 350±550	ANU-5469
7	Shell	32 500±800	ANU-5065
7	Shell (degraded)	33 300±950	ANU-5070
7	Shell (undegraded)	32 700±1550	ANU-5070

Balof 2 *Depth below surface (cm)*	*Material*	*Date* BP	*Laboratory number*
45–52	Charcoal	3 120±190	ANU-4972
70–80	Charcoal	7 680±510	ANU-4849
122–130	Charcoal	10 560±230	SUA-2502
142–148	Charcoal	9 970±390	ANU-4973
170–174	Charcoal	14 240±400	ANU-4848

Panakiwuk *Stratigraphic unit*	*Material*	*Date* BP	*Laboratory number*
1	Charcoal	640±130	ANU-5529
2	Charcoal	1 040±110	ANU-5530
2	Charcoal	1 170±70	ANU-5376
4	Charcoal	1 630±130	ANU-5531
6	Bone	8 910±690	ANU-5547
6	Bone	10 160±390	ANU-5546
8	Bone	8 480±500	ANU-5545
8	Bone	10 300±310	ANU-5544
9	Bone	6 780±1220	ANU-5542
9	Bone	8 000±830	ANU-5541
9	Bone	8 530±520	ANU-5540

Table 3c.1: Cont'd

Panakiwuk *Stratigraphic unit*	*Material*	*Date* BP	*Laboratory number*
11	Charcoal	12 930±210	RIDDL-316 (ANU-4978)
13	Charcoal	15 140±160	RIDDL-531

Note: The dates quoted here are in radiocarbon years using the Libby half life of 5568 years. The marine shell dates have not been corrected for oceanic reservoir effect; in Australia the accepted correction is –450±35 BP. All bone dates relate to the apatite fraction of the bone. All dates have been corrected for $^{12}C/^{13}C$ ratios.

and ANU-5469), 6 m away from the first sample, yielded very similar results. Three of these four determinations overlap with each other at one standard deviation. The exception (ANU-5469) does so at two standard deviations. All fall comfortably within the maximum range of 30–36 kyr BP. Above these, the single Layer 6 date (ANU-5953) confirms the impression of intermittent occupation and gradual accumulation of these lowest deposits gained in the field. The two Layer 5 dates (ANU-5951 and ANU-5952) taken from immediately above ANU-5953 indicate that there was minimal use of the site during the height of the Last Glacial Maximum. Layers 4, 3 and 2 contain the greatest concentrations of bone, stone and shell discard and are dated by ANU-5467 and ANU-5468 to the terminal part of the Pleistocene. It appears that there was little human use of Matenkupkum during the Holocene and the thin deposits constituting Layer 1 contain glass and metal artefacts relating to Japanese occupation of the cave during the Second World War. Two charcoal samples (ANU-5066 and ANU-5067) from Layer 1 yielded concordant modern results. Two further charcoal dates from Layers 3 and 4 (ANU-5068 and ANU-5069) did, however, produce anomalous modern results. These are under further investigation but we currently reject them on other archaeological evidence. Matenkupkum contains no pottery, no *Rattus exulans* remains and no Lou Island obsidian from the Admiralties group. These three data sets occur very commonly throughout the region in sites younger than ~3 kyr BP and all occur, for example, in the upper levels of the two other New Ireland cave sites discussed here.

Continued coastal and marine foraging would have been facilitated by the steeply sloping shoreline in front of the cave. Around 30 kyr BP sea-levels were *c*. –50 m compared with the present, dropping to a minimum –150 m at the Last Glacial Maximum. Rising temperatures after 15 kyr saw sea-levels approaching present levels ~10 kyr BP, finally stabilising at ~6 kyr BP (Chappell and Thom 1977: 275–91; Hope, Golson and Allen 1983: 37–60). Although these fluctuations must have affected reef and shell-bed formation, they would not have greatly increased the distance between the cave and the sea. In general, the same is true of the Balof 2 shelter, which although now ~1 km from the coast, also contains marine shells, stone tools and animal bones, scattered through its Pleistocene layers. This shelter contains 1.8 m of cultural deposits overlying sterile clay. The lowest 0.5 m of deposition is contained in a clayey matrix with lenses of bat guano; above this the matrix is greyer and homogenised by human and other taphonomic agents. A hearth dug into the basal sterile clay provided the charcoal for ANU-4848. ANU-4973 and SUA-2502 give an acceptable date for the top of the clayey unit which coincides with the approximate end of the Pleistocene. Although the Pleistocene occupation of Balof 2 may have been episodic, the amounts of human discard are not small. This point is of particular interest because in 1969 Balof 1, a shelter only a few metres from Balof 2 and in the same doline, yielded a basal date of 6800±400 BP (NSW-95) (Downie and White 1979: 763–802). Thus, although both sites were occupied in the Holocene, only Balof 2 has a Pleistocene component.

The third site, Panakiwuk, is situated in a limestone doline ~4 km from the coast. Here, 1.6 m of cultural deposits overlie sterile basal clays. Apparent roof fall has complicated the stratigraphy of the Pleistocene deposits. Human occupation, however, is demonstrated by the presence of stone tools and faunal food debris in the layer preceding that dated by RIDDL-531. Both this determination and RIDDL-316 are accelerator-mass-spectrometry dates on small amounts of charcoal from hearths. Human use of the site during this period, although clearly demonstrated, appears quite episodic. Successive layers indicate a more systematic use of the site into the early Holocene. The suite of dates listed in Table 3c.1 which relate to this period (Stratigraphic Units 6, 8 and 9) contain some anomalies, possibly related to mixing in the only available dating medium in these layers, hundreds of small rat bones. A number of these dates are considered to be minimal because of the possible presence of younger carbonates. Panakiwuk appears to have been abandoned *c.* 8 kyr BP and not reused until *c.* 2 kyr BP.

These results treble the known time-scale for humans in the northern Melanesian islands. They not only relate the initial colonisation of these islands to the initial colonisation of Greater Australia, but also suggest that the Solomon Islands to the southeast may have been colonised during the Pleistocene as well, as no greater water crossings were involved than those already traversed to reach New Ireland, or indeed to cross Wallacea to Greater Australia.

The second import of the dates presented here is that although marine and coastal subsistence patterns of late Pleistocene hunter-gatherers are often postulated, higher Holocene sea-levels have usually obliterated the evidence. Older marine shell middens are known in both southern and northern Africa (Klein 1977: 121–6; Singer and Wymer 1982; McBurney 1967), but those spanning the final 20 kyr of the Pleistocene are infrequent. The suspected rapid uplift of the New Ireland east coast and its plunging profile below present sea-levels have preserved in Balof 2 and Matenkupkum important evidence of the human exploitation of these coastal resources during this crucial period of the terminal Pleistocene.

Acknowledgements

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3d Pleistocene occupation in arid Central Australia

M.A. Smith

Puritjarra rockshelter is in the Cleland Hills, a series of low hills of Devonian sandstone outcropping in a dunefield, 75 km west of the main MacDonnell ranges (Fig. 3d.1). The site is formed at the base of a small escarpment where a large overhang provides around 400 m^2 of level, shaded floor area and which is close to the only permanent water in the hills. Occupation of the site continued until the 1930s when the area was depopulated by the migration of Aboriginal people into missions and ration depots in the western MacDonnell ranges.

Initial analysis of the site has concentrated on stratigraphy and chronology, and on a preliminary examination of associated archaeological material from two 1 m × 1 m pits (designated N9 and N10), representing half of the area excavated. All excavated material was dry sieved in the field using 3-mm and 6-mm mesh. The sieve residues were transported to the museum where the charcoal was floated off and the stone artefacts and bone material removed for analysis.

Four radiocarbon dates have been obtained from Pit N10 (Table 3d.1). For Beta-18882, Beta-18884 and Beta-19901 the material dated was scattered charcoal pieces recovered from the sediment by flotation. Beta-18883 dates a single large lump of charcoal found embedded in Layer II sediments during excavation.

The deposit consists of three well-defined stratigraphic layers (Fig. 3d.2). Extending from the present surface (0 cm) to a depth of 42 cm, Layer I is a loose, gritty, light brown sand (Munsell colour 5YR 5/8) containing lenses of rockfall, intact hearths, charcoal, flaked stone artefacts, grindstones, ochre and emu egg-shell. It gives evidence of a major increase in occupation of the region during the last one thousand years, a change shown in more detail in other sites (Napton and Greathouse 1985: 90–108; Smith 1986: 123–30).

Evidence for Pleistocene occupation occurs in Layer II, which consists of compact, fine, red clayey sand (Munsell colour 2.5YR 5/8). The lowest artefacts were recovered from the middle of this layer (66–77 cm) associated with charcoal dated to 21 950±270 yr BP. On present evidence the date of 22 440±1370 yr BP, from the lower part of Layer II, predates occupation of the rockshelter.

Layer III (101–212 cm) consists of well-rounded rubble in a matrix of loose, fine dark red sand (Munsell colour 10R 5/8). No definite evidence of occupation was found in Layer III. The full depth of the layer was not established nor was bedrock reached.

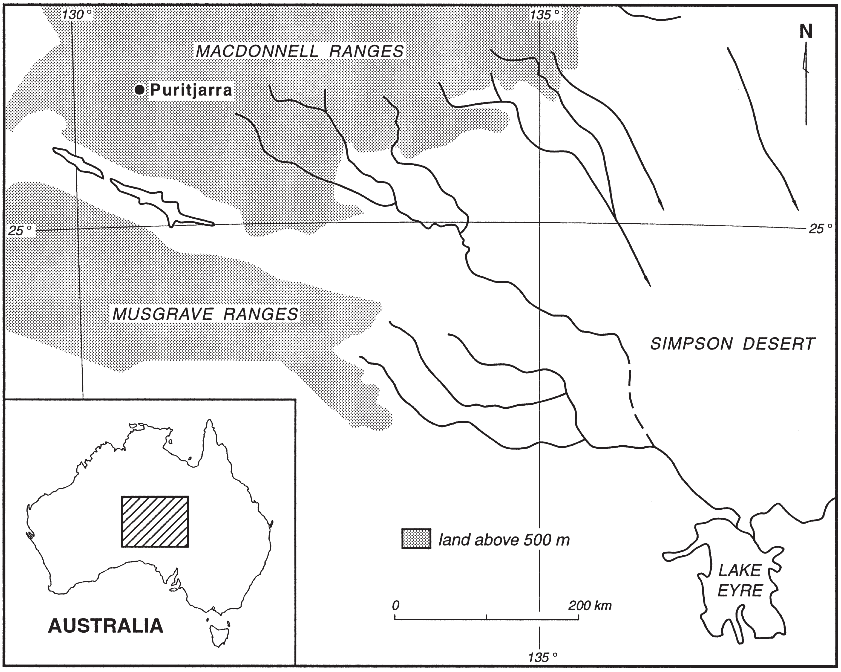

Figure 3d.1: Map of Central Australia showing the location of Puritjarra rockshelter.

Table 3d.1: Radiocarbon dates at Puritjarra rockshelter

Excavation unit	*Depth (cm)*	*Layer*	*Sample no.*	*Date (yr* BP*)*
N10/6	32–42	I	Beta-18882	5 860±150
N10/9	53	II	Beta-18883	12 020±240
N10/11	66–77	II	Beta-19901	21 950±270
N10/13	89–101	II	Beta-18884	22 440±1370

Note: Depths are given in cm below surface.

The use of Puritjarra rockshelter between 22 000 and 12 000 yr BP was low and is represented by the deposition of only a few artefacts per millennium (Table 3d.2). Generalised palaeoenvironmental data for the period from 25 000–16 000 yr BP indicates widespread drying of lakes, extensive construction of aeolian dunes and an expansion of the arid zone on both northern and southern margins (Bowdler and Wasson 1984: 183–208). Studies of pollen and sediments are planned to establish the conditions that prevailed at Puritjarra during this period.

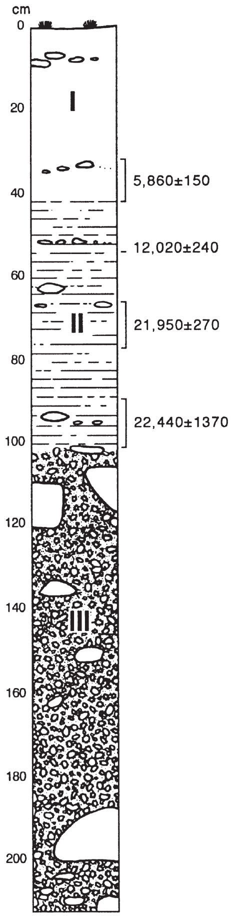

Figure 3d.2: Schematic stratigraphic diagram showing the location of radiocarbon samples. Layer I is a loose, gritty, light brown sand. Layer II consists of compact, fine, red clayey sand. Layer III consists of sandstone rubble and fine, dark red sand.

The artefacts from the Pleistocene levels suggest the production of large flakes and large flake implements. However, the presence of a small blade core, and of several parallel-sided elongate flakes from a similar core, show that this early assemblage should not simply be characterised as a large flake industry. A small piece of red pigment was found associated with the basal occupation.

From about 6000 yr BP the shelter was used more frequently but the period of greatest use occurs in the upper 10–14 cm of Layer I, estimated to post-date 1900 yr BP. This period of more intensive use is reflected not only in an increase in chipped stone artefacts and charcoal but also in the concentration of grindstones and emu eggshell in these levels.

Distinctive late Holocene artefacts, such as backed blades and tula adzes, first appear in the middle levels of Layer I (18 cm), estimated to date from about 3000 yr BP.

The significance of this site must be seen in the context of the human colonisation of the Australian arid zone. This region is presumed to have been the most difficult

Table 3d.2: The distribution and concentration of flaked stone artefacts in Layers I–III, Pits N9 and N10 combined

Layer	Volume (m^3)	No. of artefacts	Total weight (g)	Estimated no. artefacts per m^3	Estimated no. artefacts per 1000 yr
I upper	0.26	1252	1981.8	4815	656
I lower	0.64	860	2889.9	1344	210
II upper	0.55	92	2575.2	167	6
II lower	0.49	—	—	0	0
III	0.55	—	—	0	0

Note: Layer I is arbitrarily divided into an upper part estimated to post-date 1900 BP, and a lower part dating from about 6000–1900 BP. Layer II is divided into an upper part containing artefacts and a lower part in which artefacts are absent.

of any Australian environment encountered by Pleistocene immigrants from the Indo-Malaysian region and its settlement therefore provides some indication of the adaptability of these early human groups. It has been postulated that initial settlement of the desert was primarily controlled by the ability of the original immigrants to adapt to new plant resources, especially seeds (Golson 1971: 196–238; O'Connell and Hawkes 1981: 99–125), or by the time required to modify an existing subsistence pattern narrowly focused upon littoral, lacustral and riverine resources (Bowdler 1977: 205–46). The accumulation of evidence from Puritjarra, and other sites in the Pilbara (Maynard 1980: 3–8; Brown 1987), Flinders Ranges (Lampert and Hughes 1987), Strzelecki dunefield (Wasson 1983: 85–115), and elsewhere (Dury and Langford-Smith 1970: 73), now indicates that the arid zone was widely settled by 13 000–15 000 yr BP and that some desert uplands were occupied before 20 000 yr BP. Settlement of the continent in the immediate landfall era may well have concentrated upon northern and eastern Australia (Golson 1971: 196–238), regions with a broad suite of Indo-Malaysian plant food species and extensive riverine resources, but the penetration of the desert now appears to have been completed much earlier than previously thought and to have been independent of the development of distinctive seedgrinding technology (Smith 1986: 29–39).

Acknowledgements

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4 The archaeology of Sahul

Tim Murray

After over thirty years of research the archaeology of Sahul (the continent which linked present day New Guinea and Tasmania with Australia during the Pleistocene still holds many mysteries. We have seen that profound differences of opinion exist among archaeologists about the antiquity of human occupation in Australia (even about the most convincing ways of establishing this), and about the history of the human settlement of the continent of Sahul.

Mulvaney's discovery of the Pleistocene in Australia has sparked over two decades of active research in prehistoric archaeology and in related earth and life sciences which have changed our perception of pre-contact Aboriginal society, and our understanding of the history of our continent. Notwithstanding current debates over the antiquity of human occupation of the northwest of Australia, we can be reasonably confident that people have been living in Australia for at least 40 000 years. During this time far-reaching changes in climate and environment and massive transformation in the landscape of Australia have occurred. Over the same period Sahul was more-or-less continuously open for colonisation by peoples moving from south and south-east Asia.

Thus we can no longer see either the country or the people who lived on and with it as being static. In fact the complete opposite is true. We are now encouraged to perceive the ancestors of contemporary Aboriginal people as highly flexible, inventive people who came to grips with a multitude of different environments and who created a rich, diverse and immensely strong culture which continues to this day. The past thirty years have been ones of great field discoveries and patient analysis; they have also been ones where our comprehension of the nature of human behaviour occurring so long ago has been challenged.

Some challenges have been successfully met. For example, Australians have now grown more familiar with the notion that Aboriginal society has a long and rich history. Where once it was thought that Aboriginal people were an 'unchanging people in an unchanging landscape', we now know different and have used this transformation of our understanding as one of the bases for far-reaching changes in our attitudes to the indigenous people of Australia.

Other challenges, such as comprehending the complex processes which underlie that history, are still very much before us. While it is true that very little of the continental land surface has been adequately surveyed and only an extremely small percentage of this has been excavated, the enigmatic nature of the information archaeologists work with throws up major obstacles to easy understanding.

The first stems from the staggering amounts of time involved and the kinds of information prehistoric archaeologists have to work with. Together these make it difficult to discuss the prehistory of Australia in the same fashion as we would discuss the period after 1788 when historians focus on a detailed analysis of events or on short-term processes. Archaeologists working on Pleistocene materials rarely deal with 'events' in the conventional sense. Rather, they observe trajectories, tendencies or patterns, in time-slices which are far greater than the entire history of post-contact Australia.

The second obstacle has more to do with unconscious attitudes rather than with something overt like chronology and time-scale. It is difficult for us to comprehend the minds of people in the remote past, especially people who clearly lived lives so different to our own. Archaeologists have attempted to overcome this difficulty by carefully observing contemporary Aboriginal people who still live traditional lifestyles, and by discussing issues of technology and subsistence strategy with those people. This approach has provided some valuable insights, but we should not forget that today only a limited number of indigenous people, mostly in the tropics and the arid zone, live like this. Moreover these few contemporary examples are affected by the existence of the wider indigenous and non-indigenous societies with sophisticated technologies, fast communications and, of course, the cash economy. What we have today are tiny samples of the variety of lifestyles practised over the last 40 000 or so years, samples which are themselves a product of long and complex historical processes. Obviously analogies drawn from these samples need to be used with a great deal of caution.

The difficulties of working from these sources of perspective are compounded by the fact that Aboriginal societies appear not to have evolved in the same way as those of other continents. Non-indigenous Australians are predisposed to understand the evolution of human societies and cultures in a vertical sense, of social systems or cultures moving from the simple to the complex over the course of history. Three of the most significant questions asked by archaeologists are all about such vertical transformations: why did people domesticate plants and animals, why did they begin to live in larger and larger population units, and why did complex political organisations such as the modern state come about. None of these seem particularly relevant to an understanding of the human history of Australia where society and culture appear to have evolved horizontally rather than vertically.

Yet because of the continued predominance of the vertical reading of social evolution many Australian archaeologists have chosen to see the first 30 000 years as being populated by small groups of highly mobile foragers working with simple technologies and a fairly rudimentary understanding of their external environment. These behavioural patterns are held to change during the last 10 000 years when technologies are presumed (by some at least) to become more 'sophisticated', where population units are thought to get larger, and where human intervention in the environment through burning and the like is thought to intensify. There is no doubt that technologies did change, that some areas became quite densely populated and that Aboriginal people did burn country, but the evidence that this is somehow a more complex, sophisticated response than that which occurred during the Pleistocene is tenuous indeed. We do, however, have excellent evidence for change and variation in just about every aspect of Aboriginal life during the Pleistocene. Indeed, recent archaeological work in the arid zone, in the tropics and in southern Tasmania indicates that the predominance of the vertical reading of social evolution, especially as it applies to Australian Aboriginal society, should now be brought to an end.

In the Pleistocene Australia was still part of the super continent of Sahul. Given the size of the landmass and the fact that it stretched from just below the equator through 40 degrees of latitude, there was a high level of environmental diversity. The period between 60 000 and 10 000 years ago (during which time human beings are first thought to have settled Sahul) was marked by intense glacial activity in the highlands of the continent, particularly in Tasmania and New Guinea. In these areas grasslands predominated and temperate rainforest was confined to pockets at lower altitudes, a greatly different situation to today. Sixty thousand years ago the arid zone was much wetter than today, with a great deal of surface water lying in freshwater lakes such as at Mungo, and flowing through river systems which today are sandy channels. Yet this regime changed dramatically around 17 000 years ago, when for a period of about 5000 years the core of the continent was even drier than today.

The story of fluctuating environmental fortunes during the Pleistocene holds true for the rest of the continent and provides a crucial backdrop to the changes which we can observe in Aboriginal settlement patterns, technology and subsistence practices over the period. These fluctuations, especially in the arid zone and in the glaciated areas, must have posed great challenges to the people of the Pleistocene, at least as great as those faced by the original settlers of the continent. Indeed, according to the current evidence, exploration and first settlement of large areas of the continent persisted down to about 20 000 years ago, perhaps about 30 000 years after first landfall. Thus it was not simply a matter of coming to grips with environments different to those experienced in the homeland of Sunda (the super continent which comprised southeast Asia during the Pleistocene), but of also comprehending the shifts and changes in what must have become familiar environments.

The papers which have been reprinted here discuss most of the new data which have led to a re-evaluation of the Pleistocene of Sahul while making clear statements about the concepts which have driven the analysis of those data. Another important case study of regional variation in the Pleistocene of Tasmania, that of Cosgrove, Allen and Marshall, is reprinted in Chapter 6 of this book.

In the last decade just about every orthodoxy of Pleistocene human behaviour has been shaken. Archaeologists have amassed evidence of flexible, responsive behaviours in areas such as deserts and glacial regions which were previously thought to be too difficult for such people to survive in. Furthermore the old image of unplanned, ad hoc responses to the trials of life simply does not match clear evidence for purposive behaviours which are of the same order as those exhibited by Aboriginal people thousands of years later during the Holocene. Instead of a featureless landscape of human beings struggling to come to grips with their world, we are now confronted by a richness and variety of human behaviour which even a decade ago was simply undreamed of.

4a Notions of the Pleistocene in Greater Australia

Jim Allen

Despite the extraordinary explosion of knowledge of the prehistoric pasts of Australia and New Guinea in the last 30 years, any coherent picture of the behaviour of Pleistocene humans in this region has remained elusive. As White (1977) discussed 15 years ago, archaeological views of the Pleistocene had for decades earlier been predicated upon the notion that patterned human behaviour was somehow immutably written in stone tools, and if these artefacts were unenterprising and monotonous, so must have been the lives and histories of their makers. This single fact and its tacit but widespread acceptance as prehistoric archaeology expanded in the second half of this century channelled Pleistocene archaeological research in Greater Australia away from questions of variability and change, which might best have been seen in the stone tools because they constitute the most commonly available database, and into other predictable areas—hypothesising on the dates of initial human colonisation, theorising about the nature and processes of that colonisation, modelling population dynamics and measuring and comparing human fossil remains as they came to hand.

All of these latter enterprises, commendable as they might be, have suffered from little or no directed research strategy or subsequent testing. In particular, until recently, no one made a concerted search for the archaeology of the Pleistocene in Australia or New Guinea; such sites have been discovered by accident, as often as not as part of postgraduate research which has not encouraged elaboration or subsequent development. These sites have offered their discoverers the bonus of antiquity, but little else. A shining exception to this hit-and-run approach is Western New South Wales, where against a backdrop of well-researched, major environmental changes during the late Pleistocene, perhaps a dozen or so archaeologists have for two decades attempted to come to terms with an extensive but fragmentary archaeological and human fossil record. That we still await a coherent behavioural picture, glimpses of the Pleistocene soul and intellect not withstanding (Mulvaney 1981 [1990]: 286), is testimony to the limiting nature of the fragmentary Pleistocene archaeological record in that environment, as well as the taphonomic problems attendant upon its interpretation.

Two consequences have followed upon the wider situation. The first is that the Pleistocene record of Greater Australia has been largely reflected in sites separated from each other by geographical and temporal distances too great to postulate direct historical connections. Isolated and fragmentary sequences, most of which have poor or worse chronological resolution, have conspired against the development of coherent models of Pleistocene, and especially regional Pleistocene, behaviours. Instead these records have been seen to reflect human groups characterised by low population densities, undifferentiated and limited subsistence strategies and uninventive technologies. We have accepted without demonstration that humans throughout Greater Australia for at least 30 000 years must have been primitive and thin on the ground. We have reinforced this homogeneous view, up until recent years, by uncritically embracing an ill-defined and continent-wide Pleistocene lithic tradition, the Australian Gore Tool and Scraper Tradition, and dumping most of our lithic evidence into it, even though its distribution is far from continent-wide. Although now we like to scoff at the notion, derived from the 1920s but held current into the 1960s, of Aborigines as an unchanging people in an unchanging land, the 'unchanging people' view is not far removed from the still widely held template of Pleistocene people in Greater Australia—dispersed and mobile groups operating within the constraints of a basic and basically similar technology, for whom many arid, upland and island regions remained *terrae incognitae* under these same constraints; this pattern is seen to continue with little change for upwards of 25 000 years. Cosgrove *et al.* (1990) have recently criticised such views being put forward as a necessary basis from which to springboard notions of social and economic intensification in the Australian mid- to late Holocene. This last argument leads me off the track and I leave it for the moment with the observation that if there was such a Pleistocene unity of undifferentiated, small, dispersed hunter-gatherer groups in Greater Australia, then the intensification claimed for mid-Holocene Australia pales into insignificance against the transformation which occurred in its separated northern half of Papua New Guinea at the beginning of the Holocene.

The second consequence of this stimulus (accidental discovery of Pleistocene age sites) and response (ad hoc explanation) process is that the models developed for the Greater Australian Pleistocene have in most cases been minimalist models. They are, or have in the past been dominated by the shortest sea routes and the lowest sea levels between Asia and Greater Australia, the smallest viable founding populations, the accidental and most infrequent numbers of landings, and dispersal routes which require the fewest adaptations. As a *strategy*, developing minimalist hypotheses when there are few or no data is a logical procedure because they demand the fewest assumptions. However, they also require continued testing and revision. The danger with this approach is precisely that the superficial support which fragmentary data bring to such minimalist hypotheses will not be further questioned; indeed, this support often obscures the need to seek alternative explanations.

It is a measure of the quality of recent investigations into the Pleistocene in both Australia and New Guinea that it is currently the subject of many revisions of both data and interpretations. This paper seeks to review two of these, beginning with Melanesia, and contrasting it with Tasmania—those two extremities of Greater Australia which were to be isolated by the marine transgression which provides the chronological terminus for this review. This comparative approach seeks deliberately to connect two regions too often seen today as entirely separate.

The Melanesian Pleistocene

The New Guinea Highlands

Up until 1986 a hard and sharp division existed for Pleistocene sites in Melanesia. With the single exception of Misisil Cave, inland from the south coast of New Britain, which is an archaeologically limited site with a terminal Pleistocene date at its base (Specht *et al.* 1981), all reported Melanesian Pleistocene sites were confined to the New Guinea Highlands.

In a generous review of these sites in 1983, Golson (in Hope *et al.* 1983: 42–5) attempted to relate them to the only overarching model which had then been advanced. Hope and Hope (1976) had suggested that the depression of the treeline during periods of colder temperature had greatly expanded the area of alpine grasslands along the spine of New Guinea. On their fringes, between *c.* 2000 m and 3000 m above sea-level at the height of the last glaciation, an extensive forest-grassland ecotone would have provided an ideal hunting environment. Golson reviewed the prediction that sites would occur in or near this zone by looking at the eight major Pleistocene sites known in the Highlands. This review did nothing to support the prediction of Hope and Hope, and indeed provoked an alternative view that people were locating themselves in the mid-montane forests in positions which gave access to a 'vast altitudinal spread of resources extending downwards into lowland valleys' (Hope *et al.* 1983: 44). However, Golson also acknowledged that testing any model of Pleistocene human behaviour in the Highlands was hampered by the small number of relevant sites and the preliminary state of the analysis and/or publication of the data from a number of them. Golson concluded that on the evidence available it was 'impossible to say anything very specific about the nature of the Pleistocene occupation: how dense it was, whether it was perennial, seasonal or intermittent, even what range of resources was being exploited' (Hope *et al.* 1983: 44). It is disappointing that apart from three further preliminary statements on the Nombe site (Mountain 1983, 1990; Gillieson and Mountain 1983) this situation has not changed in the last eight years. No new Highlands Pleistocene sites nor any new, substantive data from the existing ones have appeared.

It is nonetheless instructive to gather together some of the disparate archaeological facts which emerged from this work. Kosipe (White *et al.* 1970) at 2000 m above sea-level and Nombe (Mountain 1983) at 1720 m above sea-level represent the two oldest known Highlands sites, both having been occupied by at least 25 000 years ago. Kosipe, an open site, is located adjacent to a high-altitude pandanus swamp where Hope (1982) recovered palynological evidence for forest clearance at 30 000 BP, assumed to be the work of humans. Kosipe has thus been interpreted as a focus for at least seasonal collection of pandanus. Nombe, a rockshelter, yielded evidence of the hunting of diverse animal species. In its earliest levels it appears that humans shared the site with other predators. These levels contain two species of the extinct *Protemnodon*, an extinct *Dendrolagus*, an unidentified diprotodontid and thylacine in association with stone tools. In the next major stratum some of these large marsupials continue in association with more stone tools, and this evidence suggests that if humans were not themselves hunting or scavenging these large animals, they must certainly have been familiar with them and their predators.

Nombe and Kosipe are thus quite different sites, each of which was apparently in or near mid-montane forests, each at high altitude and a long way (100 km plus) from the coast. Despite the fact that Nombe is *c.* 400 km northwest of Kosipe, these sites shared, 25 000 years ago, the distinctive stone artefact type commonly known as the waisted blade, but described more provocatively and accurately by Groube (1986: 172) as a hafted axe. Groube in fact distinguishes between the waisted axe (the Kosipe examples) and the stemmed axe (the early Nombe example) but concedes that the stemmed axes have a 'consistent association with waisted axes in New Guinea . . . (which) suggests they are a significantly associated form' (1986: 169). While I agree with Groube on this point, I do not here persist with this differentiation for the sake of simplicity, and continue to group them as waisted tools. Waisted axes also occur in the undated (but Pleistocene) early levels of the 1300 m a.s.l. rockshelter site of Yuku (Bulmer 1975) a further 150 km northwest of Nombe, and elsewhere in Melanesia and Australia, as discussed below.

While it may be an artefact of the limited number of sites and sequences at our disposal, there does in fact appear to be an increase in the density of archaeological evidence in terminal Pleistocene Highlands sites. Yuku, containing a wide range of forest and forest-grassland ecotone prey animals, continues through this period. Mountain (1983) reports that Stratum C at Nombe, representing the period between 14 500 BP and 10 000 BP, contains 'considerable' amounts of bone, including burnt bone, and stone artefacts and a wider range of species than before or after this period in the site. Other rockshelters, such as Kafiavana (White 1972), Kiowa (Bulmer 1975) and Manim (Christensen 1975) and open sites like Wañlek (Bulmer 1977) and NFX (Watson and Cole 1978) are not only occupied during this time, but also reflect the presence of humans in a range of upland environments as well as varying human activities, including claims for the construction of houses at Wañlek at 12 000–15 000 BP (Bulmer 1977: 65) and at NFX at 18 000 BP (Watson and Cole 1978: 35–40).

As fragmentary and non-complementary as the data may be, in general we can assume that well before 25 000 BP people were quite familiar with a wide range of upland and highland environments in New Guinea, and more particularly with the resources they contained. While, as Golson (1971a) has suggested, the cultural baggage that the earliest human colonists brought with them would have included a familiarity with many of the plant species they encountered, by this time we can also assume a reasonable adaptation to a strange and marsupial-based fauna at high altitudes and at relatively low temperatures compared with the coast. Whether this plant familiarity facilitated migration into the upland forests may only be speculated upon; irrespective of this, however, we see by this time the presence of wallabies, tree kangaroos, phalangers, bandicoots and echidnas, as well as the mammalian colonisers, bats and rats, as common elements of the subsistence regime in these sites. Such data are clear signals of distinct adaptations to non-coastal environments.

The slender amounts of evidence cannot be stretched too far, but the lateral spread of the specific artefact type, the waisted axe, in these sites, reflects either some measure of lateral connectedness along the spine of New Guinea throughout the Pleistocene or the common origin of groups for whom this implement was of importance. Groube (1988: 298–302) argues the case quite strongly for these tools having been used for forest clearance, suggesting that the available widespread evidence for forest interference in the Pleistocene cannot be seen merely as the result of hunting practices. Rather, it is the deliberate creation of small disturbed areas to promote the most useful and productive food plants which flourish in such patches:

> Restricted natural stands of food plants such as aerial yams, local bananas, swamp taro, and such tree crops as sago and *Pandanus*, could be promoted by judicious trimming, canopy-thinning and ring-barking, and perhaps, with the aid of fire, some minor felling. (1988: 299)

Groube (1988: 296–7) maintains that the forms, wear-marks around the waisting, edge damage and breakage patterns on these tools are consistent with these uses and concludes that this management or 'taming' of the forest for food plant promotion was probably established soon after initial human arrival in Greater Australia and after initial exploration of the Highlands forests—in Groube's view these are likely to have been archaeologically synchronous events which occurred at least 40 000 years ago (1988: 302). These groups were thus already on a trajectory which would result in the appearance of fully developed and apparently widespread horticultural subsistence practices in the Highlands in the immediate post-Pleistocene (Golson 1988).

Concomitantly, site location data and the faunal suites indicate a good deal of altitudinal human movement as well, best reflected in the early Holocene occurrence of marine shells at Kafiavana (White 1972: 93). Whether or not particular Highlands groups were very mobile, what little evidence we have indicates high measures of adaptation and patterning in Highlands Pleistocene human behaviour, as well as the possibility of developed networks of interaction between distant areas of both the Highlands and the lowlands of eastern New Guinea.

The Melanesian lowlands

We first move to the New Guinea lowlands, for many years a blank on the Pleistocene map of Greater Australia. In the mid-1980s Groube *et al.* (1986) published thermoluminescence dates of *c.* 40 000 BP for waisted axes found in situ between volcanic ash layers on the uplifted coral terraces of the Huon Peninsula in the vicinity of Fortification Point. This site is currently the oldest dated human site in Melanesia. The several buried examples of waisted axes here are complemented by many more surface finds, both broken and complete, and it is the total collection of more than 70 examples on which Groube has primarily based his functional interpretation of these tools, just outlined.

These relatively specialised artefacts currently provide an archaeological focus for the Pleistocene but, nevertheless, one that remains enigmatic. In Melanesia they occur from 40 000 BP to 6000 BP in the Highlands, the lowlands and the islands, appearing as undated surface finds in the Solomons (Groube 1986: 172). Their presence near Mackay in north Queensland (McCarthy 1949; Lampert 1983) where they were found in rainforest/open forest locations might be accommodated as part of a single geographical distribution which includes the more northerly Melanesian tools; their appearance as a component of terminal Pleistocene Kartan sites on Kangaroo Island, at the other end of the Greater Australian continent, however, raises some obvious issues.

Lampert addressed the question of whether the 'Australian' and 'New Guinean' waisted axes were related to each other, since Golson (1971b: 131–5) had earlier suggested that waisting as a hafting aid might be a significant technological aspect of the archaeological record on both sides of the Wallace Line. Lampert (1983: 145) thus sought to extend the comparison into Australia. Using multivariate statistics he compared the two Australian sets with that from Kosipe and concluded that each was unrelated to the others, sharing only waisting as a common trait. He argued independent invention, at least in Australia, although his argument that waisting is 'a universal method of hafting', as support for independent invention, appears to be at odds with another of his supporting arguments which stresses that waisting has only been found at two localities some 2000 km apart in the relatively well-known archaeological landscape of a country the size of Australia (1983: 151).

Groube attempted a similar comparison using, this time, the Huon waisted axes, and including as well two collections of similarly shaped tools from Botel Tobago and late Jomon Japan, considered to be hoes (Groube 1986: 169). Employing a different statistical approach, Groube arrived at a diametrically opposite conclusion to that of Lampert, suggesting (1986: 174) that the waisted axes of Australia and New Guinea are part of a single population which is distinct from the Northeast Asian set that was included in his analysis. On this basis Groube saw waisted axes as an invention in Greater Australia independent from Asian influence.

The issues raised by these two analyses are far from resolved and represent an example of the wider problems discussed at the beginning of this paper—fragmentary evidence greatly separated in space (and apparently time) coupled with poor chronological resolution. The analyses are constructed to measure similarity rather than variability, which would seem initially to demand some control over the variabilities within each of the data sets. What are the time frames of these collections? Should we expect internal variability within sets over time? What differences between sets can be explained by the different physical properties of the different raw materials used? What differences have been created by different collecting procedures (a point raised by Groube 1986: 170)? What variability in the uses of these tools may have occurred in space and time?

This last question raises the intriguing point that while Groube's functional explanation in northern Greater Australia might be extended to the Mackay waisted axes, the Kangaroo Island case would seem to require at least a lateral shift in the function of these tools from opening the canopy to sunlight in order to promote food plant growth, to some other need for forest clearance. In support of the Groube hypothesis, however, is Lampert's (1983: 151) observation that waisting is widespread in New Guinea, but not in Australia. This might be an expected distribution pattern if these tools were forest clearance implements.

What of lowland sites more generally? The occasional preservation of Pleistocene coastal sites in Melanesia, for the most part submerged by the marine transgression which followed the Last Glacial Maximum around 18 000 BP, has depended upon somewhat idiosyncratic geological events. Before and during the period of human occupation on the Huon Peninsula, tectonic uplift caused this coast to rise at a rate around 3 m per 1000 years (Groube 1986: 171, 1988: 295) and it is this uplift which has saved the archaeological remains there from drowning. On New Ireland similar processes have exposed limestone terraces along much of the east coast and several of the sites we now turn to are in caves in these terraces. Here, however, it may not be uplift so much as steep underwater coastal contours which kept these sites close to the coast during the Last Glacial Maximum, when seas fell to *c.* 130 m below present levels (Chappell and Shackleton 1986) and kept them dry and intact when it rose. On Buka Island, in the northern Solomons, the Kilu rockshelter site falls into this same category (Wickler and Spriggs 1988: 704).

Since 1985 the one known island Melanesian Pleistocene site of Misisil has been added to by seven others. On Manus, Ambrose and Spriggs have excavated deep deposits in a limestone cave called Pamwak which is still being dated but which has 2 m of cultural deposits below a radiocarbon date of *c.* 12 000 BP (Ambrose pers. comm.). Spriggs reports (this volume) that it contains among its faunal remains an introduced bandicoot, one species of rat, bats, reptiles and fish. Both Pamwak and Kilu have *Canarium* nuts preserved as macroscopic charcoal, and the latter site has also yielded artefacts with residues suggesting that they were used to process root vegetables (Wickler 1990). At Kilu, faunal remains included lizards, fish and marine shellfish (Flannery and Wickler 1990; Wickler 1990: 140–1) as well as bats, birds and five endemic rat species (Spriggs this volume).

The five remaining sites are cave or rockshelter sites in limestone on the east coast side of New Ireland. They cover a distance of *c.* 200 km between the southernmost sites, Matenbek and Matenkupkum, which are only 70 m apart, and Panakiwuk, which is *c.* 40 km from the northern end of New Ireland. In between, the site of Balof 2 is *c.* 50 km southeast of Panakiwuk and Buang Marabak is a further *c.* 50 km southeast of Balof 2. With the exception of Buang Marabak all of these sites have been reasonably reported (Allen *et al.* 1988; Allen *et al.* 1989; Marshall and Allen 1991; Gosden and Robertson 1991; White *et al.* 1991) and will not be systematically described here. What we know of Buang Marabak is that it has yielded a basal date of 31 990±830 BP (ANU-6614) and that its deposits contain shell midden throughout (Balean 1989: 7).

In these two respects Buang Marabak parallels the Matenkupkum cave, where the deposits consist of marine shell midden throughout and where multiple basal radiocarbon dates cluster at *c.* 32 000–33 000 BP. These two are the oldest Melanesian island sites so far investigated, currently followed by Kilu at *c.* 29 000 BP. The initial occupation date of Pamwak is yet to be determined. Of the three remaining New Ireland sites, Matenbek has yielded four early dates of 18 000–20 000 BP and the two northern sites, Panakiwuk and Balof 2, appear to have been first occupied around 14 000–15 000 BP. Some further qualifications of these dates are necessary to fully understand their importance.

The first qualification is accessibility. As Irwin (1991) has discussed, crossing the water barriers from New Guinea to New Ireland would have presented no problems to people who had already crossed wider expanses of water to reach Greater Australia. Similarly, while the crossing to the northern Solomons would, for the first time since leaving South East Asia, have required boats to leave one landmass before people could see the next (although New Ireland remains in sight after Buka Island comes into sight), this apparently caused no real delay in the colonisation of the Solomons. Manus, however, requires a minimum blind crossing out of sight of land for 60–90 km. This strikes my landlocked imagination as something of a quantum leap, but while Irwin acknowledges that this might have delayed the discovery of Manus, he implies that it may not have been a long delay. Initial occupation dates for Pamwak will prove interesting in this regard.

The second qualification is proximity. As stated, Matenbek is only 70 m from Matenkupkum, and in this respect the two sites might best be seen as two foci of one site. In the case of Matenbek, the dates come from the back of the site inside the cave, since the front of the site is buried beneath the collapsed cave mouth. In Matenkupkum it appears as if the earliest materials are distributed less towards the back of the cave. It is thus possible that Matenbek may have been used earlier than the available dates imply. Whatever its age, this latter site seems likely to have always been a subsidiary site to Matenkupkum. Taken in combination the two sites throw up an interesting problem: a proposed gap in the Matenkupkum sequence between 21 000 BP and 14 000 BP is partially filled by the Pleistocene occupation in Matenbek. This detracts from the suggestion that lowered sea-levels caused the abandonment of Matenkupkum at this time. As Gosden and Robertson (1991) discuss, the relevant dated portion of the stratigraphy is difficult to interpret at this point and further dating is being undertaken.

The final qualification is location. The two northernmost sites are also the two furthest from the coast. Marine resources occur throughout the Balof 2 sequence but do not occur in Panakiwuk until the sea approaches its present position, *c.* 8000 BP. Whether their inland locations might have made them less attractive site locations can be raised at this stage, although not conclusively resolved. From what we have already discussed about the Highlands sites, the 'inlandness' of Panakiwuk and Balof 2, respectively 4 km and 2 km from the coast, can only be considered trifling. Among the distinctions between the Highlands sites and the island sites, however, the faunal lists noted here for sites like Yuku and Nombe on the one hand and sites like Pamwak and Kilu on the other indicate that the move into the island world of Melanesia required yet another major adaptation; Green (in press) has noted that Papua New Guinea (discounting the extinct species that were around 40 000 years ago) is presently home to two species of anteaters, five species of wallabies, and a range of bandicoots and phalangers. Crossing the biogeographical divide of the Vitiaz Strait reduces this to one bandicoot, one wallaby and two species of phalangers. Bird species reduce between eastern Papua New Guinea and West New Britain from 225 to 80. Less well-reflected in the archaeological record is the concomitant reduction in plant species across this divide (Spriggs this volume). The effects of this pauperisation of resources on the ways in which the colonisation of the Bismarcks may have differed from Papua New Guinea are as yet barely glimpsed; thus the issues raised here must be recognised for what they are—points for discussion from a handful of sites, not a definitive prehistory.

Matenkupkum, Matenbek and, one assumes, Buang Marabak, reflect in their earliest levels a strong coastal dependence. Marine fishbones at 32 000 BP catch the imagination for their 'oldest in the world' status, but these bones are few in the earliest levels of Matenkupkum and suggest neither specialised technology (nets, lines, poisons, fish spears) nor deliberate pursuit. Fortuitous accidental or deliberate trapping or spearing on reefs on outgoing tides would account for the evidence to hand; when the more deliberate pursuit of fish develops is not clear from the data. Currently our best evidence for fishing comes from the younger Pleistocene site of Balof 2 where fish remains are found throughout the deposits. These bones include five identified families, which are all found around reefs: Acanthuridae, Carangidae, Balistidae, Scaridae and Pomacanthidae. In the Holocene levels only, but beginning early in the Holocene, three species of small sharks are represented in Balof 2 which strengthen the notions of deliberate fishing, for while they enter lagoons they are more likely to be found in the open sea (White *et al.* 1991).

Instead, the early focus appears to be the reef itself, with shellfish and echinoderms the most common food remains. Such an apparent strandlooper strategy seems hardly surprising for the earliest colonists and what we may be seeing in New Ireland is an example of the particular adaptation which involved the new arrivals in Greater Australia in the least amount of change, in the sense of maintaining continuities from their South East Asian homeland. If this is true then the question of whether Matenkupkum and Buang Marabak, so similar in their earliest dates, actually reflect initial colonisation of New Ireland becomes quite important, because it bears directly on the question of minimalist explanations—we might expect such a coast, with its familiar climate and resources to be quickly occupied, and occupied, for example, before mid-montane forests, other things being equal. But were other things equal? Was the comparatively pauperate nature of the edible land biota sufficiently important to have significantly delayed the occupation of this coast vis-a-vis the northern coastlines of Greater Australia further west?

Given what we do not know, this last question is too difficult and remains open, but on the simpler question of whether Matenkupkum and Buang Marabak represent initial human colonisation of central eastern New Ireland, I would continue to argue that the nature of the shell data from the earliest levels of Matenkupkum, Matenbek and Buang Marabak (Balean 1989: 33–4) suggest that this is the case, Spriggs' strictures (this volume) not withstanding. In Matenkupkum and Matenbek, large individuals of a large species of *Turbo* predominate early, and this species remains prominent in the record for 10 000 years in the former site, indicating that the local reef was subjected to a long period of low-level human predation. Quite clear changes in the nature of subsequent shell exploitation at Matenkupkum have been documented (Gosden and Robertson 1991), particularly in the period following the Last Glacial Maximum. The apparent lack of change to the nature of the shellfish remains between 20 000 BP and 30 000 BP certainly allows that the same low level predation could have gone on for the 10 000 years prior to the commencement of Matenkupkum and Buang Marabak; however the coincidence of the dates of commencement of these sites strengthens the alternative view.

These changes in shell use in the terminal Pleistocene are accompanied by other changes in the archaeological record. Obsidian from the Talasea area of West New Britain occurs in small but continuous amounts throughout the 18 000–20 000 BP levels of Matenbek. In the adjacent Matenkupkum cave the published age for the earliest appearance of obsidian, *c.* 12 000 BP (Allen *et al.* 1989: 554) is currently subject to review following further excavations there in 1988; in the light of the apparent gap in the Matenkupkum sequence between *c.* 21 000 BP and *c.* 14 000 BP, already discussed, this discrepancy between Matenkupkum and Matenbek appears to be stratigraphical in nature and likely to be resolved. The same argument pertains to the earliest occurrences of phalanger in these sites, discussed next. Three points are to be made about the distribution of Talasea obsidian in the New Ireland sites. The first is the simple fact that a useful raw material was being transported over a straight line distance of *c.* 350 km at least 18 000 years ago. The second is that this movement involved water transport between New Britain and New Ireland at this time—itself unremarkable in the context of Pleistocene sea travel, apart from the fact that it is the earliest known demonstration in this region of repeated and systematic canoe transport rather than accidental movement, and thus illustrates patterning in another dimension of human behaviour. The third point is that Talasea obsidian occurs in the Pleistocene levels of neither of the northern sites, Balof 2 and Panakiwuk. Whether this is a product of our small sample sizes or a real regional or site functional difference is presently unclear. However, Talasea obsidian does appear in the Holocene levels of these sites (certainly at 7000–8000 BP at Balof 2 and probably at the same time in Panakiwuk) and thus signals a definite change of some sort. On the single site samples from Manus and the Solomons so far available, Talasea obsidian reached neither of these places in the Pleistocene.

The transfer of a lithic raw material like obsidian at such a date occasions less surprise and more ready acceptance amongst archaeologists than the notion that Pleistocene hunter-gatherers may have deliberately moved live wild animals across biogeographic boundaries, although why this should be the case is not immediately clear. The evidence that these animals were moved seems to me to be quite convincing. All the New Ireland sites discussed here reflect this pattern. They all contain in their earliest levels *Rattus mordax*, now apparently locally extinct and perhaps replaced by *R. praetor*, which occurs in Holocene levels of Panakiwuk and Balof 2 and is absent from Matenkupkum and Matenbek. The phalanger *Phalanger orientalis* is clearly absent from the earliest layers of Matenkupkum, Panakiwuk and Balof 2, but is at the bottom of Matenbek. Following the earlier discussion of obsidian distribution at Matenbek and Matenkupkum, it may be that phalangers appear earlier in southern New Ireland than in northern New Ireland. The thylogale, *Thylogale brunii*, appears in Holocene layers in the northern sites, but on the Balof 2 evidence this was earlier than, and a separate event from, the appearance in New Ireland of domestic animals such as the pig. On Manus, as already noted, an introduced bandicoot is present in the Pamwak sequence.

While the data cited here are not without inconsistencies (see Allen *et al.* 1989: 556), they are still quite compelling in their implication that humans transported wild animals across water barriers in the terminal Pleistocene and early Holocene. That such animals were able to establish breeding populations need not, however, imply that this was a deliberate human policy of stocking empty landscapes. Indeed given the present disparities with species and dates of introduction it would seem altogether more probable that this was an accidental by-product of the human colonisation of these islands.

Given the evidence of both obsidian and fauna, that useful products were being transported relatively long distances under the impetus of effective sea transport by the terminal Pleistocene, it seems to me highly improbable that useful elements and perhaps whole systems of horticultural food production did not occur as early on New Ireland as we know them to have occurred in the New Guinea highlands. As Groube (1988: 298) has observed, swamp manipulation for food production at Kuk (Golson 1988) some 9000 years ago—as soon as climatic amelioration permitted at the end of the Pleistocene—'suggests that it may have been practised at lower altitudes during the Pleistocene'. Allen *et al.* (1989: 558) have examined the little evidence which might support this view and this has in turn been criticised by Spriggs (this volume). I am unconvinced by his treatment of the data that the explanations he evinces are in any way more parsimonious or compelling.

A final point concerns the lithic assemblages from these sites. As far as they have been described at all (see Freslov 1989; Allen *et al.* 1989: 552–4; Marshall and Allen 1991; White *et al.* 1991) they appear to show a good deal of inter-site variability which seems likely to reflect the different local raw material resources more than cultural continuities in terms of their manufacture and use.

As fragmentary as the island Melanesian Pleistocene data currently are, there are still clear indications in the record that quite distinct changes took place during the last 20 000 years of the Pleistocene. We may not yet be able to choose between explanations—whether Balof 2 and Panakiwuk were occupied later than Matenbek and Matenkupkum because they were in northern New Ireland, or because they were away from the coast, or for some other reason—but it is possible to see in the data a progression from initial, coastally oriented, low intensity occupation to more intensive and more extensive human use of the region. Matenbek at 18 000 BP looks archaeologically different to Matenkupkum at 32 000 BP; Balof 2 and Panakiwuk reflect different and more intensive usage at 8000 BP than at 14 000 BP. Beyond this, however, there are also hints at least of greater differences between the Melanesian islands and the Papua New Guinea Highlands than can merely be explained by simple environmental differences. Human strategies predicated on sea rather than land travel may have dictated increasing divergence between human behaviours in the islands and Highlands throughout the Melanesian Pleistocene, leading to broad spectrum and extensive solutions to subsistence acquisition on the one hand and more specialised and intensive solutions on the other.

The Tasmanian Pleistocene

The paper by Kiernan *et al.* (1983) is a hallmark in Tasmanian Pleistocene studies. Kutikina was not the first Tasmanian Pleistocene site to be reported, but along with Kenniff, Koonalda and Keilor it forms an archaeological quartet to rival the Golsonian heroes of Worrell, Weekes and Walcott. Among the many reasons for this, three are obvious. Firstly, the other two Tasmanian Pleistocene sites then known, Cave Bay Cave (Bowdler 1984) and Beginner's Luck Cave (Murray and Goede 1980), contained Pleistocene data which were not abundant. In contrast, Kutikina was, when found, artefactually richer than any other Australian Pleistocene site by perhaps several orders of magnitude. Secondly, it is in a region which was unoccupied by humans at the time of European contact, an absence which, in 1983, was soon to be recognised as spanning the entire Holocene. Thirdly, humans had apparently occupied it throughout the Last Glacial Maximum period, at a time when the nearby mountains were glaciated, a fact that had already led Jones (1981) to refer to it as 'the extreme climatic place'. Kutikina's central role in the wilderness disputes of the early 1980s, concerning the damming of the Franklin and Gordon Rivers for electricity generation, reinforced its scientific importance at the public level as well.

A decade on, Southwest Tasmania compares with western New South Wales in the extent and detail of the archaeological investigations so far carried out into its Pleistocene history. A series of surveying expeditions, carried out under the joint auspices of the then Tasmanian Parks and Wildlife Department and the Prehistory Department at the Australian National University, systematically explored the lower Franklin River, lower Gordon River and adjacent rivers (see for example Blain *et al.* 1983; Jones and Allen 1984). In the mid-1980s archaeologists in Tasmania continued surveying and test-pitting sites (e.g. Harris *et al.* 1988; Brown *et al.* 1989) and in 1987 archaeologists at La Trobe University began surveying and excavating in what has developed into the Southern Forests Archaeological Project (see Cosgrove *et al.* 1990 and references). Over 50 cave sites and 60 open sites (almost all in the King Valley) have now been recorded and at present there is every reason to assume that the vast majority (and perhaps all) of them are of Pleistocene age.

The Southern Forests Project has sought to test the geographical extent and cultural variability present in the Pleistocene sites of the Southwest. Initial excavations were carried out on the eastern fringes of the Southwest, at Nunamira Cave in the Florentine Valley, Bone Cave in the Weld Valley and at ORS 7, a sandstone rockshelter overlooking the Shannon River. (This last site, on the edge of the Tasmanian Central Plateau is, strictly speaking, east of the Tasmanian 'Southwest', but it has provided an important contrast in this research, particularly in respect of defining the Pleistocene cultural boundary between Southwest and Southeast Tasmania.) During the 1990–91 summer the project excavated sequences on the western side of the region, at Warreen Cave in the valley of the Maxwell River, formerly called M86/2 (Harris *et al.* 1988; Allen *et al.* 1990); at a rockshelter on the Acheron River, labelled ACH/84/1 (Jones and Allen 1984); and at an unnamed cave on Lake Mackintosh near Tullah discovered by a project survey in 1990. There are thus six new major sequences plus several minor ones, together with the open sites data from the King Valley, to supplement the information from Kutikina. Overall, these sites occupy an area of *c.* 15 000 km^2, with Bone Cave and the Mackintosh cave separated by *c.* 150 km.

ORS 7, Nunamira, Bone Cave and Warreen Cave are currently the four oldest human sites in Tasmania, each having extensively dated sequences extending back to about 30 000 radiocarbon years before the present (Cosgrove 1989; Cosgrove *et al.* 1990: 66). Warreen Cave has now yielded ten C^{14} dates in sequence, spanning the period from *c.* 16 000 BP to *c.* 27 000 BP. This latter date is, however, only two thirds of the way down the cultural sequence, with depth/age curves predicting dates in excess of 32 000 BP at the base of the excavated deposits. Further excavation was blocked by rocks before sterile deposits were reached. Warreen is thus conservatively as old or older than ORS 7, Nunamira and Bone Cave. While dates on the early Warreen levels and on the other sites are still awaited, this general antiquity of *c.* 30 000 BP for four sites in the region has come as something of a surprise, since none of the earlier dated sites from further west, including Kutikina, had exceeded 20 000 years in age (Jones 1990: 276–7.)

Like Kutikina, these sites are mostly extremely rich in artefacts rivalling the densities of the richest Palaeolithic sites anywhere in the world, and thus, to date, our resources have only permitted minimal sampling. Notwithstanding this, the excavation at Nunamira of about 1 m^3 of deposit recovered some 30 000 stone flakes and 200 000 pieces (or 30 kg) of bone from animals eaten at the site; in Bone Cave, 0.8 m^3 produced a similar amount of bone and more stone. Impressionistically, Warreen Cave is equally rich. In addition to this richness, the quality of the data recovered is also high. Many of the bones are whole or nearly so, enabling both a more certain identification of the species involved and also an accurate quantification of the body parts present.

Patterns are clear in the faunal data (see Cosgrove *et al.* 1990) and are also beginning to emerge from the analyses of the stone tool assemblages. By looking at the raw materials present in the various sites in relationship to their availability in the landscape, it is now possible to argue that most of the assemblages are produced from local materials; however, the minor presence of exotic materials in sites provides an archaeological measure of the interrelationship between sites and their inhabitants. The most striking example of this is the material known as Darwin Glass, found in the Darwin Crater, between the Franklin River and Macquarie Harbour. Found in the Franklin sites, it also occurs in tiny quantities in Nunamira and Bone Cave, about 100 km southeast, and in greater numbers in Warreen, the Acheron shelter and in the Lake Mackintosh cave, *c.* 75 km to the north. Similarly, a distinctive tool type found in these Southwest sites is the small thumbnail scraper. This tool occurs commonly in all the sequences (although not in the earliest layers of the older sites) and can be seen as an archaeological signal of relatedness between sites; in the west, however, it is made exclusively on quartz and in the east on chert. By measuring these similarities and differences we are gradually uncovering a system of human behaviour in the distant past which relies totally on the evidence from all rather than one or two of these sites—the seasonal indicator of emu eggshell seen in Nunamira is absent in Bone Cave, while the evidence for the processing of animal skins for clothing seen in the array of bone tools from Bone Cave (Webb and Allen 1990) is absent in Nunamira. This is a direct behavioural difference between two sites only 20 km apart which share many other similarities. East of Bone Cave and Nunamira Cave, the geographical boundary which separates Southwestern and Southeastern Tasmania today seems also to have been a boundary—ecological, or cultural, or both—in the Pleistocene. While the same animal species are found in the deposits of ORS 7, the archaeological configurations of this latter site are different. Neither Darwin Glass nor thumbnail scrapers have been found there and other patterns of site use are also different. Nor is the dramatic abandonment of the Southwestern caves around 12 000 years ago reflected in this site, which continued to be used through the recent millennia of the Holocene.

Southern Forests Archaeological Project member Richard Cosgrove has taken advantage of the extensive palaeobotanical, palaeoclimatic and geomorphological research previously undertaken in Tasmania to construct a palaeoecological model to accommodate the Tasmanian Pleistocene archaeology (Cosgrove *et al.* 1990). If this model holds (and currently it accommodates the evidence quite well) it will continue to indicate quite structured human behaviour which concentrated on the exploitation of a limited range of animals in discrete and rich grass patches scattered along limestone river valleys. Cosgrove sees the predictability of game animals in these patches as the factor which outweighed the environmental harshness of the region and kept people there through the climatic excesses of the Last Glacial Maximum. A second project member, Brendan Marshall, together with Cosgrove, is painstakingly reconstructing the faunal data into a picture of hunting strategies, prey species compositions and butchering and bone disposal patterns which equally reflect the long-term structuring of this regional Pleistocene economy.

Cave art associated with Tasmanian Pleistocene archaeology (Cosgrove and Jones 1989; Loy *et al*. 1990) as well as its archaeological richness and intactness and high latitude setting has occasioned direct comparison with the Upper Palaeolithic of southwestern Europe (Jones 1981, 1990: 281, 288, 290; Kiernan *et al*. 1983). The Southern Forests Archaeological Project is currently seeking a different perspective, trying to assess, in the first instance, the range of similarities and differences—the human behavioural variations—across the Pleistocene landscape of Southwest Tasmania and by doing so establish a basis for comparison with other Pleistocene records in Greater Australia. The data so far suggest what Cosgrove has called a 'regional management' distinctly different from other regional Pleistocene behaviours in Greater Australia. In Tasmania the archaeological record of the Pleistocene overwhelms us with its animal hunting emphasis. While we are not unmindful of the general invisibility of plant food components in such records, it is in this instance difficult to even nominate what these plant foods might have been, so few are the potential species. The hazards of such a meat-heavy diet have been previously discussed (Cosgrove *et al*. 1990: 72–3) but in the present context they carry the further clear implication of specialised and structured behaviour in this record.

Conclusion

Intellectually, we have perceived for some time a difference between the environmental limitations which imposed constraints and restrictions on Pleistocene human behaviours in Greater Australia and the cultural strategies of those behaviours. Only in the detailed and regional examinations of the archaeological record of Greater Australia will we begin to disentangle these separate strands. The cases of the tropical Highlands, the tropical lowlands and the periglacial uplands of the southernmost extent of Greater Australia demonstrate not only the adaptability of their Pleistocene human occupants but also the variability of their responses. Space does not permit extending the comparison further, but the works of O'Connor (1990) in the Kimberley region and Smith (1987, 1989) in Central Australia extend and emphasise these observations.

While we begin to perceive distinctions between environmentally determined and culturally determined variabilities in the Pleistocene record in Greater Australia, developing methods for identifying and explaining change in that record remains elusive; we can identify when Talasea obsidian reaches New Ireland sites, or when thumbnail scrapers first occur in the sequences of Southwest Tasmania, but for the most part problems of scale and time obscure specific events and causes. These problems are not new, but have only recently been brought into focus by the new emphasis on regional Pleistocene studies. Perversely it is the quality of the Tasmanian record, for example, which initially encouraged the pursuit of notions like seasonality in the Pleistocene, length or frequency of site occupancy in the Pleistocene, demography in the Pleistocene or group social interaction in the Pleistocene, but which equally rapidly showed us that a millennium of behaviour might be reflected in as little as a centimetre of deposit, even in these incredibly rich sites.

The current debate on dating Pleistocene sites (Roberts *et al.* 1990a, 1990b, 1990c; Hiscock 1990; Bowdler 1990) on the one hand emphasises these issues and on the other hand obscures equally important ones. If we are truly to understand the Pleistocene of Greater Australia we not only need to know when people first arrived here but also how quickly they spread through the country. Some of the ways in which the data mentioned here pertain to this question have been discussed elsewhere (Allen 1989), but ultimately the need to standardise disparate dating techniques assumes prominence in the current efforts to renovate the Pleistocene of Greater Australia.

This brief review has sought to emphasise that even in its infancy the concerted study of the Greater Australian Pleistocene can already demonstrate variations between regions of this huge landmass and hint at least at equally significant changes within regions over time. It suggests that we discard notions of an unchanging history for Pleistocene humans in Greater Australia. Equally, it suggests that there is sufficient reason, grounded in the existing database, for the development of hypotheses and research designs which do not accept, *a priori*, that we must contain ourselves within minimalist models merely because we are dealing with humans of 30 000 or more years ago.

In attempting to circumvent these models and interpretations, the strategy of intensive regional research advocated here appears to be one way to break the nexus between data and interpretation which have previously characterised the archaeology of the Greater Australian Pleistocene. A second and associated imperative is to recognise that the ethnographic present can tell us little or nothing about the deep past. In Southwest Tasmania we are today confronted by a landscape quite unlike that which we reconstruct for the Pleistocene. Beyond that, it is a landscape which, during almost all of the time which separates us from the Pleistocene inhabitants of that place, was apparently devoid of humans. In Melanesia, the social and physical transformations which have accompanied 9000 years of agriculture have made modelling its Pleistocene past on the ethnographic present equally difficult. Our knowledge of these pasts resides only in their archaeological records.

I wish to end this paper on a personal note. When directed by the editors to review the Pleistocene of Melanesia I felt I was being sent to worry 'the carcass of an old song'. Instead I rediscovered an area of research brimming with potential which will in the future contribute much more to our knowledge of Pleistocene human history. I also recognised that while Jack Golson has himself only rarely ventured into the Pleistocene of Greater Australia, he has been a central influence on those who have. I count myself among the indebted.

Acknowledgements

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4b The fifth continent: problems concerning the human colonisation of Australia

Rhys Jones

The last 15 years have seen a revolution in Australian prehistory. In 1961, the oldest acceptable date for human occupation in Australia was 8700 BP (Tindale 1937, 1957), several scholars being convinced that man had only a post-Pleistocene antiquity here (Abbie in Stanner and Shiels 1963, pp. 82–3), and Grahame Clark in his Olympian review of world prehistory (Clark 1961, p. 243) could find no convincing evidence for any site older than after 'Neothermal times'. In 1962, John Mulvaney obtained a 10 000-year-old date from Kenniff Cave in south Queensland, followed soon by a sequence thought in 1964 to extend back to some 16 000 years (the basal date for this site is now established at some 19 000 years ago). By 1968, carbon dates of just over 20 000 years had been obtained from four Australian sites, with six others showing terminal Pleistocene occupation of the highlands of eastern Australia and New Guinea (Jones 1968). By 1973, there were 26 sites older than 10 000 years (Jones 1973, 1975; Mulvaney 1975), and this figure has now been increased to over 35 (Fig. 4b.1), even if we restrict whole complexes of sites such as those on the lower Willandra or lower Darling river and lake systems to single entities. Several hundred archaeological sites have now had some scientific investigation mostly with radiocarbon chronologies established.

The rapid development of Australian archaeological research since the early 1960s can be followed by reading a series of reviews (Bowler *et al.* 1970; Clark 1968; Gallus 1968, 1970; Golson 1971a, 1971b; Howells 1973; Jones 1966, 1968; Macintosh 1967; McCarthy 1958, 1965; Merrilees 1968; Mulvaney 1961, 1964, 1969, 1971; Mulvaney and Joyce 1965; Thorne 1971; Tindale 1957) to assessments of the contemporary situation as interpreted by many authors (Allen 1974; Birdsell 1977; Bowdler 1977; Clark 1978; Dortch 1977; Gillespie *et al.* 1978; Gould 1973, 1977; Hallum 1977; Hope 1978; Jones 1973, 1976, 1977, 1979a, 1979b, 1979c; Lampert 1979; Lorblanchet *et al.* 1979; Mulvaney 1971, 1975; Thorne 1977; Urry 1978; White *et al.* 1979). The situation in New Guinea as it related to Australian prehistory has also been reviewed (Allen 1972, 1977; Bulmer 1975, 1977; Hope *et al.* 1976; Jones 1979c). Although completed site reports unfortunately have been slow in their transmogrification from the theses in which so many were originally written, the scope of excavated data is well illustrated by reports on Kenniff Cave, Queensland (Mulvaney and Joyce 1965), Lake Burrill, NSW (Lampert 1971), Seelands and other sites in northern NSW (McBryde 1974), Seton rock shelter on Kangaroo Island (Hope *et al.* 1977), Puntutjarpa, central Australia

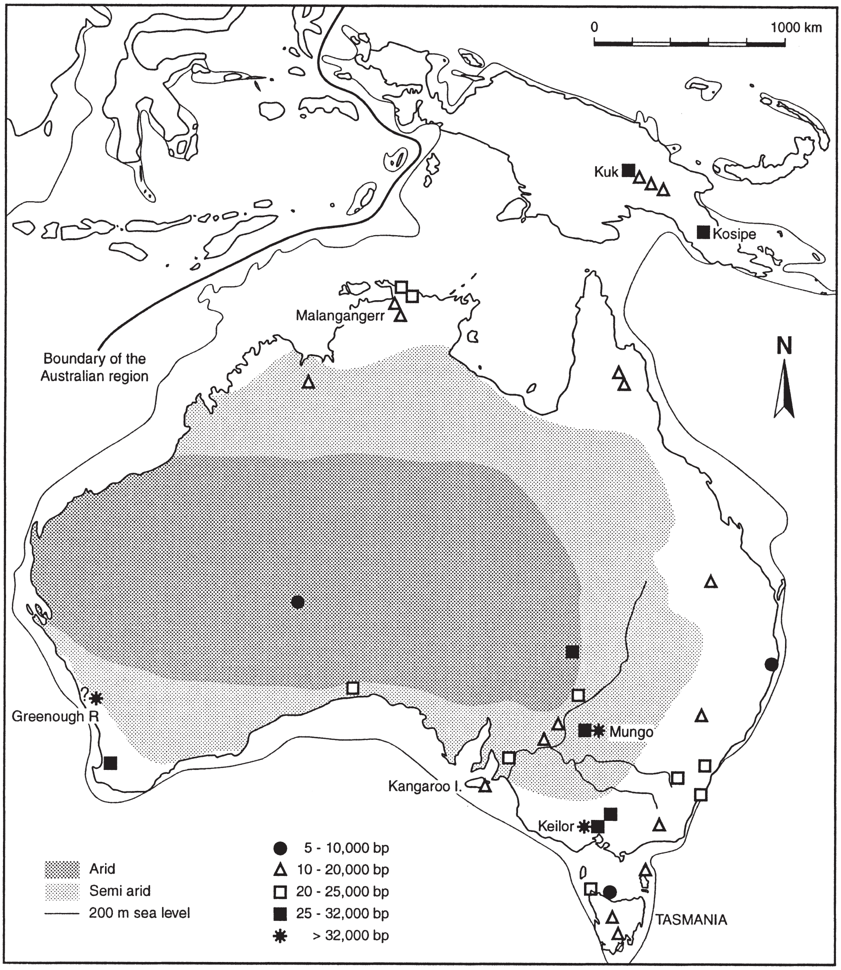

Figure 4b.1: Map showing the basal dates for the oldest archaeological sites in each region of Greater Australia.

(Gould 1978), and Kafiavana and other sites in the New Guinea Highlands (White 1972; White *et al.* 1970). Recently a mass of new information and of theoretical debate has been made available with the publication of several books of collected essays (Allen *et al.* 1977; Garanger 1979; Gould 1978; Harris and Weiner 1979; Henderson 1978; Johnson 1979; Kirk and Thorne 1976; Mulvaney and Golson 1971; Peterson 1976; Sieveking *et al.* 1976; Ucko 1978; Walker 1972; Wright 1977) which altogether contain some 150 chapters of direct relevance to current Australian archaeological research. It is likely that the broad outlines of Australian prehistory over the past 25 000–30 000 years are already known and that the theoretical problems that will engage Australian prehistorians over the next 25 years have already been established. The days of 'cowboy archaeology' may be coming to a close.

At this stage of research one set of interrelated issues dominate all others—that is to find out when man first arrived in Australia, who he was, what was the nature of his economic adaptation to the new ecological conditions which he was to find there, and conversely what was his own impact on the environment. In this brief review I can deal with only a few aspects of these problems.

Sea journeys across Wallacea

Australia once formed part of old Gondwanaland, but unlike its sister plates of southern Africa or India, it has not yet impinged upon a northern continent, and so is separated from oriental Asia by tectonically uplifted island arcs and a series of immensely deep oceanic troughs never exposed by glacial period low sea-levels (Chappell and Thom 1977). A superficial look at an atlas shows Australia merely as the largest island in a massive archipelago stretching 8000 km to the southeast of Asia (Allen *et al.* 1977 frontispiece), but the structure of the continental shelves together with the biological distributional data reveal a fundamental bilateral symmetry to this kaleidoscope of land and sea recognised since the days of A.R. Wallace. Greater Australia consists of Australia, New Guinea, Aru, Tasmania, Kangaroo Island, and many smaller islands of both the Sahul and Bassian shelves, all joined together to form a single land mass with a sea-level drop of more than 65 m. as occurred during the last and probably previous ice ages (Birdsell 1977; Chappell and Thom 1977; Jones 1973). To the northwest, the continental islands of Asia such as Java, Sumatra and Borneo, rising from the floor of Sundaland, would equally be a part of the Asian landmass at such times. Simpson, in a deceptively simple paper (Simpson 1977), has redefined the intermediate zone not as a distinct region or as a series of biogeographic lines of faunal balance, but simply as a zone of truly oceanic islands subject to well-known laws of faunal colonisation and of extinction (Diamond 1972, MacArthur and Wilson 1967).

The fossil record shows that in ancient times of all the placental land mammals of Asia, only mice and rats managed to cross through this entire archipelago to reach the continent of marsupials. However, other animals, notably the elephant-like Stegodons, crossed water straits to the islands of the Sunda Arc, Flores and Timor, where their fossils are found in gravels which may date to the middle or early upper Pleistocene (Hooijer 1967; Simpson 1977). Stone tools have also been found eroding from these gravels and from old Tjabenge industry sites on Sulawesi, showing that men had already managed to cross some substantial water barriers by this time (Glover 1973; Glover and Glover 1970; Hallum 1975; Mulvaney 1975, pp. 144–7). There still remained the final water crossing to Australia which even during low sea periods was never less than about 80 to 100 km wide (Birdsell 1977). The colonisation of the Wallacean islands and of Australia itself is probably the oldest evidence that we have in the world of the ability of man to cross substantial bodies of water, and it is almost certain that at least the final stages of this process to Australia itself would have involved the use of some kind of watercraft.

Performance of modern Tasmanian and Australian Aboriginal watercraft

Ethnographic and archaeological studies on the performances of Tasmanian Aboriginal watercraft, which were basically canoe-shaped floats made from bundles of bark, give us an insight as to the limited capacity of such simple craft to safely traverse sea crossings of only a few kilometres width (Birdsell 1977; Jones 1976, 1977). There was an inverse relationship between the width of water to be crossed to Tasmanian offshore islands and the intensity of their economic use; so that islands up to 2 or 3 km offshore were regularly visited on a seasonal basis by entire bands of people, but those requiring a voyage of 5 and up to 8 km were visited only for especially rich resources such as seals during good weather and probably by specialist hunting parties only (Jones 1977). Archaeological evidence shows that Banks Strait, 20 km wide and with strong cross currents, between northeast Tasmania and the rich islands of the Furneaux Group, was too formidable a barrier to be crossed even over the time period of 10 000 years that it has been in existence (Jones 1977, pp. 358–61).

A similar situation may have pertained with the 16 km of water separating the South Australian mainland from the large Kangaroo Island, which was unoccupied by Aborigines at the time of European contact and probably for the previous 2000 years at least. The few archaeological sites on it dating to post-Pleistocene times may be the remains of a tiny stranded population which persisted there for several thousand years before becoming extinct; or they may be the result of extremely infrequent cross-water colonisations, say of the order of a few per millennium, the difficulties of the crossing being too great for any systematic visiting or the establishment of a successful founding population (Jones 1977, pp. 341; Howells 1973; Lampert 1977, 1979; Singh 1979). In the nineteenth century, a captured Aboriginal woman did manage to swim from the island to the mainland to escape from her sealer persecutors, showing that *in extremis* people will do things which under normal circumstances are not attempted (Taplin 1878). On the Gulf of Carpentaria on the northern side of the continent, fugitive bands of Kaiadilt people on Bentinct Island, using simple rafts of dry mangrove wood, suffered an average mortality rate of 50% on two trips involving a total of 34 people making sea journeys of 13 km to another small island (Tindale 1962).

Given such simple watercraft, it is likely that there was an exponential inverse relationship between the probability of a disaster-free trip and cross-water distance (Jones 1977, p. 330), journeys of over 15–20 km having a low probability of success. Nevertheless, reference to G.G. Simpson's dictum concerning the biological colonisation of oceanic islands by the 'sweepstake route' shows that situations involving even the smallest chances can end in success if they are repeated often enough. For a successful colonisation there must be fertile members of both sexes, and McArthur *et al.* (McArthur *et al.* 1976) have shown by computer modelling experiments that the chances of survival over several generations of such tiny founding populations are greatly enhanced as the numbers of the founders are increased from two or three up to half a dozen or so, and of course such groups are highly exposed to the risks of accident.

In terms of plant foods, shell fish and fish, the recipient coastline itself would have been sufficiently familiar to colonists from the shorelines of South East Asia who presumably would also know how to tap sources of undersurface water on coastal dunes during the dry season (Golson 1971; Meehan 1977a, 1977b). If such colonists had not managed to carry fire with them and if they did not know how to make it, as was the case with both the Tasmanians and the Andamanese (Cipriani 1966, pp. 25, 65–66; Plomley 1962, pp. 11–12), then opportunities to obtain fire from dry season lightning storms may not have come for many years, causing massive stress on the capacity of such a group to survive because many of the plant foods need some cooking to remove slight toxins (Jones 1979c; Meehan 1979).

We thus have a scenario of peoples foraging on the shorelines of the southeastern part of Asia, and with some capacity to ride on water with rafts or other primitive craft, slowly and stochastically gaining access to the islands of Wallacea and eventually Australia by random processes outlined above (Birdsell 1957, 1977). The chances of success for any particular voyage would have been extremely low, and it is also likely that there had been many unsuccessful landfalls on the Australian or New Guinean coast which did not lead to viable founding populations. Man in Java at *c.* 2 million years ago, in the Wallacean islands of Flores, Timor, Sulawesi and the Philippines at say middle or early Upper Pleistocene times, and his arrival in Australia in only the late Pleistocene, gives a time-scale against which these colonising processes operated. White and O'Connell (White and O'Connell 1979, pp. 22–4), asserting that late Pleistocene watercraft in the region must have been more sophisticated than any used ethnographically by Australian Aborigines,[1] reject the model outlined here. However, they give no coherent arguments as to why this hypothesis is falsified by the data we have available, and on the grounds of parsimony it must stand.

Palaeoclimatic considerations

Australia is the world's driest continent, with over 75% of its surface area under a regime where potential annual evaporation exceeds precipitation (Bowler *et al.* 1976, p. 360). Recent geomorphological and pollen analytical research has demonstrated great changes in climate during the late Pleistocene period, especially as they affected the semi-arid zone fringing the desert core (Bowler 1976; Bowler *et al.* 1976; Kalmer and Nix 1972; Singh *et al.* 1979; Walker 1972). A key site is the now dry Lake Mungo on the Willandra Creek in the dry region of western New South Wales (Fig. 4b.1), where a local depositional sequence has been set up spanning the past 120 000 years, the most recent third of which being exceptionally well supported by over a hundred radiometric determinations (Barbetti and Allen 1972; Bowler 1971, 1976; Bowler *et al.* 1972; Hope 1978).

A dune deposit called the Mungo Unit was formed when the lake was full of water 15 m deep, so that sand was blown from its beach by westerly winds to form an immense crescentic shaped flanking sand dune, called a lunette, up to 30 m thick on its eastern shore. This lake-full stage lasted from 45 000 to 25 000 years ago and involved lakes of a total surface area of 1000 km^2 on the Willandra Creek system alone (Bowler 1976; Bowler *et al.* 1976). It was succeeded by a drying phase of oscillating lake levels of gradually increasing salinity, some of which allowed the formation of smaller clay dunes until about 17 000 years ago when the lake was as dry as it has been ever since. This 'Mungo Lacustral Phase' is now recognised from fossil geomorphic features in the ephemeral stream and playa lake systems along the entire eastern flank of the desert core from Coopers Creek to the lower Murray as well as in the subtropical north and southwestern Australia (Bowler 1976; Bowler *et al.* 1976; Kalmer and Nix 1972).

It was followed by a short period of intense aridity and of strong wind activity which, especially between 16 000 and 18 000 BP, reactivated the great anticlockwise continental dune system of the desert heartland and caused some mobile dunes to be formed even in the southeastern tablelands and northeastern Tasmania where they are now totally vegetated (Bowler 1976; Bowler *et al.* 1976).

This aridity correlated in time with the height of the last glacial period and it was caused by a combination of cold winters and strong hot summer winds from the desert, especially in the lee region of southeastern Australia. Such intensification of the atmospheric circulation pattern was a function of the equator-ward expansion of the polar ice sheets, and in the middle latitudes of the globe it caused such a phase of dust that layers of dust particles have been found in deep sea cores far out in the middle of the oceans (Bowler *et al.* 1976; Rognon *et al.* 1977; Shackleton and Opdyke 1973).

Stratified beneath the Mungo Unit at Lake Mungo is another heavily weathered dune soil, the Gol Gol Unit, again indicating a lake-full phase and believed on radiometric and palaeomagnetic grounds to date prior to the penultimate glaciation of the order of 120 000–140 000 years ago, suggesting that these climatic changes were probably cyclical, associated with the various glacial periods, several of which have now been recorded in extremely old pollen curves in eastern Australia (Bowler 1971, 1976; Bowler *et al.* 1976; Kershaw 1974; Singh *et al.* 1979). Ice sheets themselves were restricted to small areas of the southeastern highlands, Tasmania and the tops of the New Guinea mountains (Bowler *et al.* 1976; Hope and Hope 1976; Jones 1968; Kalma and Nix 1972).

The antiquity of man in Australia

Lake Mungo

The oldest well-published carbon date from an archaeological site in Australia is one of 32 750±1250 BP (ANU-331) obtained from freshwater mussel shells (*Velesunio* *ambiguus*) from a small midden in the upper part of the Mungo Unit lunette at the site of Lake Mungo itself (Barbetti and Allen 1972; Bowler 1971; Bowler *et al.* 1970; Fig. 4b.1). There are some 10 other published charcoal and shell dates ranging from 25 000 to 31 000 years BP from hearths and middens found in situ at this and neighboring sites, all attesting to human occupation of the sandy shores of the then full lake (Barbetti and Allen 1972; Bowler 1971, 1976; Bowler *et al.* 1972; Hope 1978, Mulvaney 1975 pp. 147–52).

The original archaeological discoveries made in 1969 at this site consisted of stone tools such as horse-hoof cores, steep-edged and notched scrapers, ochre, hearths, shell middens and faunal remains from camp debris, together with the cremated bones of a young woman, all from the upper part of the Mungo Unit and dated to between 25 000 and 30 000 years ago (Bowler *et al.* 1970, 1976; Jones 1973). The foraging economy of these people straddled the ecotone between water and land. Freshwater mussels were collected from the lake shore muds in large quantities to form discrete middens, and fish including perch (*Plectroplites ambiguus*) and Murray cod (*Maccullochella macquariensis*) up to 15 kg in weight were caught. From the back scrubs came emu eggs and a variety of small marsupials of species such as rat kangaroos and wallabies (e.g. *Bettongia lesueur, Lagorchestes leporides*) which survived in this area until the nineteenth century (Bowler *et al.* 1970, pp. 47–56). Such a broad spectrum foraging focused on the edges of wetlands is characteristic of the Aboriginal economic response as recorded ethnographically (Bowdler 1977; Jones 1975, 1979; Meehan 1977, 1979), and the Mungo site provides evidence for intensive lacustrine exploitation as old as anywhere in the world (cf. the situation in Africa, Clark in Harris and Weiner 1979). Apart from the absence of seed grinding dishes which appear in the sequence only some 15 000 years ago, there is a broad continuity of economic response in this region right through from *c.* 25 000 years ago to ethnographic times (Allen 1974, unpublished PhD thesis, Australian National University 1972, p. 351).

During the past 6 years, the Mungo and related sites have been subject to a major multidisciplinary archaeological study, though only a few results have yet been published (Bowler 1976, p. 59; McIntyre and Hope n.d.; Mulvaney 1974, 1975, p. 153; Shawcross 1975). On the Mungo lunette itself, excavations into lower Mungo Unit deposits below the level of the finds described above revealed some stone artefacts within it down to its base associated with high lake beach gravels (Mulvaney 1974, 1975; p. 153) 'conservatively estimated at a good forty thousand years or more', though no further details have as yet been forthcoming. Below 32 000 BP the artefacts were flakes (D.J. Mulvaney, personal communication). Bowler refers to a thin in situ midden lens of freshwater mussel shells associated with finely divided charcoal in a nearby dune section dated to between 34 000 and 37 000 BP (N 1665), and concludes that 'man's presence in western New South Wales at least by 35 000 years BP seems assured' (Bowler 1976, p. 59). Accepting this, and indeed a probable antiquity down to at least early Mungo Phase times, one might also speculate whether or not the relative paucity of remains both absolutely and in terms of range of cultural manifestations before *c.* 30 000, as opposed to those pertaining afterwards between 25 000 and 30 000 years ago, document an initial sparse occupation followed by an intensive use of the lakeshore edges some 10 000 years later.

Keilor

A very different site, but one which has been in the literature since the middle 1940s, is the terrace sequence of the Maribyrnong River at Keilor just north of Melbourne. The Keilor terrace silts, which contain stone tools and the well-known human cranium (Gill 1966; MacIntosh 1967; Mulvaney 1964), date back to some 18 000 BP. These lie unconformably on an alluvial clay deposit called the 'D clay' by Gallus (Gallus 1971/1972). Despite diagnostic problems concerning the human or natural origins of some of the stone objects in this deposit (Gallus 1971/1972; Mulvaney 1975, pp. 146–7), at least some indisputable struck flakes of human origin have been found in situ in this 'D clay' (Bowler 1976, p. 62; 31; personal observation at a field demonstration by A. Gallus and J.M. Bowler, 1971). The date of this deposit has been excellently reviewed by Bowler (Bowler 1976, pp. 62–4), who argues on both radiometric and pedogenetic grounds that conservatively it must be between 25 000 and 36 000 years old, with basal levels possibly extending back to 45 000 years ago. Excavations are now in progress to test whether or not tools can be found in situ at these or older levels where Gallus has also claimed stone tools in situ (Gallus 1971/1972).

Greenough and Murchiston Rivers

On the other side of the continent, stone tools including flakes and crude choppers have been found in situ in a heavily weathered alluvial deposit on the banks of the Greenough River in Western Australia (Wyroll and Dortch 1978). Called the 'Older Fill', this contains bands of partly silicified calcrete, and probably reflects the same geomorphic events as the 'Murchiston cement' from the nearby Murchiston River, from which Merrilees reported a stone tool as long ago as 1968 (Merrilees 1968), and which has also been receiving recent scientific attention by Lofgren and Clark. On geomorphological grounds, Wyrwoll and Dortch believe that this 'Older Fill' is of comparable antiquity to deposits from a nearby coastal alluvial sequence dated to somewhat more than 37 000 years. While systematic direct dating work needs to be done at these sites, a prima facie case is being built up suggesting an antiquity at least of the same order as the Keilor and Mungo situations discussed above, though the investigators themselves consider much higher ages to be possible.

Thus archaeologically we can demonstrate man's presence in the southern part of Greater Australia in the time period 35 000–45 000 years ago with every expectation of older dates being obtained with continuing research, so that even conservative opinion is now talking in terms of a 50 000 year antiquity for man on the continent (Hallum 1975; Mulvaney 1975, p. 128; White and O'Connell 1979; Wright 1976, p. 272). The maximum limits have also been discussed by Bowler (Bowler 1976, pp. 64–6), who points out that despite recent intensive search by several workers, no remains have ever been found either from Gol-Gol lacustrine phase lake dunes nor from the widespread Last Interglacial high sea beach systems of the east coast dated to *c.* 120 000 years ago, both being prime potential locations for foraging man. One might conclude that the archaeological evidence so far supports an entry of man into Australia of the order of 50 000 years ago.

Filling of the continent

The evidence for man's presence before 32 000 BP seems to be sparse and is so far restricted to large lakesides or rivers close to the then presumed coast, giving some support to Bowdler's model (Bowdler 1977) that the economic adaptation of the very first men in Australia may have been restricted to the coastline or analogous situations. Following such an argument, the Mungo Lacustrine Phase would have given major access to the reactivated river and lake systems which ringed the arid heart.

In contrast to this, the situation after 30 000 and especially between 20 000 and 27 000 BP shows not only a great intensification of use of the lake shores and river banks of, for example, the Murray-Darling system, but also the presence of man in many rockshelters, caves and open sites throughout much of the geographical spread of the continent. Figure 4b.1 indicates sites with basal dates of this time range in the tropical savanna of northern Australia, down the spine of the Great Dividing Range, on the eastern coastline, on some of the open plains and isolated lake country of southeastern Australia, and in the tip of southwestern Australia (Allen 1974; Balme *et al.* 1978; Bowdler 1977; Bowler *et al.* 1970; Dortch 1977; Flood 1974; Gillespie 1978; Hallum 1977; Hope *et al.* 1977; Jones 1968, 1973, 1975, 1977; Lampert 1971, 1977, 1979; Lorblanchet and Jones 1979; MacArthur and Wilson 1967; McBryde 1974; McIntyre and Hope n.d.; Mulvaney 1962, 1975; Mulvaney and Joyce 1965; Pretty 1977; White and O'Connell 1979). In many cases these traces may actually have represented the first substantial presence of man in these areas, as suggested by Hughes and Lampert (Hughes *et al.* 1977), who pointed to major erosional features such as fire-induced hill slope instability and great acceleration of rockshelter wall erosion associated with the earliest archaeological evidence in these sites.

Perhaps of special interest are the data from the other islands of Greater Australia. To the north in the highlands of New Guinea, embedded beneath volcanic ash dated to *c.* 26 500 BP, the open Kosipe site has stone tools including large bifacially flaked pieces with oppositely placed indentations which are called 'waisted blades' (Allen 1972; Bulmer 1977; Golson 1971; White *et al.* 1970). These together with a hearth and burnt stones at the base of the Kuk Swamp site (Allen *et al.* 1977, pp. 612–30; Jack Golson, personal communication) show that man had penetrated to over 1500 m above sea level before the height of the last glaciation (Hope and Hope 1976). There is also ample evidence for Late Pleistocene occupation of these highland valleys in limestone caves and rock shelters such as Kafiavana, Kiowa, Yuku (Allen 1972, 1977; Bulmer 1975, 1977; White 1972) and the exceptionally interesting Nombe, where waisted blades have been found at the base in the same layer as bones of *Protemnodon* sp., an extinct kangaroo-like 'giant marsupia', though the primary stratigraphic association of these finds needs further work (Mountain 1979). This region in early post-glacial times saw the establishment of a horticultural system involving large-scale drainage works in the floor of the swamps probably for the cultivation of taro. The earliest phase of drainage in the Kuk Swamp is well dated to 9000 BP (Allen *et al.* 1977, pp. 612–17), when New Guinea still formed the tropical northern belt of the Greater Australian continent (Jones 1979c).

To the south, the Bassian Plain was exposed by an eustatically lowered sea-level at about 24 000 years BP (Chappell and Thom 1977; Jones 1977), giving a dry road to Tasmania some 250 km to the south of the present Australian mainland. A date of 22 750±420 BP (ANU-1498) from Cave Bay Cave (Bowdler 1977) on what is today Hunter Island just northwest of Tasmania showed that man was quick to take this opportunity of extending his range to the mountainous peninsula which at about that time supported an ice sheet on its central plateau (Bowler *et al.* 1976; Hope 1978; Jones 1968).

Beginners Luck Cave in a high mountain valley in the southwest has stone tools in a deposit dated to *c.* 20 000 BP showing that men could at least seasonally exploit this country which pollen analysis indicates was then under cold steppe herbfield vegetation (Goede *et al.* 1977, 1978; Hope 1978; Jones 1977). Tools have also been found on what are now islands in the Bass Strait such as the Kent and Furneaux Groups (Jones 1977; Jones and Lampert 1978; Orchiston and Glenie 1978; Tindale 1957) and King Island (personal observation) in deposits believed on geomorphological grounds to date from the time when they formed hills rising out of the cold plain. Kangaroo Island also has carbon-dated sites back to 17 000 BP (Hope *et al.* 1977; Lampert 1977, 1979), but surface collections of the typologically archaic Kartan industry indicate a much higher antiquity for occupation of this place either as a part of the continent or during one of its several phases as an island over the past 50 000 and more years (Chappell and Thom 1977; Lampert 1977, 1979; Tindale 1937).

Thus every major ecological zone of Greater Australia had some human occupation at least by 20 000–25 000 years ago with the exception of the true desert core. Here the oldest date so far is one of 10 000 BP from the Puntutjarpa site in the Warburton Ranges west of Alice Springs (Gould 1973, 1977, 1978), but much more exploratory work needs to be done in the vast arid region of central Australia before we can posit an absence of man during the previous 15 millennia. Bowdler's view that man was not able to exploit the Australian landscape away from the coast or major rivers until terminal Pleistocene times (Bowdler, p. 234) is not supported by the site distributional evidence, though she is right in stressing that the highest human populations in the period under review were related to the richest lacustrine and coastal resources as indeed they have been throughout Australia's prehistory until modern times (Birdsell

1977; Jones 1975, pp. 22–3; Meehan 1977a, 1977b, 1979). What the effects were of the full glacial period arid phase on these people as their lakes dried up around the desert fringe is the subject of current research (Allen 1974; Bowdler 1977; Gould 1977; Hallum 1977; Latz and Griffin 1978; McIntyre and Hope n.d.; Mulvaney 1974). Whether or not such environmental pressures might have led to an intensification of the use of dry land vegetable foods, especially the grinding of grass seeds, has major implications for general theories about the origins of agriculture (Allen 1974; Bowdler 1977; Gould 1977; Wright 1976; Tindale in Wright 1977).

Stone tool technologies

Concerning the stone tools presumed to be older than about 32 000 BP, very little has been published. From the descriptions we have they seem to consist of flakes, some large and with edge retouch, together possibly in some cases with roughly flaked choppers (Gallus 1971/1972; Mulvaney 1974; Wyrwoll and Dortch 1978; personal observation at Keilor).

However, in contrast to this, where collections are large enough for meaningful analysis, stone tools at Pleistocene sites more recent than about 30 000 years ago are similar enough to be seen as belonging to a single technological complex called the 'Australian Core Tool and Scraper Tradition' (Bowler *et al.* 1970; Hallum 1977; Jones 1973, 1977; Lampert 1977, 1979; Lorblanchet and Jones 1979; Mulvaney 1975, pp. 172–97). As its name implies, it is dominated by large horse-hoof shaped or in some cases pebble core tools and by scrapers mostly with steep step-flaked edges and with notches. An element increasingly being recognised in some assemblages are small thumbnail-shaped scrapers often made from quartz (Lampert 1979), which also provides the raw material for bipolar flaked pieces, possibly little cores for tiny quartz flakes.

The common origin of both New Guinea industries and those of Pleistocene Australia is indicated not only by the general similarity of the scraper forms, now augmented by metrical studies, but also by the presence in both regions of the distinctive waisted blades (Allen 1972, 1977; Bulmer 1977; Golson 1971; Hallum 1977; Jones 1973, Lampert 1979). These are found from the earliest levels of the New Guinea highland sites as noted previously (Allen 1972; Bulmer 1977; Golson 1971; White 1972; White *et al.* 1970) and also, among other places in Australia, on Kangaroo Island, 3500 km on the southern side of the continent (Lampert 1979). Here they form an integral part of the Kartan industry of large horse-hoof cores and pebble choppers, believed by some to be the ancestral industry from which the Australian Core Tool and Scraper Tradition developed (Lampert 1977, 1979; Lorblanchet and Jones 1979; Mulvaney 1975, pp. 181–4; Tindale 1957). Such a wide distribution of a specialised tool type suggests rapid dispersal of a technological idea, and it is possible that the earliest phase of this tradition was quickly and widely established across the Australian continent approximately 30 000 years ago. Identical waisted tools, together with core tools and scrapers reminiscent of the oldest Australian ones, have also been found in Late Pleistocene contexts in South East Asia, such as at Sai Yok in Thailand, thus suggesting a potential historical source from which these technological ideas were brought to the southern continent (Golson 1971; Hallum 1977; Lampert 1979; Van Heekeren and Knuth 1967). In the New Guinea sites, there also seems to be a gradation from purely flaked waisted tools to those having ground edges, and in several sites in the tropical north of Australia, such as at Malangangerr near Arnhemland (Fig. 4b.1), small edge-ground hatchet heads with waists or grooves around their middles have been dated back to *c.* 23 000 years BP, the oldest dates in the world for edge-ground axes (Golson 1971; Mulvaney 1975, pp. 192–3; White in Mulvaney and Golson 1971), though it is likely that similar tools of comparable antiquity will also be found in South East Asia (Golson 1971; Hallum 1975); Hayden in (Allen *et al.* 1977); Jack Golson, personal communication.

Within the assemblages of the Australian Core Tool and Scraper Tradition, seen over a period of some 25 000 years and on a continent-wide scale, there was a very slow developmental pattern. As time proceeded there was a gradual diminution in the total size of tools, though the worked edges themselves tended to remain more constant (Hallum 1977; Lampert 1979; Lorblanchet and Jones 1979). Parallel with this there was a shift from the oldest industries such as the Kartan from Kangaroo Island where core tools dominated with few scrapers (Lampert 1977, 1979), through the Mungo assemblage of 26 000 years BP (Bowler *et al.* 1970), to late and post-Pleistocene assemblages with few core tools and mostly scrapers (Flood 1974; Lampert 1979; Lorblanchet and Jones 1979; H. Allen, personal communication). Within the scrapers themselves there was a parallel trend from rougher steep-edged ones to those with finer round edges, noses, etc. (Jones 1977; Lorblanchet and Jones 1979). These reflect a process toward greater efficiency which can be measured in terms of the average length of working edge per unit weight of tool, which over a period of a thousand human generations increased crudely by a factor of eight times from 0.5 mm/g to 4 mm/g (Lorblanchet and Jones 1979).

Such a process in mid-Recent times was augmented and probably accelerated by the appearance of new suites of what are loosely referred to as 'small tools' (Dortch 1977; Gould 1973, 1977; Jones 1977; Mulvaney 1975, pp. 210–37; White and O'Connell 1979) which were added onto the old stone technology. These small tools consisted variously of backed microliths, adze flakes, unifacial and bifacial points etc., which were differentially distributed across the continent but which all reflected the same technological advances—namely a transformation in the methods of hafting of the stone bits to their wooden handles (Mulvaney and Joyce 1965). Whereas it is likely that the 'tula' and other mounted adzes of the central parts of Australia were an internal development within Australia, perhaps starting some 10 000 years ago (Dortch 1977; Gould 1973, 1977, 1978; Lampert 1979; White and O'Connell 1979), other technologies such as the backed microliths which appear at about 4000 years ago right across the southern part of the continent but not in the north may have been influenced from outside by cultural processes which are beyond the scope of this paper (Golson 1971). The dingo also appeared on the mainland at about this time, showing that at least some important cultural contacts were still coming to Australia during mid-Recent times (see White and O'Connell 1979 for a contrary view).

Tasmania was cut off from these new developments by the post-glacial rising sea some 12 000 years ago. The Tasmanian Aborigines preserved in isolation over a period of 500 generations until modern times the technological ideas of late Pleistocene Australia affected both by the ecological needs of their southern latitudes and by the effects of isolation (Jones 1966, 1968, 1977a, 1977b, 1978, 1979a, 1979b; Jones and Lampert 1978). When met by the French explorers of the late eighteenth century, the Tasmanians had the simplest technology in the world (Jones 1977b).

From technology to culture

Bone tools such as stout awls and spatulae made from kangaroo and wallaby fibulae have been found from about 20 000 years ago in several sites such as Devil's Lair in the extreme southwest of Australia and Cave Bay Cave in Tasmania (Fig. 4b.1) (Bowdler 1977; Dortch 1977; Flood 1974). A bone bipoint, possibly the tip of a barbed spear head, is presumed to be more than 25 000 years old from the Mungo site, whereas bone beads and a fine bone needle date from late Pleistocene levels at Devil's Lair (Dortch 1977; Mulvaney 1975, pp. 151, 158). The peat of Wyrie Swamp in South Australia revealed seven wooden boomerangs, a barbed wooden spear head, and several hardwood double-pointed objects, possibly detachable spear tips, all dated to about 10 000 years ago, together with typical stone scrapers of the period—showing that advanced ideas about projectiles were fully recognised by these Pleistocene Aboriginal ancestors (R. Luebbers, personal communication).

The Mungo 1 girl was cremated at 26 000 years ago, her bones smashed and put into a small pit (Bowler *et al.* 1970). The coeval Mungo 3 man was buried on his side and covered with red ochre powder (Bowler *et al.* 1976). The Nitchie man of at least 8000 years ago had a magnificent necklace of scores of drilled teeth of *Sarcophilus harrisii*, the Tasmanian devil, now extinct on the mainland (Macintosh 1967; Mulvaney 1975, p. 199). There are pieces of ochre at most sites, those at Mungo going back to at least 32 000 years ago (Bowler *et al.* 1970; Mulvaney 1975, p. 152). Complex patterns of circles, other geometric designs and the tracks of birds and animals were pecked and abraded onto rock slabs in hundreds of sites, most of which are heavily weathered through age. Claims that this art style had a Pleistocene antiquity because of its similarity with that found in Tasmania, and thus the implications of a common artistic heritage older than Bass Strait (e.g. Mulvaney 1975, pp. 279–82; Ucko 1978), have been confirmed by the Early Man Site in Cape York, where a large panel of such art is older that the 14 000-year-old carbon-dated deposit which covers it (Rosenfeld in Henderson 1978; Ucko 1978).

In total darkness, 30 m below the surface of the Nullarbor Plain at Koonalda Cave, are great panels of latticework designs on the soft limestone walls scratched by people who left their torches behind, the charcoal from which having been dated to about 22 000 years ago (Gallus in Mulvaney 1975, pp. 156–7).

In terms of art, personal decoration and ideas about the mysteries of death, the Aborigines 20 000–30 000 years ago in Australia showed the same concern about things of the mind and of the soul (Jones 1973, 1977; Mulvaney 1975, pp. 279–82) as did their contemporaries on the other side of Asia.

One people or two

Behind these archaeological data lies a thorny question which has been perceived for a long time but which has still not been resolved. Are the cultural remains the product of one group of colonists or of several (Birdsell 1949, 1977; Howells 1973; Macintosh 1965, 1967; Thorne 1971, 1977)? The Mungo 1 and Mungo 3 crania, both excellently dated to between 25 000 and almost 30 000 years old (Bowler *et al.* 1976, 1972), are gracile and of modern sapient morphology, with rounded foreheads and delicate skeletal features. They represent some of the oldest evidence of modern *Homo sapiens* in the world.

In contrast to this is another group of fossil skulls whose morphological primitivity (in the evolutionary sense [Wright 1976]) was noticed since the days of the announcement of the Talgai skull in 1914, and of which Macintosh (Macintosh 1965, p. 59), following Weidenreich (Weidenreich 1946, p. 83), said that 'the mark of ancient Java is on all of them'. This morphological group has been brought into focus by the fossil assemblage at Kow Swamp, consisting of some 60 individuals of both sexes and excellently radiocarbon dated to as recently as 10 000 to 15 000 years (Thorne and Macumber 1972). For such a young date, these skulls exhibit extraordinarily archaic features, especially in the frontal region. They are large and robust, with flat receding foreheads, thick vaults and heavy supraorbital ridges which in some cases approximate to a torus. In some specimens, the standard measurement of the frontal curvature index is even flatter in this respect than the holotype Javan erectus specimens. The face is prognathous and the mandibles large with exceedingly large teeth (Thorne 1971; Thorne and Macumber 1972; Thorne and Wilson 1977).

Using multivariate techniques, Thorne and Wilson (121) have shown that all these Pleistocene hominid remains lie outside the range of contemporary Australian skeletal forms of the appropriate sex and that they fall neatly into two groups, one more gracile than any modern Australians and the other more rugged and primitive in a morphological sense (Thorne 1977, p. 189). In order to explain this, some authors have resorted to a notion of extreme morphological plasticity among late Pleistocene populations in southeast Australia, but it is strange that only the two extremes of such a putative wide range have been found and not the forms in the middle. Wright (Wright 1976) proposed that the Kow Swamp forms with their prognathism and large tooth size may have evolved out of Mungo-like ancestors due to extreme selection processes associated with eating foods with a high grit content such as roots and grass seeds in the dusty, sandy environment of the Australian plains, as opposed to the allegorical diet of 'rain-washed fruits' in the forests of the ancestral South East Asian homelands, a view with which White and O'Connell (White and O'Connell 1979) concur. Such selective pressures are posited as having been relaxed due to technological inventions such as seed-grinding stones or netting methods for fish etc. in the terminal Pleistocene period, thus resulting in the modern Aborigines of the same region being more 'modern' again than their Kow Swamp forebears (Wright 1976). However, the time available from 26 000 to 15 000 years ago seems too short for such drastic changes (Hallum 1977), especially when we bear in mind that the physical differences involved are greater than those found anywhere within the variation of the entire human species today. All of the traits displayed in the Kow Swamp series, such as flat foreheads, have not been demonstrated to be related to the same selection processes which putatively produced prognathism and large teeth etc. If man had been in Australia at least 15 000 years before the time of the Mungo population, why had these selection pressures not already operated on the Mungo ancestors, since they lived in precisely the same sandy environment as the Kow Swamp people?

Of all the alternative explanations that Thorne outlines for these two fossil groups in Australia (Thorne 1977, p. 193), I feel that the data point to the inescapable conclusion that there were two races of man in late Pleistocene Australia (Gallus 1971/1972; Hallum 1977; Macintosh 1965; Thorne 1971) and that the Aborigines at least of mainland Australia were the result of some degree of hybridisation between the two. At the present state of research, it might seem that the gracile Mungo people were the first colonists and that the Kow Swamp people came later, thus supporting part of Birdsell's theory as proposed in 1949 (Birdsell 1949). However, this would go against the general trend of human evolution as viewed elsewhere in the world where rugged forms have always been succeeded by gracile ones. Taking a world view also points to the strong implication that if man was in Australia at 50 000 or more years ago, he would have to have been of an archaic sapiens form, since nowhere do we find modern sapiens man older than about 40 000 years BP. Thus the Kow Swamp forms may represent a relic population (Thorne 1971), giving a hint of the morphology of the putative first Australians.

An hypothesis

Bringing together the various strands of the argument, I am now in a position to propose an hypothesis for the human colonisation of Australia, although many elements have been stated or are implicit in the work of several other authors (e.g. Gallus 1970, 1971/1972; Hallum 1977, Thorne 1971; Tindale 1957). Greater Australia was colonised by man probably of the order of 50 000 years ago (or even more), the process itself occurring through random journeys on primitive watercraft. The first men were archaic sapiens standing in relation to their Javanese erectus forebears in roughly the same evolutionary position as the Neanderthals did to the western erectines or Mapa man did to Pekin man. These first Australian colonists had limited technological capabilities compared to modern hunters and gatherers and so may have had a much lower population density than that observed ethnographically, or they were limited to the easier foraging areas around the wetland edges or both. Their stone tools may have consisted of roughly retouched flakes and possibly flaked choppers. They could use fire and began to set in motion vast ecological changes in the floral composition of the Australian landscape (Clark 1952; Hallum 1975; Haynes 1978; Jackson 1965; Jones 1968, 1969, 1975; Kershaw 1974; Latz and Griffin 1978; Singh *et al.* 1979; Stocker 1971). In the long run, such changes affected many species of marsupial and other fauna, placing suites of animals at risk to future environmental changes. However, the arrival of these archaic men did not cause a massive phase of extinction as posited by me previously.

About 30 000 years ago, modern *Homo sapiens* of a gracile form arrived in Australia, possibly across somewhat wider water crossings than the first men, but with watercraft no more sophisticated than those seen ethnographically in Australia. These modern men quickly spread through the continent and either replaced their predecessors or were able to occupy land not used by them. Objections that hunters cannot move through land already occupied by other hunting societies do not apply here because we are dealing with two sets of people with considerable differences in their cultural capacities. After all, modern sapiens Cro Magnon man managed to inherit Europe from its previous Neanderthal owners during almost exactly the same time period, an episode which I think reflects the same fundamental evolutionary process as the Australian one 10 000 km away on the other side of Asia. The new Mungo people had a broad spectrum economy capable of exploiting the inland wetlands possibly with additional food sources such as large fish, but they also had the technology to occupy new lands, such as the savanna plains and slopes and even the high montane valleys of New Guinea. Their stone technology was the foundational phase of the Australian Core Tool and Scraper Tradition and included waisted blades. Possibly the Kartan industry is an example of it. Bone tools were also made. They practised cremation and other complex burial customs, used ochre and expressed their artistic feelings on the faces of rocks.

There was some intermarriage between the two groups of peoples, which resulted in a population ancestral to the modern Australian Aborigines. In some environments, especially the riverine one of the middle Murray, the older group must have had a high enough population density to survive with less genetic input from outside than elsewhere until at least late glacial times, when its morphological range is exemplified by the Kow Swamp population. Even afterwards the modern Aborigines of this region held within them a greater proportion of genes of the older group than any others, which is why Birdsell chose the name ‘Murrayan’ to exemplify what he thought was a second wave of people of ‘rugged’ morphology.

An awkward problem for theories of Australian origins has always been that of the Tasmanian Aborigines. Locked away on their island on the southward side of the relatively rugged Aborigines of the mainland, their own morphology was more rounded and gracile—indeed much closer to the Mungo form than the Kow Swamp one. A solution for this problem may be one of sheer coincidence that at about the time that bands of the putative gracile and modern group reached the southeastern coast of Australia the sea-level was dropping, giving land access to Tasmania for the first time for some 30 000 years and thus allowing the new immigrants to reach a part of the continent not already occupied to some extent by a previous population. The Tasmanian Aborigines may have held within their genetic pool a closer approximation of the biological heritage of the putative second group than their cousins on the mainland, who gradually through intermarriage absorbed some of the genes of the first group. Perhaps similar processes may have been at work on some Melanesian islands such as New Britain, which has always required a cross-water access yet whose modern population seems closer morphologically to the Tasmanians than either do to the intervening Australians, yet all are of one group compared to the other races of modern man (Howells 1973). Did the second Mungo-like group represent the first colonists on this island sometime around 20 000–25 000 years ago, and are the waisted blades recovered there as surface finds legacies of such an act of Pleistocene colonisation (Golson 1971)?

This filling of the continent by modern man occupied many more ecological spaces than had probably been the case with the archaic first group. There is a consistent pattern that the first appearance of stone tools in many sites dated to between 18 000 and 26 000 years ago lie immediately over deposits devoid of man and his works but containing many bones of the 'giant marsupials' (Balme *et al.* 1978; Bowler *et al.* 1970; Flood 1974; Gillespie *et al.* 1978; Goede *et al.* 1978; Hope 1978; Hope *et al.* 1977; McIntyre and Hope n.d.; Merrilees 1968; Mountain 1979). These constituted the extinct third of the late Pleistocene marsupial fauna, whose demise must have been a catastrophic phase of faunal pauperation sometime between 25 000–30 000 and 18 000–20 000 years ago. The relative roles of climate and of man in this extinction process are the subject of intense current debate beyond the scope of this paper (Balme *et al.* 1978; Bowdler 1977; Gillespie *et al.* 1978; Goede *et al.* 1978; Hallum 1977; Hope 1978; Hope *et al.* 1977; Jones 1968, 1975; McIntyre and Hope n.d.; Merrilees 1968; White and O'Connell 1979; Calaby in Kirk and Thorne 1976), but I am left with a feeling that man, his fire-induced vegetational and erosional changes, and even his direct predation placed an additional stress on the marsupial fauna, which made the arid phase of the height of the last glacial period so much more devastating than all the others throughout the Pleistocene which had allowed the marsupials to proliferate and to radiate. Only twice in human history were entire continents, Australia and the Americas, colonised suddenly. Recent archaeological research is now placing these events into the broader perspective of the physical and cultural evolution of modern man.

4c Biogeography, human ecology and prehistory in the sandridge deserts

M.A. Smith

> Sunday September 7th 1845. Ascending one of the sand ridges I saw a numberless succession of these terrific objects rising above each other to the east and west of me. Northwards they ran away before me for more than fifteen miles . . . How much farther they went with the same undeviating regularity God only knows . . . The scene was awfully fearful, dear Charlotte. A kind of dread came over me as I gazed upon it. It looked like the entrance into Hell. Mr Browne stood horrified. 'Did man', he exclaimed, 'ever see such a place?!' (Sturt 1844–45: 72–4)

Sturt's 1844–45 expedition was the first to enter the immense dunefields that ring the centre of the Australian landmass (Fig. 4c.1). Later exploring expeditions (e.g. Carnegie 1898; Giles 1889; Lindsay 1886; Warburton 1875) were to answer Mr Browne's question. Such regions did indeed support small, and apparently flourishing, human populations. Had he known this, John Harris Browne, surgeon and savant on the 1844–45 expedition, might well have wondered how and when people had come to live in such inhospitable places. These are questions now central to current archaeological interest in the sandridge deserts.

Until a few years ago it seemed possible that the arid interior of the continent was unoccupied prior to about 10 000 to 12 000 years ago (cf. Bowdler 1977; Horton 1981). Enough archaeological evidence has now accumulated to show that the major desert uplands and river systems were occupied during the late Pleistocene (Brown 1987; Lampert and Hughes 1988; Maynard 1980; Smith 1987; Smith *et al.* 1991) and the debate has moved on to questions about the nature of this occupation (Hiscock 1989; Smith 1989) and whether or not there were significant differences in the timing of settlement in different parts of the arid zone. In an influential paper Veth (1989b) drew attention to the lack of evidence for occupation of the major sandridge deserts prior to 5000 years ago and argued that these regions would have been first colonised in the mid-Holocene. Given the paucity of evidence about the prehistory of the sandridge deserts, Veth has put forward what is essentially an a priori argument for their mid-Holocene colonisation, hinging upon the premise that before this time, there were persistent biogeographic barriers to the human settlement of these regions. The 'barrier desert' theory, as I have dubbed it, warrants critical examination because it is the cornerstone of a model explicitly proposed as a new framework for desert prehistory (Veth 1989a and 1989b).

Figure 4c.1: Longitudinal sandridges and playas in the Simpson Desert.

The purpose of this paper is to examine the ‘barrier desert’ theory in some detail. In turn I hope this will open up for discussion a range of issues concerning the biogeography, human ecology and prehistory of the arid zone. The paper is in four parts. The first considers whether the major sandridge deserts, the Great Sandy, Great Victoria and Simpson deserts, form a coherent biogeographic unit. The second part asks whether the ‘barrier’ deserts are fundamentally different to anything prospective colonists would have previously encountered in other parts of the arid zone. The third part of the paper deals with social and technological prerequisites for the colonisation of these regions. The final section reviews the archaeological evidence. Before proceeding, however, it is worthwhile outlining the key propositions of the ‘barrier desert’ theory. Places mentioned in the text are shown in Figure 4c.2.

The ‘barrier desert’ theory

The ‘barrier desert’ theory is part of a larger model (‘Islands in the Interior’) in which Veth (1989b following Smith 1988: 5–57, 293–343) argues for a post-Glacial recolonisation of the arid zone from areas which had provided refuge for human populations during the Last Glacial Maximum. It differs from earlier formulations in singling out the major sandridge deserts as regions where colonisation was independent of these palaeoenvironmental changes. Instead Veth argues that settlement of these regions was a consequence of various socioeconomic changes which took place in neighbouring

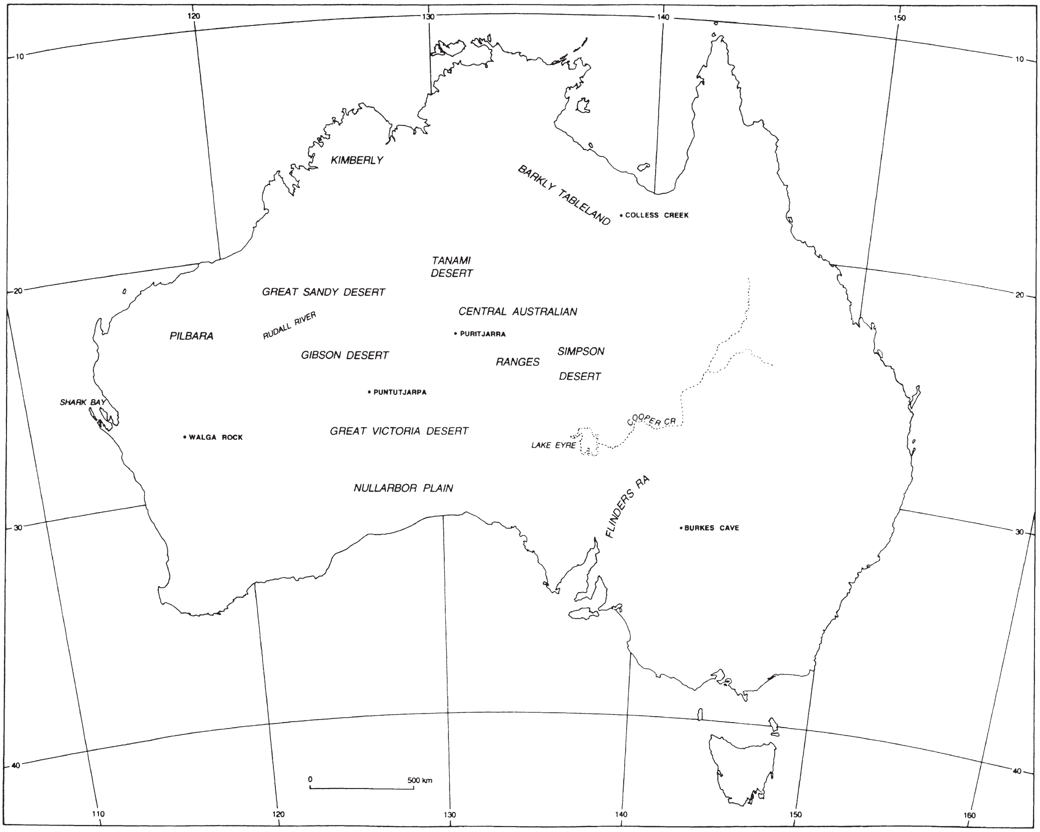

Figure 4c.2: Places and sites mentioned in the text.

parts of the arid zone during the mid-Holocene. Smith (1988) suggests that the sandridge deserts, along with other lowland desert habitats, would probably have been occupied initially in pre-Glacial times, abandoned during the Glacial Maximum and re-colonised as part of a post-Glacial expansion of population but Veth argues that sandridge deserts would have been first occupied in the mid-Holocene.

To emphasise the distinction between different arid habitats, Veth makes use of the biogeographic terms *barrier, refugia* and *corridor* (cf. Heatwole 1987). Under his scheme the major sandridge deserts, the Great Sandy, Great Victoria and Simpson deserts, are *barriers* (Figs 4c.3a and 4c.3b); the Pilbara, Kimberley, Flinders Ranges and Central Australian ranges are *refugia*; and intervening areas, including the Tanami, Strzelecki, Gibson and Tirari deserts, the Nullarbor Plain and the Barkly Tableland are *corridors*.

According to the theory, the association of dunefields with hummock grassland and uncoordinated drainage (Figs 4c.3b, 4c.3c and 4c.3d) would have posed major difficulties for human groups attempting to occupy the 'barrier' deserts. Hummock grasslands would have been an unfamiliar environment and, without the technical and ecological

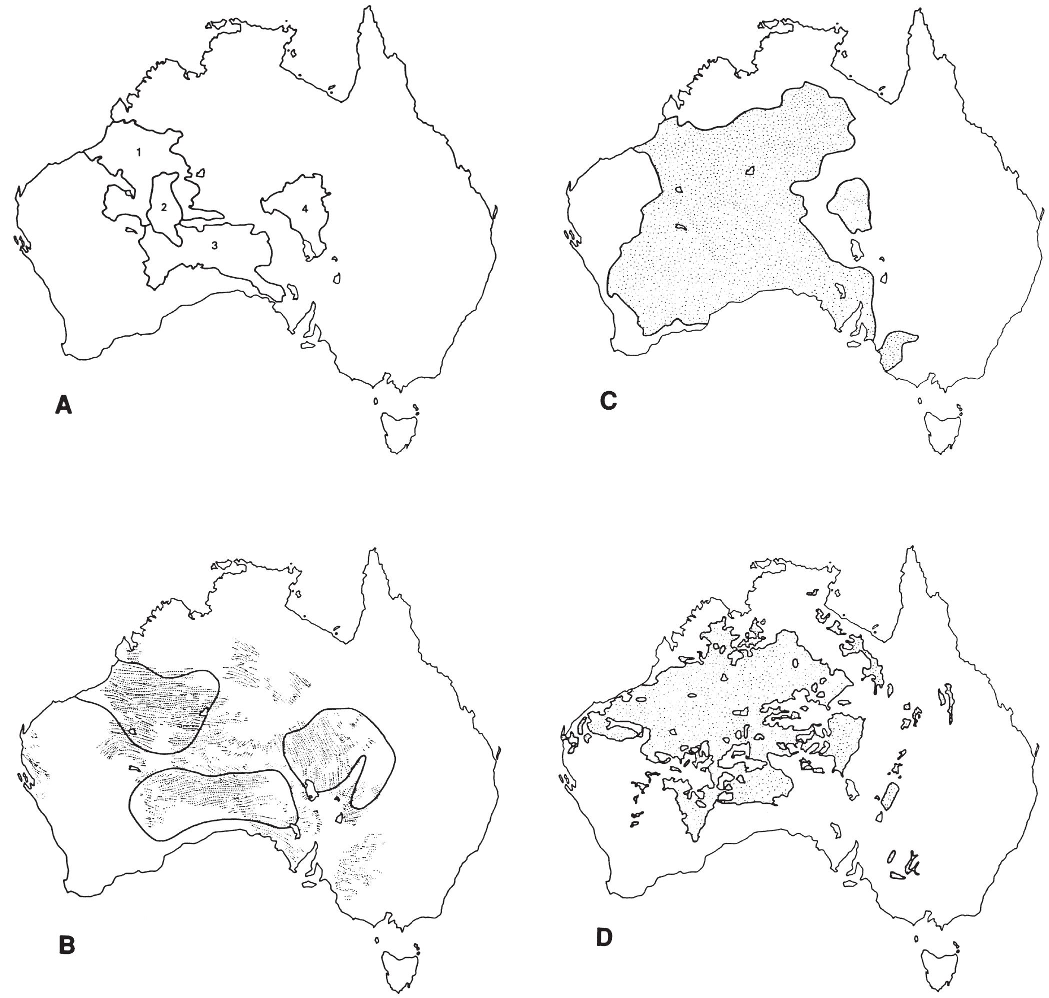

Figure 4c.3: Differences between the three major sandridge deserts.
A. Physiographic regions (after Jennings and Mabbutt 1977); 1: Great Sandy Desert, 2: Gibson Desert, 3: Great Victoria Desert, 4: Simpson Desert.
B. Continental distribution of longitudinal dunes with the 'barrier' deserts as shown by Veth (1989b) outlined.
C. Regions with uncoordinated drainage and riverless areas (after Mabbutt 1971: Fig. 6.1)
D. Distribution of hummock grassland (after *Atlas of Australian Resources* Volume 6 1990: 44).

knowledge necessary to harvest and process large quantities of grass seeds, also poor in plant food species. The spatial patterning of water resources would have been unfamiliar and vital supplies of shallow ground water difficult to tap without the technical knowledge necessary to construct wells. A range of technological, economic and social developments were therefore necessary before the 'barrier' deserts could be occupied on a more or less permanent basis. Veth (1989b: 83) lists the following.

a. the development of implements for working desert hardwoods and for processing wild seeds;
b. detailed knowledge of the distribution, seasonality and processing methods for useful seed-bearing species in the hummock grasslands;
c. the technical ability to construct and maintain deep wells to tap ground water; and
d. the emergence of extended social networks.

He sees all of these as likely to have been developed or acquired in adjoining regions, as adaptations to local conditions under pressure from increased population. This resulted in the 'emergence of regionally specific settlement and subsistence systems' (Veth 1989b: 83). We can liken the process that he outlines to one of *exaptation* whereby these changes opened up new niches for exploitation, in this case in the adjoining sandridge deserts.

The chronological framework for the theory rests on two lines of argument. Firstly, evidence that late Pleistocene groups were unable to cope with arid conditions. In support of this, Veth cites evidence from Colless Creek Cave, where the use of stone derived from sources outside the river and gorge systems ceased during full Glacial aridity (cf. Hiscock 1989). He also reminds us that initial occupation of the interior took place at a time when conditions were not as arid as at present and that the people who initially occupied the more favourable desert habitats may have been 'only partly pre-adapted to desert conditions' (Veth 1989a: 7). Secondly, he states that the prerequisite social, economic and technological shifts themselves did not take place until about 5000 BP. Veth is not explicit as to why these are necessarily part of a package of changes. But he clearly sees the need for a long period of adjustment to local conditions in the adjacent *corridors*, which themselves were only re-occupied in the aftermath of the Glacial Maximum.

Sandridge deserts as a biogeographic unit?

The first point at issue is whether the sandridge deserts actually form a biogeographic unit. By lumping together the Great Sandy, Great Victoria and Simpson deserts, Veth implies that they do and that they hold many features in common and have strong contrasts with neighbouring regions. Even a cursory search of the biogeographic literature shows that the case for this is not as strong as might be supposed.

Figure 4c.4: The Simpson Desert showing the disarticulated drainage system in the north and the belt of playas (in black) in the southeast.

Biogeographic regions

Baldwin Spencer divided Australia into three biogeographic provinces which he called the Torresian, Bassian and Eyrean provinces (Spencer 1896). Modern biogeographers have not altered this framework substantially (Archer and Fox 1984; Burbidge 1960; Heatwole 1987; Horton 1984; Johnson and Briggs 1975; Tyler 1990) though there are differences of opinion as to whether or not Cape York Peninsula or southwestern Western Australia should be regarded as additional provinces. In all of these schemes the interior of the continent is recognised as a single biogeographic province, variously labelled as Eyrean, Eremean or Sturtian. Diels (1906 cited in Carolin 1982) and Tate (1896) both proposed a preliminary subdivision of the Eremean flora into northern and southern elements and this is echoed in some more recent studies (e.g. Nix 1982). What is significant is that biogeographers do not view the sandridge deserts as a unit distinct from the remainder of the arid zone. Where biogeographic divisions are recognised within the arid zone they divide it into northern and southern zones, cross-cutting any grouping of the three sandridge deserts as a single unit.

Differences between barrier deserts

Although the three major sandridge deserts do have some obvious features in common, such as extensive dunefields, uncoordinated drainage systems and an understorey of hummock grassland (Figs 4c.3b, 4c.3c, 4c.3d), there are sufficient differences among them to suggest that they would each present different opportunities for humans in terms of their plant resources, landforms and hydrology. For example, in the Great Sandy Desert the dominant vegetation is hummock grassland, either *Triodia* or *Plechtrachne*, with an open tree or shrub layer of *Eucalyptu* in the northern part and mixed desert *Acacias* in the south. In contrast, the Great Victoria Desert has extensive tracts of mulga (*Acacia aneura*) woodland, mallee (*Eucalyptus gongylocarpa*) and *Casuarina cristata* woodland, while in the southeastern third of the Simpson Desert the dominant dune vegetation is sandhill canegrass (*Zygochloea*), rather than *Triodia* or *Plechtrachne* (*Atlas of Australian Resources* 1990). In view of the wide differences in latitude between the three deserts (19°S to 29°S), and consequent differences in light, thermal and moisture regimes, we might also expect differences in the distribution of important plant food species. In this regard, Nix (1982: 64) recognises two broad plant groups with characteristic temperature response patterns in the arid zone: one in the north and the other in the south with a wide zone of overlap in the central part of the region.

There are also significant differences in landforms and landscapes among the three regions. For example, the Simpson Desert forms a much smaller region than the other 'barrier' deserts and differs in some important characteristics from them (Graetz *et al.* 1982). Firstly, there are eight rivers: the Todd, Hale, Illogwa, Plenty, Hay, Field, Mulligan and Kallakoopah, which strike deeply into the dunefield (Fig. 4c.4). These originally formed part of the catchment of Lake Eyre but have been severed by the formation of the dunefield. However, even today the rivers that flood out into the Simpson Desert occasionally funnel large amounts of floodwater into the dunefield (Kotwicki 1986). Secondly, the southeastern part of the Simpson Desert is distinctive in that it has dunes made up of pale pelletal clay (Wasson 1983) rather than the red silicious sands which make up the bulk of the dunes in the three regions. This sector of the dunefield also contains an extensive belt of closely spaced playas.

Barrier deserts as new challenges?

At the heart of the theory is the premise that the 'barrier' deserts would have represented a new habitat, one fundamentally different to anything people would have encountered in either the glacial *refugia* or in the *corridors*. This does not stand up to detailed scrutiny, particularly if the 'barrier' deserts are compared with adjoining *corridors*.

Humans moving into the 'barrier' deserts would in many cases have found much poorer country than that they had left. It would however have been basically familiar in terms of the structure and composition of the vegetation, structuring of water resources and in its landforms. Nor would the *combination* of these features have presented a new set of circumstances for there are extensive tracts of hummock grassland, dunefield and uncoordinated drainage in areas outside the 'barrier' deserts (Figs 4c.3b, 4c.3c, 4c.3d). Even Veth agrees that prior to the colonisation of these regions people may well have visited them opportunistically during good seasons (1989a: 229, 1989b: 81). This allows plenty of scope for people to have acquired specific local information about water supplies and other resources within a few generations of occupying adjacent *refugia* and *corridors*. If there were barriers to permanent occupation of the major sandridge deserts, it is likely that these were due ultimately to the aridity of these regions rather than to their biogeographic characteristics, in other words due to differences of degree not of kind. These issues are explored in more detail below.

Floristic affinities with other desert habitats

Rather than being regions with an ancient, endemic flora and fauna, biogeographers take the view that the 'barrier' deserts have strong links with adjacent *refugia* and *corridor* regions (e.g. Greenslade and Halliday 1982; Schodde 1982). This is shown in floristic gradients running from north to south across the Great Sandy Desert (Burbidge and McKenzie 1983: 82–3 and Fig. 4c.1) and from west to east across the Simpson Desert (Fatchen and Barker 1979). The northern sector of the Great Sandy Desert shares many vertebrate and plant species with the adjacent Kimberley region, so much so that Burbidge and McKenzie (1983) suggested it could be considered as a formal interzone between the two biogeographic provinces, Torresian/Tropical to the north and Eyrean/Eremean to the south.

While it is true that hummock grasses such as *Triodia* and *Plechtrachne* are endemic genera, they are also very widespread genera (Fig. 4c.3d). Pure hummock grasslands are uncommon but an understorey of hummock grasses covers 26.9% of the continent (*Atlas of Australian Resources* 1990 Vol. 6, Table 1). Both formations extend into regions that are classified by Veth (1989b: Fig. 1) as *refugia* or *corridors* as well as those listed as 'barrier' deserts. In many cases species are shared between upland and sandridge desert habitats, as in the case of the Pilbara and Great Sandy Desert (Jacobs 1982: 288). Both *Triodia* and *Plechtrachne* are known to die during prolonged droughts (cf. Beard 1969). In fact, in a study of speciation within these genera, Jacobs (1982: 290) argues that it is probable that the hummock grasses were confined to *refugia* outside the sandridge deserts during the last glacial maximum and that they have subsequently recolonised the latter. This suggests that humans would have had a long and intimate association with these taxa in the areas Veth identifies as glacial *refugia*.

This pattern is also true of the *Acacias* which comprise an open shrub layer in the 'barrier' deserts. Across the arid zone there are a number of widespread and highly variable species, such as *Acacia ligulata* and *A. aneura*. Maslin and Hopper (1982) show that of the various species of *Acacia* found in the Great Sandy Desert a high proportion are shared with neighbouring regions, especially with the Tanami (50–64%) and with the Central Australian Ranges (41%). A similar pattern exists for the Great Victoria Desert, where 54% of *Acacia* species found there are also present in the Central Australian Ranges. Maslin and Hopper (1982: 311) also draw attention to a pattern in the distribution of groups of closely related species, in which at least one taxon in a related pair occurs around the periphery of the sandy deserts and the other within the desert.

Similarly, Buckley (1982) provides data to show the widespread distribution, within the arid zone, of species that make up the sandridge flora of central Australia.

All of this makes it seem very unlikely that humans attempting to colonise the sandridge deserts would have needed to learn to cope with an unfamiliar flora. Golson (1971) has made the point that humans moving into the arid zone would have had the opportunity in northern Australia to acquire some of the ecological knowledge essential for successful settlement of central Australia. The same would obviously be true of people colonising the 'barrier' deserts from either *refugia* or *corridors*. A further demonstration of this can be seen in a comparison of the plant species exploited for seed by Aboriginal people in the Great Sandy Desert with those used in Central Australia. At least 56% (23 species) of the plant species used for seeds of one sort or another in the Great Sandy Desert (Veth and Walsh 1988: Appendix 1) were also used for seeds in Central Australia (Latz 1982: Table 4; O'Connell *et al.* 1983). Another three plant species are shared between the two regions but were not used in Central Australia for their seeds.

Spatial patterning of water resources

Veth stresses the significance of uncoordinated drainage. Presumably the point here is that the 'barrier' deserts had not only poorer water resources but that these were also much less patterned and predictable in their spatial distribution. This is correct as far as it goes but plays down the significance of local drainage networks, such as the Rudall River in the Great Sandy Desert (Fig. 4c.2), which signal obvious points in the desert landscape in which to look for wells, soakages or waterholes.

It also assumes that without a coordinated drainage system the spatial distribution of water resources will be haphazard. This overlooks the influence of the underlying geology, including palaeochannels (Graaff *et al.* 1977), in structuring water resources. This patterning often has some surface expression in rock outcrops (e.g. calcrete) or in vegetation (Griffin 1990). One example here would be the *mikiri* soakages in the southern part of the Simpson Desert (Hercus and Clark 1986). These appear to be clustered along the northwestern margin of a chain of playas that marks the bed of a relict lacustrine basin (Loffler and Sullivan 1979). Another example is the influence of buried palaeochannels and associated shallow water tables (Arakel 1986) upon the occurrence of the *Triodia pungens* alliance in the hummock grasslands (Griffin 1990: 443).

In any case, an uncoordinated drainage system is the hallmark of much of the arid zone (Fig. 4c.3c), including some of Veth's *refugia* and *corridors*, and is not in itself likely to have been something unfamiliar to humans encountering the 'barrier' deserts for the first time.

Ranking of desert habitats

In making a distinction between 'barrier' deserts and *corridors*, the theory also assumes that dunefields are fundamentally more difficult environments for humans than other desert lowland habitats such as stony desert, sandplain or karst landscapes. This runs counter to current ecological studies which stress that topographic variety increases runoff and concentrates water and nutrients in a way which allows more frequent plant growth than on a flat landscape (Noy-Meir, 1985; Shmida *et al.* 1986; Stafford-Smith and Morton 1990). All else being equal, a dunefield should be a more productive habitat than a sandplain. In another context, Latz and Griffin (1978: Table 10) found that spinifex habitats in central Australia had the greatest variety of plant food species when compared to woodland and watercourse habitats. This indicates that neither dunefields nor hummock grasslands are invariably the poorest of the desert habitats.

Therefore, we can ask whether Veth's division of desert habitats into *barriers* and *corridors* is realistic. Are the Nullarbor Plain, Gibson Desert or Tanami sandplain really more favourable human environments than the Great Victoria and Great Sandy deserts? I think not. Veth's own data on subsistence patterns in the Great Sandy and Gibson deserts runs counter to his implied ranking of habitats. He notes that the Gibson Desert (a *corridor*) lacks edible roots and tubers such as *Vigna* and *Cyperus* and has a much more limited suite of plant foods than the Great Sandy Desert (38 species compared with 89 in the latter) (Veth 1989a: 24–5). He also notes that whereas the subsistence round in the Gibson Desert was non-seasonal and opportunistic as a response to the lack of permanent water sources in the region and low, unreliable rainfall, there was a predictable cycle of aggregation and dispersal in the Great Sandy Desert reliant upon a series of permanent or semi-permanent waters and reliable summer rainfall (Veth 1987).

Palaeoenvironmental considerations

The ‘barrier desert’ theory glosses over the potential role of palaeoenvironmental changes in altering accessibility of the sandridge deserts. Nevertheless the question remains of whether the ‘barrier’ deserts are susceptible to shifts in climate and whether they are likely to have been more accessible to humans at various times in the past. Here Veth clearly agrees that climatic fluctuations would have affected the distribution of human populations in the arid zone, predicting evidence for an ebb and flow of settlement in the *corridors*. However, he suggests that colonisation of the ‘barrier’ deserts would have depended upon specific adaptations rather than on any amelioration of climate.

On this matter it is worth considering an argument put forward by Ross (1989) that the arid zone, including the major sandridge deserts, is a very stable environment, one that is unlikely to have been transformed by any of the climatic shifts that took place during the Holocene. The implication is that despite climatic shifts, such as that from the optimal climate of the early Holocene to the cooler, drier conditions that followed, the major vegetation formations of low open woodland, chenopod shrubland, or hummock grassland would have maintained their present distributions. In this she is essentially correct because climate is not the only factor influencing the vegetation of these regions. They are also extremely poor in soil nutrients, in particular in phosphorus and nitrogen (Beard 1969; Stafford-Smith and Morton 1990; Winkworth 1967). This alone is likely to have made them unfavourable habitats for many plant species, even if the rainfall was somewhat higher than today.

Ross (1989; Ross *et al.* 1992) developed this argument to suggest that environmental changes during the Holocene would have had a negligible effect upon human occupation of the arid zone. However, I do not think we can discount the potential impact of even quite small climatic shifts upon the distribution of human populations in the region. Issues of scale are important. Although the sandridge deserts may not have been transformed, small climatic shifts would certainly have had an impact upon the distribution and availability of critical resources, in particular potable water, but also plant and animal foods. Many of the soakages, wells and rockholes in the sandridge deserts are not self-replenishing but are fed by regional catchments and local infiltration into dunes. As such they are vulnerable to shifts in rainfall patterns and changes in temperature regimes. The important point here is that it is not just scarcity of water that makes the sandridge deserts difficult for humans. Low productivity of the country is also a factor, placing a premium on residential mobility to meet subsistence needs. This mobility is underwritten by the seasonal or semi-permanent distribution of waters and any changes in this network can affect access to often quite large blocks of country (Cane 1987; Smith 1988). Changes in rainfall and temperature regimes can also have an impact upon the productivity of these areas, either in terms of fruit, seed or tuber production, changes in cover for reptiles and small mammals, and the presence of specific plant food species such as frost-sensitive fruit and tuber plant species and summer rainfall dependent grasses.

Given evidence for a strengthening of summer monsoon incursions and increased temperatures in the early Holocene (Singh and Luly 1991), one could well argue that the 'barrier' deserts would have been more accessible to humans at this time than at present. Under such conditions there may well have been a better, or at least more reliable, network of small watering points across the major sandridge deserts as well as an increase in the availability of plant foods. Palynological evidence from Lake Frome (Singh and Luly 1991) indicates that, in the southeastern sector of the arid zone, conditions after about 4500 BP were the driest since the Last Glacial Maximum. This underlines the point that, even if the sandridge deserts in their present form are seen as potential *barriers*, circumstances may have been rather different in the early Holocene and perhaps also in the period before the Last Glacial Maximum.

Prerequisites for colonisation

Leaving aside for the moment my doubts about whether the major sandridge deserts form a biogeographic unit, whether the division into *corridors* and *barriers* can be sustained, or whether the 'barrier' deserts would have represented a new challenge to humans, we can go on to review the factors that are listed as social and technological prerequisites for the colonisation of these regions. Veth (1989b: 83) specifically lists a number of factors.

a. the development of hafted implements for working desert hardwoods and implements for processing seeds;
b. detailed knowledge of the distribution, seasonality and processing methods for seed-bearing species in the hummock grasslands;
c. the technical ability to construct and maintain deep wells to tap ground water; and
d. the ability to create and maintain widespread social networks.

Seed grinding: a new technology?

Veth's most detailed argument concerns the need for a greater reliance on seeds in the 'barrier' deserts. According to the theory a shift towards more intensive use of seeds took place in the *corridors* at about 5000 BP. Implicit in this is the idea that the resulting ecological and technical expertise opened the way for exploitation of the hummock grasslands of the 'barrier' deserts.

One problem with this scenario, as we have already seen, is that the ecology of the 'barrier' deserts would have been familiar to inhabitants of *refugia* and *corridors*. We have good reasons for presuming that, in the late Pleistocene, there would already have been basic knowledge of the distribution and seasonality of most if not all of the plant food species.

If we turn to the issue of technical expertise, a fine distinction needs to be made between the appearance of specialised seed grinding implements in the archaeological record and the acquisition of the technical ability to process seeds. The latter has probably been available since the late Pleistocene as there are generalised grindstones in late Pleistocene assemblages from northern Australia (Kamminga and Allen 1973; Jones 1985; Roberts *et al.* 1990), southwest Western Australia (Ferguson 1981) and the Darling Basin (Allen 1972). I have argued elsewhere (Smith 1986) that the appearance of specialised seed grinders in the archaeological record heralds the adoption of a new mode of subsistence, not the acquisition of a new technology. From this perspective it would be valid to argue that seed grinding was a corollary of colonisation of these regions but invalid to see it as the proximate cause of these moves. It is unclear what Veth's position is on this point. If he opts for a technologically deterministic stance, with acquisition of the technical ability to grind seeds as a factor controlling the timing of colonisation, then there is no reason why the human settlement of the sandridge deserts could not have taken place well before 5000 BP. If he takes the alternative stance, he must seek other reasons for a mid-Holocene move into the 'barrier' deserts.

Finally, we might challenge the view that the ethnographic exploitation of seeds was a feature that distinguished the 'barrier' deserts from other desert habitats. Veth's own data do not convincingly show a greater reliance on seeds (including *Acacia* as well as grass seeds) in the 'barrier' deserts. For instance, seed food species comprised 48% of the plant species used traditionally as food in the Great Sandy Desert, compared with about 50% in central Australia (53% using figures by Latz 1982: Table 4 or 39% using figures by O'Connell *et al.* 1983 specifically for the Alyawarra). Veth and Walsh (1988) are also at some pains to point out that the Martujarra made substantial use of roots and tubers as staple foods. Another obvious alternative is reptiles. The herpetofauna of these regions is greater and more diverse than in comparable sandridge habitats in other parts of the world (Pianka 1984). Use of a wide range of resources is what we might expect in the 'barrier' deserts, given a correlation between environmental variability and behavioural plasticity or opportunism (Yellen 1976). Thus although seeds may have been integral to ethnohistoric subsistence patterns, this is not necessarily a special feature of the 'barrier' deserts nor does it imply that use of these resources was necessary to colonise these regions.

Desert wells and landesque capital

The second factor that Veth discusses in any detail is the ability to construct and maintain wells. Desert wells were narrow shafts dug down to reach the water table. They were often dug on an incline and were up to 7 m deep. Sometimes they were lined with straw and mud and, in very rare cases, roughly shored up. In other cases they were natural solution conduits in calcrete or limestone that were enlarged and maintained.

It is clear from the ethnography that human occupation in much of the 'barrier' deserts was reliant, at least for part of the year, upon such wells (cf. Hercus and Clark 1986). Other water conservation techniques included the cleaning out and covering of rockholes and in some cases the enlargement of rockholes using fire (Kerwin and Breen 1981).

There is little doubt that such wells were an essential part of the infrastructure of life in the sandridge deserts. Nor is there any argument with the idea that a network of such wells would take some time to establish and would represent what Brookfield (1986) aptly terms *landesque capital*. However, their construction requires detailed local knowledge rather than sophisticated technology and there is no need to ascribe a long period of technical evolution to account for the development of such wells. The digging of deep soakages would have also been part and parcel of life in desert *refugia* where it would often have been necessary to obtain water from soakages in dry stream beds. One could as well argue that the basic knowledge and the skills required to dig these wells would have been available to humans living in the Central Australian Ranges in the late Pleistocene.

The point about the build up of *landesque capital* in terms of wells, rockholes and a productive mosaic of vegetation through patch burning is an important one. It may have been necessary to establish such an infrastructure in advance of the frontier before permanent occupation could occur. Surely though this is part of any process of colonisation and in this case need not require more than two or three generations to establish.

Other technological and social factors

Two other factors are mentioned in passing as prerequisites for colonisation of the 'barrier' deserts. The first is the use of hafted implements, such as the tula adze, for manufacturing the hardwood bowls and dishes used in seed collection. Tula adzes are a highly distinctive item of desert technology and an efficient implement for working desert hardwoods (Sheridan 1979) but are hardly crucial to occupation of the region. In any case, serviceable wooden bowls, suitable for water, seeds or small babies can be made without adzes simply by detaching the bark and outer wood of a eucalypt with a cobble (pers. obs.).

The remaining factor concerns the importance of extended kinship networks in mitigating local resource stress. If we accept that extended kinship networks are the key to maintaining biological and social viability at very low population densities then there is no doubt that they are a prerequisite for not only the successful occupation of the desert but also the process of colonisation itself. People moving into new territory beyond the frontier in small family groups are likely to have placed a premium upon the maintenance of such networks. The catch here is that these networks would have been as necessary for the colonisation of desert *refugia* and *corridors* as for the 'barrier' deserts. One could argue that such structures must have already been in place in the Central Australian Ranges in the late Pleistocene to account for the survival of a human population there throughout the Glacial Maximum (Smith 1989a). This is especially so given that the overall population density may have been very low at this time, perhaps comparable to ethnohistoric levels in the Great Sandy Desert. Here again I see no a priori reason for claiming a mid-Holocene genesis for such networks.

Demographic pressure in adjoining regions

To sum up at this point, Veth has not made a sufficiently strong case that hafted woodworking implements or a shift towards intensive use of seed foods are *prerequisites* for colonising the 'barrier' deserts. Nor can he sustain the argument that extended kin networks or the ability to construct deep wells are *mid-Holocene* innovations. What we are left with is the proposition that it is demographic pressures in the *corridors* that set the stage for occupation of the 'barrier' deserts. While there is nothing especially surprising in this idea, the problem is in explaining why this necessarily took place at 5000 BP, millennia after settlement of the *corridors*, and not at say 10 000 BP, 7000 BP or perhaps even at 35 000 BP.

Testing the theory

Setting aside any criticism of the premises of the 'barrier desert' theory and simply evaluating it on its own terms, how well is it supported by existing archaeological evidence?

A rigorous test should not only deal with the predicted outcome, in this case mid-Holocene colonisation of the 'barrier' deserts, but should also deal with the processes that are specified in the theory. For example, we would expect there to be evidence for population growth in adjoining regions between 10 000 BP and 5000 BP; evidence of economic and social transformations in the corridor regions shortly before 5000 BP; and evidence that tula adzes or seed grinding implements were associated with the colonisation of these regions. We might also look at whether regions with similar water resources, landforms or plant resources were occupied before 5000 BP. Veth (1989a, 1989b) presents only a perfunctory test of the theory, restricting his discussion to whether there are any sites older than 5000 BP in the major sandridge deserts.

A review of archaeological evidence shows that other aspects of the theory are not well supported. The earliest radiocarbon dates yet available for occupation of the 'barrier' deserts are likely to be only minimum ages for occupation of these regions. There is no intrinsic reason to think that these dates reflect initial colonisation of these regions. The notion that seed grinding and the harvesting of wild seeds were part and parcel of the first moves into these regions is not well supported by existing data. Seed grinding implements do not appear in basal levels of sites in the 'barrier' deserts and do not appear in assemblages there or in neighbouring regions until about 1500 BP, several millennia later than the earliest evidence of occupation. Nor is there

Table 4c.1: Radiocarbon dates for basal occupation horizons at sites in the Great Sandy Desert (data from Veth 1989a)

Site	*Radiocarbon dates for basal occupation (years BP)*	*Sample code*
Jalpiyari	5030±60	WK-1255
Karlamilyi rockshelter	3180±70	WK-1093
Winakurijuna	900±70	WK-1158
Karlamilyi quarry	500±50	not given
Yulpul	315±150	WK-1256

archaeological evidence that social and economic transformations took place in adjoining regions immediately prior to mid-Holocene colonisation of the 'barrier' deserts. These points are dealt with in more detail below.

Mid-Holocene colonisation of the barrier deserts?

Despite some pioneering work (Cane 1984; Hercus and Clark 1986; Hughes and Lampert 1980; Smith 1988; Smith and Clark 1993; Veth 1987, 1989a, 1989b; Williams 1988) very little is presently known about the prehistory of the sandridge deserts.

The only detailed regional prehistory available for any of the three major dunefields is for the Rudall River region of the Great Sandy Desert (Veth 1989a). Five stratified sites were excavated and all have basal occupation deposits of mid- to late Holocene age (Table 4c.1) resting directly upon bedrock. Several factors immediately suggest that these results should be interpreted as *minimum ages* for occupation of the region. Firstly, there is great variation between the ages for initial use of different sites. Secondly, the lack of a stratigraphic record for the period before 5000 BP means that there is no direct evidence bearing on whether or not the region was occupied prior to this date. Thirdly, deposits appear to have begun to accumulate at Karlamilyi rockshelter and at Jalpiyari as a result of lintel blockfall which rules out any simple correlation between the onset of sedimentation and human use of these sites. There is therefore no indication in this evidence that the full span of occupation in the region is represented.

Although the Rudall River investigations are a step in the right direction, one might also question whether these sites are well placed for testing the 'barrier desert' theory. According to Veth (1989a: 20–1) the Rudall River 'is the only extensive riverine complex entirely within a desert region of Western Australia' and is 'well watered with a relatively high density of permanent waters when compared to other regions of the Little/Great Sandy Deserts' (1989b: 89). Veth's ethnobotanical work makes it clear that local Aboriginal people preferentially foraged for plant foods in the watercourses and floodplains of the river rather than on the dunefield. Maps of physiographic regions show the nearby Throssel, Broadhurst, McKay and other ranges as a peninsula of range

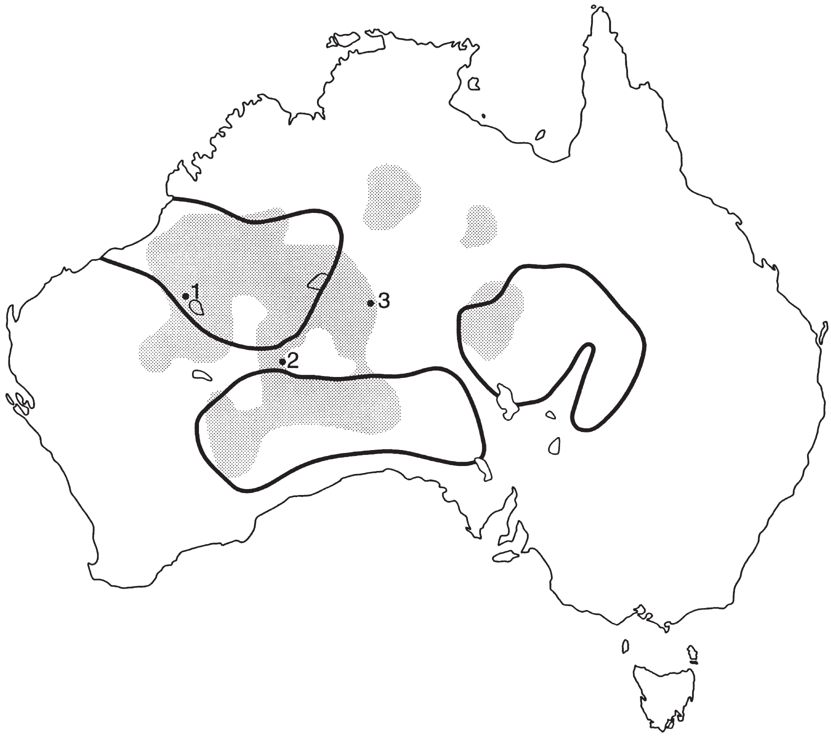

Figure 4c.5: The distribution of areas which combine dunefields with hummock grassland and uncoordinated drainage (shaded) contrasted with the boundaries of the 'barrier' deserts as shown by Veth (1989b: Fig. 1) (solid line). 1: The Rudall River sites, 2: Puntutjarpa rockshelter, 3: Puritjarra rockshelter.

country projecting into the dunefields from the Pilbara (see Beard 1969: 693; Jennings and Mabbutt 1977). Therefore, I think it is fair to ask whether the Rudall River area has the particular combination of hydrological circumstances, landforms and plant resources that are specified in the theory.

This raises a more general problem with the way in which Veth tests the theory against archaeological data. Figure 4c.5 shows that there are substantial differences between the boundaries of the sandridge deserts as shown in Veth's Figure 1 and the actual distribution of areas which combine dunefields with hummock grassland and uncoordinated drainage. The discrepancy is serious enough to cast doubt on his conclusions about the temporal distribution of sites in such areas.

Less is known about the archaeology of the other major sandridge deserts. There are no radiocarbon dates for occupation in the Great Victoria Desert. The first series of dates for occupation of the Simpson Desert and its margins (Smith and Clark 1993) cluster within the last 3000 years but data is too sparse yet to tell whether this pattern represents a regional trend and if so what it may mean.

The most compelling piece of archaeological evidence against the idea that the sandridge deserts were first occupied in the mid-Holocene is the long Pleistocene and early Holocene record of occupation at Puritjarra rockshelter in the western part of Central Australia (Smith 1987, 1988, 1989a). As Figure 4c.5 shows, Puritjarra is in an analogous position to the Rudall River sites *vis-à-vis* the sandridge deserts and is in an area which perhaps more closely conforms to the physical criteria specified in the 'barrier desert' theory. Similar points could be made about Puntutjarpa rockshelter, which has a record of occupation extending back to 10 000 BP.

Social and economic transformations in refugia and corridors

The most obvious example of some form of social and economic transformation taking place on the periphery of the 'barrier' deserts is in the Central Australian Ranges (Smith 1988), where the appearance of seed grinding implements in archaeological sites coincides with a substantial increase in occupation of sites beginning about 600 to 1400 BP. Similar changes are registered at sites in the *corridors*, such as at Walga Rock (Bordes *et al.* 1983) and Burkes Cave (Allen 1972: 138–218) and are mirrored in changes in site use in the Rudall River sites (Veth 1989a: 175). Some form of reorganisation of activities at large ceremonial sites also appears to have taken place in the Central Australian Ranges within the last 1000 years (Smith 1988: 269–92). Although these are exactly the sorts of changes that we might have expected to find under the 'barrier desert' theory, they are clearly too late to be a contributing factor to a mid-Holocene colonisation of the sandridge deserts. No comparable changes are evident in the mid-Holocene.

Tula adzes, seed grinders and colonisation

If the exploitation of seeds or the development of hafted woodworking implements was integral to colonisation of the 'barrier' deserts, we could expect to recover seed grinding implements, or tula adzes, in the earliest archaeological levels in these regions. This is clearly not the case, even for the Rudall River sites. According to Veth (1989: 174):

> No formal grinding bases . . . were recovered from the excavations. All of the other formal implements, i.e. backed pieces and tula and burren adzes, occur in very low numbers and are associated with deposits which, from the respective depth/age curves for each site, are assumed to be less than 1500 years old.

Karlamilyi is the only one of the stratified sites where grindstones are present at some depth. At this site fragmented grindstones and mullers are reported to be present down to spit 14, bracketed by radiocarbon dates of 3180±70 (WK-1093) and 1120±50 (WK-1092) (Veth 1989: 133) and presumably dating to about 1500 to 2000 BP.

Even in the Central Australian Ranges there is little evidence for the presence of seed grinding implements before about 1500 BP (Smith 1988: 334–8) or of tula adzes and other elements of composite hafted implements before about 3000 to 3600 BP (Smith 1988: 333). For earlier evidence of seed grinding implements we need to look at sites located on the eastern margin of the semi-arid zone, where there are several examples dated to about 3000 to 3500 BP (summarised in Smith 1989b: 311) and perhaps earlier (Balme 1991).

Population growth in refugia *and* corridors

The lack of sites with an uninterrupted record of occupation between 10 000 BP and 5000 BP makes it difficult to test the proposition that population density in *corridors* and *refugia* increased at this time. Only Puritjarra rockshelter has a continuous record of changes in site use during this period. Use of this rockshelter substantially increased at about 7000 BP (Smith 1988: 129–32). The use of Puntutjarpa rockshelter may also have increased from about 7000 BP (Gould 1977). Though the evidence is limited it is not inconsistent with an increase in population density in these regions in the early Holocene.

Adaptation to aridity in the Pleistocene

Veth cites the marked impact of full Glacial aridity upon patterns of land use in the Colless Creek area as evidence that late Pleistocene groups were not fully adjusted to aridity. It is more probable that this documents particular local circumstances, the sharpness and amplitude of environmental changes in this region and the availability of alternative resources, rather than supposed pan-continental levels of adaptation.

Other archaeological evidence suggests that, on the contrary, late Pleistocene groups were capable of adjusting to a wide range of circumstances within the arid zone. For example, Bowdler (1990) suggests that well-developed inland economies, based on macropods and emu eggs, might have been in place in the Shark Bay area of Western Australia by 25 000 BP or earlier. In the Central Australian Ranges a human population appears to have been present throughout the Glacial Maximum (Smith 1989a). Throughout the late Pleistocene and early Holocene the occupants of Puritjarra rockshelter would have been dependent upon the resources of the surrounding spinifex sandhill habitat. People using the Strzelecki Desert/Coopers Creek region in the late Pleistocene (Smith *et al.* 1991; Veth *et al.* 1990) would also presumably have been reliant upon the resources of the surrounding dunefields for at least part of the year. Although the riverine corridors of Coopers Creek and Strzelecki Creek were no doubt an important focus of this occupation, I doubt whether humans could have relied exclusively upon the resources of these comparatively poor riverine habitats. Periodic use of the surrounding dunefields may well have been an integral part of land use in this region even during the late Pleistocene.

Discussion

Direct archaeological evidence about the prehistory of the arid zone is still sparse and speculative models, theories or hypotheses have a heuristic role in helping to organise our data, explore it in new ways or identify priorities for further research. The 'Islands in the Interior' model was put forward as a new framework for desert prehistory and enthusiastically accepted in some quarters (e.g. Ross *et al.* 1992). Its most original element *vis-à-vis* earlier models is the concept of 'barrier' deserts and I have therefore focused my attention upon this aspect. It presents an a priori argument for mid-Holocene colonisation of the three great Australian sandridge deserts: the Great Sandy, Great Victoria and Simpson deserts. In this paper I have attempted to show that although the 'barrier desert' theory is useful in drawing attention to regional variability within the arid zone it does not provide a good framework for understanding events in the sandridge deserts. A review of the biogeography, human ecology and archaeology of these regions highlights problems with the theory as it stands. These are summarised below.

Firstly, the regions that are grouped together as 'barrier' deserts do not form a natural biogeographic unit in the sense implied in the theory. They have greater floral and faunal affinities with adjacent regions than with each other. The presence of floristic gradients into the sandridge deserts from adjoining regions and the widespread distribution of key plant species such as *Triodia* and *Plechtrachne* point to the fallacy of portraying these deserts as sharply defined regions with an ancient endemic flora. Humans moving into the 'barrier' deserts, especially those moving in from the *corridors*, would have found an environment basically familiar in terms of the structure and composition of the vegetation, structuring of water resources and landforms. Nor would the combination of these features have presented a new set of circumstances, for there are extensive tracts of hummock grassland, dunefield and uncoordinated drainage in areas outside the 'barrier' deserts. The distinction between *barriers* and *corridors* is especially difficult to sustain, whether it be based on biogeographic grounds or on the constraints these regions place on their human populations.

Secondly, there is the issue of whether specific social, economic or technological adaptations were necessary to colonise these regions and whether the time frame specified in the theory is realistic. That extended kin networks and deep wells may be essential for living in these regions is not in dispute. However, there are no grounds at present for claiming these as exclusively mid-Holocene developments and certainly nothing to suggest that they were developed as part of a package of changes at this time. A reliance upon seeds as an adaptation to hummock grasslands is cited as a special feature of the 'barrier' deserts. This runs contrary to ethnographic data showing that the bulk of plant species used for their seeds are the same as those used in other desert habitats and that reliance upon seed foods in the 'barrier' deserts is no greater than in these other areas. There are also good reasons for thinking that the technical expertise and ecological knowledge to utilise seed foods is likely to have been available from the late Pleistocene. Seed grinding may turn out to be a corollary of the first moves into the 'barrier' deserts but is not likely to have been the proximate cause of their colonisation. The wider issue here is whether adaptation is about acquiring a finely honed economy, a specialised technology or specific strategies, or whether adaptation is about having flexible and opportunistic responses to changing circumstances and being able to map rapidly onto local resources and new terrain. Pleistocene Australians probably had the latter. If the major sandridge deserts posed problems for humans it was because these regions are arid and impoverished rather than because of any fundamental differences in landforms, vegetation or patterning of water resources.

Thirdly, if we turn to the archaeological record, there is no indication that:

a. the earliest radiocarbon dates yet available for occupation of the 'barrier' deserts reflect initial colonisation of these regions;
b. seed grinding implements, which are present only after about 1500 BP, were connected with this process;
c. social and economic transformations took place in adjoining regions immediately prior to the mid-Holocene.

What the archaeological record does show is a long period of occupation on both western and eastern margins of the Great Sandy Desert, the latter in a broadly comparable environmental setting.

If the 'barrier desert' theory is unconvincing as an a priori argument for mid-Holocene colonisation, where does this leave the prehistory of the sandridge deserts? The principal alternative scenario posits that fluctuations in climate, in so far as they have an impact upon critical resources, such as potable water, are likely to have been the major factor affecting the accessibility of these regions to humans. At various times resources in the sandridge deserts, along with other lowland desert habitats, may have been so poor, variable and dispersed that a viable hunter-gatherer population could not be sustained. At other times these constraints would have been relaxed somewhat, making the deserts accessible to a small, mobile human population on a more-or-less permanent basis. On current palaeoenvironmental reconstructions it seems likely that the 'barrier' deserts would have been accessible sometime prior to the Last Glacial Maximum and became so again in the terminal Pleistocene or early Holocene. Therefore, we might expect re-colonisation of the major sandridge deserts to have been an integral part of the post-glacial settlement of the surrounding *corridors*. This may have proceeded very rapidly in empty territory, with groups budding off well before any population pressure was felt (cf. Birdsell 1957; Rindos and Webb 1992) as studies of other colonising populations have found (e.g. Irwin 1992; Jarman 1986; Stodart and Parer 1988). If this is the case we should expect to eventually find archaeological sites dating to at least 10 000 or 12 000 BP in the major sandridge deserts.

The challenge now is to see whether the prehistory of the sandridge deserts can be uncovered in some systematic fashion, giving due attention to both the timing and process of colonisation, rather than be slowly bludgeoned into shape by the haphazard accumulation of new field data.

Acknowledgements

This paper was originally given at a seminar at the Australian National University in May 1990 and is presented here in a revised form. I am indebted to Peter Veth for his thought-provoking theory and take full blame for any raised blood pressure my critique might cause. I wish to thank Jane Balme, Dick Kimber, Anne Clarke, Mike Morwood and Bert Roberts for commenting on a draft and Marg Friedel, Graham Griffin, Steve Morton, Elizabeth Williams, Bob Wasson, Gary Dunnett, Nancy Sharp and Peter Veth for useful discussion of the issues raised in this paper. Figures 4c.2, 4c.3, 4c.4 and 4c.5 were drawn by Ian Faulkner.

5 Late Holocene Australia and the writing of Aboriginal history

Christine Williamson

The archaeological investigation of late Holocene Australia has been dominated by what has become known as the 'Intensification Debate'. This long-lasting argument has involved archaeologists in seeking an understanding of the causes and significance of changes in prehistoric Aboriginal society during the last several thousand years. Consequently, three of the four papers reprinted here relate to these arguments. However, while we should acknowledge the importance of that debate, particularly during the early 1980s, I will argue that in order to write the Aboriginal history of Australia we need to take a more inclusive approach to what has occurred on the continent in the last several thousand years. This inclusive approach involves integrating the archaeology of pre-contact Aboriginal Australia with that of the contact period and even into the twentieth century. The paper by Murray, reprinted here, is seen as being a part of this agenda.

The 'Intensification Debate' can be seen as a reaction to the functionalist approaches to the analysis of prehistoric societies that dominated world archaeological theory in the 1960s and 1970s. In the late 1970s archaeologists became increasingly concerned that explanations for social change that had occurred in the deeper past seemed overwhelmingly deterministic. According to these approaches societies were viewed as systems made up of interacting subsystems that operated to maintain equilibrium or homeostasis. As a result any impetus for change within the system was seen to be due to external pressures rather than driven by forces within the system itself. This systems model was widely criticised as being dehumanising, of placing the motive forces for human history away from human beings and investing them in vast impersonal forces such as environment or population change. In criticising this approach Bender observed that, 'at best, a systemic analysis serves to describe the working of a given system, it neither explains the genesis of the system, nor its subsequent transformation' (1981: 150).

In the late 1970s and early 1980s many researchers started to look to other explanations for change in human societies, especially those of the post-Pleistocene period. This period was seen as having heralded dramatic changes in societies all over the world —in particular the move towards the adoption of agriculture. This trend was described by Flannery as 'the broad spectrum revolution' (1969). While all might be agreed that these Holocene societies experienced tremendous change over a comparatively short stretch of time, there was a predictably wide range of opinion about how to describe this change and then how to explain it. Intensification was defined simply by Bender as an increase in productivity for a given area. It seemed self-evident that economic 'intensification' had occurred, however, there were disagreements as to what the mechanisms leading to such a change were. Archaeologists had been debating the causes of this change since the nineteenth century. They had tended to settle on explanations which saw the intensification as being the result of either an increase in human knowledge about animals and plants and the subsequent adoption of agriculture as a better way of life, or as a necessary reaction to population pressure and consequent resource stress, often due to environmental changes associated with the end of the last glaciation.

Again, archaeologists such as Bender were quite happy to recognise that environmental factors and population pressure doubtless had a role to play. However, explanations that focused only on these sources of change took no account of the influences of processes of social differentiation that might be seen as being 'internal' to those societies. Inspiration was sought from social theories that had their foundations in Marxism and in the many varieties of structural-functionalism and structuralism which were at that stage very popular in social anthropology. Thus, in contrast to the deterministic environmental/ecological explanations of the systems theorists, archaeologists with a focus on historical materialism argued that explanations for change needed to focus upon the social relations of production and the basically competitive nature of human society (Zvelebil 1986: 10; Bender 1981: 149).

It is an important point to note that many of these theories regarding the appearance of agricultural societies in the post-glacial period originated in Europe and the Middle East. Here the transformation from Mesolithic foraging societies to relatively complex Neolithic food-producing societies had manifestly occurred. There could be no doubt about the direction of the trajectory of human societies evolving from the simple to the complex although, even here in the heartland of food production, there was a tremendous range of variation in both the rates and the complexities of change. The explanation of this trajectory was quite straightforward. An intensification in social relations and increased productivity and technological innovation were seen to lead towards population growth, food storage, increased sedentism and the development of social stratification with the ultimate outcomes being agricultural and complex societies and the emergence of states (Zvelebil 1986: 8). Given the dramatic nature of climatic changes at this time, most explanations ultimately saw the origins of this process in post-glacial environmental changes.

In order to maintain the validity of the progressive line of reasoning many non-agricultural late Holocene societies were argued to have in fact contained the seeds of an agricultural society. The archaeology of South West American Indian groups, non-agricultural European groups and so on were examined for markers, or take-off preconditions, of the transformation towards an agricultural society. This tenuous assumption also coloured the debate within Australia where researchers frequently pondered whether Aborigines were well on the highway to farming only to be cut off by the road

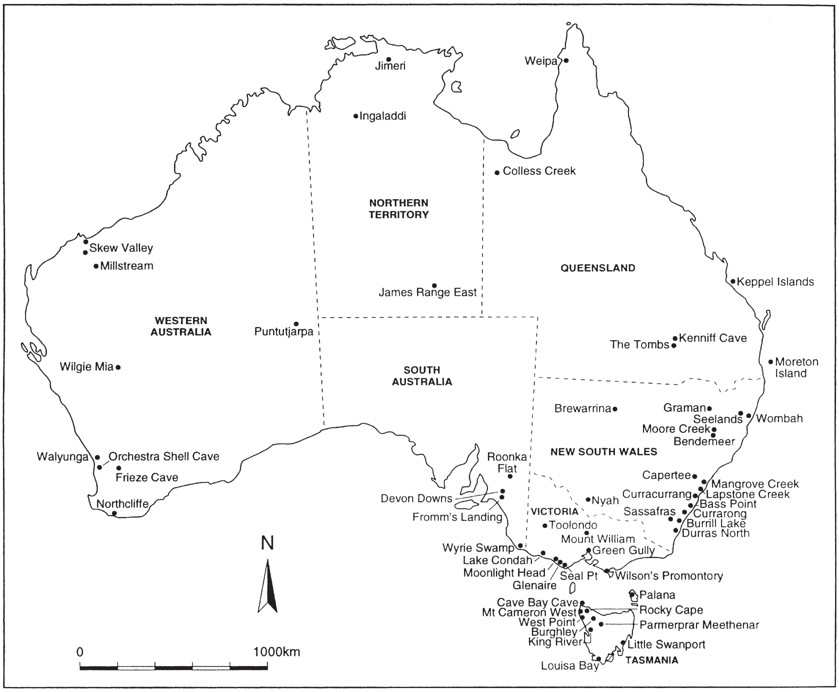

Figure 5.1: Holocene sites in Australia

block that was the arrival of Europeans. Davidson has argued that the view that an agricultural lifestyle is inevitable over a hunter-gatherer lifestyle is a hangover from the social Darwinist theories put forward in the colonial period as justifications for the invasion of lands that were already occupied by non-agricultural peoples (1989: 74).

In Australia a number of changes *are* evident in the archaeological record of the Holocene. Most scholars agree that the Holocene sees a number of changes in the archaeological record, including:

- an increase in the number of sites and the intensity of their use in many areas of the continent;
- an overall expansion into new areas that were generally considered to be more marginal (including the use of offshore islands);
- changes in some food procuring and processing techniques;
- a major change in stone tool technology with the appearance of the Australian Small Tool Tradition which replaced (or was added to) the Core Tool and Scraper Tradition that was thought to have remained essentially unchanged throughout the previous period. Ethnographical records and other eyewitness accounts have also been used to demonstrate that at contact large-scale gatherings, regional exchange and interaction networks and ceremony were important features of Aboriginal society. However, the significance of these changes, their timing, and their explanation

forms the framework of the debate about transformation of post-Pleistocene societies in Australia.

The concept of 'intensification' was first introduced to Australia through the work of Harry Lourandos, who had been influenced by Bender's discussion of shifts in alliance networks as the explanation for the adoption of a more intensive subsistence economy by Middle American foraging societies. Lourandos specifically rejected the view of researchers such as Birdsell who proposed that Australian Aboriginal society had been basically static for 40 000 years. Lourandos and Ross argue of the intensification debate that 'it focused attention and research upon questions of change and dynamics within hunter-gatherer societies of the past, especially regarding demographic, socioeconomic and sociocultural factors' (1994: 54). Lourandos was keen to demonstrate similarities rather than differences between late Holocene Australian hunter-gatherers and agricultural societies and argued that changes in the late Holocene could best be explained as the consequences of economic growth *following* a restructuring of social relationships that placed extra stress upon the economy and thus production (1985). This idea was taken up by a number of researchers who carried out regional archaeological analyses aimed at identifying intensification of social relations in the archaeological record (Ross 1981; Williams 1987; David *et al.* 1994; Morwood 1984; Attenbrow 1982). Most of the research was restricted to eastern Australia and the general model constructed from this data then applied on a continent-wide scale. Recently Edwards and O'Connell (1995) have argued that one of the reasons that models of socially driven change are popular in the explanation of late Holocene Australian patterns is that here, unlike other areas of the world, the appearance of 'broad spectrum' changes is unassociated with any major environmental disruptions.

The model of socially driven change was immediately attacked on a number of fronts. Most researchers agreed that the late Holocene changes formed a 'package' but disagreed that the key factor in their appearance was social. In contrast more traditional systems-based models were applied that argued for economic change forced by environmental pressure and/or population growth. Some argued that the late Holocene in Australia was a period of exponential population growth and that changes could best be explained as a feature of this population pressure (Beaton 1983, 1985; Hughes and Lampert 1982). Others believed that the environment of the Holocene was more variable than had previously been thought and that shifts in ecology could account for the observed changes (Jones 1975; Smith 1986; Rowland 1983). An extension of this argument was that one or two changes in food gathering and processing technology, in response to changing ecology, allowed for population increase and subsequent intensification (Bowdler 1976, 1981).

Hiscock also argued that the use of stone discard rates as an indicator of the intensity of site use was invalid as technological change can easily account for variability without invoking the notion of intensification (1981, 1986). Others argued that 'intensification' was probably an inappropriate concept to use in the Australian context as, compared to what occurred in other societies in other areas of the world in the post-Pleistocene period, the Australian culture remained relatively stable and intensification occurred only in the narrowest sense of the term (Beaton 1983: 95).

It was also argued that information about society is not easy to derive from the stones and bones of the archaeological record and that to separate the social from other elements of the system was not analytically valid (Beaton 1983: 95; Hutchet 1991: 49). Some researchers even questioned the validity of the late Holocene 'package' and a number of 'spoiler' arguments were put forward. These ranged from attacks on the supposed synchronous timing of events, to the operation of post-depositional factors and site formation processes in the selective destruction of older material, especially in older sites (Bird and Frankel 1991; Godfrey 1989; Head 1983).

The debate was not one sided. Lourandos and others responded by providing long lists of apparently synchronous changes in the late Holocene archaeological record such as the appearance of new site types (mounds), new harvesting technologies (fish traps, poison removal, shell fish hooks), new foods (bogong moths, Macrozamia) and so on. They also argued that environmental changes did not correlate with the changes in the archaeological record, and therefore changing environmental pressures or population increase could not be posited as causal factors independent of cultural factors (Lourandos 1985). Using ethnographic analogy they interpreted the late Holocene data as demonstrating the expansion of social relationships as played out in the increased importance of ritual and large communal gatherings supported by the new foodstuffs appearing in the archaeological record from this time period. They also indicated that skeletal evidence supported the view that the late Holocene was a period of dietary stress for many Aboriginal populations and interpreted this as indicating population pressure driven by these new social relationships.

Notwithstanding the strength of this counterattack, it was never made clear what prompted the social changes that were thought to have driven changes in population and economy in the first place. It is fair to say that after a decade of discussions the causal links remain unexplored. One is left with the unsatisfactory impression that these social changes are seen as being part of the biological evolution of modern *Homo sapiens sapiens*. The implicit argument behind much of this work was that such social changes are cumulative and form part of an evolutionary scheme of intensification from simple foragers, through complex collectors to agriculturalists. Indeed Williams contends, in her paper on the appearance of Western District mounds after 2500 BP (and the claim that they represented sedentary or semi-sedentary village sites), that the demonstration of such traits traditionally thought to characterise more complex groups 'throw much light on variation within the hunting-and-gathering mode of production and on pathways between hunting-and-gathering and agriculture' (1987: 311).

This brief discussion of the 'Intensification Debate' serves to contextualise the papers reprinted below, but it also serves to highlight the basically progressive and linear evolutionary nature of the models that have been used to explain late Holocene change in Australia. So pervasive is this often unstated assumption that Aboriginal societies were treading the conventional pathway of social and cultural evolution that evidence that is seemingly contrary has tended to be overlooked. For instance many Holocene sites, while showing clear evidence for either first occupation or more intensive occupation during the late Holocene, also show a subsequent decrease in the intensity of occupation in the last 2000–1500 years (Hiscock 1986). This feature has been noted by researchers in a number of different areas, however, such potentially disturbing data has not served to seriously disrupt the intensification argument. Surely if an increase in site numbers and site use in the late Holocene is interpreted as an indicator of increased population, increased social relations and so on then the drop off noted over the last 2000 years must indicate a reversal of these trends if the argument is to maintain its logical consistency?

I argue that the intensification debate has in fact obscured rather than clarified interpretations of change in the late Holocene. Too much emphasis has been placed upon a single identifiable marker (the appearance of backed blades), and there have been too many attempts to correlate all other changes with what many see as an archaeologically instantaneous point in time. The result of this lopsided investment of energy and interpretive strategy is that little emphasis has been placed upon investigation of earlier time periods and the changes also evident within them (this point has also been made by Bird and Frankel: 1991). Bailey has argued that the approach of Bender and Lourandos, among others, sets up an unnecessarily sharp division between hunter-gatherers and agriculturalists. Rigid binary classifications of this kind tend to emphasise discontinuities and change with explanations that highlight the disruptive, rather than looking towards gradual process and long-term trends (1981: 1). In my view change is a constant feature of the Australian archaeological record of all time periods. However, evidence for change should not be interpreted within the straitjacket of progressivist models, but rather be interpreted in a more non-linear fashion.

Research focused on the contact period, and the writing of Aboriginal history for the entire duration of occupation of the continent rather than just the prehistoric component, demonstrates the need to revise traditional frameworks of explanation and interpretation as applied to the archaeology of Australia. It is now well understood that pre-contact Australia has often (wrongly) been viewed as static and unchanging for most of its history with any change that was evident having been forced from outside of the system. Even the more social approaches of workers such as Lourandos and Ross view the contact period as a time when everything stopped and any progress that was being made was halted. Changes once again were forced on Aboriginal Australia from outside—this time by the dominant European culture.

Writers of Aboriginal history, both historical and archaeological, have tended to highlight the divide between pre- and post-contact, between prehistory and history, by choosing the time of invasion as their starting point. However, as Braudel has stated, prehistory and history are one and the same process and any division is, by necessity, artificial (1989: 20). Generally when dealing with Aboriginal Australia the prehistorians stop with invasion and the historians and historical archaeologists, on those few occasions when they actually chose to consider Aborigines, start with it. When writing Aboriginal history it is important for the archaeologist to cross this artificial divide. It has been argued that in order to understand what happens to an indigenous society at contact it is necessary to investigate pre-contact adaptations, the situation during contact, and also what happened after (Bartel 1985: 12). Consequently the archaeology of contact must cover a longer temporal span than the actual period of contact itself.

In my view the best way to approach the writing of an Aboriginal history that encompasses both the pre-contact, contact and post-contact periods is through exploring the notion of trajectories. Trajectories, following Clarke, are defined here as temporal sequences of transformations in culture systems (1968: 42). Any system can ultimately be shown to be either a time or space transformation of another system state as archaeological systems tend to be continuous (Clarke 1968: 45). In arguing this I am not saying that a particular archaeological 'point' is predetermined by or totally explicable from what preceded it. What I am arguing is that a particular 'point' in any archaeological system will bear the footprint or echo of what preceded it. Any system will therefore be constrained, to a certain extent, by where it has been in terms of the possible directions that it may take in the future. Consequently, the study of pre-contact situations may enable us to delimit some perameters for the post-contact period. Put another way, 'whereas the products of cultural replication are not predetermined by existing circumstances, the consequences of their existence are delineated at several levels of process' (Fletcher 1992: 42). Clearly investigation of this kind would be dependent upon the scale of analysis adopted by the archaeologist. There is already a wide literature, both within archaeology and external to it, on resolving time-scales and the impact of different scales on analyses and interpretations. It is not appropriate to reiterate all of that here, however, the point needs to be made that when attempting to identify archaeological trajectories, scale of observation becomes critical.

For example, an examination of prehistoric Aboriginal society and modern Aboriginal society will reveal vast differences that in many ways cannot be understood or explained in terms of one another. It is not possible to see how things moved from point A to point B. However, if we start to fill in some of the missing points we can begin to get some idea of trajectory and, consequently, history. However, this is not history in the conventional sense. It is not a seamless narrative of events, causes and effects. It is an archaeological history of artificially linked discontinuities. As Eddington has stated, 'we often think that when we have completed our study of *one* we know all about *two*, because "two" is "one and one". We forget that we still have to make a study on "and" ' (1958: 104). We should not be led into the trap of thinking that because we have filled in some of the points and gained some idea of trajectory, that breaking down history and prehistory into progressively smaller and smaller units will allow us a better understanding. Ultimately the writing of Aboriginal history is structured by the resolution of the archaeological record.

The approach advocated above is radically different to that usually taken by Australian archaeologists in that it implies no directionality in Aboriginal history but rather opens the door to an exploration of possibilities. Contact can be seen as a 'bifurcation point' in Aboriginal history, a point where major external pressures led to an opening up of the system to new possibilities. From this point a number of different trajectories were possible but those which were actually followed were the product (at least in part) of the history of the system. In my view the history of Aboriginal Australia is filled with such points and therefore the writing of Aboriginal history must be the tracing of trajectories and possibilities rather than the tracking of a single evolutionary process halted by the arrival of Europeans.

5a Holocene environments and prehistoric site patterning in the Victorian Mallee

A. Ross

Introduction

Throughout their long occupation of this continent, Aborigines have had to adapt to a variety of fluctuating environments. This paper examines the changing environmental conditions during the Holocene in the Mallee of northwestern Victoria, and discusses the influence that these environmental changes may have had on the population distribution in the area as reflected in archaeological site patterning.

Today the Mallee is the driest region of Victoria, with a rainfall of 350 mm per annum in the south and only 250 mm in the north. Most of the rain falls during the cool winters; summers are hot and dry and droughts are common. There are at present only two natural sources of permanent fresh surface water in the Mallee, the Murray River and Lake Hindmarsh (Fig. 5a.1). Throughout the area there are numerous ancient lake systems and palaeochannels which are indicative of wetter environments in the past. These features make the Mallee an ideal area for a study of adaptions to changing water supply.

The study area

The Mallee district lies within the Murray Basin, a circular plain bounded to the east and south by the Highlands (inset, Fig. 5a.1). The Basin may be divided into two basic landscapes, the riverine plain (Riverina) to the east, and the low lying Mallee to the west. The Mallee owes its flatness to the Pliocene inundation of the Murray Basin by the sea (Jones and Veevers, in press). Pliocene marine sediments are veneered by the aeolian Parilla Sand, a series of NNE-SSW aligned ridges representing beach strandlines of the retreating sea in the terminal Pliocene (Idnurm and Cook 1980; Gill 1973). It is these sands which form the substrate of the Mallee landscape.

The study area (Fig. 5a.2) covers approximately 30 000 km^2. The most common surface features in this area are the east-west aligned linear dunes. These were formed from the reworking of Parilla Sand during periods of extreme aridity (cf. Bowler 1978). The final phase of linear dune activation probably occurred 20 000–15 000 BP (Bowler

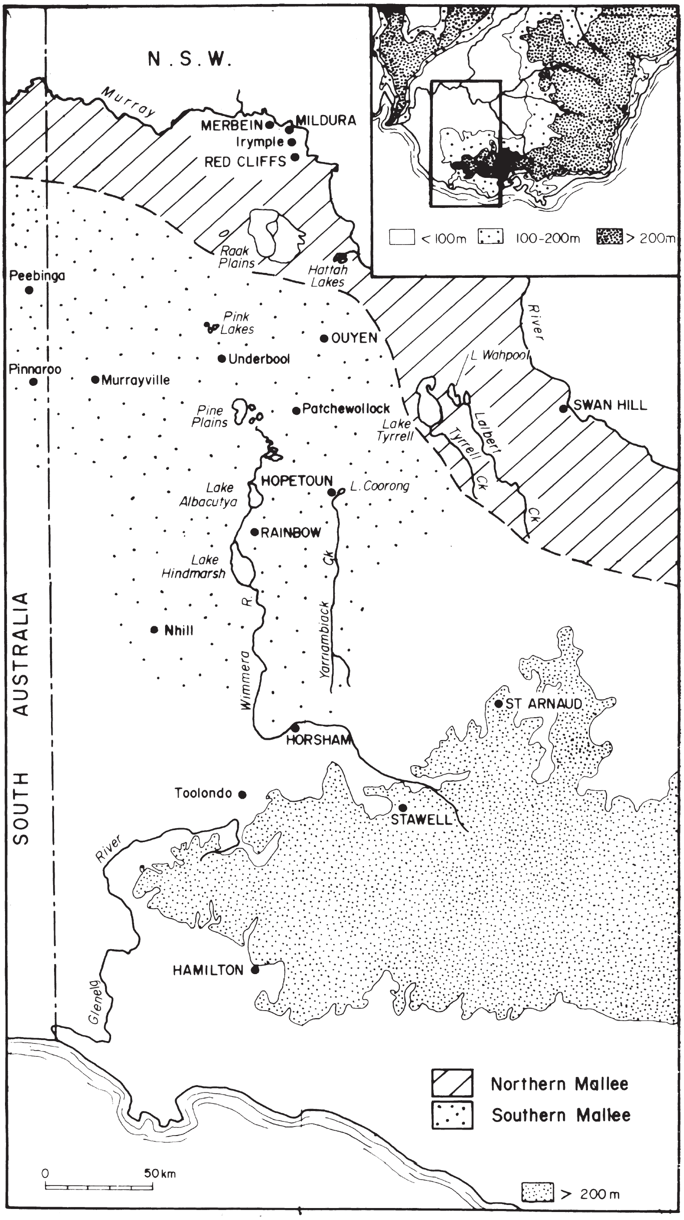

Figure 5a.1: The location of the study area, and other places mentioned in the text. Inset: Location of study area in the Murray Basin.

1976; Bowler *et al.* 1976; Bowler 1978). In the south, large parabolic dunes appear to have overridden the linear dunes west of Outlet Creek some time after the period of final linear dune activation (Fig. 5a.3). The irregular dunes to the east may be of similar age.

The ancient lake systems are another characteristic feature of the area (Fig. 5a.2).

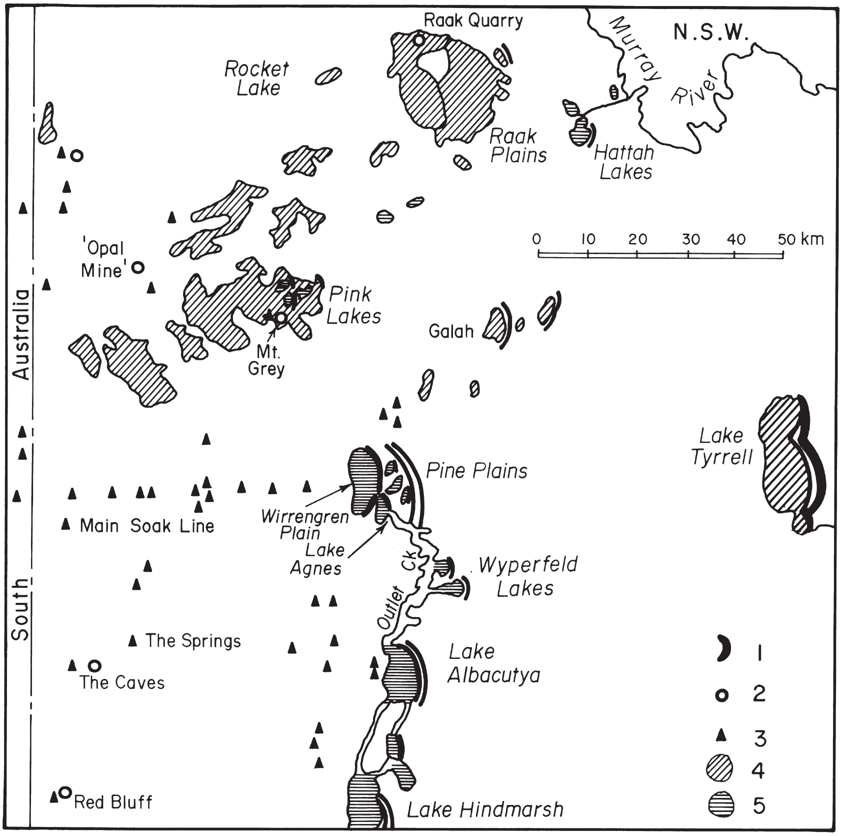

Figure 5a.2: Location of major areas and sites mentioned in the text. 1. lunettes; 2. stone sources; 3. soakages; 4. dry lakes; 5. wet lakes.

Lake Hindmarsh in the south is a large, usually closed lake on the Wimmera River, which rises in the Grampians and flows north. Lake Hindmarsh overflows from time to time down a channel known as Outlet Creek. This watercourse runs through Lake Albacutya and thence through the small lakes of Wyperfeld National Park (here termed the Wyperfeld Lakes), eventually terminating in the Pine Plains system. There is no surface outlet from Pine Plains, and the Quaternary Wimmera has never flowed into the Murray. Some flushing of these lakes by groundwater flow may have been important when the lakes were full (P.G. Macumber, pers. comm.). In historic times floods have rarely reached Pine Plains.

The Outlet Creek/Pine Plains system is the major surface water system of the Mallee. The other lakes, such as Raak Plains, the Pink Lakes and Rocket Lake, were formed primarily by ground water discharge (Macumber 1980).

Soaks, tapping near-surface ground water and runoff are the other major sources of water in the Mallee. Soaks and water-holes are found at or near the ground surface

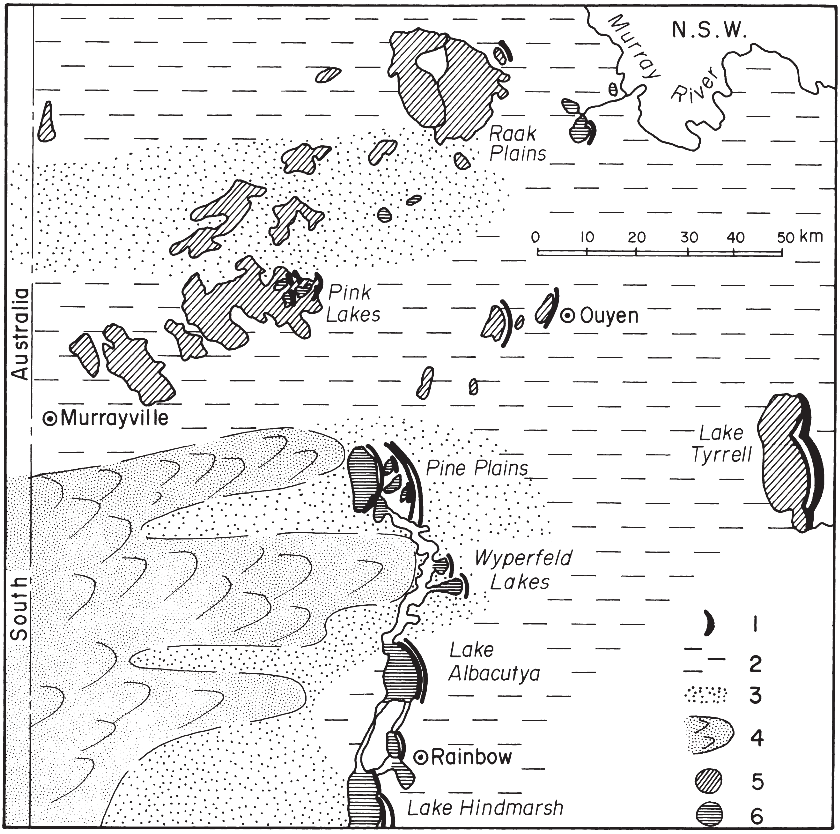

Figure 5a.3: Geomorphology of the Victorian Mallee (drawn from aerial photographs). 1. lunettes; 2. linear dunes; 3. irregular dunes; 4. parabolic dunes; 5. dry lakes; 6. wet lakes.

throughout the study area, but the main concentration is to the west of Pine Plains where shallow ground water is fresh rather than saline (Fig. 5a.2). These soaks were probably vital to Aboriginal life when Outlet Creek and associated lakes were dry.

Past environments

Prior to 35 000 BP the Mallee was somewhat wetter than present, with most of the lakes full and fresh (Bowler 1973, 1978; Bowler and Magee 1978; Bowler *et al.* 1976; Macumber 1980). The period from 35 000 BP to 20 000 BP saw the onset of global cooling and associated desiccation. Lakes and rivers gradually dried or became saline, and sand dunes became devegetated and mobile. By 18 000–15 000 BP most of the now semi-arid parts of southeastern Australia were virtually devoid of any fresh water (Bowler 1976).

It was not until the terminal Pleistocene and early Holocene that environmental conditions seem to have ameliorated. The presence of a suite of molluscan fauna on an 11 m high beach at Pine Plains indicates a very wet period existing in the Mallee from 12 000 BP to 7000 BP (Macumber 1980). The six species of mollusc found on the beach have been identified by Dr B. Smith of the National Museum of Victoria as *Plotiopsis balonnensis*, *Velesunio* sp., *Physastra gibbosa*, *Glyptophysa aliciae*, *Gyraulus meridionalis* and *Corbiculina australis*. All species are found today in the slow or intermittently flowing streams and billabongs of the Murray–Darling system. Although ostensibly fresh water species, all are tolerant of the salinity fluctuations in the present Murray system. This suggests that Pine Plains, when full, was a series of fresh, or perhaps slightly brackish, lakes with gently flowing water.

Since the Pine Plains lakes are the terminal lakes of the Wimmera–Outlet Creek system, high water levels such as occurred 12 000–7000 BP have far-reaching implications. All the lakes upstream from Pine Plains must have been full to overflowing at that time, making the whole system a corridor of water and life through the dunes. An early Holocene date on mussel shell (*Velesunio* sp.) from the now dry or saline lakes on Raak Plains (see below), and similar ages for high lake levels along the Darling River system to the north (J. Hope and J. Balme pers. comm.) and at Kow Swamp to the east (Macumber 1977) suggest that this event was not confined to the Wimmera, but was a widespread phenomenon.

The wet phase does not appear to have continued for long. A sudden drop in lake levels at Pine Plains is suggested by evidence of an 11 m high beach dated to 7460±120 BP (SUA-763) and a 1 m high beach dated to 7280±105 BP (SUA-764)[1] (Macumber 1980). Most of the Mallee was dry again by about 7000–6000 BP. From that time the Mallee has been essentially semi-arid. The presence of *Velesunio* in the Wyperfeld lakes at 2500–1000 BP (see below) indicates a more recent wet phase in northwestern Victoria, but the scale of this event was far smaller than that of the early Holocene. How then did the Aboriginal inhabitants of northwestern Victoria react to these environmental changes?

Adaptation to environmental variability

> Hunter/gatherers are as much a part of the ecosystem in which they operate as are the geographical features or the animals and plants, so that it can be assumed they will make adjustments comparable to those made by other parts of the biome which expand into new, perhaps empty regions when conditions are favourable and fall back onto the reservoir areas at times of adversity. (Clark, 1980: 529)

Many workers in arid and semi-arid parts of the world have seen human adaptation to environmental change in this light.

Clark (1980) in Africa, and Allchin, Goudie and Hegde (1978) in India, on a long time-scale (stretching back into the early Pleistocene in Africa), and Allen (1974) in Australia and Yellen and Harpending (1972) in South Africa, on a short, basically seasonal time-scale, have suggested that during periods of climatic optimum people will be dispersed throughout their range, living in small groups around all available water resources both temporary and permanent. During dry times the population congregates around the larger and more permanent water sources until the supply becomes too meagre to support such a population. At such times of peak aridity there is little or no occupation of the arid areas, and the surviving population is forced to retreat to a large lake or river which becomes a refuge for human and beast.

On the basis of this hypothesis, a particular pattern of site distribution in north-western Victoria could be expected. Sites which were occupied during dry phases should mainly occur near areas of permanent water such as Lake Hindmarsh and the soaks; on the other hand, during wetter times sites should occur throughout the area.

Sampling procedure

It is a truism to suggest that any site surveying methodology must be designed to meet the particular requirements of research strategy, as well as the vagaries of the tract of country being surveyed.

> There is no single best sampling procedure for regional surveys. The sampling procedure must take into account at least these important parameters: the information desired, the distribution of that information in space, cost of obtaining samples, and degree of precision needed, etc. (Read, 1975: 60)

The nature of the Mallee country produced probably the greatest constraint on the sampling procedure followed for this study. A methodology of 10% or 20% random quadrat sampling was not possible owing to the large scale of the area, the inaccessibility of much of the dune country in the west, and the lack of fresh surface water. Therefore this study used a modified version of the random transect survey of ecologically stratified zones advocated by Judge, Ebert and Hitchcock (1975), Plog (1976) and others (see Mueller 1975; Flannery 1976a). This method proposes that the field area be stratified into geomorphological/ecological zones, which are then surveyed by either randomly selected or regularly spaced transects cutting across the 'ecological grain'.

The field area was divided into four geomorphologically stratified zones:

1. Outlet Creek/Wyperfeld Lakes/Pine Plains
2. Soaks

3. Salinas and other ground water discharge areas
4. Desert dunes

Each zone was surveyed using random transects. However, the modification of the strategy outlined above was that the transects were not determined using any statistical system, nor do many of the transects cut across the ‘ecological grain’. Existing roads, tracks and fire trails became the basis for all surveys. Once again the inaccessibility of the dune country, the dense vegetation and the lack of fresh water made such an approach essential. Indeed Flannery (1976b) has suggested that in difficult country such ‘trail transects’ are suitable substitutes for traditional surveying techniques:

> Attempting to sample the lowland Maya jungle [or Mallee desert dune country] by .5 km^2 quadrats would border on lunacy. Even if you succeeded in actually doing it, no one would ever believe you. . . . the only hope for probability sampling in such tropical [or desert] areas would be to use *transect samples* . . . In some respects, the surveys . . . along trails . . . are a form of ‘transect’. (1976b: 159)

Therefore, by using detailed maps of tracks and fire trails in the Mallee, aerial photography and local knowledge, the four stratified zones outlined above were surveyed by ‘random trail transects’.

The main water system of the Mallee from Lake Albacutya north to Pine Plains was surveyed most intensively. With the aid of present and retired landowners, Rangers and their staff and local ‘desert enthusiasts’ to augment the information from maps and air photographs, I believe that at least 70% of this area was surveyed and most of the visible sites recorded. This survey was further augmented by the examination of artefact collections made by many of the residents of the area. Some of these collections are extremely well catalogued and provenanced.

Soaks and ‘native wells’ were also surveyed in detail, with army maps providing additional information to the fire trail maps and air photographs. All soaks marked on these maps were visited and intensively surveyed, as well as some located only on air photographs, which also proved to be accessible. A flight over the area in February 1981 confirmed my notion that over 70% of the soakages in the area had been surveyed.

Salinas and other ground water discharge areas were also given a particular emphasis for surveying. Most of these areas occur in the north of the field area, and the majority of these are marked on bush-fire trail maps and are clearly visible on air photographs. Most are incorporated into leasehold grazing land, and assistance from lease-holders and local ‘desert enthusiasts’ was invaluable. Park Rangers and Soil Conservation Authority personnel were also guides in this difficult country. Over 70% of ground water discharge areas were surveyed.

Surveying the desert dune country was the most difficult and least rewarding part of the fieldwork. The main tracks followed for this part of the survey were those from Rocket Lake to Underbool and Cowangie (near Murrayville), from Murrayville to Nhill through the southern Big Desert, the 'border track' following a straight line along the 'vermin proof fence' marking the Victoria–South Australia State border, and the east–west track from Peebinga to Rocket Lake (Fig. 5a.1). Although wind-scoured ground and dune blowouts occur along these tracks, no sites were located unless near to water. But even here there was not a one-to-one relationship between water and sites. Isolated sources of water located several tens of kilometres from the main water areas showed no signs of Aboriginal occupation. Places such as Red Bluff in the south west of the survey area and the Caves west of the Springs on the Nhill-Murrayville track (Fig. 5a.2), where suitable flaking stone as well as water is available, were particularly noticeable in their lack of evidence for occupation.

In summary I would estimate that over 70% of the water resource areas were surveyed intensively for archaeological sites. Although less than 5% of the dune country was surveyed, the lack of water in this part of the field area would make the likelihood of sites occurring here rather slim. Only three systematic surface collections were made from sites located; most of the detailed observations on artefacts from other sites were made in the field.

The archaeological evidence

All archaeological sites so far located in the Mallee are surface scatters, found on dune blowouts, on lake-side sediments, or on aeolian ridges around salinas where grass cover is thin. This means that interpretations cannot be made with the same confidence as from excavated sites. Given these limitations, to what extent does the archaeological evidence support the predictive hypothesis outlined above?

Apart from a possible early occupation of Lake Tyrrell during the wetter period prior to the Last Glacial Maximum (see below), the earliest prehistoric sites in the Mallee date to the period of high lake levels in the early Holocene. These sites are at Raak Plains in the northern part of the Mallee, and date to 7650±110 BP (SUA-766)[2] on the basis of a radiocarbon date on *Velesunio* sp. found with hearths and artefacts.

Raak Plains is a saline ground water discharge area, and throughout most of its existence was totally unsuitable for human occupation. However, 8000 years ago at least some of the salinas must have held fresh or only slightly brackish water (B. Smith, pers. comm.) to a depth sufficient to support *Velesunio* in the quantities seen to have been exploited by the Aboriginal population of that time. Since the Raak system has never had a surface inlet or outlet (Macumber 1980), fresh water probably came into the lakes from increased seepage and runoff from the surrounding dunes as a result of the higher rainfall regime which existed in the Mallee from 12 000 to 7000 BP (ibid; J.M. Bowler, pers. comm.).

All the sites at Raak Plains appear to predate the period of geometric microlith tools. The artefacts are of the large core tool and scraper tradition, with small horse-hoof cores, large flake scrapers and large utilised flakes dominating the tool assemblage. Some of the thick shell of the mussel *Alathyria* has also been worked and must have been a useful raw material in an area where suitable flaking stone is available but not common. Most of the tools at Raak Plains are made on coarse grained siliceous sandstone and quartzite, or indurated ironstone. The ironstone crops are out at the northern end of Raak Plains (Fig. 5a.2) as well as elsewhere on the plain. The siliceous sandstone and quartzite may also be local, being from the Tertiary Parilla ridges (R. Vernon, pers. comm.), however the exact source of all raw materials used is as yet unknown and some material may have been imported from outside the Mallee region.

All other known sites in the Mallee are much younger than those at Raak Plains. Tools and flakes at these later sites are small; most artefacts measure less than 25 mm in length. Geometric microliths are present but not numerous. Artefact collections made by many local landholders consist primarily of microliths and small heavily retouched flakes, as well as hatchet heads and grinders. Only one collection, an extremely well catalogued and provenanced collection of material from sites south of the Wyperfeld Lakes, had any artefacts typical of the Raak Plains assemblage: two large horse-hoof cores and several large scrapers among some 3000 small tools and geometric microliths from Lakes Hindmarsh and Albacutya. Given that many of the collectors concentrate on large artefacts such as grinders and hatchet heads which are easily seen from the tractor or saddle, the fact that almost no large flaked tools appear in the collections is significant.

On the basis of the lithic assemblage it seems that these southern Mallee sites are unlikely to be older than 4500 BP, which is the earliest date for most other microlithic sites in southeastern Australia (Johnson 1979). Two radiocarbon dates on *Velesunio* from sites at the southern end of the Wyperfeld Lakes of 2310±80 BP (SUA-1109) and 1470±80 BP (SUA-1110) add support to the dating by typology.

The raw material for the stone tools at these sites is rather different from that employed at Raak Plains. Both coarse- and fine-grained siliceous sandstone and quartzite were used as well as chert and jasper. The sandstone and quartzite may come from local Parilla outcrops at quarries such as those at Mt Grey and the ‘Opal Mine’ at the southern gateway to the Sunset Desert (Fig. 5a.2). However, stone from both these sources usually flakes poorly and it is more likely that the fine-grained material at least was imported from outside the Mallee region. Chert and jasper dominate the material at these later sites, as opposed to Raak Plains where it is a minor component. Chert is certainly not found in the Mallee (R. Vernon, pers. comm.), and it must have been imported; however the source of this raw material has not yet been determined. This need to import material may be one of the reasons behind the high incidence of small flakes at these southern sites, and for the large amount of retouch occurring on these flakes.

Thus there are two main types of sites in the Mallee: the Raak sites dating to the wet period from 12 000–7000 BP and composed of large flakes and cores of coarse

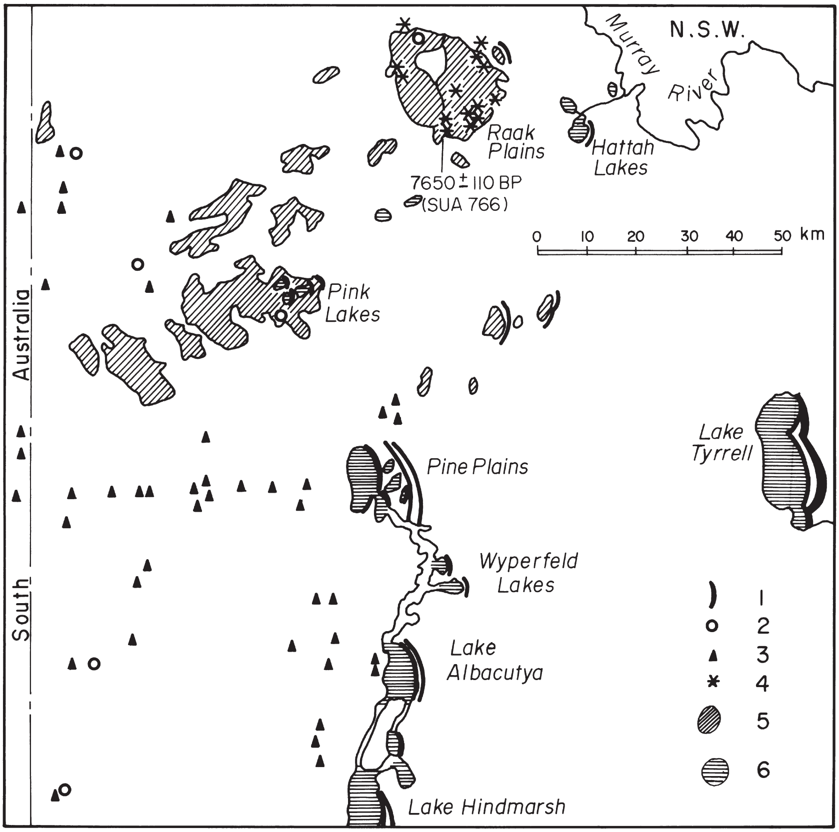

Figure 5a.4: Distribution of early sites. 1. lunettes; 2. stone sources; 3. soakages; 4. sites without geometric microliths; 5. dry lakes; 6. wet lakes.

grained sandstone, ironstone and quartzite; and the more recent sites of the drier period after 6000 BP composed of small flakes and geometric microliths of both coarse and fine-grained sandstone and quartzite as well as chert and jasper.

The distribution of the sites is shown in Figures 5a.4 and 5a.5. Early sites occur right across the Raak Plains complex but have not yet been found anywhere else in the Mallee. The later sites occur mainly along Outlet Creek and the Wyperfeld lakes, around the lake shores at Pine Plains, and along the main line of soaks west of Pine Plains. Other smaller sites are found in the area around Murrayville and the Pink Lakes district, although not associated with the Pink Lakes themselves.

The predictive hypothesis, generated from Clark's 1980 model, suggests that sites should be found around permanent water such as Lake Hindmarsh and the freshwater soaks in dry periods, and should occur throughout the area in wet periods. Clearly this is not the pattern reflected in the overall distribution of sites in the Mallee.

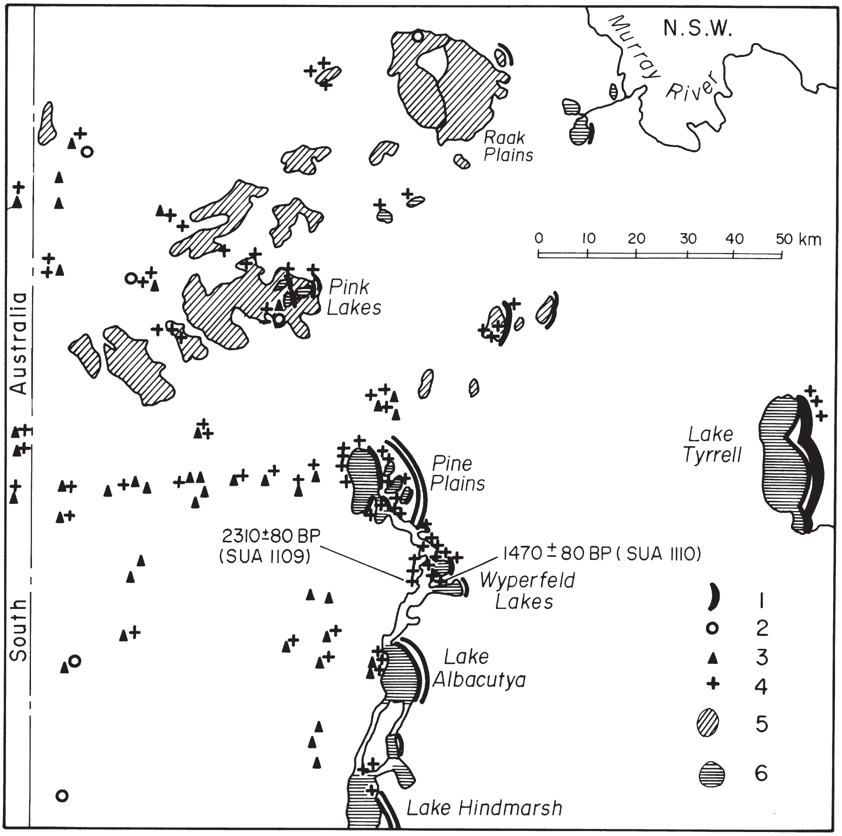

Figure 5a.5: Distribution of recent sites. 1. lunettes; 2. stone sources; 3. soakages; 4. sites with geometric microliths; 5. dry lakes; 6. wet lakes.

For the northern part of the Mallee the hypothesis does seem to be applicable. Sites along the Murray River near Merbein and Irymple (Fig. 5a.1) date to 16 120±200 BP (SUA-964), 13 340±170 BP (SUA-963) (Coutts 1980) and 11 250 BP (Thomas 1969), indicating occupation of this permanent water resource through both dry and wet periods. The occupation of Raak Plains appears to have been the result of Murray River tribes expanding their range during a time of increased fresh surface water availability. Once Raak Plains began to dry and become saline the area was abandoned and the population retreated back to the Murray.

However, occupation of the Wimmera River and the Wyperfeld–Pine Plains system seems to have occurred much later than the high lake level period. Although the system 2500 to 1000 BP must have been somewhat wetter than present, as shown by the presence of *Velesunio* populations where there are none today, there was no occupation of this lacustrine environment during a most substantial lake full period 12 000 to 7000 BP.

Thus, in spite of ample fresh water in the lakes and creeks of the Wyperfeld–Pine Plains system from 12 000 to 7000 BP, it appears that Aboriginal populations were not exploiting these areas at all, or else were doing so in such low numbers that occupation is now archaeologically invisible.

The discrepancy between the observed site patterning in the Mallee and the pattern expected from the hypothesis can be accounted for if the paucity of early sites is a function of later geomorphic activity—that is, the areas *were* occupied, but the evidence has been obscured.

That the absence of sites is a consequence of environmental conditions, rather than a real reflection of site patterning, is very likely in an area where the ground surface is continually changing. Surface sites can easily be destroyed or obscured. In the Mallee, sand from the crests of dunes is moving much of the time, especially after fire and drought, and movement of the dune crests covers and uncovers sites regularly. However, to cause *all* sites older than 4500 BP to be totally obscured, more than dune crest mobilisation would be necessary. Is there any evidence for a major aeolian event after the 12 000 to 7000 BP wet period which could have destroyed all evidence for occupation of the southern Mallee at this time?

The large parabolic dunes west of Outlet Creek could well have been active after 7000 BP. From the air these dunes appear to override the linear dunes to the north and, although this may not *necessarily* indicate two different phases of movement (as parabolic dunes tend to overrun linear forms even during simultaneous movement (J.M. Bowler, pers. comm.)), the very sharp boundary between the two dunefields suggests separate periods of mobilisation (Fig. 5a.3). That the linear dunes were stable for long periods in the late Pleistocene is indicated by four phases of carbonate precipitation in these dunes (Churchward 1961, 1963a, 1963b, 1963c), the final phase of which was towards 15 500 BP (R.J. Wasson, pers. comm.). One or more phases of parabolic dune activity after 15 000 BP cannot be excluded, so that a phase of middle Holocene burial of sites is possible. Unfortunately no datable deposits have been found in the parabolic dunes.

Today Outlet Creek swings away to the northeast from Lake Albacutya to wind its way around the eastern side of the massive wall of sand at the forefront of the parabolic dunefield. North of the parabolic dunes it swings back to the northwest before entering Pine Plains (Fig. 5a.3). There is a good chance that the present channel has existed only since parabolic dune migration ceased, in which case the original position of the channel could well have been to the west of its present position, following a straight and more direct route between Lake Albacutya and Pine Plains. If a burial of the original Outlet Creek has indeed occurred, then all evidence for the occupation of the creek prior to dune activation would also have been buried.

However, Outlet Creek is the only part of the southern Mallee which may have been affected in this way. Pine Plains in particular has several exposed surfaces dating prior to the introduction of geometric microliths. Lake shore sediments have been deflated down to expose the beaches of the high lake level (7460 BP), and early dune deposits at 'Snowdrift' on the edge of Wirrengren Plain (Fig. 5a.2), where radiocarbon dates on carbonate exposed at the surface date to 17 820±215 BP (SUA-1111a) and 13 895±165 (SUA-1111b), should certainly produce evidence for occupation prior to 4500 BP if it had occurred. Yet only microlithic and recent Mallee assemblages have been found on these blowouts. The two eroded lunettes around Lake Agnes on Pine Plains (Fig. 5a.2) also yield recent material only, yet at least one of these lunettes must have been in existence prior to 12 000 BP since there has been only one period since 12 000 BP when lunette-forming conditions existed at Pine Plains (cf. Bowler 1973). Further up Outlet Creek, south of the parabolic dunes, the Pleistocene lunettes around Lakes Hindmarsh and Albacutya have also provided evidence for more recent lithic assemblages only.

Therefore, several exposures of Pleistocene sediments occur in the Mallee in the form of lunettes, lake beaches and weathered linear dunes. However, it is only around Raak Plains, and possibly Lake Tyrrell, that any evidence for occupation prior to 4500 BP occurs. The evidence from Lake Tyrrell is tentative. Artefacts on the surface of the lower lunette, which date to 31 700±1140 BP (SUA-559) at the base, and from between 27 780±730 BP (SUA-783) and 22 000±370 BP (SUA-558) at the top (P.G. Macumber, pers. comm.), consist of a mixture of archaeological material with both small artefacts typical of the late Holocene Mallee sites and larger artefacts similar to those from Raak Plains being found. No date is available for this material.

Apart from the possible parabolic dune migration and the evidence for some minor lunette formation *c.* 6000 BP at Lake Wahpool (Fig. 5a.1), and possibly at Lake Agnes, there has been little depositional activity anywhere in the Mallee since 7000 BP. Nearly all the geomorphic activity in the Mallee since the early Holocene appears to have been largely restricted to deflation of dune surfaces, so that the *revealing* of sites is in fact more likely than their obscuring.

On archaeological and geomorphological evidence, therefore, it would appear that, if occupation of the Mallee other than Raak Plains had occurred prior to 4500 BP, it was most probably a small-scale event which is not now archaeologically visible. It may be that the recent Mallee sites *do* indicate a late occupation, and that the absence of early material is real. What, then, could account for the delayed occupation of such a potentially rich resource area as the Outlet Creek/Wyperfeld/Pine Plains system?

Population pressure and settlement of the Mallee

It appears that suitable environmental conditions alone were not sufficient to bring about the occupation of the southern part of the Mallee. A likely additional trigger for occupation is population pressure.

Before discussing this proposed population pressure model for Mallee settlement, a brief survey of evidence for late Holocene population increases and occupation of other parts of southeastern Australia will be made.

Traditional views of the stability of the Aboriginal population of Australia have recently been challenged by Hughes (Hughes 1980; Hughes and Djohadze 1980; Lampert and Hughes 1974) and Lourandos (1976, 1977 and 1980), who have argued that population changes did occur in certain environments and that these fluctuations may be related to changes in energy-harnessing techniques.

There is evidence for increased population pressure on the resources of the south coast of New South Wales around 4000 BP (Hughes and Djohadze 1980, Lampert and Hughes 1974). Sedimentation rates in both rockshelters and open sites began to increase gradually between 6000 BP and 4000 BP after the sea-level reached its present height, but show a marked increase after 4000 BP (Hughes and Djohadze, 1980; Lampert and Hughes, 1974: 232–4). This increased rate of sedimentation, which has been shown to reflect increased intensity of site use (Hughes 1980), corresponds with a marked increase in both total artefact numbers (Hughes and Djohadze 1980; Lampert and Hughes 1974) and in manufacturing tools alone, excluding geometric microliths (Hughes 1980; R.J. Lampert, pers. comm.). Furthermore, there is a dramatic increase in the total number of sites occupied on the coast after 4000 BP, and Hughes has suggested that this also reflects an increase in population at this time (P.J. Hughes, pers. comm.). Lampert and Hughes argue that this population increase was related to the inception of the small tool tradition and the development of a more efficient fishing technology (1974: 233–4).

Elsewhere in eastern Australia evidence for late occupation may also reflect an increase in population. In her work on the Southern Uplands, Flood found only one site older than 4000 BP, the site of Cloggs Cave (Flood 1974, 1980): 'No site so far discovered on top of the coastal ranges or on the tablelands goes back more than 4000 years' (Flood 1980: 279). The types of artefacts found on these recent sites, both in shelters and in the open, show a 'marked difference' from those found at Pleistocene sites elsewhere in Australia, the main differences being the smaller size of all tools at the recent sites, the presence of backed blades and other geometric microliths and the absence of horse-hoof cores and fewer scrapers (Flood 1980: 279). These differences between early and recent assemblages observed by Flood are identical to those observed in northwestern Victoria between the earlier Raak Plains sites and those of the southern part of the Mallee.

Flood has outlined three hypotheses which could explain this dearth of sites in the Southern Uplands prior to 4000 BP (1980: 281–2):

1. That earlier occupation did occur in the Southern Uplands but that evidence is not visible;
2. People followed the Bogong moths into the highlands as climatic conditions ameliorated and the moths moved to higher ground above the retreating snow line;
3. That population pressure on the resources of the coast forced a movement of people into the uplands *c.* 4000 BP.

Although each of these hypotheses now needs to be tested, the fact that bedrock or a considerable depth of sterile basal clay was reached in most of Flood's sondages (Flood 1980: 323ff) would make it unlikely that, in the rockshelters at least, the evidence for earlier occupation was destroyed (cf. Hughes 1980), making the first hypothesis rather less attractive. The second hypothesis is also unattractive when the climatic conditions of the Holocene are considered. The migration of the Bogong moths to high ground during post-glacial amelioration would have occurred by 12 000–7000 BP, since this was the period of greatest climatic amelioration in the Holocene. If people had followed the moth into the uplands, evidence for initial occupation should be expected to date from at least the early Holocene. At present, the third hypothesis has the most support from evidence both in the Southern Uplands and on the coast.

Bowdler's (1981) recent synthesis of Uplands research generally further emphasises the evidence for a population increase in the eastern Australian Highlands *c.* 4500 BP.

In southwestern Victoria, Lourandos (1980) has examined the effects of change in energy harnessing techniques on population intensity. He describes the development of water control systems at Mt William and Toolondo (Fig. 5a.1), which allowed a much higher return on eel fishing than earlier techniques had permitted. However, the drains were more important than simple eel traps—they were a form of 'swamp management'

> coping with excess water during floods and retaining water in times of drought. . . . An extension of eel range, by providing access to further inland swamps and waterways, would have led to an increase in the annual production of eels. (Lourandos 1980: 254)

Although such niche expansion may not have had an immediate impact on population density, Lourandos argues that it is plausible to expect that long-term trends in population density would be influenced by this greater resource stability (Lourandos 1980: 256).

It is significant that Toolondo and Mt William lie between the coast (where more sedentary groups live) and the inland (Lourandos 1980: 255). The area provided access to a range of environments, including the northern ranges and the Grampians. Any increase in population density in this wetlands area could have triggered an expansion into the more northerly districts. Lourandos has likened this situation to Binford's 'population frontier or adaptive tension zone' where:

> Population growth within the area occupied by the parent group might well be so great that daughter communities would frequently be forced to reside in an environment which is incompatible with their particular cultural adaptation. (Binford 1968: 331)

Binford suggests that at this point 'adaptation along population frontiers' occurs (Binford 1968: 331).

The only date for the eel trap at Toolondo is very recent, 210±120 BP (GX-4785), 'indicating the final stages of the drain's operation' (Lourandos 1980: 253). However, the introduction of water control systems may have begun as a result of the climate in southeastern Australia being slightly drier than present *c.* 3000 BP (Lourandos 1980: 255). Evidence of low lake levels at Lake Keilambete (Bowler and Hamada 1971), increased solifluction activity in the highlands (Costin 1972) and Tasmania (Derbyshire 1972), increased swamp pollens on Mt Buffalo (Binder and Kershaw 1978) and increased slope instability in the Southern Tablelands (Williams 1978) point to a change in environmental conditions around 3500 to 2500 BP and possibly earlier. Evidence from the Snowy Mountains indicates that it may have been windier during this period (Costin *et al.* 1967; Walker 1978). The resulting increased evaporation would have brought further pressure on water resources and Aboriginal populations in marginal areas.

In order to maintain population levels in marginal areas such as Toolondo, adaptation to the drier conditions would have been needed:

> Without the elaborate water controls which ensured against periodic variations in water availability in these marginal areas, the local Mount William population would have been denied its resource base for large scale seasonal gatherings and possibly ceremonies. Climatic variations, as have occurred in precipitation within the area, and also possible demographic shifts, would have aggravated the situation even further. (Lourandos 1980: 255)

Assuming then that environmental factors forced an 'artificial niche expansion' in the area south of the Mallee some 3000 years ago, a plausible sequence of events might be that some movement of people into the wetter Grampians occurred at the same time, followed by further population expansion into the Mallee once wetter conditions returned to the area *c.* 2500 to 1500 BP (Bowler and Hamada 1971; evidence of *Velesunio* in the Wyperfeld Lakes, see above), especially since the new energy harnessing techniques remained in use (Lourandos 1980).

This new hypothesis for the triggering of population movement into the Mallee must now be tested. In order to do so a far more detailed study of local environmental conditions in western Victoria, particularly in the southwest, must be carried out, and detailed archaeological work in the Grampians and surrounding areas would need to be undertaken. If the hypothesis is to hold, evidence for a dry period 3500 to 2500 BP should be found and substantial occupation of the Grampians should be a late event dating little earlier than 3500 BP. Although little archaeological work has been carried out in the Grampians, the limited evidence is so far consistent with this speculation. Excavated sites have yielded no evidence for occupation prior to 3500 BP, with sites dating to 3330±100 BP (SUA-584) at Black Range, 1620±100 BP (SUA-533) at Glen Isla and 820±95 BP (SUA-583) at McKendrick Mound (Coutts and Witter 1977). With occupation of the Grampians it would have been only a matter of time before the Wimmera River, and hence the Mallee proper, was explored and settled to the extent that remains of occupation could become visible archaeological sites.

An alternative hypothesis is based on dates from southwestern Victoria in general (M. McIntyre, pers. comm.). Apart from Lake Bolac, which dates to >12 000 BP, and one coastal site between 7000 and 4000 BP, all sites in southwestern Victoria so far dated were occupied after 4000 BP. If the Mallee region was indeed occupied from the south, then it may have been a result of a *general* population increase in southwestern Victoria in the late Holocene, rather than increased population pressure around the Toolondo area alone. Either way, a late occupation of the Mallee resulting from population increases to the south is proposed.

Linguistic evidence from the ethnohistoric period in western Victoria lends support to this proposed pattern of settlement (R.M.W. Dixon and J. Laycock, pers. comm.). Evidence indicates that the languages of inland western Victoria were genetically related. Languages spoken in the Mallee and inland southwestern Victoria show a high percentage of cognate vocabulary, generally greater than 80%. On the other hand, the shared vocabulary between the languages of the Mallee area and the Murray River from around the Hattah Lakes downstream to the South Australian border (Fig. 5a.1) is extremely low, ranging from 10–15%.

Where two groups have more than 70% shared vocabulary they are considered to speak dialects of the same language; where less than 40% of the vocabulary is shared, the implication is 'that the two languages have been in contact for a relatively short time, and that they are not closely related genetically' (Dixon 1980: 255).

The picture is complicated, however, by a high percentage of shared vocabulary, around 80%, between the Mallee language and that of the Murray River peoples east of Lake Tyrrell. Nevertheless, the fact that a major dialectal boundary occurs between the Mallee and the Murray at this point has significant implications for long-term population movements between the Mallee, the Murray and inland southwestern Victoria in general, and does not imply a contradiction to the model based on archaeological data (R.M.W. Dixon, pers. comm.). A detailed analysis of the relationship between linguistic and archaeological evidence will be presented in a future publication.

In general, therefore, although it would not be valid to suggest that the linguistic evidence from the ethnohistoric period is necessarily applicable to the prehistoric period, it appears that the Mallee population was linguistically more closely related to those people from inland southwestern Victoria than it was with the Murray River peoples, either to the north or east of the Mallee. Although the linguistic evidence cannot be said to actually support the notion of initial occupation of the Mallee from the south, it is generally consistent with the model.

The evidence for likely population increase in inland southwest Victoria, late occupation of the Grampians and ethnohistoric language affiliations in western Victoria all point to a late occupation of the Mallee as a result of a northward expansion of the people from the Western District of southwestern Victoria.

Conclusion

Archaeological evidence from the Victorian Mallee does not support the hypothesis that widespread occupation of the area occurred during peak wet times. During the wet period from 12 000 to 7000 BP sites are only found in the northern part of the region along the Murray River and around Raak Plains. The early occupation of Raak Plains could well have been due to the expansion of peoples from the Murray into a greater area with the onset of wet conditions, but if expansion at this time occurred any further south there is no archaeological evidence for it. With the advent of drier conditions and the salinisation of Raak Plains after 7000 BP the population would once again have retreated to the Murray. To this extent the predictive hypothesis for site patterning is supported. But on the scale of the Holocene, and for the Mallee as a whole, a modification to the hypothesis is required.

It is suggested that the variable of population dynamics must be added to the model. It is proposed that drier conditions in inland southwest Victoria around 3500 years ago increased population density in southwestern Victoria generally at this time, and population pressures west of the Grampians associated with the development of water-control mechanisms at about the same time allowed for an expansion of Aboriginal populations from southwest Victoria into the Grampians. Occupation of the Wimmera and thence the entire Mallee would have occurred as soon as the arid conditions ameliorated. In this way, the initial late occupation of the Mallee as a whole may have been in response to different conditions from those which initiated the earlier short-term occupation of the drier northern Mallee.

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5b Perspectives on 'trends toward social complexity in prehistoric Australia and Papua New Guinea'[1]

Norman Yoffee

'Ladies and gentlemen: let us bow our heads in a moment of prayerful thanksgiving for the wisdom you are about to hear.' So began a paper presented at a recent meeting of the Society for Biblical Literature by a theologian and New Testament scholar, notorious for his vanity as he was acclaimed for his scholarship.[2] Before this paper, ladies and gentlemen, I humbly crave your indulgence and patience, both for the exceeding modest observations you are about to hear and the length of time it will require to make them.

The task set me by the organisers of this panel is one of comparison—a formidable but honoured goal in anthropological circles. Specifically, I am to review some new studies on the evolution of social complexity and so to represent trends in such analyses; from this perspective I am asked to comment on the presented studies of sociocultural change in prehistoric Australia and Papua New Guinea. It will be appreciated that, as a Mesopotamianist, struggling to understand historic forms of Babylonian social behaviour and their prehistoric roots, I find these comparative tasks are by no means straightforward. Nonetheless, I do consider that Mesopotamian cases profit through cross-cultural comparison with other politically organised societies. Further, I am interested in general questions of social evolution and in this domain it would be unwise to be too Mesopotamio-centric.

It is, of course, not news that the sorts of social change in Sahul, as are observed in these papers, did not lead to complex societies (those socially heterogeneous and economically stratified (McGuire 1983) and politically centralised ones (Yoffee 1979).[3] However, it may also be doubted that Sahulian societies represent earlier 'stages' in the evolution of complex societies. Early developments in such evolutionary trajectories seem, indeed, quite unlike those processes of change, and especially the important constraints on them, that are consistently observed in prehistoric Australia and Papua New Guinea (Yoffee n.d.). All too commonly the theory of social evolution has functioned simply as an idea of progress and has been pictured as a step-ladder from which our 'contemporary ancestors' (Service 1975: 18; Fried 1974 [1960]: 25) descend into the archaeological record. Few evolutionary studies have attempted to appreciate the varieties and paces of societal change that exist, to measure probablistic trajectories of growth, and to assess constraints on growth without the assumptions that extinct societies are the fossilised representatives of essential types best known through comparative ethnography.

It is clear, therefore, that comparison—for example, both among complex societies and between complex and non-complex ones—must be considered, not just performed; indeed, anthropology has often suffered from lack of rigour in controlling comparisons, a matter that Eggan discussed in a famous essay 30 years ago (Eggan 1954). We need to say very explicitly what are the goals of comparison, in what terms can the comparison be undertaken and, perhaps most critically, what may be the value in delimiting the areas in which comparison is transformed productively (rather than mechanically) into contrast.

Now, both Australians and foreigners have meditated on these points. George Collier, in introducing a most useful monograph on Inca and Aztec states, portrays the situation thus:

> The manner in which scholars now regard generalization and comparison has to be understood in light of the . . . reasons for which they initially set them aside. First, one cannot generalize about what one does not understand. Social science ideas can serve to inspire interpreters . . . but . . . unsophisticated work on insufficient sources with the newest . . . methods and concepts is . . . nothing but an amusing intellectual game . . . Willingness to generalize and compare thus makes sense to the degree that empirical understanding of these [cultures] in their own terms has matured. (Collier 1982: 2)

For an Australian perspective I turn to that peerless philosopher of the bush and sometime detective, Napoleon Bonaparte. In *Bony Buys a Woman* our hero is in the vicinity of Lake Eyre investigating the disappearance of a child and the murder of her mother. In the course of things, Bony visits an Aboriginal camp where, on an evening, as the campfire gleamed, the 'headman', Canute, was telling a story on his dijeridoo.

> [Bony] heard, and saw the pictures, because he knew the story. Thus he could follow and interpret the sounds issuing from the dijeridoo. But when Canute told another story of which he was ignorant, the sounds were of no help, told him no story, but did create pictures of flat water, waving tobacco bush, wind stirring sand grains. (Upfield 1983 [1957]: 49)

In the one end, Bony solves the case and in the other one Collier compares at full speed. As for me, the issues of double-comparison on which I am about to embark are far from clear and I think that has to do with the state of evolutionary theory within which such comparisons must be structured. Consequently, this paper will consist not only of actual comparisons, but also of reflections on the viability of comparative strategies. For those of you who feel that not enough is known, in their own terms, of the particular cultures being compared, this paper will be of small comfort. My own impression is that the quality of recent studies both on the rise of civilisations and those on sociocultural change in Greater Australia are at a rather high level. At least we are beginning to know what we need to know and can so better structure our subsequent investigations.

In the first part of this paper I shall very rapidly survey the most recent literature on 'trends toward social complexity' in those societies that were, or became, by anyone's definition, highly stratified and politically organised: ancient states and civilisations. Naturally, I shall be unable to pause over advances in understanding specific sequences of sociocultural change. I intend only to depict themes of investigation in which change is measured and assessed. Of course, I cannot pretend to be exhaustive in this depiction, nor do I claim to understand all the nuances specialists in each area must confront, nor do I control all the secondary literature of such research. I do claim, however, that I have reviewed a significant group of the most recent monographic literature on the evolution of social complexity on a worldwide basis—with the sole proviso that these recent monographs address questions of social evolution fairly directly.[4]

In the second part of this paper I shall address the issues brought forth in the papers that were presented at the Australian Archaeological Association meeting at Tallebudgera. Naturally, I shall not deal with problems of Sahulian culture history nor shall I comment on the recovery and primary analysis of the data. Rather, I shall try to compare the foci of evolutionary concern in these papers with those perceived trends in the aforementioned studies. Those already preparing to winge that it is unfair to compare monographs (some the result of long years' research) with brief symposium presentations will have missed the point of the exercise. My goal here is not to award any trophy for the real archaeology of social complexity and so to judge other entrants to be immature, unbalanced, perhaps dominated by volatile acidity, or even contaminated by bunch rot—if I may use these vinous metaphors so as to hold the attention of my oenophilic friends. I am interested rather in the variety of evolutionary theories that may be productively used to assess social change; or, to put it another way, I propose to consider what kinds of theories are contextually appropriate to explain particular forms of change.

Some recent studies on the evolution of social complexity

In the attempt to synthesise 'trends toward social complexity' it is necessary both to identify the distinctive features of such sociocultural changes and the categories within which investigations of trends (or processes) may be empirically referred. From a review of recent studies I suggest (following Runciman 1982) that the most important necessary and jointly sufficient condition that separates complex societies from non-complex ones is the emergence of socioeconomic and governmental roles that are emancipated from real or fictive kinship; that is, the basis of relations between the occupants of those governmental roles and those whom they govern is not ascription. The congeries of such governmental roles, usually denominated the state, includes the quality of paramount and enforceable authority and that of permanence—or at least more than a temporary stability of the governmental structure. According to Runciman, it may be further observed that the process by which such governmental relations develop depends on the cumulative accretion of power available to incumbents of prospective governmental roles. For the purposes of evolutionary investigation here, it is argued (also following Runciman) that power can be subdivided into three, and perhaps only three, forms: economic, societal and political power. Finally, these three varieties of power reinforce one another and the state does not depend on only one form. It is the combination of economic productivity—the control over the sources and distribution of subsistence and wealth—along with the segregation and maintenance of the symbols of corporate legitimacy and the ability to impose obedience by force that mark together the essential qualities of states. It is the process toward such socioeconomic differentiation and cultural and political integration that is to be traced and explained in the archaeological record: whence come these varieties of power and what constrains them into social co-existence?

First, economic power is created through a process of horizontal specialisation in the means of subsistence, including a diversification of tasks in the production, storage and distribution of goods. Intensive land use, usually associated with farming and the production of disposable surpluses and so storehouses, are key subjects of investigation. Elaborations in long-distance networks of exchange are generally found to accompany inequalities in access to basic productive means. In addition to representing inequality, however, the acquisition of prestige goods also becomes an institution requiring specialisation and organisation and thus a means by which status is created. The resulting economic inequalities in power, abundantly documented both on vertical and horizontal scales in the societies reviewed here, provide potential avenues for the exercise of power outside kinship networks.

Societal power refers initially to the horizontal segmentation of social structures and thus entails a consideration of numbers of people and population growth. Societal power also refers to the establishment of territorial interactions—the development of 'interaction spheres', nucleation into urban complexes, and most critically the creation/adaptation of certain symbols of cultural and political commonality. Ceremonial buildings, artistic, glyphic and literary representation not only link peoples and settlements beyond factors of kinship, but confer honour and prestige on those who maintain these symbols. Those people with unequal access to the symbols that legitimise social life and who are thus able to command goods and labour ostensibly on behalf of the community, but especially for their own ends, exercise societal power.

Political power refers to the ability to impose force throughout a community through specialised, permanent administrators. These bureaucrats and other clients of the dominant estate, i.e. the ruling estate, occupy their offices through means of recruitment beyond the co-existing systems of ascription and enterprise. Other household systems (*oikos*-estates) responsible for local decision-making exist, but do not maintain the overarching political power exercised by the dominant (royal) estate.

I shall now very briefly illustrate how these evolutionary trends are observed in recent studies. Although within these trends I do not seek to isolate any particular cause of social complexity (trade, warfare, religion, population growth or anything else) and although the three forms of power are and must be mutually reinforcing, this evolutionary approach is not a subset of systems theory. Furthermore, it does not rely on a taxonomy of stages and levels—which is a good thing as an inspection of this panel's papers brings out most happily.

I shall briefly discuss two trajectories toward economic power, agricultural and mercantile activity. In the first, land and labour become characteristically divided between a dominant estate and other household organisations, each of which is structured in ranks of managers and dependents. Most dramatically, in Inca society, dominant estates were established throughout the empire and worked by local labour in order to provide tribute of various sorts, especially in the production of cloth, for the government in Cusco (various papers in Collier, Rosaldo and Wirth 1982). Throughout the empire, however, local elites retained their traditional roles and in some cases the famous *mit'a* colonies of transplanted ethnic groups were not installed. Similarly, in Mesopotamia, the 'great households' of rulers and temples, employing teams of agricultural labourers and craftspeople, never completely organised the local economies in which were embedded ascribed and private estates (literature in Yoffee n.d.).

The route from agricultural production to economic power, which lay in all instances in converting stored wealth to systems of dependencies, differs in the various examples. In the central Maya lowlands the last decade's research has yielded evidence of intensive terracing and raised fields which have confounded the once traditional questions of how Maya city-states could be supported through slash-and-burn agriculture (Harrison and Turner 1978). In Mesopotamia, by contrast, agricultural systems were extensive (another reversal of expectation), dictated by the need to alternate fallow seasons and avoid salinisation of irrigated fields. The characteristic system of large-scale ownership of lands by corporate households seems to have arisen as a response to the need to shift people across the landscape to meet this condition (Hall nd; Yoffee n.d.). In southern Mexico, it is noted that small Mixtec statelets depended on large-scale irrigation schemes, while the relatively large Zapotec state at Monte Alban flourished by intensifying small-scale water spreading activities (Flannery and Marcus 1983). Clearly, the agricultural bases of economic power could vary greatly. The only constants are the production and management of reliable surpluses.

I shall reluctantly truncate a variety of remarks that can be made about ancient mercantilism (Yoffee 1981). Let me only note that Old Assyrian traders in Mesopotamia did not base their activities on the monopolisation of any product, but rather were skilled in transporting goods from where they were plentiful to where they were scarce and so derived profit from the resulting comparative costs advantage. Just as these traders were organised entrepreneurially and wholly outside the state's bureaucracy (see, most recently, Larsen 1982), so were the Aztec *pochteca* independent social units (Blanton *et al.* 1981: 238). Of course, the state was no disinterested spectator of the activities of each of these traders, but that is another subject. In China, for reasons I shall note, the state did monopolise bronze technology (Chang 1983); the Incas were similarly absolutely keen on cloth (Murra 1962). In Teotihuacan, however, where obsidian extraction and circulation seemed so important, and when in the Classic period 600 distinct workshops in the city have been identified and these employed about 12% of the population, the state still seems not to have controlled all obsidian-related activities (Spence 1981; Blanton *et al.* 1981).

Let me conclude this brief section on economic power with the reminder that trends toward economic inequality in production and exchange have been measured in differing sizes of residences, accompanying features and artefacts, and in mortuary furniture. The mortuary studies are especially interesting, not only because economic inequality can be assessed therefrom—though this task is by no means straightforward—but they also imply that by taking luxury goods out of a living system, a continuity is imposed to continually acquire new luxuries and maintain high levels of local production. In this analysis, one can see the important connection between agricultural inequality and mercantile activity that leads to new forms of economic power. The vital horizontal component of societal power in the various cases reviewed can be ascertained in both material and ethnohistoric studies. Examples are of Aztec *calpulli* (Carrasco 1982), Inca *ayllu* (Conrad and Demarest 1984), various named ethnic groups in Mesopotamia (Kamp and Yoffee 1980), the Zapotec barrio and many distinct apartment complexes at Teotihuacan (Millon nd, with literature). The various territorial agglomerations of these socially, economically and politically differentiated groups is one subject that is quintessentially appropriate to the study of complex societies. Thus, interaction spheres have been postulated since the 1890s for the Andes, now rekindled in Murra's 'vertical archipelago' (1980). Vértesalji (1984) and I are variously interested in the idea in Mesopotamia, while Kohl has written persuasively of a 'Western Asian world-system' (1979).

For the present brief discussion, however, it is perhaps more important to concentrate on the ideological side of societal power. As Godelier has argued, religion is 'part of the internal armature of . . . relations of production', that is, not something external to the economy (Godelier 1977: 10). Several studies have taken up this theme in the examination of community-embedding symbols, from great monuments like Mesoamerican pyramids to ceramic horizon-styles. These symbols are sociocultural, not political. In both Mesopotamia and in the Mayan region, for examples, these kinds of symbols, and the economic and cultural interactions they represent, went beyond the characteristic political organisations of autonomous city-states. Nevertheless, things look a lot less theocratic in Mesopotamia and in Teotihuacan than once they did. Cowgill, in particular, has pointed to the Ciudadela in Teotihuacan as a state ceremonial complex. It has a massive plaza capable of holding 100 000 people and ceremonial architecture and decoration, but also an absence of residential apartments and unchanging form through about 20 generations of rulers. In contrast, the so-called 'Street-of-the-Dead-complex', a structure that was often renovated, possessing large and fine apartments at its core and with accessible administrative rooms on the periphery, looks a good candidate for a royal palace (Cowgill 1983). The evolution of such societal differentiation leads one to look for intra-elite struggle over the symbols of such power in complex societies.

The trends toward political power and the connections with economic and societal power are nowhere better illustrated than in ancient China. If I understand the gist of recent studies (especially Chang 1983; Keightley 1983), the argument is that the earliest Chinese villages, based on farming, were themselves units of kinship which then segmented through time into a hierarchical ranking of opposing networks. For reasons of defense and expansion, ascriptive ties broke down and new organisational orientations began to supersede those of kinship. Rulers were able to promote their claims over non-kin dependents through their superior abilities to communicate with the world of dead ancestors. They effected these claims essentially by controlling writing, notably used on oracle bones, and most importantly by monopolising bronze technology. This last was especially significant since it was precisely through the bronze vessels and their highly charged decorations that the path to the ancestral world could be trod.

Shang dynasty kings travelled constantly through newly gained territory, sacrificing to local spirits and demonstrating their legitimate rights to rule. For Keightley, Shang administration was 'patrimonial'; that is, administrators were closely connected to the person of the king. By Chou and Han times, however, the professionalisation of the bureaucracy was profound. Semi-autonomous literati carried the keys to legitimacy, the Mandate of Heaven, and so maintained, within limits, the authority to rule beyond any single ruler. They could remake the characteristic governmental institutions of Chinese civilisation in the aftermath of the collapse of particular Chinese states (Hsu nd).

Although much more could be said about the evolution and development of political power (obviously), I only juxtapose with the above Blanton's argument that Monte Alban was a 'disembedded capital,' that is, a new city founded precisely to be an administrative center (a Zapotec Canberra), and not evolved from humbler economic and social roots (Blanton 1983). Although perhaps not everything has been satisfactorily explained in Blanton's formulation (see Santley and Arnold 1983, Yoffee nd a), this comparison of Monte Alban with the growth of the Chinese state does show that various trajectories could be taken in the achievement of a structurally similar exercise of political power.

To end this cursory review of literature on the rise of social complexity, let me belabour the obvious: the scale of the investigations. The city of Teotihuacan at AD 500 occupied a nuclear site of 20 km^2 and had an estimated population of 200 000 people; at about the same time, Monte Alban occupied 6.5 km^2 with 30 000 people (Blanton *et al.* 1981); Chengchou had a walled core of 340 ha with a 24 km^2 area of connected service settlements (R. Fletcher, pers. comm.). Whether one agrees with Wright and Johnson's method of locating three-tiered administrative hierarchies as manifestations of states (1975), the object of seeing social complexity as depicted in relationships of urban centers and their connected hinterlands has been rightly influential. It is an absolute misuse of the method, however, to simply rank sizes of settlements alone and so 'find' states (Cordy 1981, Cordy and Ueki 1983).

Lastly, I note that the sources used in recent studies of state formation include various means of writing and/or recording. The issue is not that non-complex societies are more purely archaeological because the investigator does not have to demonstrate that he or she is literate, but that complex societies develop means of communication and record-keeping for functional and structural purposes. It is, however, vital to note that there are written sources and written sources. In the Collier *et al.* volume, ethnohistorians clearly reject the reports of Spanish codices and capital-centered accounts, which regularly depict the conquered Mesoamerican states as totalitarian and bloodthirsty, and so justify Spanish conquest and practices of religious conversion. Rather than rely on these sources to interpret Mesoamerican and South American corporate structures, they turn to mundane tax records, pay lists and legal cases.

Even though I have not obeyed the Shakespearean injunction, 'if it were done—'twere well it were done quickly', I now turn to the papers presented at Tallebudgera in the perspective of the evolutionary trends that I suppose can be perceived from the above studies.

Papers presented at Tallebudgera

Since A.B. Knapp's paper at Tallebudgera (to be published elsewhere) dealt with matters of comparison, I may open the discussion with a consideration of trade in the eastern Mediterranean and in Melanesia (represented in this volume by Jim Allen's essay). Knapp's comparison rests on the points that trade in copper is the engine that results in social complexity in Cyprus and that, in some coastal Melanesian situations, trade provides the organisational reasons for social existence. Of Knapp's avowed formalist orientation, one is reminded of the well-known article by Renfrew (1975) in which are diagrammed ten discrete kinds of trade that are each correlated with the degree of centralisation in its society. For archaeologists of such persuasion, trade serves the function in their research that sex did for S. Freud (or that it serves for certain entrepreneurs in Sydney's Kings Cross): if you work at it hard enough you can solve most of the problems of human existence. Unfortunately, the research on coastal Melanesian trade (see Allen this volume for references, especially Allen 1984, Irwin 1978) has generated some very sticky ointment for the hyper-formalist archaeological flies: the societies in which trade seems most important, in terms of energy expended and percentage of population involved, are not states at all. Allen has explicitly remarked that several of Renfrew's types of trade co-occur in the Motu system, a point that I've also made in describing Mesopotamian trade. Allen also notes that it is the social constraints on growth, that is, not formalist economic reasons at all, that explain the important facets of the Melanesian trading systems, especially the reasons why there are ceilings on their growth. In the Cypriot comparison Knapp has argued that some social inequalities pre-existed the burgeoning of the copper trade and that such trade was in fact developed only in the nexus of international forces that were set in motion by Western Asian states in the early and mid-second millennium BC. Obviously, the situation in Cyprus is quite unlike that in Melanesia. Further, one can easily see that trade alone cannot be separated in an analytical tunnel and so explain (much less predict) forms of social and political complexity.

The three papers on Papua New Guinea can be economically considered in a group. Peter White (this volume) nicely brings out some important implications of the Kuk research: that the early management of resources in the highlands and resulting long-term productivity ought to be correlatable with increasing amounts of social complexity (see Golson 1983, 1977). He queries, however, whether the ethnographically attested 'big man' systems that depend on such economic production are, or indeed ever will be, archaeologically visible. Although, at contact, certain inequalities of power and wealth are documented, White points out that big men do not seem to have larger houses and more stuff than others. The real issue concerning White is whether archaeology can be or even should be an illustration of the ethnographic record. I concur, I think, with White's judgment: just as archaeologists don't set out to discover the material correlates of support groups that are mobilised beyond principles of patriliny to refer to a prize-winning essay of D.K. Feil (1984), so archaeologists should free themselves from the dubious goals of digging out big men, chiefs and the like. The problems of unequal distribution of materials, or lack of them, in the archaeological record are just as real as problems of perishable status that are archaeologically invisible—and, in the end, the archaeological data are perhaps more important in the assessment of long-term social changes than are the evanescent materials and emic systems with which ethnographers work. Archaeologists can and should use ethnographic research, but they must use it within their own rigorous standards and in their own distinctive research agendas.

Daryl Feil's paper (a version of which will be published elsewhere) well represents new trends of how ethnographers use archaeological materials. Feil shows that eastern and western highlands are 'clinally' demarcated along a number of institutional lines. In further using the Kuk findings he ponders the time-depth that may have provided the 'pre-adaptations' for differences that were then accentuated by the introduction of the sweet potato. Two issues strike me from this excellent paper. First, I find it interesting that a skilled ethnographer like Daryl Feil relies more on environmental factors to explain social variability than have the other (archaeological) authors herein represented. Following Feil, it is also interesting to note that more people live in the western highlands, have more pigs and cultivate more intensively, while it is in the east that villages are more nucleated. In considering 'constraints on growth' it is perhaps useful to see that these critical institutional factors of productive intensification, population growth and nucleation do not come together in the PNG highlands. Second, there is no guarantee that there is an unbroken and direct trend from prehistoric to historic systems in PNG, especially since so many external factors seem crucial in molding the ethnographic present. It should be at least as interesting to see how the past is different from the present and explain this as it is to trace continuities.

For his part, Ian Lilley (this volume) queries whether the classic 'clinal' distinction between Polynesian chiefs and Melanesian big men is valid, or at least whether such a distinction tells us all we wish to know about the differences in these systems. In his brief, but provocative, study, Lilley notes aspects of complexity in Siassi that do not fit the classic big man type; like White he is concerned that the symbols of such status may not be readily visible in the archaeological record. I think Lilley has overstated the matter, however, in arguing that the Siassi *maron* is a kind of 'chief'. Ascription and authority are not the sole characteristics of chiefs and chiefs don't exist apart from so-called 'chiefdoms', which have the kinds of economies that are not present in Siassi (Carneiro 1981; see also Kirch 1984). What Lilley is arguing, in effect, is that the stage/level terminology has tended to impede, not further, research into the similarities and differences in Oceanic social systems, by simply ignoring awkward data (see also Yoffee n.d.). One area in which such research can profitably be done is in Melanesian ethno-archaeology of the sort Lilley is now undertaking.

Moving north, John Craib (this volume) presents a very satisfying discussion on the matter of social complexity in Micronesia. In challenging the venerable notion that *latte*, by their very existence, denote complex social organisations, he has looked soberly at both the ethnohistoric evidence and the real distribution of megalithic architecture. In a straightforward and, for me, convincing manner he breaks downs all analyses into their constituent assumptions and analogies and examines the empirical referents to each. Having rejected the standard reasons given for *latte* structure and function, he is now putting the pieces of the puzzle back together. This is no trivial pursuit since he must examine constructions, their variations, the associated artefacts and the patterning of the structures. We can safely expect a mature assessment of social structure in Micronesia and new directions of research there to come from Craib's work.

Probably the major issue in assessing putative 'trends toward social complexity' in prehistoric Australia is associated with the term 'intensification'. I shall group the two papers that were presented in Tallebudgera (Ross, this volume; Williams, this volume, concerns material related to the paper delivered at Tallebudgera) with a very brief appraisal of the important work of Lourandos, whose use of the concept of 'intensification' in Australian prehistory provides the context for the Tallebudgera papers.

The term 'intensification', of course, implies that some relations, usually social or economic ones, change in time. For example, more food is produced or more labour is utilised per unit of land per unit of time. 'Intensification' also is a relative concept since the intensification of production in system A (from time a to time b) may seem quite insignificant when compared to intensification of production in system B. In reference to the matters discussed in Tallebudgera, but also generally valid, it is evident that not all changes described by this term can be considered tantamount to 'trends toward social complexity' and that there may be constraints on the possible outcomes of certain kinds of 'intensifications'.

In her specific consideration of the appearance of many sites in northwest Victoria after about 4500 BP, in contrast to the few sites there before this time, and in the environmental situation of increased aridity, Anne Ross conveniently sets out the expectations of competing theories to explain this phenomenon. Since, in her view, traditional hunter-gatherer theory, especially that of optimal foraging strategy, predicts that more sites and more durable sites will be found in improving climatic circumstances, this theory must be rejected. She considers, rather, that increased management of resources in southwest Victoria, as argued by Lourandos (1985, 1983, 1980), might plausibly lead to a long-term trend to population density and migration to the Mallee. In Australia, as we all know, social fission is the classic response to population pressure on exceedingly fragile resources.

Now all this may indeed be plausible, but it may also be possible to reverse the argument, or at least tilt it a few degrees. In times of environmental difficulty, one might well expect new degrees of cooperation among otherwise hostile groups. This activation, and demographic clustering, of potential social ties in difficult conditions might lead to a budding-off of some groups into marginal areas; so might a deteriorating environment alone impel the migration of groups into previously underexploited, if uninviting, niches. Neither of these hypothetical scenarios depends on a population growth model, but rather one of demographic shift. Although I did not spend much time on the matter of scale in the first part of this paper, it is probably relevant to note here that the Mallee sites seem to be almost entirely lithic scatters and mostly quite small. If this is 'intensification', it is a very unintensive form of intensification; it might even be preferable to eschew this term entirely in reference to the social and economic changes in the Mallee that Ross has described.

Elizabeth Williams is similarly occupied with the nature of changes in the mid- to late-Holocene period in Victoria. She reviews the data on artificially constructed mound sites and discusses the functions of these, which was mainly to raise living and cooking activities above productive wetlands and boggy environs. She makes the very clear epistemological point that the contrasting explanations offered to explain such data—independent population growth and agglomerations for ceremonial purposes—are both able to incorporate her findings. If a model cannot be falsified, as we all know, it also cannot convince. Let us look at the models again.

Lourandos argues that late-Holocene water-control systems in southwestern Victoria, especially in the construction of fish and eel traps, imply intensified socioeconomic and demographic stability. By intensification, he means greater control over the environment, an elaboration of productive strategies that in part can be correlated with increasingly difficult environmental conditions (somewhat reminiscent of a Childean oasis theory to explain the origins of domestication in western Asia). But the major issue for Lourandos is surely his notion of an intensification of social and ceremonial networks from which, in order to support the circulation of materials and marital partners in permanent, 'semi-sedentary' locations (Lourandos 1983: 84; 1980: 249), new productive strategies are set in motion. It is this social intensification that results in greater productive stability, plausible population increase, and the breakdown of egalitarian institutions into a gerontocracy (Lourandos 1985: 406). I hope this brief summary does, if not real justice to his interesting ideas and research, at least no serious damage to them.

Lourandos further argues that, while population growth is a necessary precondition of social growth, population growth is usually a common effect of other causes. If we have learned anything from Cowgill (1975a, 1975b), it is that population growth can not be abstractly modelled either in steady rises or in sudden spurts. There are ways to control growth and the onus is always on the side of having to explain why population grows, not just in noting that it does grow. It is further plausible that relative stability in settlements is often a factor that contributes to population growth. For me the most important part of Lourandos' thesis is that one cannot simply derive economic and social institutions from environmental factors, no matter how simple the society. Rhys Jones' argument of Tasmanians deciding not to eat fish (Jones 1978) and the many !Kung San studies showing that choices are made among the possibilities of edible foodstuffs demonstrate that hunter-gatherer ecology is not easily reducible to optimal foraging. In complex societies, of course, the situation of settlements often has little to do with carrying capacity, and site catchment studies do not explain the location of pilgrimage sites, administrative centers and the geometry of settlement patterns. Lourandos has very impressively called our attention to the fact that in prehistoric Australia one must account change to conscious modes of adapting, not just conserving, the environment or reacting to environmental change.

The most elegant evidence for alteration of the landscape and intensification of production in Greater Australia is supplied by Golson's work at Kuk. For those interested in complex societies, the Kuk material may provide the final refutation, not just of the Wittfogel hypothesis that control of water leads to states, but also that intensive alterations of the environment inevitably lead to high degrees of social stratification. While some Wahgi (and certainly other PNG societies) are not easily pigeonholed as 'big-man' types, neither were there systematic courses toward economic, societal and political power that were divorced from the constraints of kinship. Also, 'rituals of intensification', such as Lourandos reviews in the ethnographic literature of Australia, classically emphasise temporary groupings of people. In cross-cultural studies, however, these 'rituals of intensification' celebrate the fusion of allies so as to maintain loose kin ties and are mainly concerned with personal rites of curing and passage. The emphasis in such ceremonies, normally conducted by elders, is on personal or group prestige and these are not easily translated into formal institutions for decision-making (Blanton *et al.* 1981: 183). For hunter-gatherers of various types, it seems quite normal to seek allies, maintain relationships and bank-account such ties as contingencies against the vicissitudes of life.

It seems difficult to derive population growth from alliance models. Neither the epigenetic model of Friedman and Rowlands (1977) nor the work of Meillassoux (1972) seem to do more than presuppose that existing social asymmetries can be reified into ranked hierarchies through unequal exchange patterns. The concern in prehistoric Australia, in any case, is whether social asymmetries and institutional stratification exist at all. Finally, from my end of the telescope, it is hard to see the expected result of social complexity that is the prediction of this postulated process of social and economic intensification (see note 3): no great inequalities in economic power are observed, no privileged access to symbols of community legitimacy that integrate societal heterogeneity and, of course, no politically specialised roles that are divorced from the web of kinship. In the fall-out from the 'Man the Hunter' approach to hunter-gatherer studies (see especially Bender 1978), investigations of prehistoric Australian societies must obviously explain such mid- to late-Holocene changes as there were in social, economic and demographic variables and they must do so within contextually appropriate realms of theory. Those theories that are useful in framing such explanations, however, must be different from those appropriate in understanding the social transformations that resulted in complex societies. From my end of the telescope at least, it seems most interesting and important to determine the constraints on the kinds of changes that took place in prehistoric Australia, to investigate why there was no evolutionary spiral of the sort that produced ancient civilisations (Yoffee n.d.).

Conclusion

It is clear, as several of the papers have stated for themselves, that the issues involving social change in prehistoric Australia and Papua New Guinea are not the same as investigations of social complexity in the kinds of cases reviewed in the first part of this paper. The Tallebudgera papers, however, provide positive and valuable examples of how the archaeology of non-complex societies has matured from the old New Archeology days. At that time archaeological theory seemed concerned overwhelmingly with non-complex societies, and social change was explained largely as responses to environmental stress. Archaeologists dealing with complex societies were pressed to understand change in the framework of such theories derived from cases of hunter-gatherer and horticulturally based societies (or were simply classed as antiquarians), and we have only recently, but forcefully, freed ourselves from such theoretical priorities. It would be ironic now for Australian archaeologists to apply theories used to explain complex societies to their own very different data and problems; the matters of change within hunter-gatherer societies in Australia are complex and interesting in their own terms as the papers of the Tallebudgera panel have shown. Furthermore, some of the papers have demonstrated that the evolutionist schemes that have tried to ascribe social change within a kind of speciational paradigm of emergent sociocultural stages have masked some of the most interesting problems with which archaeologists have to deal: they sacrifice data that are 'anomalous' from evolutionist 'types' on the altar of holistic social change. Finally, these papers indicate how vital are the connections between the archaeological and ethnographic evidence in this Austral part of the world—without necessarily subordinating one class of data or intellectual activity to the other.

Ian Lilley has reminded us that my colleague in Tucson, Bill Rathje, was 'stunned' by the lack of data in New Guinea with which to analyse trade and compare its effect on the nature of social stratification with that in Mesoamerica (Rathje 1978: 171). Evidently, Rathje had not appreciated that formal similarities in long-distance trade between Melanesia and Mesoamerica need not at all imply similarity in evolutionary trajectories or the use of undifferentiated evolutionary theory. Comparison, as I have stressed, works two ways and contrast has its own rewards: much is learned by considering why societies are *not* comparable. Research on the constraints of growth are as important and as interesting as study of growth cycles. Whether archaeology as a discipline continues to grow or not may hinge on this sort of realisation.

5c The cemetery as symbol: the distribution of prehistoric Aboriginal burial grounds in southeastern Australia

Colin Pardoe

Burials are a common part of the cultural remains left by previous societies and as such have a great deal of potential for archaeological study. As the last and most disruptive of the 'rites of passage' (van Gennep 1960), death is of considerable significance to any society and we stand to learn much from the examination of a society's physical response to death. For the archaeologist, especially in Australia, this means an examination of burial: customs, location, variation.

The archaeology of burial is often oriented toward individual cases, with the analysis of skeletons, graves, grave goods and social status. Meehan (1971) studied in detail the form and distribution of burials throughout Australia and remains the prime source of information on the subject. To date though, there exists no Australian study that has treated the cemetery as a discrete entity.

I propose here a definition for cemeteries in southeastern Australia and map their distribution and chronology. Known aggregrations of burials in this region are described and evaluated in terms of the definition. Some are assessed to be cemeteries: others are not.

I then examine the cultural and economic role of cemeteries and how this might be interpreted in an archaeological context. To this end I explore the proposal that cemeteries are linked to economic circumstances, as well as religious. Discussions of burial practice in Aboriginal Australia have invariably focused on religious ideology rather than on the more pragmatic economic aspects. My observational position does not deny religious causation. Indeed, it does not make sense to separate economic and religious spheres—the relationship between environment and culture is an elaborate dialectical process.

From an archaeological perspective I am not interested in causal elements. The reconstruction is more concerned with the interaction of these differing aspects. Once religious ideas emerge from social/economic relationships (or wherever), they have an independent social life and may be manipulated in social and economic contexts. In this paper, environmental and economic aspects of burial will be demonstrated to play a major role.

Saxe (1970) links cemetery behaviour with specific resource distributions, large and dense populations, and a social organisation characterised by descent groups which are corporate, localised and unilineal. In this sense, cemeteries will be viewed as symbolic markers of group affiliation and, through that, land ownership. Cemeteries are found only where large groups and permanent but finite resources coincide.

I will refer to broader social theory by placing these observations within Peterson's (1986) model of inclusion/exclusion. The River Murray populations will be seen as groups promoting exclusion and limiting their membership through descent. This contrasts with social and territorial organisation in other areas, where access to resources is maximised by extending kinship ties.

Finally I set out a diachronic reconstruction of social organisation throughout the Holocene. It is difficult to extrapolate social organisation from archaeology and this prehistory is based on archaeological features, skeletal biology and anthropology. It is an outline for an archaeological region that will bear examination from other studies and which follows Witter (1984) in his overview of 'cultural adaptive areas'. One of the consequences of this regional view is an explanation of the morphological changes evident in this part of the world in terms of social organisation and population structure rather than migration theories. I will point to an indigenous biocultural origin for the distinctive populations of Kow Swamp and Coobool Creek, rather than to the island of Java.

Defining cemeteries

The term 'cemetery' (or graveyard) requires definitions both in structural terms and in archaeological practice. As a structural feature it remains undefined in the Australian context. Cemeteries are usually associated with sedentary, agricultural societies, not hunter-gatherers, although this is not always the case (Pardoe 1980). Many of the southeastern cemeteries I will describe are thousands of years old and betray their presence only by the graves. In contrast, cemeteries in other parts of the world are often more easily defined by some form of monument such as barrows, mounds, pits, megaliths or headstones (Bradley 1981; Chapman 1981).

I have arrived at the following criteria for defining cemeteries from the study of known sites in the region, my own research and the need to apply archaeological techniques and methods of analysis to difficult data. These four practical criteria are: number of burials, contiguity, boundedness and exclusivity of site use.

First, there must be a significant number of burials, though the absolute number is rather difficult to pin down and is site dependent. The more graphic observation that a cemetery consists of a number of burials has generally sufficed elsewhere, but in the southeast where there are usually no other indicators of the site and where burials are a common occurrence in riverine sand formations, the number of graves is not critical. This will become apparent in the site descriptions, and so these sites must be assessed in light of the other criteria.

Second, cemeteries may be defined as places where there are contiguous burials: that is, a single location where graves are adjacent. This is difficult to qualify, but depends on density of burial and boundedness of the burial site. With many burials close to one another it is possible to be reasonably sure that they are not random events. That is, the probability of one burial is dependent on the fact that other known interments are in the same place. Thus, even over 1000 years, 40 burials could take place, each based on the memory of a preceding one.

Contiguous burials may also be defined in distinction to burials that are essentially random, independent events (in a statistical sense). For example, the 103 burials at Willandra Lakes (Clark 1985) come from thousands of hectares and hundreds of kilometres of lunette and dune formations. The fact that there are many graves has to do with length and density of occupation. The graves are not, however, associated one with another. (It may well be that lunettes were known to be the final resting place for some individuals and there may have been reasons that determined burial location based on rank, grade or other status. But it is equally true that no *single* spot was considered to be preferable or necessary. Nor is any grave unequivocally related to another in time or space. That is, we cannot point to two graves and say that one might have been placed there because the other was.)

Third, graveyards are sites where the burials are bounded, either by landform or in the actual distribution of graves. The density of burials should decrease fairly quickly at the edges of the site.

Fourth, cemeteries should exhibit exclusivity. During their time of use, they should not have been occupation areas as well and so should not have the mundane material remains associated with living areas. By way of illustration, burial on the abovementioned lunettes and along water margins generally follows density of site occupation. More burials are to be found in areas of increased site density. Burials and occupation sites both cluster at outlet creeks and inter-lake stretches. It is clear that on the Willandra and Darling river systems, burial and site density are highly correlated rather than mutually exclusive.

The idea of exclusivity as a defining feature of graveyards is implicit in most mortuary studies and accepted as given in ethnographic examples. The explicit statement of this criterion is usually unnecessary: we all know that cemeteries have one use. However, the particular situation in this study area (eroding landscape, long time spans, large variation in burial location and preservation) demands as much detail as possible in the definition of these archaeological features.

Saxe's hypothesis

Saxe (1970) brings an analytical rigour to mortuary studies that not only highlights his interest in general rules of societies, but also allows the archaeologist to operationalise his hypotheses. Both Saxe (1970) and Tainter (1978) make the point that since variation in burial distribution is attributable to social factors, then study of the same distributions may provide insights into social organisation. Saxe's idea on the use and reason for cemeteries is simple (once thought of) and succinctly put:

> To the degree that corporate group rights to use and/or control crucial but restricted resources are attained and/or legitimized by means of lineal descent from the dead (i.e. lineal ties to ancestors), such groups will maintain formal disposal areas for the exclusive disposal of their dead, and conversely. (1970: 119)

In other words, cemeteries are a symbol of the rights of groups to resources that are not distributed equally, either in space or between groups.

Descent groups have existed as a form of social organisation throughout the whole of Aboriginal Australia. However, the extent to which descent groups are fluid or corporate, dispersed or localised, cognatic or unilineal varies from region to region.

Peterson, in his analysis of Aboriginal territorial organisation, distinguishes between desert groups in areas of low population density where descent patterns emphasise inclusion, and those larger social and residential groups in richer environments where 'descent becomes an indigenous model of group structure and a primary mode for the transmission of rights' (1986: 153). In such corporate groups, the emphasis of the descent pattern is on exclusion. I propose that burial in cemeteries is one factor in that process of exclusion and serves to reinforce lineality.

While most of the studies deriving from Saxe's hypothesis have been at the level of intra-site variability, a few analyses, including that of Saxe himself, have been concerned with inter-site or interregional patterns. Goldstein (1976) applied this hypothesis ethnographically, and to Mississippian society in Illinois (AD 900–1400). Her results demonstrated a general association of cemeteries or formal disposal areas, with groups characterised by lineal descent patterns. She noted that the converse was also the case, but not as assuredly: most groups maintaining lineal descent patterns had formal graveyards.

Charles (1985) found further agreement in the same area with the evolution of cemeteries in Mid to Late Woodland (500 BC–AD 900) corresponding to 'territorial saturation of the lower Illinois River valley'. He also argued that the distribution of cemeteries was intimately linked to the distribution of resource territories.

On the other hand, in an analysis of prehistoric southern Ontario social organisation, Spence (1986) noted the rise of cemeteries in Terminal Archaic times (1400–900 BC) without any seeming economic or population stimulus. He concluded that even though graveyards may have eventually become linked with resource control and intergroup competition, their origin may have been internal and religiously based.

In this discussion, Saxe's hypothesis will be considered in terms of the relationship between cemeteries, size and distribution of population, environment and social organisation for the River Murray cultural area. This reconstruction will be relevant to current investigations of 'evolving social relations' and material and technological innovations in the mid-Holocene.

There is a potential for skeletal biological studies to test this reconstruction and the probability of the archaeological results. The implications of localised lineal descent groups, marriage patterns, inbreeding and differential burial on variation in skeletal morphology in Australia are great. Therefore our interpretations of the patterns of variation must take such implications into account. The timing and nature of the proposed social organisation is highly suggestive of a cultural origin for the distinctive Kow Swamp-Coobool Creek groups as well as later populations in the central Murray.

Archaeological data on cemeteries

To see how this proposed association between graveyards, corporate descent groups and resources applies to Australia, I have selected for study the southeast (as defined in McBryde 1984) and in particular the Murray/Darling Basin.

The information on burial grounds is uneven in distribution, often poorly documented and varies in scientific content. The basic data come from museum records, study of the Murray Black skeletal collection[1] (Sunderland and Ray 1959) and my own archaeological investigations (Pardoe 1985a, 1985b).

One of the problems with studying this particular set of sites is that many of them were not excavated by archaeologists and the archaeological information that would be most useful is missing. However, I have studied the material itself, both the individual skeletal remains and their location. I have also talked with Professor I.J. Ray, who was involved in the original collection of the Murray Black collection at the University of Melbourne (Sunderland and Ray 1959) and local residents of the Murray valley.

All the Murray Black sites are likely to have been concentrations of burials. I have determined this in two instances: Lakes Victoria and Benanee. From these two sites it is apparent that Black's methods were only appropriate to large-scale excavation. He was not likely to have circled a lake excavating at random, but apparently settled on large concentrations. Furthermore, from my own ongoing study (Pardoe 1985a), it is apparent that large numbers of exposed but uneroded burials could not be found by traversing vast areas. To excavate 100 burials in a field season, he would have been exposing contiguous burials. I am convinced of the burial distribution for most of these sites, while others I have surmised on the basis of Black's methods and his correspondence with the Australian Institute of Anatomy.

The following is a description of most of the burial sites known in southeastern Australia that have multiple interments (Fig. 5c.1). I have only included sites which contain information on the four criteria outlined above.

Kow Swamp

Kow Swamp (Thorne 1975) is one of the particularly resource-rich lakes associated with rivers in the Murray-Darling basin. The lake is characteristic in having a regular ovoid shape, a sand/clay lunette along the eastern margin and one or two outlet creeks. These creeks connect the lake to the river and enable flood waters to fill the basin.

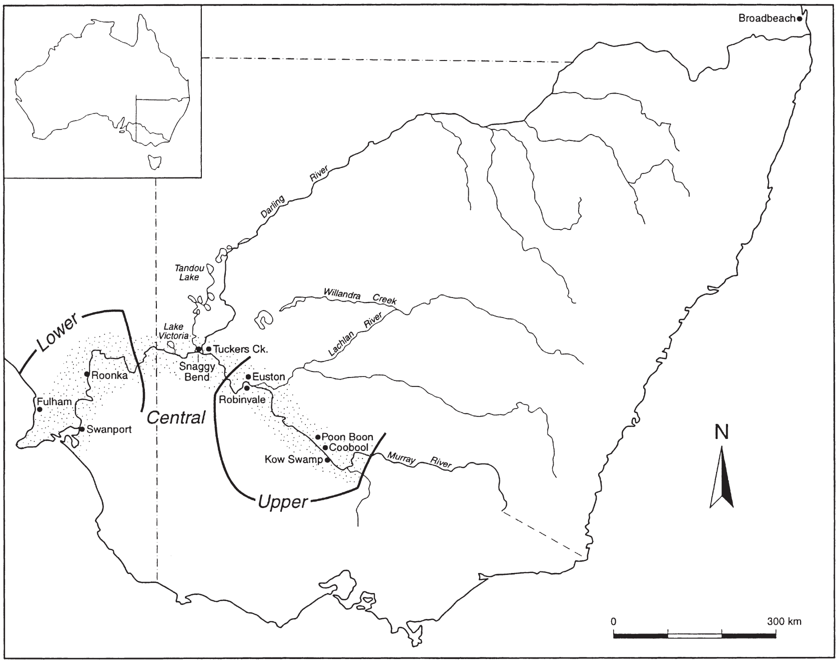

Figure 5c.1: Cemeteries in the southeast. The divisions of the River Murray are based on biological differentiation (Pardoe 1984), social organisation (see text for references), and archaeological and environmental data (Witter 1984).

The Kow Swamp burials occur on either side of Taylor's Creek, the outlet channel that punches through the lunette about halfway down the eastern margin. The 22 to >40 burials that occur in this small area are not the only ones on the lake. The well-known Cohuna cranium was found on the north shore, but the Kow Swamp burials form by far the largest portion on the lake edge. The density of burials at the main burial spot on Taylor's Creek is one grave per 11 m^2. This value, and all the other density estimates, is derived from the *in situ* evidence, in this case the 11 undisturbed graves and the minimum area that encompasses them. The density of burials could be higher, for the rest of the skeletal material comes from a disturbed layer with no certain provenance.

Coobool Creek

Coobool Creek (Sunderland and Ray 1959; Brown 1982). The 70 carbonate–encrusted crania retrieved from 'Doherty's Hut' at Coobool Crossing on the Wakool River were dug by George Murray Black, who excavated many of the sites listed below. None of these have archaeological provenance, stratigraphy or site plans. The only information for some is the relationship between site location and year of excavation (L.J. Ray, pers. comm.).

The Coobool Creek material was dug in 1949 and 1950; the 1950 sample I accept as Pleistocene-Holocene boundary in age and contemporaneous with Kow Swamp. The 1949 sample is assuredly younger and is provisionally accepted as late Holocene in age. It is not possible to determine whether these two samples come from the same locale, or from ones slightly different.

Baratta Tulla

These are two adjacent stations just north of the River Murray. From Black's notes it is apparent that many more that 60 burials were in the two areas he excavated, but 60 is the number of individuals in the collection with sure provenance. I am not sure yet whether he excavated at more than two spots to secure the majority of these burials. At least 50 skulls come from Baratta and perhaps as many as 100. From Tulla he records 70 and these apparently come from one locality.

Lake Poon Boon

Nothing is known of this site. I have not surveyed the area nor contacted people resident in 1947, when Black excavated here. As with many of the other areas sampled in this collection, the 139 individuals from Lake Poon Boon may come from a few locations in the area. One hint is the presence of dark mineral staining to most of the skeletons. This is characteristic of inhumation in clay rather than sand and may indicate that the burials did not come from sand deposits at the lake itself. This would not preclude burial in a prior channel or other landforms associated with the lake.

Robinvale/Euston

Burials abound in the Euston-Benanee-Robinvale area. Sunderland and Ray (1959: 47) mention that 'nine burial grounds were excavated in this area, all of them being in sand or a sandy loam type of soil'. These would have been dug in 1946 by Black, who also led an expedition in 1937. The more than 400 excavated individuals come from 9 graveyards, with an average of 44 graves per cemetery.

One of these locations is certainly Washpen Creek, the outlet for Lake Benanee. I surveyed the edge of the lake in 1985 and found 11 burials spaced along the lunette. There appears to be a small concentration of burials on the northernmost part (Pardoe 1985a, Clark and Hope 1985).

Bowdler (1983) excavated part of a graveyard on the south side of the River Murray at Robinvale, uncovering the remains of 11 graves. She estimates a density of one grave/0.3 m^2, with between 245 and 1400 burials in the site. The latter estimate is based on 'the extent of relatively high, flat ground available' (p. 38). This number is definitely too high given that cemeteries I have observed do not cover the available landform. I consider 245 individuals to be a maximum estimate.

The age of the Robinvale cemetery is at least 3 kya. Bowdler suggests that this is an intermediate date, with some burials stratigraphically older while sediment and camping debris cap the burials.

Snaggy Bend

The cemetery at the Murray-Darling junction has been monitored by Peter Clark for a number of years and has been recently described (Clark and Hope 1985). Although salvage operations by the NSW National Parks and Wildlife Service have attempted to halt the erosion, it is too late for the majority of burials, which are now completely fragmented and windblown, with little opportunity for future archaeological investigation.

The number of burials recorded at Snaggy Bend is 157, with probably no more than 200 representing the sum total. These are situated in a sand dune that was originally perhaps 2 to 3 m in height and 120 m in diameter. The cemetery is located 500 m north of the River Murray and 5 km west of the Murray-Darling junction. It is about 500 m west of a billabong that was likely to have been a prior channel of the Murray. The burials are in the central section of the dune and are bounded within this landform. The density of interments is estimated by Clark and Hope (1985) to be one grave/5 m^2.

The site contains recent burials as well as a 10 kya, date (on shell) associated with one burial.[2] It is probably misleading to suggest a continuous and even distribution of burials through this long lifespan. Most of the graves appear to be in an upper unit and therefore would not date beyond the younger radiocarbon date of 6890±100 yr BP. Clark and Hope suggest that many of the burials are less than 150 years old. While some are associated with European goods, their assessement is based on stratigraphic position (in the upper red-brown soil). Other burials from the lower part of the red-brown soil are partially mineralised. This is not necessarily an indication of age of the burials, but of depth of the grave.

Lake Victoria

The Lake Victoria-Rufus River-Lindsay Creek complex is similar to Robinvale-Euston in that many skeletons associated with a number of graveyards are present. Some part of the collections are directly attributable to restricted localities, but much is muddled by the lack of attention paid to recording site information at the time. There are over 570 individuals represented in museum collections for this general area.

Black dug at Rufus River in 1938 whence he sent 200 crania to the Australian Institute of Anatomy. In 1940 he dug at Ned's Corner and Lindsay Creek, these collections also going to the Institute of Anatomy. In 1941 to 1943 he was again in Victoria digging at Lindsay Creek and Lake Wallawalla (the Chowilla area mentioned by Sunderland and Ray 1959: 46–7). He was also at Rufus River and Lake Victoria in 1944 and 1945. The skeletons exhumed in these years went to the University of Melbourne.

There is no way of knowing how many excavation sites there were, nor where the burials came from. I spent a short time at Lake Victoria in 1984 and 1985 recording some burials and have found two spots where a large number of skeletons were dug out (many remain). One of these is in the extreme south end of the lake and the other is in a small isolated dune bordering Rufus River. Clearly Black dug major sites at Lake Victoria (south end), Ned's corner on Lindsay Creek, Rufus River itself and Lake Wallawalla.

Near the junction of Lindsay Creek and the River Murray, Blackwood and Simpson (1973) found six and 16 skeletons less than 500 m apart in a 1 km long north-south tending dune of loose yellow sand. The larger group of burials (site 19c at Lindsay Creek) covered an area of about 12 m^2 with a density of one grave/0.75 m^2.

Blackwood and Simpson (1973) recorded a concentration of 14 burials from the northeast corner of Lake Victoria, coming from a red sand unit in the lunette (site 8). Even though the palaeosol was clearly Pleistocene, a radiocarbon date for the burials was less than 600 years. The 14 burials come from an area of very roughly 240 m^2, yielding a density of one grave/17 m^2. Wallpolla Creek was the site of 24 skeletons being found (site 16B). The density of burials at this location I have estimated at one grave/47 m^2 (from their Fig. 5c.2).

Although I have identified a number of areas that had graveyards, I am in the unenviable position of not being able to state how many there were, nor how many burials were in each. The more than 370 individuals recovered from this area must have come from no less than three areas (Lake Victoria, Rufus River and the River Murray-Lindsay Creek anabranch system) and anywhere up to a dozen cemeteries.

Roonka

Roonka Flat is a low, wide, sandy expanse on the river's edge in the cliffed region of the Murray. The dune proper is next to the river and is about 400 m in length. It is bordered by the river and by floodplain and swamp. Most of the 142 burials have been found here, with few coming from the river margins for some distance to the north and south. The burials are about as dense here as at Snaggy Bend, with one grave/6.4 m^2 in the excavated area. The site has been used as early as *c.* 18 kya, but it seems that the bulk of burials come from a more restricted period of time. Pretty (1977: 297) suggests that the main period of cemetery use spans the last 7000 years. He further elaborates the cultural sequence with two points of immediate interest.

First, the chronology points to an early occupation phase of *c.* 18 kya without burials. This is Roonka I. Roonka II was used exclusively as a cemetery from between 7 and 4 kya and Roonka III appears to have not only burials but considerable occupation debris as well. This latter phase spans the 4 kya to the present. I should note that this sequence is based on burial form tied to a very few radiocarbon dates and very difficult stratigraphy. Most of the burials are not assigned to a phase.

The second point is that burials increase sevenfold in an undeflated section of the dune from six (Roonka II) to 41 (Roonka III). Pretty has subdivided phase III into earlier and later components based on burial features and their form. While this is an uncertain procedure given the diversity of burial patterns in the Southeast (Meehan 1971), it is worth noting that the presumably later component (IIIb) has 33 burials compared to eight in the earlier (IIIa).

Swanport

The site of Swanport seems to have generated a great deal of misunderstanding in the archaeological literature. It was one of the early victims of sand quarrying, yet the original publication (Stirling 1911) gives an adequate summary description of the site. It has also been called an epidemic graveyard (presumably by those who have read no more than the title).

The site was on a dune made up of three layers. The upper sand is dark from organic midden deposit with apparently a weak carbonate development, although these small nodules could have been the result of soil reworking by digging/burrowing. The lower sand is red with thin layers of broken river mussel shells and is underlain by a dense carbonate horizon (‘a layer of imperfectly consolidated travertine limestone about 2ft, in thickness’, p. 8). The burials were dug through the red soil from the black and generally lay just above or on the carbonate layer. The bones have no carbonate encrustation, nor are they mineralised. Although it is not safe to overgeneralise, they are certainly less than 9000 years old on this basis, as dense carbonate formation is likely to predate this time. In fact the burials are likely to be much younger than this. There has never been a radiocarbon date for this site, a somewhat surprising fact in light of its importance in skeletal studies in prehistory (as well as anatomy and dentistry). Swanport was not a ‘plague pit’ resulting from a smallpox epidemic as it is clearly pre-contact.

Most of the burials were found in a 50×30 ft (135 m^2) area (Strirling 1911: 9). A minimum number of 90 individuals were recovered, although Stirling estimated there would be as many as 160 in the total. The latter estimate is probably more accurate, since the incomplete, broken and disarticulated remains have not been recorded. In any event, the density of graves in this cemetery would have been no less than 1/1.5 m^2 (n=90) and as high as 1/0.8 m^2 (n=160).

Fulham

The suburb of Fulham is on the banks of the River Torrens in Adelaide. The site was salvaged in the 1950s by members of the University Faculties when land reclamation was underway (Professor T. Brown, pers. comm.). Apparently on low sandy ground, the burials have no chronological or archaeological provenance. The bones are well preserved and free from carbonate encrustation or mineralisation. It is now impossible to determine if all or only part of the cemetery was saved, however the bulk of the burials did come from a single restricted area. Further work on the provenance of burials from Fulham will be necessary to get a better view of this particular site, especially for the number of burials.

Broadbeach

This is one of the only graveyards recorded for the east coast. While Haglund (1976: 80) suggests that there are others in the immediate area, such as that at Bundalla, she also says that single burials are the rule further south. This is supported by McBryde (1974).

The site of Broadbeach is on a narrow sand ridge about 1.5 km from the coast. It rises about 1 to 1.5 m above the surrounding marshy areas. Streams bound the ridge to north, east and northwest. The top of the dune was the only place where burials were found and in the 225 m^2 excavated 140 graves were found, some being multiple interments. This yields a density of one grave/0.6 m^2. Broadbeach differs from the rest not only in location, but in age. It seems to span a little more than 1000 years, although the construction of a detailed chronology has not been possible.

Lake Tandou

The Tandou lunette site (TL I; Allen 1972: 232–4) is one of the few Darling River sites that has been considered as a cemetery by virtue of burial density and circumscription. Unfortunately, it is also the one part of the lake margin to be completely destroyed by irrigation earth-works. Thus, in my survey of the lake as part of a larger archaeological project (Pardoe 1985a), I was not able to examine this area of the lunette.

The site is described by Allen: 'Numerous scattered human bones on the site are evidence of a large number of burials which have eroded out of place along the ridge of the lunette' (1972: 232).

As with many of these sites, Tandou Creek cuts through the lunette at this point. There are problems with interpreting this spot as a graveyard and the first of these is the number. There are ten plus an indeterminate number of individuals. These are located along the exposed ridge, so presumably more are there. The size of the site is difficult to determine, but then that is an ongoing problem of archaeology in the western region of NSW. I estimate it to be roughly 70 m in diameter and covering most of the transverse distance of the lunette. The burials are not restricted to this spot, however, as was noted for Kow Swamp. I found 39 burials along the 28 km length of the lunette, and another survey in 1981 by Jane Balme and Jeanette Hope recorded a further 21 not evident in 1985. Tandou Lunette I (TL I) is a local concentration among burials that encompass the whole lunette.

Summary data on cemeteries

Figure 5c.1 maps the cemeteries known in the southeast as well as a few areas of above-average burial density that have been referred to as graveyards. It is clear that true cemeteries are restricted mainly to the River Murray. What might be thought to be possible exceptions, around Willandra Lakes and Tandou Lake on the Darling River, do not fulfil the criteria for cemeteries and as will be elaborated below should not be classed as such.

The Broadbeach burial ground on the east coast is the only clear exception to be found outside of the River Murray corridor.

There are some other records of burial grounds in the southeast, but these are generally incompletely described. Poor as our information is for the ones listed above, it is worse for others such as Nap Nap, Koonadan, one in the Macquarie Marsh region and so on.

On the River Murray, cemeteries seem to be clumped in at least three main areas. In the Upper Murray region there are perhaps six or more. In the Euston area the collections are likely to come from ten, while further downstream at Lake Victoria there might have been as many, or more.

Table 5c.1 lists the burial sites and an estimate of the chronology, which more than anything highlights the urgent need for a detailed dating program.

In Table 5c.2 I have listed the density of burials for those with sufficient data. For true cemeteries the density ranges from a low of about one grave/11 m^2 to a high of 1/0.3 m^2. Tandou Lunette I, Wallpolla Creek and the site at the northeastern edge of Lake Victoria could all be discounted as cemeteries. Their density is very much lower and given the large number of burials at lunettes, local concentrations might be expected. This is so because considering each interment as a random, independent event would yield exactly such a pattern for these lunettes. Each burial would be determined by camping location, season and a host of other factors. It is possible that, given the amount of time available, each burial could also be chronologically independent of all others, however this is perhaps less likely.

Discussion

Using the four variables of number, contiguity, boundedness and exclusivity, it has been possible to extract a large amount of information from burial locations. The main result has been in deciding which sites constitute cemeteries and which do not. In the first part of the discussion I will address both the definition, with applications to resource management, and the distribution, with applications to predictive modelling. The second part will be an analysis of the cemetery data in concert with their chronology, skeletal biology and Saxe's hypothesis to present a reconstruction of prehistoric social organisation. I believe all this material taken together is sufficient to define an archaeological region, the River Murray Corridor.

Table 5c.1: Cemeteries and localities with numerous burials in southeastern Australia (mainly River Murray). Burial numbers are estimated from museum skeletal samples and should be considered as minima. The sites of Roonka and Broadbeach are more accurately counted. Dates with question marks are estimated, others are based on available radiocarbon dates.

Cemetery	*Number of individuals*	*date (kya)*	*Reference*
Kow Swamp	40	13–9	Thorne 1975
Coobool	70	13–9	Brown 1982
Coobool	90	6?–2?	Sunderland and Ray 1959
Baratta/Tulla	120	?	Sunderland and Ray 1959
Lake Poon Boon	139	?	Sunderland and Ray 1959
Robinvale/ Euston/ Lake Benanee	$\bar{x}$=44	6–2?	Bowdler 1983, Sunderland and Ray 1959, Pardoe 1985a
Snaggy Bend	150–200	10-modern	Clark and Hope 1985
Lake Victoria/ Rufus River/ Lindsay Creek		5.3–0.8	Sunderland and Ray 1959, Blackwood and Simpson 1973
Roonka	142	7-modern	Pretty 1977
Swanport	90–160	?	Stirling 1911
Fulham	60+	?	
Broadbeach	200	1.3-modern	Haglund 1976
Locality			
Tandou	15+?	?	Allen 1972
Lake Victoria	14	0.75	Blackwood and Simpson 1973
Wallpolla Creek	24	5.4–4.2	Blackwood and Simpson 1973

Definition of cemeteries and resource management

The number of burials in a given area does not vary randomly. If burials were distributed randomly over the landscape, that is without regard to the presence or absence of other burials, then the probability of observing two burials in an area of given size would be less than that of observing one. The probability of observing three burials together would be less than that for two, and so on until the probability of observing more than ten together becomes negligible. In fact, the distribution of burials in a given area would follow a *truncated Poisson distribution*. From my own observations and a

Table 5c.2: Density of burials in cemeteries (upper) and localities with numerous burials (lower). Density is estimated as the number of square metres occupied by one grave: I grave/x.x m^2. The figure in brackets is anupper estimate.

Site	*Density*	*Reference*
Kow Swamp	11.0	Thorne 1975
Robinvale	0.3	Bowdler 1983
Snaggy Bend	5.0	Clark and Hope 1985
Lindsay Creek	0.8	Blackwood and Simpson 1973 (site 19c)
Roonka	6.4	Pretty 1977
Swanport	1.5 (0.8)	Stirling 1911
Broadbeach	0.6	Haglund 1976
Lake Victoria	17.0	Blackwood and Simpson 1973 (site 8)
Wallpolla Creek	47.0	Blackwood and Simpson 1973 (site 16b)
Lake Tandou	40.0	Allen 1972 (site TL I)

review of the literature, this distribution holds only partially. Most of the areas with more than one burial do not have two or three, but dozens. The form of this distribution indicates clumping, a phenomenon well known in sampling theory. It indicates that non-random factors are affecting the spatial distribution of the observations: that the presence of a grave is, in part, determined by the presence of other graves in the immediate area. This is mainly common sense, concurring with our implicit understanding of what a cemetery looks like.

However we need to be able to decide if all the interments in an area (which might be very large) were made with the understanding that the spot was indeed a cemetery. Contiguity and boundedness are variables that may be measured archaeologically and so may help to decide whether the burials were placed with reference to other graves and a known locale. Even accounting for vagaries of preservation, the difference in burial density between cemeteries and other localities is striking (Table 5c.2).

Exclusivity as a criterion for defining a cemetery is only superficially easy to apply. With stratigraphic control, it is simple to image a cemetery, or any feature, as being spatially and temporally distinct from any other feature. However, in the western region of NSW most features, graveyards included, are on erosional surfaces: deflating features confound the stratigraphy. We are forced to give relative measures of density of site occupation. I will contrast the material remains in those localities where burials are independent and reflect site usage with what I consider to be true cemeteries.

TL I on Lake Tandou not only has more than 11 burials spread over a large area with an older cremation and a number of younger burials, it is also characterised by nine stone implements, an *in situ* grindstone, a small shell midden and presumably a large quantity of stone debris. Allen (1972: 234) makes it clear that much of this is chronologically as well as spatially associated.

Site 8 on the northeast corner of the lunette at Lake Victoria (Blackwood and Simpson 1973: 102) has a number of 'concentrations of dispersed mussel shells' but otherwise no artefacts. Site 16b at Wallpolla Creek displays an even larger midden nearby, but also had 'small scattered areas of shell and burnt clay in the area surrounding the site' (p. 101).

My own observation on the Darling River is that burials are part of the overall scatter of material remains. In any deflated area burials are surrounded by hearth remnants, faunal remains, stone tools and their debris and grinding implements. Clearly, these are now chronologically muddled, but just as clearly an area with a number of burials (distributed through time) also contains a large number of artefacts, also distributed through time. From the *in situ* evidence and the disturbed material that has deflated (that is, later material), burials are neither spatially nor temporally separate from the rest. Any given area has been used through time for different sporadic events; a burial, a tool workshop, a seed grinding station, an overnight camp.

The cemeteries on the other hand have had really only one function through the ages: burial of the dead. Most stone tools associated with the dead are grave goods. Evidence of fire is not found so much in hearthstones as in crematory or votive functions. Shell and faunal remains take the form of small ritual offerings, not middens. Haglund (1976) reiterates through her text the lack of material remains not *directly* associated with burial.

This is all well and good, except that there is evidence of tool-making (flaking debris) at Roonka and Kow Swamp. There are hearths and food remains, mainly in the form of shell middens, at Snaggy Bend, Kow Swamp, Roonka and Swanport. There is soil alteration, the development of a humic layer indicating an occupation horizon, at Swanport, Roonka and perhaps Snaggy Bend.

It appears that exclusivity, like boundedness, density and number, is not an immutable quality of cemeteries. Exclusivity needs to be considered in the context of other local occupation areas and with the knowledge that all events in these sites are telescoped into a very thin layer with poor stratigraphy. Furthermore, we need to consider the grey zone of what constitutes religious, ritual behaviour in the material remains and what constitutes the mundane. Are a small hearth and shell midden part of an overnight camp, the inhabitants of which, blissfully unaware of their ancestors lying beneath them, have made on a sandy rise? Or are they the remnants of a graveside ritual, the sharing of a last meal with the deceased family member? There is no archaeological test to distinguish the two sets of behaviour, so I will simply state as opinion my view that the material remains at these cemeteries are insufficient to be construed as 'occupation'. Twelve hearths and three shell middens (one dating to pre-cemetery times) at Snaggy Bend do not constitute even minimal evidence for behaviour unassociated with the cemetery. The hearths, shell middens and stone tools at Roonka are far more prevalent in the immediately surrounding areas than in the cemetery proper. In the latter, many of the artefacts are considered to be grave goods.

It is unlikely that these cemeteries represent nothing more than random inhumations over a very long time span (circa 13 kya) in very densely populated areas. This may certainly be the case for some spots such as the Willandra Lakes, Menindee and others that we know very little about archaeologically. However, burials are not distributed over huge stretches of the rivers in this fashion. These graveyards stand out well above the background distribution of single interments to be found in most sand deposits.

From a practical viewpoint, it might be argued that these burials in cemeteries are a matter of convenience in times of flood. This is plausible given that primary burials are the main form of interment in most of these graveyards. However, it does not explain the lack of burials in other similarly high structures (including mounds). While cemeteries have a function in containing pollution where land is restricted, there are many cases here where actual living space is not encroached upon by floodwaters. The argument that cemeteries exist in areas of high population density and less mobility for the purpose of tidying the landscape cannot be denied. Few complex behaviours serve only one purpose.

How many burials in a group constitutes a burial ground or cemetery? This seems important in considering the problems in Cultural Resource Management with sand quarrying of potential cemeteries. As with many quantitative questions it is difficult to answer finitely. But it is possible to answer that question with reference to known data and to Saxe's more theoretical approach. The dividing line is with orders of magnitude. All the cemeteries seem to be large, probably with hundreds of burials. Furthermore, the answer cannot be based solely on number, but it must include the other criteria of contiguity of graves, their boundedness and the use of the site. We should not consider nine burials in a rockshelter a cemetery, nor 103 dispersed through the lunettes and source bordering dunes of the Willandra Lakes. I reiterate that the greveyard itself is a tangible entity of symbolic value different from the interred graves. Perhaps, like all symbols, cemeteries have a value beyond that of their constituent parts.

Distribution of cemeteries and predictive modelling

Clearly, burial grounds are tied to the River Murray. Future investigation may prove them to be associated with tributaries of the system, although in the case of the Darling River this has not proven so (Pardoe 1985a).

There are a number of reasons to explain this distribution. However, I find only one compelling, the rest being ancillary factors. It would seem reasonable to suggest that here is one of the few places in the world with extreme variations in resources over a very small area. In other words, one could walk from a river stocked with unimaginable wealth in fish, fowl and invertebrates to a parched, drought-stricken plain in a matter of minutes. In those drought times when the river became the only reliable lifeline for food and water, access to such resources would be controlled by the resident groups.

The Saxe hypothesis would suggest that legitimation of this control of the river lay in the claim to ownership as handed down from ancestral groups. The cemetery would be proof of unilineal ties and no doubt would have been highly visible and maintained over very long periods of time. Similarly, in resource-rich areas of northern coastal Australia the ethnographic literature shows localised unilineal descent patterns to be a predominant feature of social organisation; in this area also, elaborate burial practices emphasise lineal ties to specific tracts of land. A more detailed comparison with these areas will be included in a future paper.

Looking to the coast we might ask why cemeteries do not seem to exist (keeping in mind the exception of Broadbeach) in an area at least as densely populated and resource rich. Perhaps the resources are not as crucial, in terms of survival or access. Access to marine resources in coastal areas may be less restricted or perhaps mediated in different ways. Burial grounds have been found or were known about in the Moreton Bay region (Haglund 1976: 80), but to the south single burials were the rule (McBryde 1974).

> Apparently, the distribution of burials as located by archeologists is not simply a matter of 'luck' or preservation: the spacial distribution and density of the dead is plausibly linked to the operation of that society's ecological system. (Saxe 1970: 234)

A very general predictive case can be made from the location of cemeteries as opposed to single burials. This follows Saxe directly in assuming that graveyards will be closely associated with restricted resources such as riverine systems in a dry environment or perhaps particular marine resources along the coast, stone deposits in stone-poor areas and labour intensive fish traps. The list should be long but not endless. Thus, one should not expect cemeteries to be associated with quarries in a region of abundant stone sources, but they might be found in regions where stone is rare.

Our regional distribution of cemeteries in the southeast is probably very accurate: we are not likely to find *any* away from rivers. Where they occur *on* rivers is presently under investigation in my own study (Darling River) and the National Parks and Wildlife Service (NSW) study headed by Jeannette Hope and Terri Bonhomme (Riverina). I doubt there are taphonomic reasons for any purported non-preservation in non-riverine environments. All those trudging surveys over the hinterland are recording an accurate reconstruction of mortuary practice.

I find it surprising that cemeteries have not been recorded on coastal regions (excepting perhaps the Coorong, which as I suggested for the Adelaide region is biologically part of the River Murray Corridor). It is not yet possible without more information to decide whether the proposed attendant social organisation was not in place (as suggested theoretically by Saxe and ethnographically by Goldstein (1976)) or whether cemetery behaviour was not appropriate or took a form that is not archaeologically visible.

Chronology

Reconstructing a chronology for these cemeteries is difficult. The extremely small number of dates makes a general overview tenuous and a detailed within-site chronology out of the question. In order to look at the development of burial grounds as a cultural system my analysis depends on subjective assessment of some of the sites as well as on the few radiocarbon dates.

First, the earliest dated cemeteries are Kow Swamp and by analogy Coobool Creek. Thus, we may be sure that by 13 kya, burial grounds and all they represent are in place in the Upper Murray. Any data earlier than this are exceedingly rare, but perhaps the early dates at Roonka (18 kya) and Snaggy Bend (10 kya) in advance of fully developed cemeteries might be taken to indicate that they did not arise much, if any earlier than 13 kya. None are known earlier than this time and, conversely, all earlier dates are associated with single burials.

Second, in each of these sites we are faced with an internal chronology, a multi-generational time depth. In the few cases with enough information, it appears that site usage increases with time: the 18 kya date at Roonka (unassociated with a burial) and the 10 kya date at Snaggy Bend are rare, isolated events stratigraphically remote from the rest. Most burials from these two sites should probably be assigned to the last 7 kya. At Roonka, Pretty (1977) suggested increasing usage within this time period. In fact, the earliest burial phase (Roonka II) is probably on a par with Kow Swamp in terms of density, while a tremendous increase occurs in the latter part of Roonka III after 4 kya. This would split Roonka in Figure 5c.2, producing a greater dichotomy between older (>7 kya) and later (<4 kya) cemeteries.

Coobool Creek, across the River Murray from Kow Swamp, is divided into two components: an early group dated by association with Kow Swamp to between 13 and 9 kya and a later undated group, perhaps from a different cemetery—we don't know. At any rate, the later sample is the larger one and this might be an indication of greater cemetery behaviour.

Third, a regional chronology can be based on a dichotomy between the early sites of Kow Swamp and Coobool Creek and the rest of the River Murray cemeteries clustered at between 7 and 2 kya. At face value, this suggests that cemeteries arise in the Upper Murray at the end of the Pleistocene and from here spread throughout the corridor to become common by 6 kya at the latest. The anomalous Broadbeach burial ground is established late by comparison.

Considering the evidence for Pleistocene occupation in the Moreton Bay region it is surprising that Broadbeach is not older: another time-lag?

Fourth is the relationship between date and density of burials (Fig. 5c.2). From their first development at *c.* 13 kya burial grounds undergo a tremendous increase in density: not only intra-site, but throughout the region. It is very tempting to view this as evidence of population increase and I would take both cemetery behaviour and increasing density to indicate just that. On the other hand, it is just as possible to imagine a

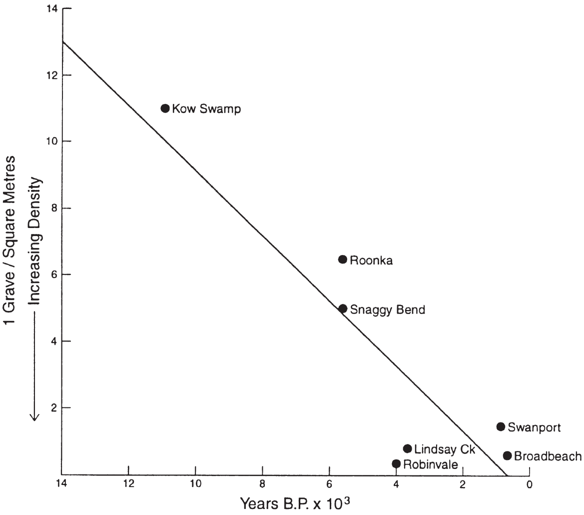

Figure 5c.2: Plot showing the increase in grave density through time. Note that density is calculated as grave per square metres, and so density increases in the direction of the arrow. The regression line is calculated from least squares and is of the form density = –1.0 + .001 (years). The regression coefficient, r = 0.90. Variation over time 'explains' 82% of the variation in burial density (that is, $r^2 = 0.82$).

static population that produces increasingly dense cemeteries through time for the trivial reason that more and more bodies end up there. Also, the development of localised corporate unilineal descent groups does not necessarily result from population increase (see Stockton 1983 on demographic adaptation; Lourandos 1983). While the analysis presented here cannot resolve the debate over the primacy of population increase vs intensification, my impression is that any population increase preceded cemetery behaviour. The cemeteries themselves arise as the symbols of a new age responding to more densely packed populations lining the River Murray corridor. Those populations are evolving a new order to cope with the increasing difficulties of resource exploitation, ownership and control.

Resources, population and social organisation: outlining the model

Saxe relates formally circumscribed areas for the exclusive disposal of the dead to an environment containing crucial but restricted resources. Access to these resources is controlled by corporate groups which legitimise their rights by means of lineal descent from the dead, their ancestors. His model, which appears to be applicable world-wide, has three generalised (or undefined) variables and one specific. These are resources, population, social organisation and cemeteries. I will summarise each of these.

1. The Murray Valley Corridor is a rich and relatively stable environment offering permanent water and a variety of resources throughout at least nine months of the year. In the Central Murray area (Fig. 5c.1), the rich river resources bordered for huge distances on either side by arid country not only permit, but enforce, a strong river focus. In the upper reaches of the Murray these resources become less restricted as they are more evenly distributed over the countryside.

Radcliffe Brown describes the narrow strip of country on each side of the Murray River, from its mouth to a point some distance above the Murrumbidgee Junction, as ‘the most favoured area of Australia, for a people obtaining their sustenance as the Australian aborigines do’ (Brown 1918: 231). Allen’s excellent work on the Darling and Willandra regions to the north of the River Murray (especially 1980, 1983, 1986) describes resource availability. The applicability of this reconstruction to the Murray is based on the historical documents used by Allen (see references in the above and 1972), which often are observations of the River Murray itself.

Allen’s work details many of the food resources available in the region, with the normal archaeological emphasis on animals rather than plants. The two contrasting environments are a lush river supporting fish, yabbies, mussels and waterfowl, with a margin of river red gum and black box trees that would house possums, other small mammals and goannas. Vegetable foods, for which the evidence is minimal, include grass seed, nardoo and small tubers: there was certainly much more. Set against this narrow corridor is the arid scrubland of mallee or perhaps saltbush. This latter environment is in fact highly productive, with a large range of land mammals from 40 kg kangaroos down to rabbit-sized hare wallabies, bandicoots and rat kangaroos. Ranging alongside these are emu with their eggs, lizards and presumably a number of tuberous plants and other vegetable sources now mostly vanished.

This mallee or scrubland away from the river is, however, unpredictable. The uncertainty of resources is underscored by an even more arid phase between 5500 and 2000 years ago (Ross 1985). Resources are not restricted, but widely scattered over the land. They are not critical because there are episodic periods of abundance and scarcity.

It is also worthwhile to remind ourselves that humans are tropical animals and one of the few mammals in this area not adapted biologically to semi-arid conditions. People are tied to water and notwithstanding the potential sources in the surrounding plains (for instance mallee roots, Cairns 1859), it is to the permanence of the rivers that they must retreat for much of the summer and in times of drought.

2. The second generalised variable in Saxe’s model is population and it is implicit. Resources are critical and/or restricted only in the context of the numbers of people using them. Furthermore, the differential in number between riverine groups and those in more arid or otherwise less favourable areas is important. One of the first anthropologists to discuss population size was Radcliffe Brown and he describes the strip of country along the River Murray as having been ‘the most densely populated part of Australia before the days of white settlement’ (Brown 1918: 231). For the Lower Murray he estimates a maximum population density of two to the square mile, with ‘hordes’ of an average 60 people ranging over an ‘estate’ of not more than 50 square miles (but see Peterson 1986 on horde, clan and band definitions).

Ecological evidence presented by Birdsell (1953) demonstrates the relationship between rainfall, size of group area and size of group. The expected size of group area decreases with increasing rainfall. He further detailed the Murray-Darling tribes, showing that their tribal-area ratio was greater than expected and that this density (and therefore population) was an order of magnitude greater than arid, non-riverine groups; in fact 20 to 40 times greater (1953: 184–8).

Biological evidence favours large populations for riverine groups. Webb's useful study of disease patterns (1984), in concert with demographic analysis (Butlin 1983), points directly to large, densely packed populations. Estimates of riverine population have skyrocketed to the point that the rivers appear to have been saturated, with hinterland populations orders of magnitude smaller. Differentiation and gene flow networks are also only interpretable in this scenario, with much smaller populations away from the rivers (Pardoe 1984).

Other archaeological studies of stone tools and faunal remains as economic indicators have focused more on the question of population increase rather than population size (Ross 1985). In fact, Ross has stated that from such material evidence 'reconstructions of absolute prehistoric population size are virtually impossible' (1985: 81). This assessement has also been very general in that specific cases such as southwestern Victoria (Lourandos 1983), northwestern Victoria (Ross 1981) and the Darling River (Allen 1972) have been generalised to an undifferentiated 'southeast'. At this point we might benefit from more regional perspective; as is apparent in this paper, I do not believe any archaeological knowledge we may have of the River Murray Corridor can be generalised to the southeast. This corridor forms a distinct archaeological region and a cultural area. Although the evidence all points to an extremely dense population for the River Murray Corridor, and I relate cemetery behaviour to population pressure on resources, the proposed chronology outlined above would suggest that in this area the impact of population stress began to occur as early as the late Pleistocene.

It is of interest that the mythology and oral history of River Murray groups contain many references to population pressure and to people moving down the river corridor (see Tindale's (1974) discussion of Crow and Eaglehawk legends). We could probably also learn from a legend recounted by the Yaralde tribe at the mouth of the River Murray:

> The origin of this name [Yaralde] and that of the neighbouring tribe (the Tanganalun) is explained by a legend. The ancestors of the two tribes are believed to have come down the Murray River. When the Yaralde reached the present country of the tribe they came upon the sea and they said, in their dialect, *Yarawalangan?*, 'Where shall we go now?' (Brown 1918: 226)

3. So far, two of the variables in the Saxe hypothesis, abundant resources and population pressure, have been shown to exist in the study area. The third, the existence of cemeteries, has been the major focus of this paper. If then the Saxe model is valid we should expect the cultural stimulus for cemetery behaviour to derive from corporate groups who legitimise their territorial rights by means of lineal descent from the dead.

4. Local group organisation and the relationship of groups to territory has been the major focus of anthropological studies since contact. Long before land rights legislation in the 1970s made such studies imperative, the relationship between social organisation and land use and ownership had been firmly established, with religious belief and ritual serving to reinforce this relationship.

An Australia-wide modelling of this relationship between social groups and territory has always been problematic, particularly because of the variable impact of contact throughout the continent. Broad generalisations have emerged however, and these are addressed by Peterson in his recent monograph on territorial organisation (1986).

A relationship between environment and form of social organisation is clear. In resource-rich areas such as coastal Arnhem Land, tropical woodlands and the south-east, where the environment can support a significantly larger population density than the arid zones, groups are larger, their territories are smaller and more clearly defined, a greater degree of boundary maintenance is practised and the groups are corporate in nature.

Peterson's inclusion/exclusion model of social organisation is environmentally based. In arid zones where resources can be unpredictable and precarious, a form of local organisation which extends social and territorial networks over a wide area and facilitates easy movement across more fluid boundaries is crucial to survival.

In rich areas, and particularly those which abut poor environments, social organisation tends to exclusivity. In these areas are found those indicators of corporate social organisation: unilineality (predominantly patrilineal); localised exogamous groups who are more likely to reside within clan territory and to have clearly defined boundaries and elaborate procedures for crossing them; shorter marriage distances and more prescribed patterns of exchanging marriage partners between neighbouring clans; and other structures most commonly associated with corporate groups such as patriarchy, polygyny, hereditary transference of power, intergroup competition and warfare.

The River Murray Corridor, already shown to be a rich environment, was inhabited by a large number of groups. Pre-contact social organisation can only be hinted at from a small number of specific studies.

The social organisation of the Yaralde and adjacent tribes of the Lower River Murray were described by Radcliffe Brown (1918: 225–41). These groups were comprised of exogamous patrilineal local clans, each permanently occupying a certain area of country over which it had exclusive rights of ownership: 'No one might hunt over the country of a horde other than his own, or fish in its waters, except on the invitation or with the permission of the owners' (p. 228).

Elsewhere (1930: 451–4), Radcliffe Brown relates the unique kinship system in the area to the solidarity and autonomy of the local clans. Although the system ensures solidarity with six related clans, 'the absence of moieties and sections means that the relation in which a man stands to distant clans, or to those with which he has no near genealogical connection, is indeterminate' (p. 454). Thus the kinship system promotes exclusivity. These observations stand in direct opposition to what has been seen as a generalised Australian Aboriginal situation, which is imported from the Centre and north, where social organisation has been extensively studied. Unlike the Centre, people here are *not* related to everyone else through a moiety or section system. They maintained very close ties with the clans of father, mother, wife and so on, but the rest were strangers. Furthermore, there are other implications, both social and biological, stemming from the close binding of clans and clan exogamy. From an individual viewpoint, relatives would be in a small cluster of clans. From a lineage perspective, clans would be linked into clusters through descent.

Greater population density in the area and the larger number of individuals in each clan are suggested by Radcliffe Brown to relate to these special characteristics of Yaralde kinship. Drawing on ethnographic evidence from Radcliffe Brown and Howitt (1904), Elkin (1938) defines a small area on both sides of the Lower Murray from about the New South Wales border to its mouth which is characterised by 'an absence of moieties and sections, and the operation of strongly accented exogamous localized patrilineal clans' (p. 422).

Berndt and Berndt (1985: 33) contrast local group territories in central Australia where game and vegetable foods were not concentrated or plentiful with those in the Lower Murray: 'On the Lower River Murray . . . such areas were smaller, but richer, and boundaries consequently firmer'.

Boundary maintenance was an important aspect of Murray social behaviour. It is significant that many tribal names along the river comprise the reduplicated terms for 'no', as noted by both Tindale (1974: 131) and Radcliffe Brown (1930). Tindale also noted that river people strictly controlled access by people from the mallee scrubland to the river, allowing them to drink only at specific places (p. 134). Warfare is another, perhaps more extreme form of boundary maintenance and is recorded by early explorers in meetings with river people and also by Le Souef (see Tindale 1974: plates 43 and 44).

There existed among the Jaraldi (Yaralde) of the Lower Murray an elaborately formalised system of 'courts' which operated at both the patriclan and interclan level (Berndt 1965: 177–81). Berndt sees this as one of the few examples of centralised authority in Aboriginal Australia.

Skeletal biology: assessing the model

The Saxe hypothesis has been shown to provide us with both a useful tool for approaching the study of cemeteries and a model for a regional prehistory. A relationship between population, resources, social organisation and cemeteries exists in the River Murray corridor. However, it is the skeletal biology of the people buried in the cemeteries which can provide further information for assessing the validity of the regional model. Genetic information encoded in the bones of these prehistoric inhabitants can tell us something more about the prehistory of this region.

I have already referred to Webb's study of palaeopathology in the southeast (1984) which shows that population density and strain on resources on the River Murray is evidenced in the skeletal record by such indicators as Harris lines, enamel defects, trauma and arthritis. The potential of physical anthropology to elucidate prehistoric social organisation is also considerable. We need not accept that this 'invisible baggage' (Bowdler 1981) is outside the province of prehistory.

A few studies relating genetic variation and social organisation have been made within Australia and are relevant to the theme of this paper. White (1979) studied this relationship in contemporary Arnhem Land, emphasising

> the relationship between genetic diversity and ecologic features in Aboriginal Australia. [I]nterand intratribal marriage patterns appear to be strongly associated with habitat type at both the tribal and local group level. These patterns in turn, are governed by demographic factors such as population density and tribal size, both of which are also influenced by the physical environment. In Arnhem Land for instance, coastal territories provide a more favorable food economy for hunter-gatherers, with considerable resource diversity in space and stability through time. These coastal tribes tend to be smaller, with higher population densities and shorter marriage distance than those tribes living inland, particularly those in the arid interior. (1979: 451)

Genetic heterogeneity, or relatively greater differences between groups compared to variation within them, is related to corporate social organisation, particularly localised patrilineal clans, polygyny and shortened marriage distances. It is also related to an environmental substrate of relatively richer coastal resources.

My own study (Pardoe 1984) of genetically based variation in prehistoric skeletal morphology throughout Aboriginal Australia shows southeastern populations to be distinguishable from those in the rest of the country. Furthermore, the River Murray samples were linked by an absolute similarity and by clinal variation running the length of the river and crosscutting other regional similarities. Even so, the range of variation is considerable and paradoxically, the River Murray samples exhibit some of the greatest diversity in the continent while still maintaining a regional similarity. This range of variation is explicable by reference to a model of corporate social organisation as discussed above and a particular gene flow pattern based on territorial organisation.

To the extent that these groups bury their own in cemeteries, the individuals should be closely related. That is, lineal ties through time and higher inbreeding coefficients in the larger endogamous group should lead to a greater relatedness for contemporaneous individuals and for the lineage. At this point it is probably not possible to discern matrilineal versus patrilineal descent archaeologically (but see Lane and Sublett 1972) and I will assume that the manner of reckoning descent is immaterial to this investigation.

The problem of identifying the group of interest as a biological unit is well known in population biology (Harpending 1974, Cadien *et al.* 1976, Fix 1979). Our samples of populations (that is, the skeletal remains) may reflect lineages within populations rather than the populations themselves and therefore could be biased. The problem is compounded by the knowledge that we are dealing with small groups. If these cemetery samples are indeed lineages, or subsets of related people within the population, then we should expect different patterns of variation from samples constructed in other ways. In concrete terms, samples such as Swanport, Coobool or any of the defined cemetery groups should be considered as large samples of extremely finite and well-defined populations. On the other hand, samples made up of aggregations of individuals from a large area may be contrasted as smaller samples of larger, less well-defined populations.

Variation of any genetically based features should be less *within* and greater *between* these cemetery samples. Furthermore, samples derived from individual burials should be more variable than those samples that might be lineages. Sinclair (1977) noted that the River Murray cranial samples exhibited greater heterogeneity, or between group variation, than Victorian ones and I have demonstrated the great variability along the River Murray (Pardoe 1984). In fact, this region shows some of the highest diversity in the continent. Unfortunately, that study did not compare within-group variability. Thorne (1975, 1977) found representatives of older populations to be far different from more recent groups, but did not address group variability. Brown (1982) demonstrated that within-group variation was as large in the Pleistocene Coobool Creek sample as in more recent groups. However, these too were samples from cemeteries. As will be amplified below, I would suggest that Kow Swamp and Coobool Creek are different because they are representatives of distinct descent groups. The differentiation results from *in situ* development.

Although the complexities of morphological differentiation are great, the pattern seen on the River Murray would be explicable within the cultural framework proposed here. Although I have suggested elsewhere that differentiation could be the result of unilineal gene flow and the attendant possibilities for extreme differentiation, it is also possible that such differences are exacerbated by a cultural system favouring endogamy[3] and lineally inherited rights to resources along the River Murray. Less marriage with groups in arid areas or away from the river reinforces the notion of exclusivity concerning river rights. At the same time, heterogeneity along the river can be explained in terms of increased reproductive isolation and shorter marriage distance which was seen by Sinclair and White (1984) as associated with resource rich habitats.

The hazy line between culture and biology would seem to have evaporated in this instance where a cultural hypothesis of group endogamy based on mortuary practices can be tested by operationalising the genetic corollaries. In this case it is the assessemt of within and between group variation of the people who invested their river rights in their ancestors' graves.

Evidence from skeletal biology has been used recently in an archaeological analysis of social evolution and I will discuss this in some detail. Lourandos (1983) offers us a model for southwestern Victoria, which he extends to the wider southeast. He proposes intensification of economic and settlement patterns throughout the late Holocene which derive from increasingly complex and competitive social networks.

Lourandos tests his models using a range of data drawn from ethnohistory, archaeology and physical anthropology. I have used a similar range of data to examine a hypothesis which also relates economic, demographic and social patterns of prehistoric life. While I would caution against the application of a regional study to the whole southeast, let alone (as Lourandos does) to the whole of Australia, my results for the Murray River Corridor fit into an overall picture of high population density, increasing pressure on resources and complex corporate social organisation. There is nothing in my data, however, which would support the primacy of the role of social relations in Holocene changes, nor indeed do I consider it useful to attempt to isolate one variable from the complex interaction of humans with their environment. Furthermore, the chronology which I have suggested for cemetery use and its social and economic role would suggest that the patterns of behaviour which Lourandos attributes to the late Holocene are to be found as early as the late Pleistocene in this region.

Lourandos' use of data from physical anthropology to support his model does require comment, particularly since he makes reference to information from River Murray skeletal samples. Drawing on Thorne (1976, 1977), Lourandos discusses a dichotomy between Pleistocene populations in the southeast which 'appear to have had a wide range of morphological variation' and Holocene populations which 'were more homogeneous overall, showing a narrower range of variation' (1983: 91). Thus skeletal information is used to reinforce his theory of evolving social relations. Further, by indentifying the first half of the Holocene as the period when morphological homogeneity was developing (in the Murray corridor), Lourandos uses skeletal evidence to support his view of the primacy of social relations over economic and environmental factors in the explanation of cultural change, since the economic aspects did not appear until later in the Holocene (in southwestern Victoria).

Evidence for Pleistocene variation in Australia is based on a dichotomy between the Kow Swamp–Coobool Creek complex and (two skulls from) Mungo. Recent non-metric analysis has shown the Coobool Creek sample to fit within the ranges of morphological variation in the southeast, and more specifically in the Upper Murray. While, as I have stated above, the southeast can be distinguished morphologically from the rest of the country, it is highly misleading to conflate this into southeastern morphological homogeneity. River Murray populations exhibit a very high range of variation throughout the Holocene and my own non-metric study shows the Pleistocene Coobool Creek sample to fit with the other southeastern material despite the considerable time difference.

This new biological evidence does not contradict Lourandos' theories on the complexity of social relations in the southeast. It does encourage a rather different interpretation of how these social relations work. Lourandos (1983: 91) has suggested that 'evolving social relations' caused widening of marriage patterns and biological 'homogenisation' of later Holocene Aboriginal populations. While I fully agree that social organisation has a profound impact on biological structure of populations and vice versa, it should be evident that 'evolving social relations'—intensification, settlement, population increase—all part of Lourandos' model, would decrease marriage and migration distances. He wrongly assumes that 'evolving social relations in Australia included the widening of marriage systems leading to multiple marriages with partners drawn from far flung regions' (p. 91). Therefore he errs in assuming 'that early (Pleistocene) marriage systems were more restricted with increasingly widening networks developing in the later (Holocene) stages. In the former case gene flow would be restricted and in the latter increased' (p. 91). In fact it is the reverse that is most likely the case: marriage systems were increasingly restricted. Exclusivity and group (tribal) endogamy would restrict gene flow and promote differentiation.

I have suggested that social relations in the Murray Corridor were characterised by exclusion rather than by inclusion, that this process of exclusion is related to resource control, that gene flow becomes restricted and leads to differentiated populations. This is supported by what we know of Murray River local group organisation and kinship systems at contact. The elaboration of social organisation is related to boundary maintenance, to formalised marriage exchange patterns, to methods of regulating inter-group tensions and competition, and to maintaining the integrity and strength of the localised lineages or clans. Cemeteries are an important expression of this.

If we accept this model, then the dichotomy between Pleistocene and Holocene southeastern populations disappears. Instead, the known Pleistocene population (Mungo) should be seen as a generalised Aboriginal precursor of a system which became increasingly established in the Murray corridor throughout the Holocene.

The Kow Swamp–Coobool Creek complex is the first of these corporate groups and is highly differentiated. The people of Kow Swamp and Coobool Creek were culturally, biologically and visually distinct. They heightened, or accentuated, this distinctiveness by pronouncing the sweep of the forehead (Brown 1981, but see also Thorne 1975). Later groups along the River Murray continue to exhibit extreme differentiation (Sinclair 1977, Pardoe 1984).

Conclusions

Cemeteries are distinct entities in Aboriginal prehistory. Their presence is defined by numbers, density, boundedness of the site and its relation to living areas. Numbers are in terms of tens or hundreds. Density should be greater than one grave/10 m^2 in order to demonstrate contiguity and the attendant probability that knowledge of other graves in the area is likely to have been an important if not the main factor of burial location. The sites are highly bounded such that density decreases markedly at the edges. This would support the idea of systematic burial. Camp sites, middens, hearths and stone tool working areas do not figure heavily in these graveyards. By contrast, areas like Willandra

Lakes, the Lower Anabranch Lakes or the Menindee Lakes show increased numbers of burials in areas of greater occupation: single burials are single unrelated events occurring more frequently in areas of more frequent habitation.

Our current knowledge of cemeteries is sketchy, but the distribution is a main cultural feature of the River Murray Corridor which spills over to the Adelaide region (in keeping with biological connections; see Pardoe 1984: 167, 187). The definition of archaeological regions or 'cultural adaptive areas' has been made by Witter (1984) for the southeast. He includes the River Murray Corridor, divided into upper and lower segments, as one such sample. I believe the present study supports this regional interpretation.

Other cemeteries probably exist in the Riverina, such as perhaps Koonadan near Leeton and up in the Macquarie Marshes region where anecdotal information is suggestive (Gresser 1966). Cemeteries do not seem to have been a feature of the Darling River system: Lakes Tandou and Menindee do not qualify as such. This raises the possibility that the Murray-Darling junction, represented by Snaggy Bend and Tucker's Creek, was not within the Paakantji (Barkindji) sphere of influence.

The particular trait of establishing graveyards is first known to us from the terminal Pleistocene, perhaps 13 000 years ago. Although the data necessary to establish this will be scarce, it is perhaps instructive to note that Roonka reaches cemetery status over 10 kya after its first occupation. I think these graveyards arose, probably in the Upper Murray, by 10 to 13 kya and spread subsequently throughout the corridor, possibly around 6 to 7 kya, to become well defined and numerous in the last 4 kya (Fig. 5c.2).

The social organisation of the River Murray people, perhaps as far back as 13 kya, was clearly designed at least partly around some form of corporate descent group—an established group reckoning kinship and membership through common ancestry, and one which acted as an individual in social and political affairs. Whether these are lineages or clans is beyond this analysis, but in the terms of Saxe we should probably view these groups as territorially based and their burial grounds as one of the symbols validating corporate ownership of that territory.

In contrast, whatever the land-based organisation of coastal groups to the south and east and of arid zone groups to the north and west, they did not use the cemetery as evidence of group affiliation and resource control. In the arid zone, it is unlikely that efficient use of resources can be made by restricting them, while on the coast resources may not have been scarce. On the other hand, different forms of organisation may have precluded the establishment of the localised unilineal descent group and its symbol.

This very economic view of social behaviour is only attractive because of our restrictions in the archaeological record. When we ask why cemeteries exist it may not be possible to answer in structural or religious terms, even if we include these same terms in our reconstuction of prehistoric society/social structure. These wider issues will be taken up elsewhere.

Both the biological evidence and the limited ethnographic and historical evidence support the concept of localised corporate groups which define themselves by the process of exclusivity. I have suggested that this process is revelant to an analysis of the 'robusticity debate': Kow Swamp is not an archaic Pleistocene relict population, but the forerunner of a modern, socially complex, dynamic system that promotes endogamy, one-dimensional gene flow and enhanced diversity.

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5d The childhood of William Lanne: contact archaeology and Aboriginality in Tasmania

Tim Murray

Introduction

In recent years, perhaps as a consequence of the self-reflective turn in archaeology and concern for the social and political implications of their practice, archaeologists have been more outspoken about their own emotions. They have never been completely silent. Nilsson's (1868) feelings of revulsion towards Lapps were hardly hidden, Worsaae's (1849) nationalistic fervour, Childe's (1933; 1944) horror of Nazi archaeology and Grahame Clark's (1936; 1939) rejection of Childe's Soviet sympathies were real enough. But a post-processual archaeology seems to claim that our emotions (our responses as people?) can be valuable in deconstructing archaeology and archaeological knowledge.

Relationships between archaeologists and post-colonial indigenous peoples have also been fraught with emotion in recent decades. A large literature surveys the topic, usually reduced (ineffectively as it transpires) to a 'debate' about 'who owns the past' (see e.g. McBryde 1985, 1992; Murray 1992, 1993). Competition between indigenous peoples and archaeologists over the control of heritage does still occur (given that issues of identity arise, it is hardly surprising); nevertheless control over sites and artefacts is only part of the story.

Matters of interpretation are much more difficult. Banning unpopular accounts, or granting access only to those archaeologists whose interpretations are 'correct', courts the charge of censorship and, perhaps more seriously, can lead to a loss of moral authority. The lesson that the past does not belong to any single group, hard enough for archaeologists to learn, is all the more difficult for those who consider that their very identity might be threatened by disputes over interpretation—disputes over which they may have very little control. Australian Aboriginal people now play in the game of interpretation, and can enhance their participation by producing interpretations of Aboriginal history and archaeology which meet their own needs (see e.g. Tasmanian Aboriginal Centre Inc. 1991; McKellar 1984; Benterrak *et al.* 1984; Shaw 1981). However, the fact remains that they *share* the field of interpretation with others who may well have different viewpoints and agendas (e.g. Mulvaney & White 1987; Morphy & Morphy 1985).

In this context we might argue that polyvocality is a sign of relevance and significance, and that to repress it would be to reimpose the artificial consensus of positivism and empiricism. However, it is quite another thing to comprehend the emotional consequences of a relativism of interpretation, particularly for those who have rather more at stake than an expansion of contemporary social theory or fun games with signifiers. Of course, part of the power and attraction of polyvocality is connected to the sense of instability of concepts and categories it can bring, and instability that does much to reveal the consequences of previous consensus accounts as limiting or restricting our imaginations as archaeologists. However, instability has its downside, especially for people who may be the victims of clear (and unresolved) inequalities of power and resources. For them polyvocality may threaten the frameworks which underwrite identity and self-determination.

There is no formula solution for these problems beyond a commitment among the disputant parties to communicate, and for them to accept the discipline communication implies (Murray 1993). This discipline, with its emphasis on self-reflection, the building of theory and the exposure of the 'hidden' criteria which allow us to determine the plausibility of archaeological knowledge claims, becomes all the more important in the field of contact archaeology. Here the conceptual field is crowded, and the stakes higher.

In Australia the overcrowded field is a consequence of a shared history, and the arguments turn on matters of identity. Over the last two decades the shared history has dramatically changed its form and content, and identity has proved chimerical. Both indigenes and invaders have a keen interest in their respective identities. After 200 years of searching, neither have proved to be singular or immutable. Aboriginal people seek to redefine themselves both in terms of their deeper pasts, as well as their more immediate pasts and presents (see e.g. Beckett 1988; Keefe 1988; Palmer 1987). Given the historical experience of dispossession, and of living 'with the white people' (Reynolds 1990), many Australian Aboriginal people do not lead traditional lives. Many have European, Melanesian or Asian blood relatives. Yet all are considered to be Aboriginal people, with Aboriginality defined largely in terms of a person's identification with place or community and their acceptance by that community.

For the non-Aboriginal population, creating (or even locating) a sense of the Australian identity has been something of a field sport since the nineteenth century. Forays into painting, song, literature, landscape and particularly Test Match cricket are frequently undertaken to locate the state of 'Australianness', even after massive post-war migration has broadened the base of non-Aboriginal Australia from predominantly Anglo-Celtic towards a highly complex multicultural mix of people drawn everywhere from Afghanistan to Zimbabwe.

In recent years support for a Republic of Australia, which would involve cutting ties with the British monarchy, has gradually overcome the view (which was widespread until the late 1960s) that British institutions were required to ensure the proper functioning of new societies which were thought to have very little history and even less intellectual sophistication. An important part of this package was the notion that colonial science should always be derivative from the metropolitan, and that the really significant questions, perspectives (sometimes even data) either came from the metropolitan or were best pursued there.

Only lately have Australians begun to question both the inherent superiority of metropolitan institutions and their cultural products. The fact that Aboriginal and non-Aboriginal people have begun to understand that in a very real sense they define each other is both a product and a cause of that questioning. Such great changes to the cultural landscape of Australia can be frightening as well as liberating. Assuming responsibility for Australia's past as well as its future has meant that both segments of the population are feeling their way into their new roles here, still defining their emotional responses to a shared past which has rarely risen above indifference and domination, but which frequently has been far worse. Information about this shared past has, until recently, been lacking.

A decade ago Henry Reynolds in *The other side of the frontier* (1982) began to change white perceptions of the Australian frontier, particularly of the complex role played by Aboriginal people in the European colonisation of their country. Using archaeology, ethnography and oral and written historical testimony, Reynolds showed how dangerous it was to generalise about the nature of the nineteenth-century Australian frontier, giving a tremendous fillip to contact archaeology as well as to contact history. In the 1961 conference which laid the ground rules for a broad program of research into Australian Aboriginal life (Stanner & Shiels 1963), no priority was given to contact archaeology as a field of research; there was not even a mention of the need to write Aboriginal history. We now more clearly understand that Aboriginal history has been the hidden history of Australia, and the primary task of the contact archaeologist and historian of Aboriginal Australia is to uncover it. Frequently this information has the capacity to cause great distress to both Aboriginal and non-Aboriginal people; it also poses deep moral questions, such as finding a workable legal basis for recognising the prior ownership of Australia by Aboriginal people.

These are emotional matters for both the producers and the consumers of archaeological knowledge. The purpose of this paper is to exemplify the process whereby the discovery of Aboriginal history and contact archaeology has helped foster a climate where such new frameworks might be built. The example is drawn from the contact archaeology of the northwest Tasmanian frontier and recent research on the Van Diemen's Land Company (VDL).

The VDL Company (1825–50)

The Van Diemen's Land Company was (with the Australian Agricultural Company) one of the two great joint-stock companies to undertake private colonisations in Australia during the nineteenth century. Formed in 1825 in London with a paid-up capital of £1 000 000, the VDL had the primary purpose of producing fine wool for export to the London market. The motivations for the investment are complex, and the machinations undertaken by the Court of Directors in promoting the scheme and establishing the Company are described in detail elsewhere (Meston 1958; Murray 1988; Le Couteur 1978; Stokes 1963). In 1825 the VDL was granted 250 000 acres in northwest Tasmania, none of which had been traversed by European explorers. It was not even known whether the land would be suitable for the Company's purpose. Led by its Chief Tasmanian Agent, the redoubtable Edward Curr, large sums of money were expended in infrastructure development (from making roads to laying out towns and ports), importing stock and clearing primary temperate rainforest. By 1851 the Company, effectively bust, had largely ceased its pastoral and agricultural operations. Again, for reasons too complex to be related here, it has survived to the present day—albeit in a dramatically altered form (see Murray 1988; Stokes 1963). Yet during this quarter-century the European landscape of northwest Tasmania, and the economy of the region, was created by VDL policy. The VDL was also the primary agency for the destruction of traditional Aboriginal society in the northwest.

Initially archaeological research on the VDL Company focused first on the development of managerial strategies among transnational entities, which came to be a hallmark of the industrial age. The VDL Company, with its detailed reporting and complex (perhaps cumbersome) management, represents one (unsuccessful) attempt to control activities taking place very far away, within the confines of mid-nineteenth-century transport and communication. In this case managerial decision-making was on a six-month time-lag, and there are significant implications flowing from this nearvacuum of decision and response *vis-à-vis* the development of corporations with a shorter cycle of decision-making.

The second focus was on the material consequences of these VDL policies. The VDL stands as an excellent example of a private colonisation in Australia—one of a very few world-wide which has survived into the present with a complex interplay of state and market that had a tremendous impact on the human geography of northwest Tasmania in the 'making of the northwest Tasmanian landscape'. As the VDL was the primary agent of European colonisation in the northwest of Tasmania, the imprint of the aspirations of its directors and managers is found in the names of towns, the size of land-holdings, the designs of farm houses, the placement of roads and ports, even in the bloodlines of local blood stock.

In the first phases of research Aboriginal history and contact archaeology were seen as being peripheral to the larger task of integrating policy, company ideology, landscape and material culture into a convincing account of the first 25 years of VDL operations. Historical research (e.g. Plomley 1966; Ryan 1981) demonstrated that clashes had occurred between the VDL servants and the local Aboriginal population, but in the absence of detailed investigations of the VDL archive by Lennox (1990) and subsequently by myself, there seemed to be little that would survive archaeologically of this period of conflict. Indeed, I understood Burghley as being primarily a European site, and thereby (probably unconsciously) excluded an Aboriginal dimension. Again unconsciously, I was subscribing to the view that European settlement had wiped the slate of history clean; it had created the country anew. Thus initial investigations at Burghley conformed to a model of doing the archaeology of European Australia. Aboriginal involvement in this research was considered to be necessary only if unambiguous evidence of Aboriginal activity was found on the site, and the historical research indicated that an Aboriginal occupation of Burghley was highly unlikely.

Excavation of the site has refocused research into the VDL away from a primary concern with ideology and landscape into a broader investigation of the consequences of dispossession of the local Aboriginal people. These consequences extend beyond the murders and deportations, which were a central feature of the frontier experience in northwest Tasmania. My concern is to understand how the factor of conflict, between white and black and between convict and gaoler, master and servant, also shaped VDL ideology and policy. Excavation of Burghley has revealed more of the hidden history of the contact period in Tasmania and provided an opportunity for recognising the validity of a much broader Aboriginal interest in the archaeology of European places, particularly in places where there may be no direct evidence of Aboriginal activity but which were, nonetheless, founded and maintained in opposition to Aboriginal people.

Burghley and the Tasmanians 1827–42

Burghley, in the heart of the Surrey Hills area of the VDL grant (see Fig. 5d.1), was the first of the stock camps established in the area in 1827. Its abandonment by the Company around 1839 marked a change in land-use strategies, as the VDL attempted to reduce the level of its involvement in the Surrey Hills. The Surrey Hills were a natural focus for a research project examining institutional failure, because it was in the Hills that the VDL failed spectacularly, after the highest hopes.

The Surrey Hills were first explored by Henry Hellyer in 1827. An avid reader of German romantic poetry, he equated spectacular scenery with quality farm land (Meston 1958: 22–8). His reports of park-like uplands ringed by snow-capped mountains stirred the imagination of a VDL management needing a place to de-pasture the first consignment of sheep which were enduring the sea voyage from England.

It transpired that the park-like aspect was the result of intensive Aboriginal burning (the subject of a separate, but related research project: Murray *et al.* 1989), that the soils were too poor for agriculture, that the remnant nothofagus and schlerophyll forests were ever-ready to re-colonise the grasslands, that it snowed in summer and that the Aboriginal inhabitants of the Hills were in a mood to resist dispossession. Nearly all of the first consignment of sheep froze to death in the first year, a devastating blow to the fledgling enterprise. The loss in self-confidence was greater. Henry Hellyer's romantic sensibilities were never to recover. Although a superb surveyor and a highly

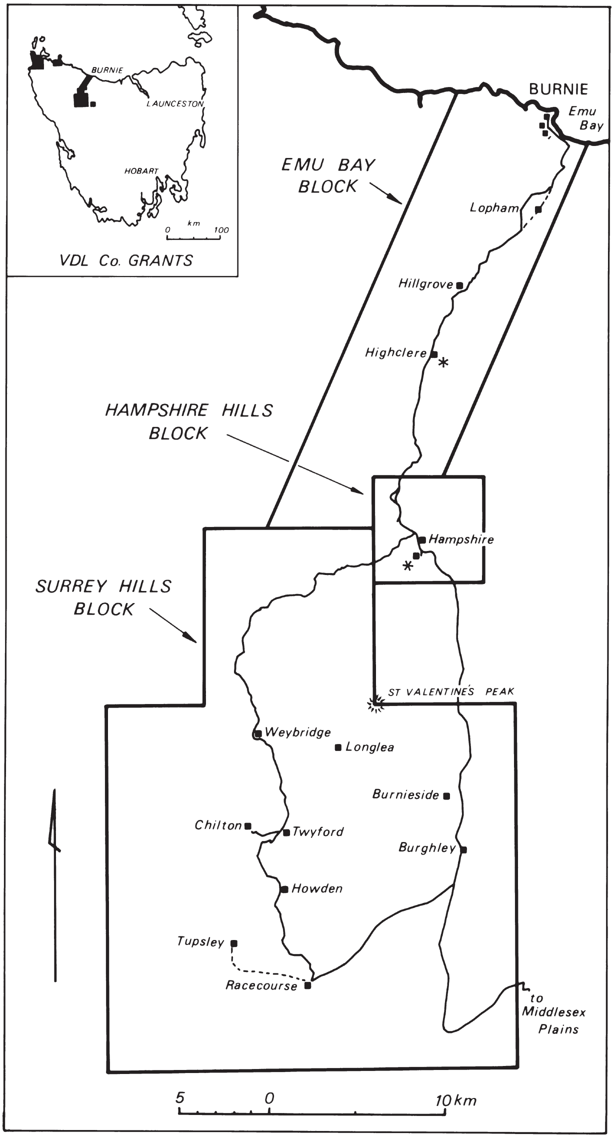

Figure 5d.1: The VDL Company holdings in the Hills.

competent leader, especially in the incredibly tough country of upland northwest Tasmania, he succumbed to melancholy and blew his head off some three years later (Rollins 1988).

Burghley was excavated between 1988 and 1990 (Fig. 5d.2). After two seasons the outlines of a house attached to a more substantial chimney-butt of mortared rock have been defined. Other important features are a midden, a drain and two cobbled areas. So far the stone and bone tools, bottle glass, ceramics and the faunal assemblage have

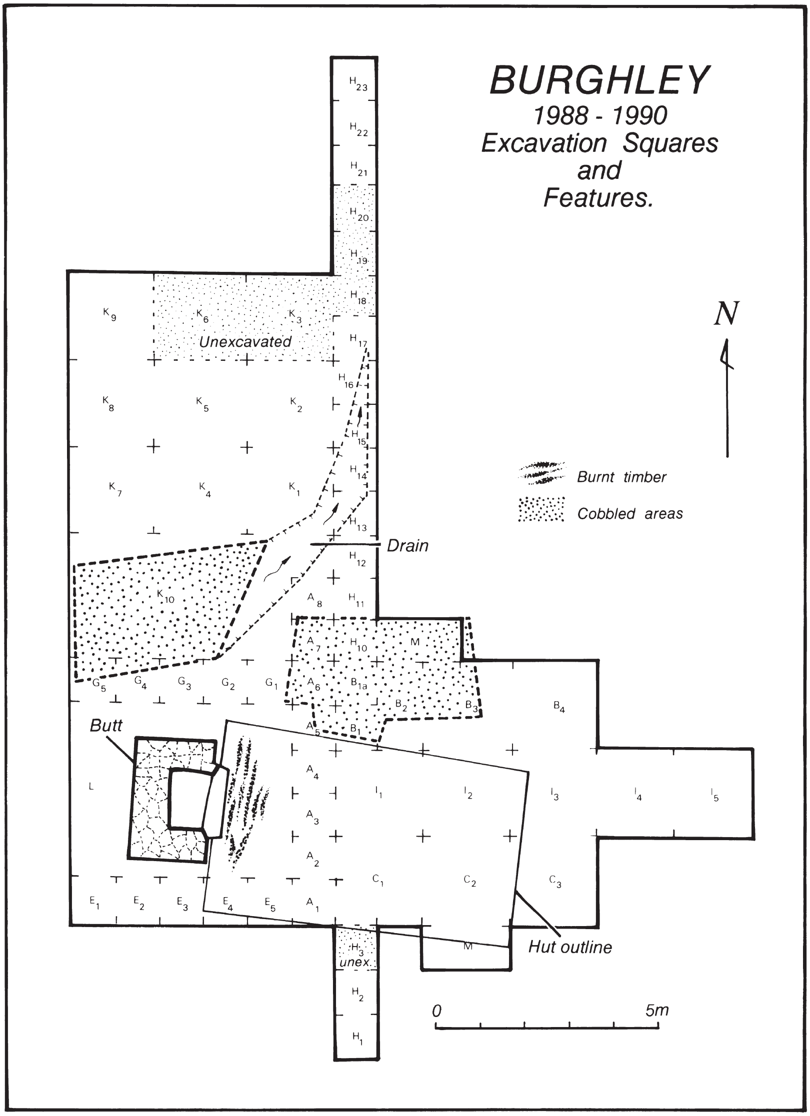

Figure 5d.2: Plan of the site of Burghley after excavation.

been closely examined. We have located some of the features which were very briefly outlined by George Augustus Robinson in the best description (we do not have an illustration), made on 24 August 1830 (Plomley 1966: 206):

> Burghleigh (*sic*) establishment consists of several wood buildings enclosed within a fence, and a ploughed paddock, and is one of the principal sheepwalks of the Company in the Surrey Hills.

Lieutenant Governor Arthur made some pithy observations in the *Hobart Town Courier* of 7 February 1829:

> In the evening of this day the party reached Burghley, the Company's stock hut at the Surry (*sic*) Hills. It lies in an open forest which has but little feed for sheep, and nothing to recommend it either in a picturesque or useful point of view.

Excavation has shown the house was destroyed by fire, and that the building was inhabited by Aboriginal people after its abandonment by the VDL.

The evidence for burning is the charred remains of the floor boards and beams which lie at the base of the chimney butt, surviving the conflagration because of the collapse of the chimney itself. Further evidence that a single fire destroyed the building comes from the charcoal hazes which are all that remains of the walls and the foundations of the house.

Aboriginal occupation after abandonment by the VDL is supported by three lines of evidence; each taken in isolation is circumstantial and unconvincing. Taken together, the argument for an Aboriginal reoccupation of Burghley *after* its abandonment by the VDL is compelling.

Stratigraphic evidence

Large numbers of Aboriginal stone artefacts were found on the site, in the main clustering around the house. At the base of the chimney butt a quartz manuport was wedged between the fallen chimney stones and the charred floorboards in association with a broken clay pipe, the remains of a tin can, a spoon and a musket flint (see Fig. 5d.3). After the first excavation season we felt that the stone tools were from an earlier (pre-European) occupation; the evidence from the second season clearly indicates that the bulk of the stone tools were deposited after the site was abandoned by the Company servants.

That said, a stone tool was recovered from a level 10 cm below the foundations of the house, along with an *in situ* deposit of charcoal, which has given an uncalibrated radiocarbon determination of 3370±90 BP (Beta-38 780). On this basis we can, at the very least, argue that Aboriginal people occupied the site prior to the contact phase of Tasmanian history. Further work in an associated research project (Cosgrove 1992; Hartzell 1992; Murray *et al.* 1989; Murray & Cosgrove 1991; Pickering 1991) confirms a complex settlement system linking shelter sites and open sites such as Burghley which may well have a long Holocene history.

A final, related item of stratigraphic evidence comes from the contents of the chimney itself. Its lower segments and the hearth itself were clogged with fine ash and animal bones (the bulk from indigenous taxa such as possum). This deposit, appearing to have been created by residents not cleaning out the hearth, may have led to the chimney catching fire and the house burning down.

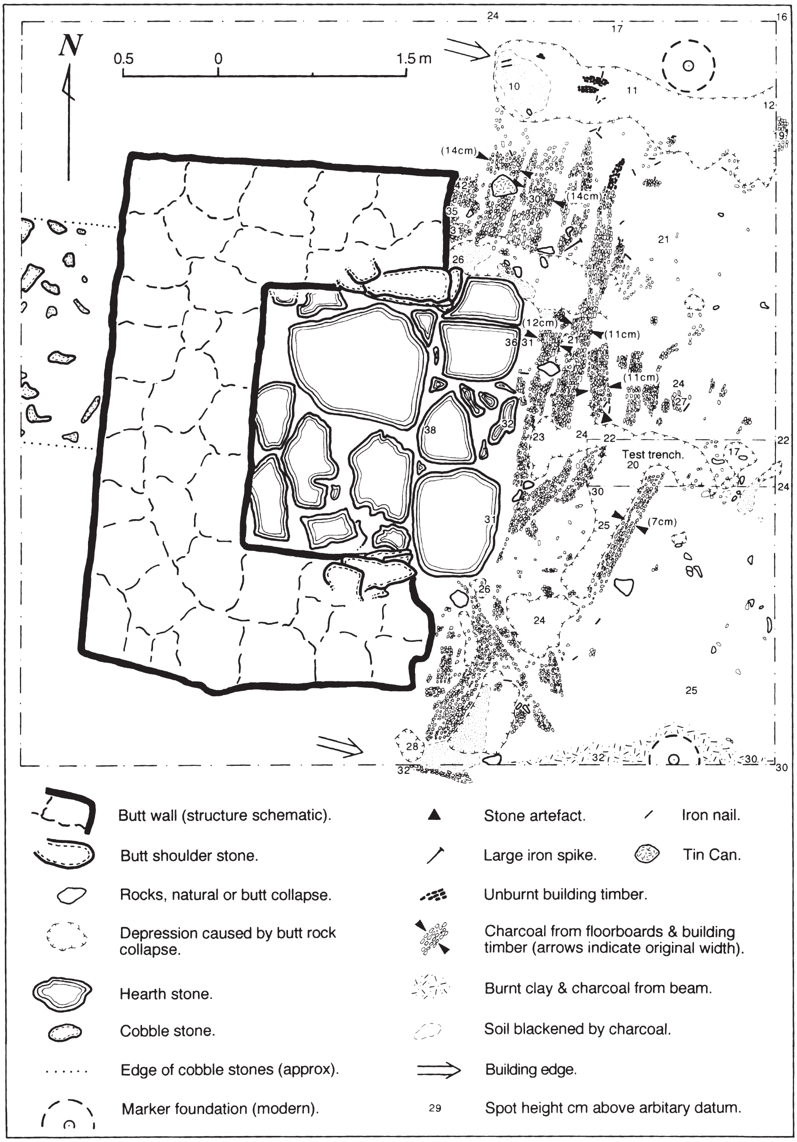

Figure 5d.3: Detailed plan of the excavated butt and associated artefacts.

Documentary evidence

Given the remarks of George Augustus Robinson (Plomley 1966) and Company servants that the Surrey Hills was a favoured hunting ground for the tribes of northwest Tasmania, a pre-contact site at Burghley was a possibility. But the evidence for the mixing of Aboriginal and European material culture was surprising. All the documentary evidence to hand indicates open warfare between the Aboriginal people and the Company servants shortly after Burghley was built that continued until the very last of the Aborigines were deported to Flinders Island in 1842. A shared occupation at Burghley is out of the question. Burghley was regularly attacked, and its occupants were driven off on more than one occasion. A good example is the events around 25 September 1828, when at half-past three in the morning Henry Hellyer reported (from Emu Bay) to the Company headquarters at Circular Head (further to the west on the coast) (VDL 23/2: 304):

> On my arrival here this afternoon September 24 two of the men from Burghleigh had reached this place in a most deplorable state namely Gunshannon and N. Russell and at this moment at half past five poor Murray and McGuffee literally covered with blood have just crawled from the same place. The natives attacked them yesterday afternoon 23rd at 3 o'clock and left one or two of them for dead and the poor lacerated have been ever since getting thus far.

Geoff Lennox (n.d.) has tabulated the violence between Company servants and Aboriginal people on the Company lands which occurred between 1827 and 1842. He has noted a sharp escalation of conflict in the Surrey Hills from 1828 until George Augustus Robinson's 'mission of conciliation' reached the tribes of the northwest between February and November 1832, and when the bulk of the indigenous population was induced to leave their lands. During this period there was a sharp divergence between the 'official' policy of the Company (and especially of its Tasmanian Agent, Edward Curr) and the reality of life in the bush, where the behaviour of some shepherds and convicts veered between an arrogant disregard for Aboriginal lives (and especially of the liberty of Aboriginal women) and a justified fear of reprisal.

Lennox estimates that the bulk of the attacks by Aborigines were reprisals for murders, rapes or abductions by servants of the VDL. The incident reported above was held by Lennox to be a response to the shooting of an Aboriginal woman by two Company servants earlier that September. Although there was occasionally a measure of panic among the VDL servants, for the most part they were able to 'retaliate' in an 'effective' manner, kicking off a spiral of violence where actions of almost unimaginable savagery took place against a background of official policies of conciliation. Yet there was always some ambiguity. After the attack on Burghley, the Court of Directors in London expressed their sympathy to the wounded but emphasised (VDL 1/4: Despatch No. 93, 6 April 1829, Directors to Curr) that

> every measure should be tried to conciliate and civilize the natives to make them your friends instead of your enemies, this is no doubt a difficult task but still it is the duty of the Company to attempt it, and if they can be brought into a state of comparatively social comfort it will be conferring upon them a greater boon than the value of the range and

> hunting of the Lands of which they will be deprived, and of which the Company will have possession. The Court cannot too strongly urge these attempts upon you, they are aware of the difficulties which have increased since the natives and the Servants have come in contact and blood spilt; they are also aware that a knowledge of the strength and power of the Company must first be proved to exist and fully impressed upon the natives and on that account the court send you by the *Friendship* some Fire Arms, particularly pistols which they conceive will be of more use than muskets because they can be carried about the person; you will therefore be fully prepared for war and possessing power, you will leave no steps untried to prevent hostile contact with the Natives, and to promote friendship and conciliation.

This ambiguity might have resolved a conflict of interest in London between the commercial goals and the Christian values of the Court of Directors. It had little impact in the Tasmanian bush. A great divergence between official policy and the reality of life on the frontier occurred right across southeast Australia in the first 40 years of the last century. However, it had different effects in Tasmania than elsewhere. Although it is true that large-scale massacres as well as the almost casual taking of individual Aboriginal lives occurred in Tasmania as well as in other places, in the northwest of Tasmania all attempts at binding Aboriginal people into the web of pastoral life failed dismally. The contrast between the Cape Grim massacre, where approximately 30 Aboriginal people were murdered by Company servants on 10 February 1828, and that of the infamous Myall Creek massacre which took place in northern New South Wales a decade later, is instructive (Morris 1992; Murray in press a: Chapter 2). At Cape Grim 'wild' blacks were murdered, while at Myall Creek 30 Aboriginal people (some of whom were working for and sleeping with the local white people) were slaughtered in the station stockyards. It is this lack of success in showing the Aboriginal people the 'blessings of civilized life', and the change from policies of 'conciliation' to deportation, which make it unlikely that Aboriginal people lived with whites at Burghley.

An infamous example of callous reporting, and of a confused official response to a straightforward case of murder, concerns the death of an Aboriginal woman at Emu Bay on 21 August 1829. Alexander Goldie, the Company's chief agriculturalist, reported how he had gathered his men together and made a concerted effort to 'take' some Aboriginal people (VDL 5/2: 210, Despatch No. 100, 16 November 1829, Curr to Directors; Lennox 1990: 181):

> For this purpose I took my horse and the men, one gun and a couple of axes. On getting within 200 yards of them we were observed and they began to make off. I ordered the men to keep outside while I took the scrub. This had the effect and the Natives kept along the sands. Russell fired at one just as she was taking the scrub and shot her. She was very badly hit about the bottom and the belly, and she must soon have died.

> I rode down another woman in the scrub and before I returned with her the men had killed the other. The woman that was shot had a child about 6 years old (a girl) which we also got.

Curr, his employer, wrote of this to Goldie (CSO 1/326/7578: 109–11; Lennox 1990: 181–2):

> It has never occurred to me since I have been in the service of the Van Diemen's Land Company to read a more revolting detail than that contained in your letter and the manner in which this barbarous transaction is related without one word of disapprobation being expressed against your associates in the deed, or one word of regret, is scarcely less-offensive to every feeling of humanity than the deed itself.

Yet for all Curr's objections to those in authority (particularly Lieutenant Governor Arthur) no action was taken against Goldie, or the men with him. Indeed, Lennox has uncovered further evidence that Goldie lied to Curr (and that Lieutenant Governor Arthur had also lied), in that the Aboriginal woman had been 'finished off' with an axe (Lennox 1990: 191). The reason for the lack of action seems simple enough: public opinion would not stand for the hanging of white men for the murder of blacks when a state of war existed in the colony. The spiral of violence continued without effective check, no matter the fears held by the Government and the directors that the news of such unpunished outrages would reach the philanthropists in London and the Aborigines Protection Society. Even after the majority of the Aboriginal population of the northwest had been deported around 1832, with a later expedition in 1834, trouble could still flare up as old scores were settled and huts pillaged. As late as 12 August 1841 Curr was seeking to remove the remaining Aboriginal people from Company land—peacefully if possible, but with main force if necessary (see VDL 5/6: 39–41, Despatch No. 234 Curr to Directors).

Material culture evidence

The third line of evidence is the pattern of material culture found at Burghley. The mixing of Aboriginal and European material culture might be partially explained by earlier Aboriginal material being brought up in excavating foundation trenches for the house. Other material culture evidence is decisive (Murray in press b). Glass tools emulate classic Tasmanian forms made on stone. Musket and pistol flints are flaked into Aboriginal forms, and two traditional forms of bone tools are made on dog and bovid (introduced species). The midden has wallaby and horse bones in the same stratigraphic units. The midden (and the clogged chimney) might tell a story of Europeans adapting to life on the frontier and broadening their diet, but cannot explain the glass tools, the modified gunflints or the quartz manuport near the chimney butt.

Consequently, I think it likely that Burghley was reoccupied by Aboriginal people after its abandonment by the VDL around 1839. The decision to wind down company operations in the Surrey Hills was largely taken for commercial reasons. The primary object of the company had been the production of wool for English mills requiring that large numbers of shepherds live on the runs spread over the Surrey Hills. When the greater proportion of the company's flock died in the Hills due to inclement weather, the company moved to open-range cattle grazing which required fewer employees, and therefore fewer places to house them.

The Aboriginal people at Burghley

Who were the Aboriginal people who reoccupied Burghley? By 1835, George Augustus Robinson, whose role in the deportation of Aboriginal people to Flinders Island is well documented (see e.g. Plomley 1966), was claiming that, with the possible exception of some small groups, the entire Aboriginal population of Tasmania had been removed to Flinders Island; the Black War was over. Nevertheless reports from VDL settlements during 1836 of further violence and robbery compelled Robinson to despatch his last mission. Late in 1836 Robinson's sons found a family group (a man, a woman and four or five children) but could not persuade them to surrender (Plomley 1966: 926–7). Violent clashes continued in the Hills until 1842.

On 10 December 1842 William Gibson, the newly appointed Superintendent of the Van Diemen's Land Company, wrote to the Court of Directors of the demise of traditional Tasmanian Aboriginal society (VDL 5/7: 111, Despatch No. 23, Gibson to Directors):

> The court will be glad to learn that the natives who had hitherto been so troublesome were captured upon the 4th instant near the River Arthur and forwarded them yesterday to Launceston, their party consisted of a middle-aged man and female, two males about 18 and 20 years of age, and three male children between 3 and 7 years old.
>
> This very desirable object has been accomplished by two men who are in the habit of frequenting the coasts of this island for the purpose of catching seals and who were accompanied by two women, natives of New Holland. It was principally through the instrumentality of the latter that they were successful and the moving cause of their exertions was the hope of getting a reward of 50 pounds which I had ventured to offer on behalf of the Company if the Aborigines were taken without violence and which I trust the Court will approve of my having paid them.
>
> These were the only natives at large in this colony and I can scarcely express the satisfaction which their removal gives me as well as the comfort and security it affords to the Company's servants and property.

The records clearly state who these people were. The man was known as John Lanna, his wife was Nabrunga, the five children were Banna, Pieti, Albert, William and

Figure 5d.4: Photograph of William Lanne by Chas. Woolley, 1866, copied from a carte-de-visite. (© Tasmanian Museum)

Frank. John Lanna and his family, it seems, were the people contacted by Robinson's sons behind Cradle Mountain in 1836. The timing is crucial. After 1834 Robinson's evidence indicates only a small family group was left free in northwest Tasmania, which coincides with a marked decline in incidents of violence (until 1839). We do not know the exact date of European abandonment of Burghley (by the late 1830s it was an outpost of only minor importance); a likely date is around 1839. It is a reasonable conjecture that the people who left the Aboriginal artefacts found at Burghley, and fought the guerrilla actions against the VDL after 1836, were this family group. Only William and Banna survived internment at Flinders Island (Plomley 1987: 882). William Lanne (Fig. 5d.4) was to live until 1869, the last full-blood Tasmanian Aboriginal man to die.

Burghley and Lanne

Our discovery of evidence for Tasmanian Aboriginal people reclaiming Burghley, and of their adapting traditional lifeways to a dramatically different world, was made all the more striking by the strong possibility of a connection to William Lanne. Lanne's death on 4 March 1869 prompted the Hobart *Mercury* to reflect:

> We chronicled yesterday an event such as we believe it has never been the lot of a British journalist to record, namely the death of the last man

> of his race—the total extinction we may say, of a once numerous division of the human family. Exceptional as the announcement appears to be, it derives an additional and melancholy interest from the circumstance that it is unlikely to stand unique for any lengthened period in the history of British colonisation.

Although some were obviously struck by the sadness of the occasion, as memories of the wildness and abominable behaviour of the Black War were now suppressed or dimmed, others saw Lanne's death as a boon to science. For this reason his passing does not end our story. Lanne's body was dismembered (in the interests of science) while laid out in the morgue at Hobart Hospital. By 8 March it was also apparent that body-snatchers had excavated Lanne's grave to get the rest of the corpse. Judging by the contemporary accounts there was no doubt that the corpse was being fought over by agents of the Royal Society of Tasmania and of the Royal College of Surgeons in England (see Ryan 1981). Both groups had questions which urgently needed answering (such as the degree of relationship between Tasmanian Aboriginal people and the gorilla), and Lanne's skeleton was vital scientific evidence. The fact that various 'extraneous' organs were rumoured to have been souvenired by the guilty parties was 'regrettable'. The Hobart *Mercury* of 11 March was moved to bitter parody in a fairly typical piece of doggerel called 'Lanny's Ghost', the last two stanzas of which pulled no punches:

> The cock it crows! I must begone—
> Kind friends, we must now part,
> But yet, I'm yours in death: altho'
> some MD holds my heart.
>
> Don't go to seek me in my grave
> Or think that *there* I be;
> They have not left one atom there
> Of my ANATOMY.

This parody recently became all the more bitter. After extensive negotiations between the University of Edinburgh and the Aboriginal and Torres Straits Islander Commission, the Melbourne *Age* of 19 January 1991 reported that William Lanne's skull would be repatriated to Tasmania. Notwithstanding a row between the Tasmanian Aboriginal Centre (TAC) and the Federal government over protocol and proper respect, there was a general belief that the 'torment of Lanne's spirit' would soon cease. Five days later the newspapers were reporting confusion in Edinburgh and in Canberra over whether the skull in question really was Lanne's. The correctness of identification was questioned in the light of possible poor accession practices in the Museum, the loss of records in a fire and a possibility that Lanne's skull had never gone to Edinburgh but was, instead, donated to another institution. Investigations using the technique of photographic imposition were conducted in Edinburgh at the University's request. The researchers were unequivocal that the Edinburgh skull was not Lanne's (Brocklebank & Kaufman 1992).

In an effort to sort the mess out and to recover something of Lanne and his life after leaving Flinders Island for Tasmania (he had become a whaler), the TAC privately commissioned research into the Edinburgh and London collections, and into the companies engaged in whaling from Tasmanian ports during the mid-nineteenth century. This research is vital for adding to the store of information about the history of the Tasmanian Aboriginal people *after* the collapse of traditional Aboriginal society. The investigation of contact sites such as Burghley gives us the chance to gain a more richly textured understanding of European and Aboriginal life on the frontier during the holocaust of the 1820s and 1830s. Burghley, and other VDL Company sites in the Surrey Hills, are places through which we can locate the past-in-present experience as both Aboriginal and non-Aboriginal Australians seek to comprehend the consequences of a shared past. In this way the active involvement of Aboriginal people in contact archaeology has become a feature of practice in recent times, extending to situations where Aboriginal groups have requested State and Federal funding bodies to support Aboriginal requests for such research.

Concluding remarks

In the 1984 Trevor Reese Memorial Lecture at the Australian Studies Centre at the University of London, Henry Reynolds, the pre-eminent historian of Aboriginal Australia, chose to discuss the role of Aboriginal people in Australian historiography between 1955 and 1983. Reynolds wanted to re-examine W.E.H. Stanner's claim of 16 years before, that Australian historians had been silent about the interactions between whites and Aboriginal people, a silence that verged on neglect (Stanner 1969: 25):

> Inattention on such a scale cannot be explained by absent-mindedness. Rather it is a structural matter, a view from a window which has been carefully placed to exclude a whole quadrant of the landscape. What may well have begun as a simple forgetting of other possible views turned under habit and over time into something like a cult of forgetfulness practised on a national scale.

Reynolds' title, *The breaking of the great Australian silence*, indicates how much has changed since Stanner's understandably harsh assessment. Reynolds reviewed some of the causes of change, linking the reassessment of colonialism which followed decolonisation with the great increase in Aboriginal political activism, although Reynolds would be among the first to argue that such activism had always been there. He also stressed the great importance of Aboriginal archaeology and anthropology as providing clear evidence of a long and complex history and of the tremendous variety of Aboriginal social and cultural forms.

Notwithstanding these advances, there is a widespread view that Australian society has a great deal further to go in understanding the consequences of the European conquest of Australia. Enlightened public servants and politicians have begun to see that Aboriginal history and Aboriginal archaeology have a very significant role in this quest for understanding. Between 1 January 1980 and 31 May 1989, 99 Aboriginal and Torres Strait Islander people died in the custody of prison, police or juvenile detention institutions. A Royal Commission into Aboriginal Deaths in Custody was established in October 1987 in response to public concern that this number of deaths was disproportionately high, and the explanations offered by the responsible authorities seemed evasive. The Royal Commission also inquired into the highly controversial deaths of two other Aboriginal men, John Pat and David Gundy. Eliot Johnson, the chief Royal Commissioner, noted that it was a commentary on relations between Aboriginal communities and the police that foul play was popularly suspected in the majority of cases.

In the Royal Commission's final report, which spans five volumes of discussion and recommendations, Johnson spoke at length of the over-representation of Aboriginal people in the prison system and the lack of care taken of them when they were inside—a direct consequence of 200 years of interaction with whites which had completely marginalised Aboriginal people in Australian society. Johnson felt the only solution was for Aboriginal society to empower itself, to take control of lives and to have pride to face the past and the future. A crucial element of empowerment was for Aboriginal people to retain and expand their culture and their identity.

In this 'self determination' Johnson saw a central role for Aboriginal history and Aboriginal archaeology working in two directions: first, to the benefit of Aboriginal people in that both studies would help them explore issues of Aboriginality in possibly new and helpful ways; secondly, to help the non-Aboriginal community to understand more about what happened, as well as to dispel myths about Aboriginal society which are still with us. Sites such as Burghley, with their unique window on to the world of the frontier, can serve both ends, but it seems to me that their greatest value stems from their status as places where a shared history between Aboriginal and non-Aboriginal began.

Contact sites are concrete representations of the beginnings of this shared history as well as the beginnings of the more recent phase of *Aboriginal* history. By extension, such sites are significant documents for all Australians, notwithstanding the fact that there is no single or overarching account of that significance. This argument for significance has great force at a time when strong links are being made between the drive for an Australian Republic and the formal resolution of a state of conflict between white and black Australia which has continued since the first white settlement over 200 years ago. In this account the same colonial society which provided the context for dispossession, genocide and unrelenting attacks on the 'other' was also responsible for the oppression of other segments of the non-Aboriginal population. As such, the remains of colonial society have to be identified and dealt with before a lasting peace born of mutual understanding and regard can come about. Contact archaeology provides the framework for contemplation about these matters too.

Archaeology and written documents have been used here to expand our understanding of the Tasmanian frontier during a time of great violence, and to assist a reflection about matters of emotion and identity. But there is more to the story than this. The discoveries at Burghley chart the dying moments of traditional Tasmanian society which had begun 37 000 years before. Even though Tasmanian Aboriginal people had been the victims of a holocaust which had begun barely 40 years prior to the abandonment of Burghley, Lanne and his family were able to adapt and to re-create the substance of their culture at what had become an alien place, and with alien materials. We do not know very much about what these people felt about the destruction of their world, save a report from the last 'Friendly Mission' in 1836 that they were reluctant to surrender because they were frightened of other Aborigines who had been deported to Flinders Island (Plomley 1966: 926). What we do know is that at the time of their greatest test, greater even than the intense cold period around 20 000 years ago, or with the incursion of the rainforests into the alpine grasslands about 10 000 years ago, these people—contrary to one celebrated analysis (see e.g. Jones 1971)—seem not to have been suffering the effects of a long, slow strangulation of the mind supposedly brought about by thousands of years of isolation. So, out of all that sadness, there is something.

Acknowledgements

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Part II
Special Studies

6 Change and variation in human ecology

Simon Holdaway

Palaeoecological research has formed a focus for recent studies of Tasmanian Pleistocene prehistory (Bowdler 1984). The paper reprinted here presents a preliminary version of a model subsequently developed by Cosgrove in his doctoral dissertation (published as Cosgrove 1995) together with research undertaken by other members of the Southern Forests Archaeological Project (see e.g. McNiven *et al.* 1993). The models made very good sense of the evidence that was to hand some five years ago. In the intervening period more sites have been excavated and further research has been completed on the existing collections. Inevitably, this has led to the need to modify some aspects of the original model. But what remains uppermost is the strength of the model as an example of the way palaeoecology can be applied to archaeology. It is also the most thoroughly worked example to date that makes use of sources other than ethnographic models or analogues to interpret the deeper prehistory of Australia.

In this essay I will discuss the methods Cosgrove and his colleagues adopted in constructing their model, provide some background concerning the theoretical perspective they utilised, and comment on the distinction between environment and culture as mechanisms for change in Tasmanian prehistory.

Construction of the model

The authors adopted palaeoecology as a source of inspiration for model building because there were no ethnographic or archaeological analogues from Australia or elsewhere to the archaeology of southern Tasmania during the Pleistocene. The cave sites which form the database of the model provided long chronologies implying continuous use of the region for thousands of years, which was then followed by a clear absence of occupation from around the end of the Pleistocene until the European invasion in the nineteenth century. Furthermore, by Australian standards the sites were exceptionally rich including substantial faunal remains. These data provided the inspiration for constructing a model independent of the archaeology but against which it might be compared.

The sources for the model are well detailed in the Cosgrove *et al.* paper—and in subsequent publications (Cosgrove 1995: 98; Cosgrove *et al.* 1994). The results of pollen analyses from late Quaternary deposits are more numerous in Tasmania than in any other part of Australia and these are used to sketch the vegetational history of the southwest. In doing so a central dichotomy is introduced between the relatively higher rainfall environment of this region and the drier southeast of Tasmania. It is not pollen analysis, however, that forms the crux of the palaeoecological model Cosgrove *et al.*

introduce, but work on grassy habitats by Kirkpatrick and his colleagues (Kirkpatrick 1986; Kirkpatrick and Duncan 1987).

Kirkpatrick has taken issue with those who characterise Tasmania during the height of the last glacial as a region akin to the steppe environments of the northern hemisphere. Based on the ecological requirements of current-day grass communities, Kirkpatrick has argued that the only places likely to have supported grasses in the interior southwest valleys during the late Pleistocene are the regions underlain by limestone or relatively fertile alluvial soils. This observation forms one of two key aspects of the palaeoecological 'Patch' model. The other is the present-day behavioural ecology of *Macropus rufogriseus* (the red-necked wallaby), the species that forms the major prey animal represented in the southwest archaeological sites. This animal is described as being sedentary when compared to other macropods, and as being tied to forest edges with access to grass and herb lands. Combining Kirkpatrick's work on grasslands, with the behaviour of M. *rufogriseus*, Cosgrove *et al.* were able to interpret the palaeoecology of the southwest as an area of resource patchiness where relatively sedentary animals would have been tied to patches of grasslands interspersed with low trees and shrubs in the river valleys.

In this model the relatively high rainfall of the southwest is distinguished from the southeast where the environment was drier and more drought prone and where the resources are modelled as less predictable and more generally distributed across the landscape. Some critics (Thomas 1993) have missed the significance of the association between grassland patches and the behaviour of M. *rufogriseus*. Yet it is this association that not only permits a link to be forged between the Patch model and the archaeological data (through the presence of M. *rufogriseus* in the sites), but also allows Cosgrove *et al.* to explain the near abandonment of the sites at the end of the Pleistocene. It is not, as Thomas claims, that the re-establishment of the rainforest which drove Aboriginal people from the caves. Rather it was that the replacement of grasslands by forest severely reduced the numbers of the primary human prey species.

The Patch model is constructed following the standard palaeoecological technique of using studies that reconstruct past environments on the basis of modern analogues from known environments (See Birks and Birks 1981: 28; Delcourt and Delcourt 1991: 3). Thus the model is a form of analogical reasoning and is based on modern rather than fossil flora and fauna. Other palaeoecological techniques rely on a search for patterns in the fossil record itself rather than the search for modern analogues, and it is in this area that significant advances have been made in the reconstruction of Tasmanian palaeoenvironments in recent years.

As detailed in summaries of palaeoenvironmental studies from southeast Australia (Kershaw 1995), Tasmania (Colhoun *et al.* 1994; Porch and Allen 1995) and New Zealand (Anderson and McGlone 1992), vegetation patterns at the Last Glacial Maximum in these regions are now characterised as grasslands with some heath and shrub taxa. In effect it is now suggested that the patches in late Pleistocene southwest Tasmania would have been the trees rather than the grassland. At the same time there is some evidence to suggest that occupation of the southwest archaeological sites was not continuous but rather occurred preferentially during periods of relatively wetter climate (Holdaway and Porch 1995, in press).

Incorporation of the results of these studies may require that the palaeoenvironmental aspects of the Patch model be revised, however the key aspect of the relationship between grasslands and the behaviour of M. *rufogriseus* remains. It is likely to continue to form an important mechanism for reconstructing the behaviour that led to the formation of the archaeological sites.

The theoretical perspectives underlying the patch model are aspects of both North American ecological anthropology and British economic archaeology. Many archaeologists have sought to characterise the environmental context of their sites, however early attempts were little more than qualitative descriptions of habitat types (Foley 1977). The model developed by Cosgrove *et al.* is clearly more than this. It moves from palaeoenvironmental reconstruction (summaries of reconstructions of the vegetation and climate) to the proposition of a palaeoecological model (the Patch model discussed above) where it is the inter-relationships among organisms that characterised past environments that is the critical discovery. The strength of the model is seen in the hypothesised relationship between grasslands and the behaviour of M. *rufogriseus* but the Patch model is less explicit in how this relationship may have changed through time.

Palaeoecology is of more interest to archaeologists than simply palaeoenvironmental reconstruction because emphasis is placed on the inter-relationship between populations and resources (Foley 1977). Human palaeoecology extends this concept further by incorporating the interrelationship among a number of behavioural subsystems such as subsistence, technology, settlement pattern and demography (Jochim 1979). The degree to which Cosgrove *et al.* are able to develop a model of human palaeoecology is limited by the preliminary nature of their study. In 1990 the specialised studies that have now appeared in print were only just getting underway (for example Marshall and Cosgrove 1990; Webb and Allen 1990). Nonetheless, there are a number of themes developed by Cosgrove *et al.* that emphasise human environment interactions. This may be seen in the link that is developed between seasonality of occupation at Nunamira (interpreted as late winter or spring—frequently a marginal time for temperate hunter-gatherers) and the evidence for bone breakage interpreted as evidence for fat acquisition. It is also apparent in the analysis of stone artefacts where these are considered in terms of raw material distribution, although the conclusions drawn are limited by the preliminary state of the data analysis.

The human palaeoecological model that has been developed likens the prehistoric inhabitants of southwest Tasmania to other hunter-gatherer groups living in temperate and subarctic environments where late winter and early spring were times of resource scarcity particularly in terms of the quantities of fat in the diet. Because M. *rufogriseus* lived in a relatively moist climate it would have been in relatively good health and an attractive resource in terms of the investment of time and labour by prehistoric Aboriginal people. Whether human culling practises employed on these species were such that M. *rufogriseus* populations did indeed remain substantially unchanging for approximately 20 millennia as Cosgrove *et al*. have suggested will only be answered by the results of archaeological research currently underway.

Environment or isolation

Cosgrove *et al*. argue that the Patch model operated over something like 20 millennia. This implies a high level of environmental constraint on the Aboriginal groups who occupied southwest Tasmania during the Pleistocene (see also Porch and Allen 1995). Such notions are not new to Tasmanian prehistory. There has been a long debate concerning the effect of severance from the mainland involving theories based on environmental constraint versus cultural isolation (Jones 1977; Allen 1979). Nor have such ideas been limited to Australian archaeology. Much the same debate developed a number of years ago when Sutton and his colleagues argued for environmental constraint leading to similarities in the economic adaptations of a number of peoples occupying regions like southern New Zealand, the Chatham Islands and Tasmania in the south of the Southern Hemisphere (Sutton and Marshall 1980; Sutton 1982; compare Anderson 1981). The questions posed in these studies are fundamental to archaeology in general. To what degree is human cultural variation environmentally constrained by hunting and gathering in temperate regions? Put another way, to what degree are palaeoecological and palaeoeconomic models able to detect cultural variation in regions characterised by similar climates and economic resources? Such questions are among those that make the archaeology of the region discussed by Cosgrove *et al*. so interesting.

The paper reprinted here will continue to be important for two reasons. First, because it stands as an example of how to construct a palaeoecological model which incorporates archaeological and contemporary behavioural-ecological data. Second, because it is an example of how to develop such models into a study of human palaeoecology. Various aspects of the Patch model continue to be tested and, as is the way with all scientific models, modifications will continue to be made. But the paper will continue to be a fruitful intellectual mine for archaeologists in future years.

6a Palaeoecology and Pleistocene human occupation in south central Tasmania

Richard Cosgrove, Jim Allen & Brendan Marshall

Introduction

The distribution and make-up of Australian Pleistocene Aboriginal populations have been characterised as widespread, with low densities possessing little regional variation over large areas of Australia (White & O'Connell 1982: 72). This assumption has been supported in part by the nature of the archaeological evidence, geographically diverse and normally preserving few material remains. Contemporary demographic models suggest that Pleistocene Aboriginal populations exploited a narrow resource base and were highly mobile, so that site occupation, particularly as reflected in the archaeological record, is ephemeral; socially these groups are characterised as egalitarian, and their social networks and ceremonial institutions are portrayed as less complex than those of mid-Holocene Aborigines (Lourandos 1983: 88; 1985: 398). Thus there is a clear trajectory of increasing socio-economic complexity from foragers (read Pleistocene) to collectors (read mid-Holocene) (Lourandos 1987: 158).

We believe that such notions of hunter-gatherer socio-economic organisation and change are inevitably unidirectional; they blend different behaviours over enormous time-scales and promote ideas of modal behaviour while denying the existence of alternative, systematic regional behaviours with inherent variability. Further, we agree with Soffer that 'by equating foraging behaviour with egalitarian sociopolitical relationships, by merging the means of the two, we eliminate the range of variability present in each construct' (1987: 492). We now know that the range of variability associated with early modern humans is larger than previously recognised. Recent evidence suggests, for example, that there is no one-to-one correlation between the advent of modern *Homo sapiens* and particular stone tool classes (Trinkaus 1986; Foley 1987). Although Australia was peopled by essentially modern humans, Gowlett (1987: 215) points out their stone artefact morphology and technology 'could come out of the African or European Lower Palaeolithic', a pattern recognised by earlier researchers (Jones 1977: 190–1; White 1977: 24). This contrast is extended by the observation that early Aboriginal colonists must have had an efficient marine technology at least 40 000 years ago to cross into Australia (Jones 1987) and an artistic tradition probably more than 30 000 years old (Dorn *et al.* 1988; Nobbs 1988). These paradoxes have direct implications for ideas of unidirectional trajectories that are based on limited sets of value laden archaeological data and assumptions to explain changes in human behaviour.

By their unique geographical position and Holocene isolation from the rest of Australia and the world, Tasmanian Aborigines have, for over a century, provided models for reconstructing Pleistocene human behaviour (e.g. Sollas 1911: 70). While there are good reasons for questioning this practice, not least being the actual nature of Tasmanian Pleistocene human behaviour which we discuss in this paper, the practice continues. Lourandos, for instance, sees Holocene southeast Tasmania as a regional exemplar of the likely character of Pleistocene Aboriginal populations and suggests that the Pleistocene southwest Tasmanian site of Kutikina Cave (Kiernan *et al.* 1983) represents transient inland hunting indicative of Pleistocene Aboriginal behaviour (Lourandos 1985: 397).

If this were so we would see little variation in the archaeological record between the two regions. Clearly this is not the case. Recent evidence from Pleistocene sites in southeast and southwest Tasmania demonstrates a degree of archaeological richness and variability not previously reported in Australian Pleistocene sites (Goede & Murray 1977; Goede *et al.* 1978; Murray & Goede 1980; Jones 1984; Kiernan *et al.* 1983; Blain *et al.* 1982; Jones & Allen 1984; Jones *et al.* 1988; Allen *et al.* 1988; Cosgrove 1989; Cosgrove & Jones 1989; Allen 1989; Allen & Cosgrove 1989; Allen *et al.* 1989). The 41 cave and open sites now recorded from these areas indicate the continuous occupation of these zones between 30 000 and 11 000 years ago. Before 1987, although over 20 radiocarbon dates were reported from eastern Tasmania, none were older than 10 000 years. In that year we initiated the Southern Forests Archaeological Project, aimed at investigating this temporal paradox and also investigating, elaborating and verifying the propositions raised by previous research, particularly that done at Kutikina Cave.

The study area

Our study area lies within southern central Tasmania and its choice has been predicated on a number of things. It lies at the division of two environmental zones in southern Tasmania: to the west a fold-structured geology vegetated by temperate rainforest, and to the east a fault-structured geology vegetated by dry sclerophyll forest (Kirkpatrick 1982) (Fig. 6a.1). It thus gives us a central pivot point from which to compare and contrast the variability of southwest and southeast Tasmania. As ethnographic or archaeological analogues in Australia for the deposits found in southwest Tasmanian sites are completely absent, we use a palaeoecological model as a conceptual framework generated from previous archaeological data and information on soils, vegetation and animal ecology. By understanding some ecological aspects of the zone during the Pleistocene, we may be able to observe links and relationships between the static archaeological record and dynamic human behaviours that produced them. Before describing

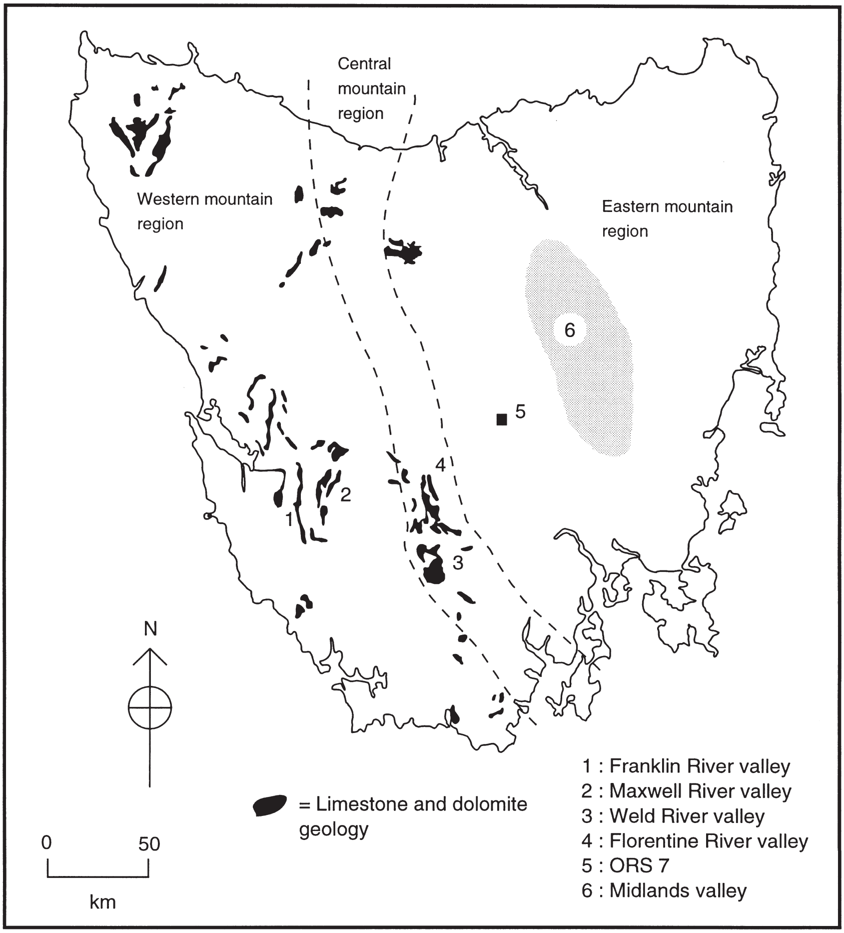

Figure 6a.1: Tasmania: regions and karst geology (after Kiernan 1988).

this model we present the various strands of independent palaeoenvironmental information available for Pleistocene Tasmania to demonstrate the ecological dynamics involved.

A palaeoecological model: climate, vegetation, subsistence, settlement

Although the relative merits of palynology for reconstructing past vegetation patterns, and especially the distribution of grassland, have been questioned by Kirkpatrick (1986: 235), palynology is a useful starting point for discussing environmental changes during the time period (10 000–50 000 years BP) under examination (Dincauze 1987: 277).

Analysis of pollen data taken from soil profiles at various lowland locations on the Tasmanian west coast indicate vegetation between >44 000 years BP and 25 000 years BP consisting predominantly of alpine-subalpine herb, heath and shrub species. At this stage the mean annual temperature was *c.* 5°C colder than present, and the climate was wet (Colhoun & van de Geer 1986, 1987a, 1987b; Colhoun 1985a; 1985b). Between 24 000 years BP and 20 000 years BP the dominant species were wet heath and herb communities but increasing values for herb and grass pollen appear after 22 000 years BP up until the Glacial Maximum at 18 000 years BP (Macphail & Colhoun 1985; Gibson *et al.* 1987). The geographic extent of these grasslands is of importance to our model and will be discussed in detail below. The tree-line at about 21 000 years ago was depressed by at least 230 m in the west coast region. The temperature at this time has been estimated to be *c.* 6°C below average temperatures today while the climate was drier than the preceding period. Between 14 000 years BP and 11 000 years BP tree and shrub species become more important until rainforest taxa become dominant after 11 000 years BP (Macphail 1975, 1979; Macphail & Peterson 1975; Colhoun & Moon 1984). The climate during this latter period was warm and moist.

In the east, especially the Midlands Valley, pollen data suggest that the vegetation cover between 25 000 years BP and 10 000 years BP varied much less during this period than in the west. A progression has been suggested from grassy woodland to grassland and back again to grassy woodland with *Eucalyptus* sp. more dominant than at any time previously (Sigleo & Colhoun 1981). On the southern edge of the Tasmanian Central Plateau, alpine and subalpine grasslands were at least 300 m lower than present day limits of about 1000 m (Macphail 1975: 299). The climate around 18 000 years BP was relatively cold, dry and windier, especially in the northeast of Tasmania where it is suggested that average wind speeds were 8 km/hr greater than at present (Bowden 1983). Sand dune development in southeast Tasmania has been dated to *c.* 15 000 years BP, a time just after postulated maximum aridity (Sigleo & Colhoun 1975). In the northeast, lunette formation continued until about 8300 years BP (Cosgrove 1985). These changes were due to low sea-levels, a high proportion of moisture was intercepted by the west coast mountains increasing the precipitation gradient between east and west (Bowden 1983). Even allowing for an average annual rainfall reduced by 50% during the Last Glacial Maximum (Galloway 1986), the west coast would have received a mean annual rate of at least 1000–1500 mm compared to 300–400 mm in the east. This latter zone was probably subject to periods of drought stress during the glacial maximum as evidenced by inland dune and lunette building episodes (Bowden 1978a, 1978b, 1983; Colhoun 1978, 1982; Kirkpatrick 1986: 239). The climate in the area at this time, classed as glacial-arid (Macphail 1975), may have restricted grasslands to areas on deeper soils, with higher moisture leaving sandier substrates vulnerable to erosion.

This ecological variation has obvious implications for the structure and distribution of exploitable energy (animal and plant resources) across the landscape for hunter-gatherers prior to, during and subsequent to the Last Glacial Maximum (Foley 1977:

171; 1981; Gamble 1984: 224; 1986: 42, 64; Jochim 1979: 84). Especially relevant, as noted above, are the past distributions of productive grassy habitats in southwest Tasmania and the role these may have played in resource availability and distribution. Kirkpatrick (1986: 235) notes a myth that extensive areas of western and southwestern Tasmania were covered with open herbfield and steppe (Kiernan *et al.* 1983: 30). He recognises that due to widespread soil infertility in western and southwestern Tasmania, the dominant glacial vegetation complexes were almost certainly stunted woody, sedge and heath taxa like those that cover the alpine and treeless subalpine regions today (Kirkpatrick & Brown 1984; Kirkpatrick 1986: 239). Only in the east and parts of the northwest and the narrow, exposed western continental shelf would grassland—chenopod vegetation and short alpine herbfields—have been extensive (Hope 1978; Sigleo & Colhoun 1981; Macphail & Colhoun 1985; Colhoun *et al.* 1982).

Jones (1984) and Kiernan *et al.* (1983) have recognised a general association between fauna and steppe as a basis for the character of the rich deposits in Kutikina Cave. However, the only places likely to support grass in the interior southwest would have been in valleys underlain by limestone and on relatively fertile alluvial soils (Fig. 6a.1) (Kirkpatrick & Harwood 1980; Kirkpatrick 1982: 268; 1986: 237; Kirkpatrick & Duncan 1987).

In view of this we put forward an alternative model based on the requirements of, and interaction between, fauna, flora, soils, temperature and fire. It has been shown that the major animal exploited by humans in southwestern Tasmania was the red-necked wallaby, *Macropus rufogriseus* (Kiernan *et al.* 1983; Jones 1984; Allen *et al.* 1988). This animal, no longer common in the southwest vegetation complexes, now occurs only in very low numbers in open shrubland and sedgeland (Hocking & Guiler 1982). A principal reason given for the abandonment of the southwest sites is the reduction or displacement of the wallabies when grasslands were eliminated by invading rainforest around 12 000 years ago (Kiernan *et al.* 1983). An important aspect of the ecology of these animals is that they are, in macropod terms, extremely sedentary, with an average home range of 15–20 ha. Within their range, animals remain focused on a particular area for periods up to 2–3 years, shifting their centres of activity less than 30 m (Johnson 1987: 131). This can be compared with home ranges of *c.* 10 sq. km for larger Australian mainland red and grey kangaroos, who shift their centres of activity between 900 m and 1060 m (Priddle 1988; Priddle *et al.* 1988). The red-necked wallaby range changes little from season to season and from year to year. Most animals position themselves close to forest edges and sheltered gullies which provide good protective cover, escape routes and proximity to feeding areas (Southwell 1987: 28; Johnson 1987: 128). An important biological requirement to support sizeable aggregations of these wallabies is the existence of grasslands and herbfields, as this is the major food on which they graze (Jarman *et al.* 1987: 11; Kirkpatrick 1983: 75; Strahan 1983: 239; Gibson & Kirkpatrick 1985: 96; Southwell 1987). It also appears that they have a wide altitudinal range. Gibson & Kirkpatrick (1985) found a high correlation between wallaby grazing activity and the distribution of short alpine herbfields on snow patches on Mt Field at an altitude of 1200 m.

Grasses and herbs require fertile soil and reliable drainage (Kirkpatrick & Duncan 1987; Bowman *et al.* 1986; Ellis 1985, 1986; Ellis & Gravley 1987). Today no grasses grow on the silicious soils of southwestern Tasmania, although they occur as sparse individual clumps of refugia on limestone outcrops in the Weld, Franklin and Maxwell Rivers (Kirkpatrick & Harwood 1980; Kirkpatrick & Brown 1987). From these refugia, during cold phases, the grasses would only colonise the deeper, more fertile soils along alluvial flats or ground on restricted limestone geology which coincidentally contain caves and rockshelters (Kirkpatrick 1986: 237; Middleton 1979).

The identification of mechanisms involved in long-term maintenance of grassland is important in understanding the reasons for abandonment of southwestern Tasmania by Aboriginal peoples at the end of the Pleistocene. Ellis (1985; 1986) suggests that an increase in fire frequency on grasslands can lead, under model conditions, to a grassland sub-climax whereas a cessation would lead to successive colonisation by heathland and tea-tree to eucalypt and possibly rainforest. This has been observed to take place in less than 150 years in areas of northeast Tasmania (Ellis 1986). In higher alpine areas, perhaps approximating palaeo-vegetation characteristics in terms of plant cover, fire in the short term increases the dominance of herbaceous species like poa grasses but in the long term degrades shallow fertile soils, although deeper substrates may not suffer as markedly (Kirkpatrick 1983; Kirkpatrick & Dickinson 1984). Their disappearance at the end of the late Pleistocene has been explained by the invasion of rainforest species on to these grasslands when the climate became wetter and warmer 12 000 years ago (Kiernan *et al.* 1983; Jones 1988).

This explanation is difficult to sustain on anthropogenic grounds. It is significant to note that rainforest is extremely fire sensitive and after burning can be effectively excluded from its former range by systematic, regular firing (Bowman & Jackson 1981; Jarman *et al.* 1982; Hill & Read 1984). Humanly induced fires have destroyed extensive areas of western Tasmanian rainforest in short periods of time over recent decades (Jackson 1978: 98–101). Given the abrupt abandonment of all cave sites in southwestern Tasmania around 12 000 years BP, it is perplexing that some areas, particularly the more fertile Florentine River valley close to the eastern edge of the southwestern zone, were not kept open by fire and utilised well into the Holocene. Gilbert (1949) has pointed out that any fires that occur in the Florentine valley today will be severe in summer when under the influence of hot, dry, strong northerly winds. Gilbert (1959: 141, 134) argued for abandonment of Aboriginal firing in the valley 200 years ago to explain the progressive replacement of relict grassland by eucalypt forest over this time. Thus the dates of 12 000 years BP may only represent the abandonment of one type of Pleistocene economic strategy focused on cave sites. The presence of a large quartzite core and several hornfels flakes lying on the undisturbed floor in Nunamira Cave (formerly Bluff Cave) may indicate ephemeral visits after the sealing of the underlying deposits by calcite *c.* 12 000 years ago. Later evidence of Holocene Aboriginal use of

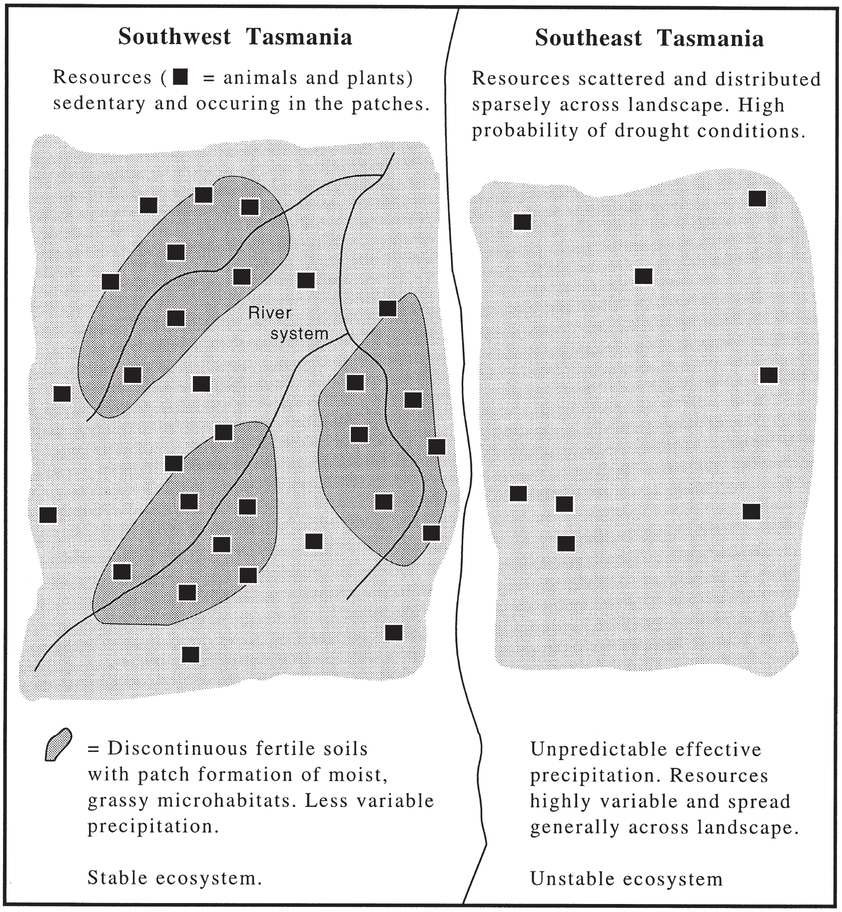

Figure 6a.2: A suggested palaeoecological model for southwest and southeast Tasmania.

the area may be found in open sites (e.g. Kiernan *et al.* 1983: 28) and this awaits further investigation.

We therefore suggest that the palaeoecology of the southwestern region at any point in time may have been characterised by resource patch-richness with sedentary animal and plant food interspersed between low trees and shrubs in the river valleys with a consistent and effective moisture input (Kirkpatrick & Brown 1987: 548). In south-eastern Tasmania where it was drier and drought prone, with widespread sparse grasslands, the resources were probably dispersed, unpredictable at times and more generally distributed across the landscape (Macphail & Jackson 1978; Sigleo & Colhoun 1981; Kirkpatrick & Duncan 1987). These contrasting patterns are represented in Figure 6a.2.

We assume however that these conditions were not stable and almost certainly varied on micro- and meso-scales through time and space although the magnitude of such shifts is unknown. This view is supported by theories of present and past ecosystem dynamics where complex influences such as climatic, edaphic and anthropogenic factors contribute to long- and short-term variability (Kirkpatrick & Brown 1984; Bowman & Brown 1986; Foley 1981; Pickett & White 1985: 374; Delcourt & Delcourt 1983; Dodson 1989). The usefulness of our model is its base on evidence, for the most part, independent of the archaeological data; it forms an ecological framework in which we believe it is possible to investigate intra- and inter-regional variability and concomitant human behaviours.

Regional archaeology

The project has so far concentrated on the karst formations of the central Florentine and the Upper Weld River valleys (Fig. 6a.3). Currently we have four excavated sequences to work on: Nunamira Cave (formerly Bluff Cave) in the Florentine and Bone Cave in the Weld, together with a previously excavated cave site M86/2 from the Maxwell River valley located to the west, and site ORS 7 positioned in the central southeast on the edge of the Central Plateau. It is recognised that the cave sites almost certainly represent only one part of a varied human behavioural pattern to be found in the area. The rugged nature of the terrain and the remoteness and inaccessibility of the region, coupled with almost impenetrable vegetation, all combine to limit the chances of locating sites. As an example, a total of 850 person-days were spent locating 28 cave sites and 6 open sites in four river valleys over a period of six field seasons.

The caves discussed here were all excavated without prior knowledge of their artefact richness. They were discovered by systematic sampling and searching of exposures of limestone and dolomite, and chosen for excavation because of intactness, location and the specific questions of the project. They are neither exceptional in size nor content within the range of southwest Tasmanian Pleistocene sites as these are presently known.

The four sites have all been reported in preliminary fashion (Cosgrove 1989; Allen *et al.* 1988; Allen 1989; Harris *et al.* 1988; Allen *et al.* 1989); here we reiterate only those aspects of site location, description and dating relevant to the discussion.

Nunamira Cave in the Florentine River valley and Bone Cave in the Weld River valley are both *c.* 400 m a.s.l., and are located on the eastern side of the western Tasmanian environmental zone previously described. A little further to the east and in the eastern environmental zone, site ORS 7 is slightly higher at 440 m a.s.l. and is positioned on the edge of the Tasmanian Central Plateau. The fourth site, M86/2, is in the Maxwell River valley, further west and at a slightly lower altitude. At the height of the last glaciation ice may have descended as low as 800 m (Colhoun 1985: 42) in this general area.

Site ORS 7 is a large sandstone shelter. The remaining three sites are limestone or dolomite caves. In contrast to some of the southwestern caves like Kutikina, both

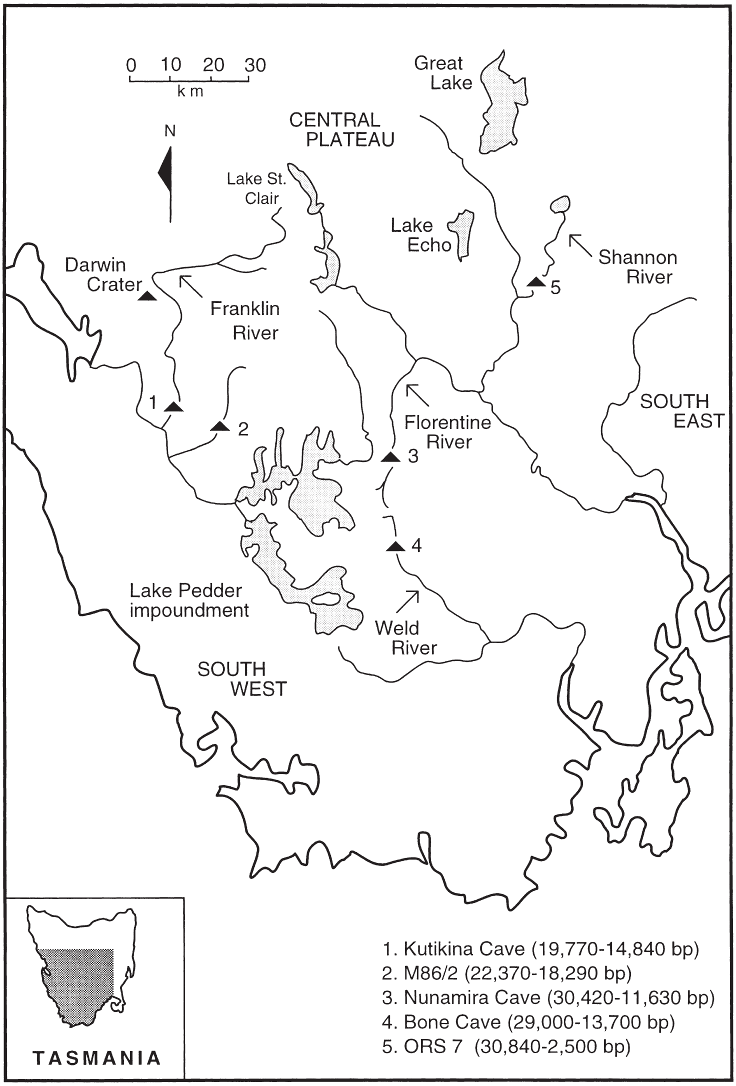

Figure 6a.3: South central and southwest Tasmania: regions and sites. **1** Kutikina Cave: 19 770–14 840 BP; **2** M86/2: 22 370–18 290 BP; **3** Nunamira Cave: 30 420–11 630 BP; **4** Bone Cave: 29 000–13 700 BP; **5** ORS 7: 30 840–2500 BP.

Nunamira Cave and Bone Cave have small chambers which would have acted to constrain the activities of occupants at any one time. The archaeological deposits therefore are likely to exhibit significantly less variation across their area, caused by different uses of different parts of the caves, than in larger caverns. This reflects directly on the representativeness of the excavated samples and some of the sequential changes.

Thirty-five radiocarbon determinations have been published for these sites. The test excavation at M86/2 has not yet reached the basal levels, but 90 cm of deposit have provided five dates in sequence from 18 290±290 BP (Beta-26961) to 22 370±470 BP (Beta-26962). The basal deposits in Bone Cave, Nunamira Cave and site ORS 7 are respectively 29 000±520 BP (Beta-29987), 30 420±690 BP (Beta-25881) and 30 840±480 BP (Beta-23404 & ETH-3724).

As can be seen, the dates of initial occupation at these three sites all overlap at two standard deviations; they extend human antiquity in Tasmania beyond the *c.* 22 000 BP date for Cave Bay Cave (Bowdler 1984) and the *c.* 20 000 BP limit previously determined elsewhere in southwest Tasmania (Jones 1988: 35).

Terminal dates for Nunamira Cave and Bone Cave conform to the general date of *c.* 12 000 years ago for the abandonment of seven of the eight dated southwestern cave sites. Site ORS 7 on the other hand has occupation continuing into the latter part of the Holocene.

Artefact density of archaeological deposits

Following excavations at Kutikina Cave, Kiernan *et al.* (1983) emphasised the extreme artefactual richness of the deposits, especially compared to the other Pleistocene Tasmanian sites like Cave Bay Cave and Beginners Luck Cave (Murray & Goede 1980). This richness is not unique to Kutikina, but appears to be a characteristic of many southwestern Pleistocene cave sites. The 0.25 cu. m of excavated deposit in M86/2 has produced 9500 pieces of artefactual stone and *c.* 30 000 pieces of bone; Nunamira Cave has yielded *c.* 30 000 stone flakes with densities of between 50 and 80 per kg of soil and *c.* 30 kg or 200 000 pieces of bone in *c.* 1.0 cu. m of deposit. Bone Cave is estimated to have produced a similar amount of bone and more stone from 0.8 cu. m of deposit. Various explanations present themselves. For example we might propose that caves were preferred over more exposed living locations for protection against cold; or that under the palaeoecological model we have advanced, since animals were congregated in patches, so were their human predators likely to use adjacent sites more frequently or for longer periods.

At Nunamira Cave, however, the richest bone deposit occurs in the upper level after 16 000 years BP and increases towards 13 000 years BP. At M86/2 abandonment occurs at 18 000 years BP, the period of intense cold, yet only 5 km away at Kutikina occupation is at its most intense. This suggests that at these two sites there is no necessary correlation between the intensity of glacial conditions and cave use. At present, however, we have no general explanation, and we will need a better understanding of the archaeology of the whole southwestern province before we can propose them. Meanwhile this archaeological richness is a two-edged sword, providing on the one hand excellent samples for various analyses but threatening, on the other, to limit the questions by the sheer bulk of the archaeological resource. While sampling small sites

Table 6a.1: Species represented in faunal remains from three sites

Species	*Nunamira Cave*	*Bone Cave*	*Site ORS 7*
Antechinus spp.	×	×	×
Cercartetus spp.	×		×
Mastacomys fuscus	×	×	×
Rattus lutreolus	×		×
Pseudomys higginsi	×	×	
Pseudomys spp.	×		×
Muridae	×	×	×
small mammal (0–0.9 kg)	×	×	×
Tachyglossus aculeatus			×
Isoodon obesulus	×	×	×
Perameles gunnii			×
Ornithorhynchus anatinus	×	×	×
Dasyurus spp.	×	×	×
Hyrdomys chrysogaster			×
Bettongia gaimardi			×
Pseudocheirus peregrinus	×	×	×
Potorous tridactylus	×		
medium mammal (1–4.9 kg)	×	×	×
Thylogale billardierii	×		
Macropus rufogriseus	×	×	×
Macropus giganteus (?)	×	×	×
Vombatus ursinus	×	×	
Dromaius diemenensis	×		×
large mammal (5–50 kg)	×	×	×

may overcome the problems of areal variability, it also at present prevents fully perceiving that variability which is fundamental to reconstructing a coherent past human behaviour in the region.

Site ORS 7 is again different from the southwestern sites in its distinctly lower density of artefactual remains. While the species list of animals represented in site ORS 7 is, with minor differences, similar to other sites further west (Table 6a.1), specific variations occur, especially including quantities and processing strategies. There are also significant technological and raw material distinctions to be drawn between the site ORS 7 stone tool assemblage and those further west. In short, site ORS 7 reflects a distinctly different archaeological signature from the southwestern Pleistocene sites and supports the idea that the eastern border of the southwestern geographic zone also marked a human behavioural boundary in the late Pleistocene.

Fauna

Table 6a.1 illustrates the presence/absence of animal species in the sites under discussion. While this information will be more useful when it is quantified through time for each of the sequences, preliminary analysis allows the following tentative observations:

- While each of the deposits is slightly different, we are confident that the vast majority of the faunal remains reflects human activity. In Nunamira Cave we can recognise a contribution of *c*. 12 000 mainly whole and unburnt bones from small mammals in the upper units which probably derive from owl pellets (Dodson & Wexler 1979; Marshall 1986; Hoffman 1988). The majority of these bones were found under the overhang at the front of the cave, which is very suitable as an owl roost. Similarly, much of the highly fragmented mammal bone from the top levels of Bone Cave indicates Tasmanian devil (*Sarcophilus harrisii*) activity (Douglas *et al*. 1966; Marshall & Cosgrove forthcoming). However, where the presence of humans is demonstrated by hearths and numbers of stone tools, these non-human predator indications are minimal or disappear. In these layers the bones exhibit characteristics of human manipulation—burning, patterned breakage, transverse incisions interpreted as cut marks and impact notches on the margins of some fractures.
- Among the humanly deposited bone there is a preponderance of *Macropus rufogriseus* throughout the sequences from the three southwestern sites being discussed here. The wombat (*Vombatus ursinus*) is a common minor element. In respect of these two animals these sequences are similar to the Kutikina fauna (Kiernan *et al*. 1983; Geering 1983). Beyond this, M86/2, Nunamira Cave and Bone Cave all contain a wider range of minor prey animals than Kutikina, including platypus, emu, Tasmanian native hen and native cat. The kangaroo, *Macropus giganteus*, no longer present in this part of Tasmania, may also occur in small numbers.
- In Nunamira Cave only, the presence of emu eggshell is of particular importance because it probably indicates the expansion of grassy habitats after *c*. 20 000 years ago, and also because it suggests human occupation in late winter/early spring at this site (Dove 1925: 221–2, 300; 1926: 213, 290–1).
- Similarly, the presence of the wallaby *Thyogale billardierii* at Nunamira Cave late in the sequence and the very late appearance there and at Bone Cave of the ring-tail possum *Pseudocheirus peregrinus* are probably indicating an increase in vegetation cover in this region in the terminal Pleistocene. The absence throughout most of these sequences of the possum, ubiquitous in faunal assemblages in southeastern Australian sites, is particularly striking and indicates the great difference between the Late Pleistocene environment and the Holocene environment in central Tasmania.
- Among the *Macropus rufogriseus* remains the apparent differential representation of body parts in many sequences suggests initial off-site processing of these animals. This pattern may represent differential treatment of bone and its breakage. Whether

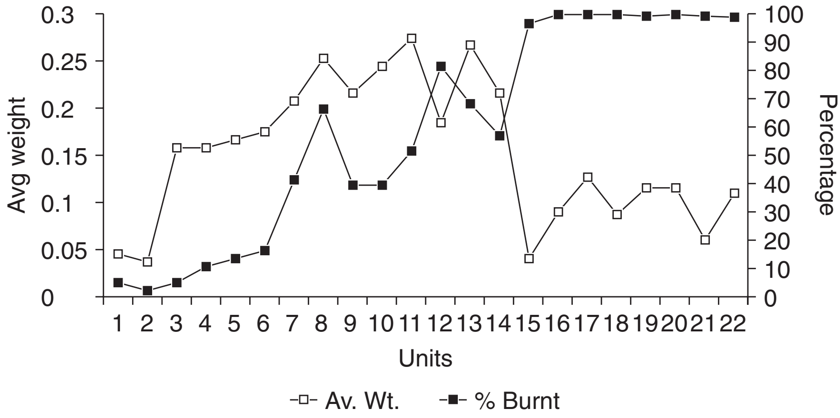

Figure 6a.4: Nunamira Cave: changing character of bone assemblage over time. The stratigraphic units forming the horizontal axis are numbered from the top of the site downwards, so deposits are older as one moves to the right.
The open squares record the average weight in grams of the bone fragments from each stratigraphic unit (left vertical axis). It is always small, and is smaller in the early deposits.
The solid squares record the percentage of the bone fragments from each stratigraphic unit that is burnt (right vertical axis). Almost all bone in the early deposits is burnt.

this pattern changes through time needs to be tested further. One very striking field impression at both Nunamira Cave and Bone Cave is that bone is more fragmented and more frequently burnt in the earlier layers. This has been quantified for Nunamira Cave. As can be seen from Figure 6a.4 the percentage of burnt bone decreases through time while the average fragment weight increases. Using a different measure, Figure 6a.5 reflects the same decrease in burning of bone. These changes take place in Nunamira Cave in the period 24 000–21 000 years ago, as the cold intensifies, and at about the same time in Bone Cave. The M86/2 sequence reflects the more recent Bone Cave and Nunamira Cave configuration.

A striking aspect of this configuration is the consistent and regular breakage pattern of the wallaby marrow-bearing bones. Long bones were systematically smashed, resulting in helical fractures to the diaphyses, while metatarsals and phalanges were split longitudinally. Such patterns conform to experimental and ethnographic studies of bone marrow extraction (Noe-Nygaard 1977; Binford 1981: 148–61; Johnson 1985; Lyman 1987; Todd & Rapson 1988). It is presently unclear what the distinctly different earlier and later patterns of bone refuse may mean. Since this change occurs in both Bone Cave and Nunamira Cave, some 25 km apart, it is not site specific. Also, because both sites are small, it is unlikely to be merely a coincidental pattern of activity area changes in each of the sites. Because the same species is involved throughout all the sequences, the change is not due

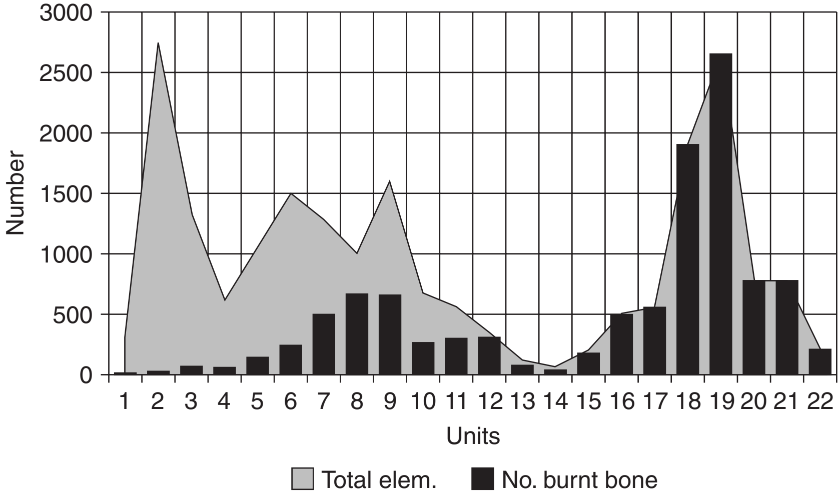

Figure 6a.5: Nunamira Cave: changing character of bone assemblage over time. The stratigraphic units forming the horizontal axis are numbered from the top of the site downwards, so deposits are older as one moves to the right. The vertical axis gives the absolute number of bone fragments from each unit, and the absolute number of those which are burnt.

to different exploitation of different species. It would seem to us to reflect either a general change in site use over time or a specific change in the processing procedures and economic utility of M. *rufogriseus* over time. While it would be convenient for our argument to be able to suggest the intensification of marrow extraction processes through time (see discussion below), the earlier smashed and burnt bones do not suggest any less complete or less efficient use of marrow. More recent, more specific marrow targetting might be implied if it can be shown that marrow-bearing bones were selectively returned to sites in the upper levels, but not in earlier ones.

- No bones of extinct giant marsupials have been recognised in any of these assemblages.

Stone industries

While the characteristic stone tool types originally used to define the 'Australian core tool and scraper tradition'—single platform, steep-sided cores ('horsehoof cores') and 'steep-edged', 'flat' and 'notched' scrapers (Bowler *et al.* 1970: 49–52)—could be extracted from some of the site assemblages reviewed here, the individual assemblages also exhibit much variability not encompassed in this blanket description. Some of this variability stems from the availability and physical reduction of different raw materials and will require a good deal of analysis to quantify in detail. Here we focus on some of the inter-assemblage similarities and differences which combine sites into a southwestern Tasmanian Pleistocene province but which also indicate distinctions between them. While this discussion moves us towards the notion of a southwestern stone *industry*, perhaps as a regional variant of the Australian core tool and scraper tradition, the distinctiveness of the faunal exploitation pattern seen in these sites is matched by the distinctiveness of the stone assemblages.

Stone raw materials exemplify inter-assemblage similarities and differences. Quartz is the predominant raw material found in western sites in the southwest, comprising 97% of the artefactual stone from M86/2. Quartzite, and tiny amounts of chert, crystal quartz, hornfels and silcrete also occur in these sites. In the eastern sites quartz is very uncommon; instead, Nunamira Cave reflects the local availability of fine-grained cherts, quartzites, silcretes and hornfels. Similarly, in Bone Cave fine-grained quartzite tools predominate, the raw material source being the river in front of the cave. Crystal quartz, found in the Weld Valley, is also common in this site. This last material is travelling north to Nunamira Cave, while hornfels and silcrete are coming into Bone Cave from the north and possibly the east. While sourcing studies designed to test these propositions are only now commencing, these preliminary identifications suggest that local sources are predominantly used and that similarities in raw material distributions in these sites will diminish with distance. Thus Bone Cave and Nunamira Cave are more similar to each other in respect of stone raw materials than to the western sites, although the tiny amounts of chert, hornfels and silcrete in these latter sites may eventually be shown to derive from the east. We may therefore be looking at the incidental movement rather than the deliberate transfer of some or many of the stone materials within the overall region; even so the very number of these types of raw materials may in the future provide a measure of association between sites and therefore the patterns of human movement between them.

Intentional movement of one stone material does seem likely. The meteorite impact site in the western part of the southwest, known as the Darwin Crater (Fig. 6a.3), is the source of the impactite Darwin glass (Fudali & Ford 1979). This material has so far been found in sites in six southwestern river valleys, and in all the sites discussed here with the significant exception of site ORS 7. So far M86/2 has yielded 10 pieces, 5 pieces have been recovered from Nunamira Cave, and a single piece was found at Bone Cave. At 100 km from the source, Bone Cave is currently the most distant site containing Darwin glass, and this straight-line distance may have been doubled by the most convenient ground route.

Among the largely amorphous stone industries in these sites, one artefact type is distinctive. The small, round 'thumbnail' scraper noted in Kutikina (Kiernan *et al.* 1983: 30) is a common element in all of the excavated sequences except in site ORS 7, where there are none. In the western sites it is made exclusively of milky quartz, while in Nunamira Cave and Bone Cave it is made almost equally exclusively of fine grained chert (Fig. 6a.6). Beyond this distinction, however, it is an unusual artefact in Australian Pleistocene stone industries simply because it is a tool type likely to possess

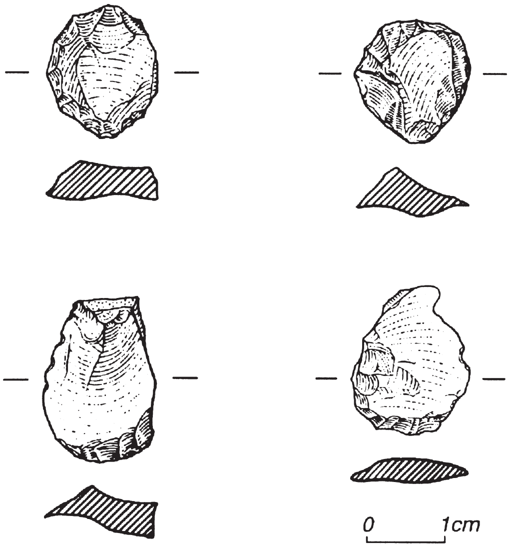

Figure 6a.6: Thumbnail scrapers from Bone and Nunamira Caves.

Table 6a.2: Dates of introduction of Darwin glass and of thumbnail scrapers to four sites

	Kutikina	*M86/2*	*Bluff Cave*	*Bone Cave*
Darwin glass	*c.* 17 000	19 670±340	>24 190±410	[insufficient data]
thumbnail scrapers	*c.* 17 000	18 290±290	21 410±240	<23 130±460

both spatially and temporally restricted distributions. It also distinguishes these assemblages from the wider Australian core tool and scraper tradition.

Jones (1988: 36) dates the appearance of both Darwin glass and thumbnail scrapers in the Kutikina sequence at *c.* 17 000 BP. The newer evidence does not support the notion that these introductions were simultaneous nor that the appearance of thumbnail scrapers reflects an 'artefactual disconformity at the assemblage level' (Jones 1988: 36; 1989: 770). Table 6a.2 lists the presently known introduction dates for these items in the sites being discussed. The discrepancies between sites seem less likely to reflect real temporal variations than the inadequacies of our present samples.

Neither Darwin glass nor thumbnail scrapers occur in the earliest levels of any of these sites. Understanding whether the former conferred technological advantage, or carried some other behavioural meaning, is a future task—as is determining the function(s) of the latter. While thumbnail scrapers may be reflected in Tasmanian Holocene assemblages, such as at Rocky Cape and Sisters Creek (Jones 1965: 195, 197; 1966: 7), the source for Darwin glass possibly became inaccessible and/or was dropped from the raw material repertoire.

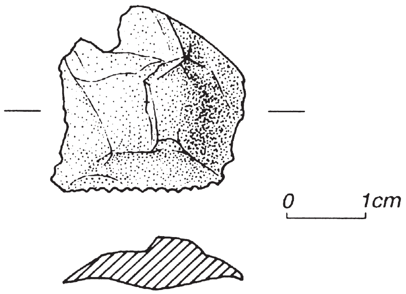

Figure 6a.7: Denticulate flake from Bone Cave, >23 000 years BP.

In terms of wider distributions, Bowdler (1984: 122) reports a quartz thumbnail scraper from Cave Bay Cave site at *c*. 19 000 BP, and Wright (1970: 87) recovered 18 thumbnail scrapers, 16 made on quartz, in his Green Gully excavation near Melbourne, Victoria. These river-terrace deposits, poorly dated, are thought to be older than 8000 years BP (Mulvaney 1975: 172). Whether these associations are fortuitous or not, neither of these latter sites suggest other obvious connections or reflect behaviour patterns like those in the southwestern sites. Thumbnail scrapers appear more widely in Australian Holocene contexts, although detailed analysis might separate the Pleistocene and Holocene groups (Wright 1970: 87).

A single and atypical flake with a denticulated edge, from a Bone Cave level older than *c*. 23 000 years BP, is the first indication of simple pressure flaking seen in Tasmania (Fig. 6a.7). This technique is rare in Australian Pleistocene industries, but a very similar piece from a *c*. 20 000 years BP context at Burrill Lake shelter on the New South Wales coast is illustrated by Lampert (1971: 52). The five Pleistocene examples of such tools from this latter site include three with double edges and have been designated 'saws' (Lampert 1971: 28). Two others have been recorded from Devil's Lair in Western Australia dated to 12 000 years BP (Dortch 1984: 52). Later Holocene examples of similar tools have also been reported (White & O'Connell 1982: 70).

Bone tools

Bone points and other bone tools from M86/2 and Bone Cave have been described by Webb & Allen (forthcoming) who propose that these tools had a systematic usage and importance in the activities undertaken in some of these sites. Suggested uses include skin processing activities, cloak toggles, marrow extractors and possibly spear points. Interestingly, no bone tools were found in the Nunamira Cave excavations.

Discussion

In our model we argue that the ecological make-up of the southwest at any one time during the late Pleistocene provided a potentially concentrated, sedentary animal resource. At first glance, the faunal evidence from all excavated sites suggests this, with high concentrations of smashed bone mainly from one species, the red-necked wallaby. It is of importance that the proposed model for the region also predicts a juxtaposed mosaic of ecotones. In this case, there is a general expectation for species richness and diversity to be relatively high (Wiens 1985: 184; King & Graham 1981), especially in patches of well watered, grassy microenvironments surrounded by scrub and low lying trees. It might be expected that human exploitation of a part of that range of habitats available would still produce the remains of an array of animal species. Although this is true to some extent in Nunamira Cave, Bone Cave and M86/2, the numbers of systematically exploited species are few. The availability of the emu and its apparent low-level exploitation at Nunamira Cave is a case in point. In this instance there appears to be no necessary correlation between those resources that form the subsistence base and the predicted range of resources available.

Emu eggshell suggests the seasonal human presence in at least at one cave site, corresponding to the most stressful period of the year, namely late winter-early spring. It has been shown that for hunter-gatherers living in high latitude, temperate, subarctic and arctic zones during late winter and early spring, diets become marginal and inadequate especially when lean meat is relied upon for energy (Speth & Spielman 1983). Because of higher metabolic rates, correspondingly higher calorific needs and a deficit in fatty acids, the physical condition of humans and other animals at this time is reduced, especially in sharply seasonal environments (Speth & Spielman 1983: 2). In the absence of plants in late winter, the reliance on a purely protein-rich diet can have detrimental effects on hunter-gatherer physiology (Speth 1987; Noli & Avery 1988).

Wallaby meat, like kangaroo, is extremely lean, with limited fat deposits around the kidneys, in the marrow and on the back and tail (Sinclair 1988; O'Dea 1988). Driessen (1988: 16) noted a significant drop in the kidney fat of female red-necked wallabies during the winter period and lower kidney fat deposits in individuals inhabiting dry areas than those in wetter zones. Males on the other hand put on condition towards the end of winter, presumably in readiness for mating (Driessen, pers. comm.). Field studies suggested that deposition of kidney fat was also related to pasture quality. The evidence suggests that wallaby populations living in higher rainfall areas on more fertile ranges appear less stressed and have an enhanced physical condition. Females in lower rainfall areas have fewer young at foot than those living in wetter zones (Driessen 1988: 23). The moister habitats in the west of Tasmania during the late Pleistocene may have been crucial to the aggregation and maintenance of healthy animals in southwest Tasmania.

A further advantage that may have been bestowed on the consumers of macropod meat is its potential to dilate the blood vessels and increase blood flow in response to cold. O'Dea (1988) has found that experimental human subjects fed on a diet of kangaroo meat showed increased levels of arachidonic acid in their plasma. This acid is 'believed to modulate the thrombosis tendency by affecting platelet aggregation and blood flow' (O'Dea 1988: 142). For subjects given a diet of lean southern fish the opposite occurred, with a reduced blood flow in response to cold. O'Dea cautiously concluded that the presence of such long chain polyunsaturated fatty acids (PUFA) may protect against thrombosis. Although it is unknown whether the Pleistocene Tasmanians had inherently higher metabolic rates such as those found in contemporary Eskimo and Patagonian populations (Kirk 1983: 158), the presence of PUFA might have had a dietary advantage for humans living in high latitude, glacial Tasmania.

The availability of lean meat does not, however, solve the dietary gaps created by the late winter and early spring seasonal fluctuations and the absence of plant carbohydrates in what was then a subantarctic environment. The possible availability and use of plants such as the small daisy yam *Microseris scapigera*, now present in Tasmanian subalpine grasslands (Jackson 1973: 71), cannot be ruled out (Bowdler 1981: 104). No evidence for plant use is preserved in any of the sites.

The lack of animal fat and carbohydrates in the diet would increase the likelihood of protein poisoning (Noli & Avery 1988: 396). A potential solution to this dilemma is the intensive processing and extraction of bone marrow for consumption as it contains essential fatty acids such as linoleic acid needed in the metabolism of protein while the inclusion of fats and/or carbohydrates enhances the effects of protein-sparing (Speth & Spielman 1983: 13):

> Protein-sparing is an important nutritional consideration when ingested protein is being utilized for providing energy. Since the body's needs must be fulfilled before protein needs can be met, under conditions of marginal or inadequate caloric intake, the amino acids of ingested protein are degraded, and the nonnitrogenous residues are converted to glucose or fat or are oxidized directly to meet the body's needs. This utilization of amino acids for energy makes protein unavailable to the body for its normal uses, and thus body protein is not replenished. Under conditions of severe caloric shortage, skeletal muscle protein will also be broken down to provide glucose for organs that do not use fat for energy.

The body parts which are well represented and systematically broken in the sites are wallaby tibia, femurs, metatarsals and humeri. In ungulates and kangaroos these elements contain relatively high levels of marrow (Binford 1978: 152, 188; 1981: 150; O'Connell & Marshall 1989; Jones & Metcalf 1988). This technique, at a high risk time of the year, might enable the dietary gap between winter and summer to be bridged.

It is unclear at present why there was preferential selection of one species at the expense of others such as wombat, the much larger grey kangaroo, pademelon and the emu—all grassland species known to be present in the river valleys, particularly the Florentine, from their minor presence in limestone pitfalls and caves. The long bones of these animals contain enough marrow to be an attractive addition to the red-necked wallaby for exploitation. The male wallabies on better-watered pasture gain in physical condition towards the end of winter, and this may have been a key factor in their exploitation. Horton has also suggested that hunting success decreases because of these larger animals' unpredictable and nomadic behaviours, their larger ranges and their smaller group make-up (1981: 23). On the basis of these criteria, the red-necked wallaby would have been an attractive investment of time and labour, as the returns from this sedentary resource may have been predictable throughout the year. Although it is not known whether there were different animal compositions and abundances during the late Pleistocene, the forthcoming excavation of a natural animal pitfall in the Florentine River valley may shed some light on this question.

The resolution and assessment of palaeo-diets is, however, difficult, even with richly preserved food refuse and/or stable-carbon isotope analysis of human collagen (Collier & Hobson 1987). The latter measures diet at a specific location, while the former only reflects diet over long periods of time and almost certainly fails to indicate the total economic strategy. It is possible that the wallabies performed an important role as a relatively sedentary food source at stressful periods in the subsistence round and in this sense could be seen as a glacial, middle-latitude animal correlate of 'low-key, dependable vegetable resources' (Bowdler 1981: 100). Although we recognise that there is a danger in overemphasising terrestrial animals in the diet at the expense of marine and plant foods, it is equivocal at present as to what role these latter items played in the total southwest Tasmanian Pleistocene economy. In addition, the recovery of bone tools adds another dimension to the processing and manufacturing activities at these caves.

This suggests that these sites, far from being the product of limited activities and 'transient economies' (Lourandos 1983: 88), show a degree of economic structuring not yet recorded elsewhere in Pleistocene Australia. These signatures are different from Pleistocene sites so far excavated in southeastern Tasmania and appear to have their own unique behavioural pattern. It is unreasonable to propose this latter case as representing the likely behaviour of Australian Pleistocene human populations (Lourandos 1987: 158). In addition, the southwest residues are very different from those found in widely dispersed Pleistocene inland karst cave sites around Australia such as Cloggs Cave in Victoria (Flood 1980: 254), Devil's Lair in Western Australia (Dortch 1984), Walkunder Arch in North Queensland (Campbell 1984: 176), and Colless Creek in Western Queensland (Hiscock 1984). The Tasmanian Pleistocene southwest cave deposits are in many ways as different from these sites as late Holocene southeastern Tasmanian sites are from those of similar age on the mainland. It may be time to look more closely at the subtle differences between early Australian sites rather than continuing to highlight the similarities of late Pleistocene residues and by implication the continuity and unidirectional vectors of Pleistocene human behaviour.

Conclusion

Preliminary investigations of archaeological sites in south central Tasmania have demonstrated that humans in the late Pleistocene occupied the upland periglacial areas of

Tasmania at least 30 000 years ago, some 10 000 years earlier than previously supposed from sites in the western valleys of the southwest. Results from two sequences (Bone Cave and M86/2) support findings and conclusions previously advanced for the sites of Nunamira Cave and site ORS 7 (Cosgrove 1989). The high densities of archaeological remains in these sites, comprising rich faunal assemblages of large identifiable bone, distinctive stone tools and location-specific stone raw materials, already enable us to distinguish the Pleistocene southwest Tasmania from other Pleistocene regions in Australia. We can also distinguish eastern and western zones in the archaeological signatures within this Pleistocene province. We have thus established an archaeological basis for examining inter-site associations both within the province and across its boundaries.

This is merely one avenue of enquiry which the rich and distinctive nature of the archaeological record in southwest Tasmania will allow us to pursue. Others for future exploration are as diverse as investigating economic management strategies by studying the death age distributions of wallaby, or techniques of capture (e.g. netting and snaring versus selective spearing), to the testing of models of food sharing and hunting group compositions by prey body-part analyses.

For the moment we are concentrating on further testing the ecological model put forward in this paper, which we see as best explaining the extreme climatic locations and the concentrated and repeated exploitation of a particular animal species—two of the more important characteristics of these sites. Among the implications of this model, if accepted, is that the pattern of humans placed in the landscape to intercept migrating herd animals, a model frequently encountered in northern hemisphere Palaeolithic reconstructions, was reversed in Tasmania. Here, in our model, humans moved between discrete grassland patches to hunt 'ecologically tethered' animal resources. The implications of this strategy are far-reaching, suggesting for example that culling practices were employed 30 000 years ago which allowed wallaby populations to maintain themselves under the ecological constraints of changing environments and human predation for nearly 20 millennia. Such ideas challenge notions of Pleistocene (foragers) behaviour in Australia. We see in our favour that our model and interpretations derive from three independent and data-rich disciplines; we have combined current evidence from botany and zoology with the artefacts in these southwestern sites to suggest that Pleistocene hunters in upland periglacial Tasmania were not merely concerned with the random exploitation of a meat resource. Rather, the data force us to confront notions of deliberate organisation and strategies which reflect risk-lowering subsistence techniques in a food-productive and seasonal environment.

This model is not without its problems. For example we do not yet understand the place of marrow extraction in the perceived strategy, nor how it may have changed through time—particularly if other food sources, such as fat-rich seals, were available on the coasts during the Pleistocene winters. At present we do not have all the answers, nor yet even all the questions. Southwestern Tasmania has the archaeological resources to provide many of both.

Acknowledgements

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7 Revitalising artefact analysis

Peter Hiscock

Throughout this century Australian archaeologists have debated the nature of types and the efficacy of typological classification. In each decade this debate has been manifested in a slightly different form, reflecting contemporary concerns and the current problems in explaining assemblage variation. Hayden's (1977) paper from the 1974 Australian Institute of Aboriginal Studies conference is one of a number that accompanied a new phase of debate about the interpretation of implement form and assemblage variation. He used ethnographic observations of stone use by contemporary people as his means of addressing this debate, as did a number of other researchers whose influence rivals Hayden's (e.g. White 1967; O'Connell 1977; Gould 1966, 1980). Nevertheless, while Hayden may have seen his work demonstrating the value of ethnoarchaeology, the continuing importance of the paper lay in the re-evaluation of conventional views of prehistoric implements. The issue upon which he was focused was the interpretation of the morphology and relative abundance of traditionally recognised implement types. Because descriptions of archaeological materials had often been confined to identifying implements, and the culture historical conclusions based on the presence or abundance of those implement types, the interpretation of Australian prehistory was tied to the interpretation of implements. In the 1970s the most common interpretation of implements presumed that specimens of each implement type were created according to strict guidelines of style, designed and used as functionally specific tools. Hayden (1977: 178) characterised this view as

> . . . the still pervasive and subjective feelings among archaeologists that stone tools could not simply have been used in a totally profane or simple minded fashion. Although this is a subjective evaluation I think that most archaeologists, somewhere in the seat of their limbic systems, *feel* that most stone tools were carefully crafted, and that the stone tool maker was doing his best to make a tool worthy of his ancestors, or himself, or his group, or something else. Perhaps more importantly, archaeologists expected that prehistoric men were striving after the particular form that they crafted, and that all else was waste, or 'debitage'.

Hayden's observation of artefact manufacture and use led him to doubt such notions, and accept instead a number of propositions that were to him surprising. Four 'surprises' have been influential and illuminate the debates about artefact interpretation:

1. Stone artefacts may have been treated in an entirely profane manner by their makers.

2. Formal implements, typically retouched flakes, may be poor indicators of the range and frequency of artefact use in an assemblage. Implements may constitute only a small proportion of the artefacts in an assemblage that were actually used, and a number of different morphologies could all be employed for the same function.
3. Retouching is often a means of rejuvenating a dysfunctional edge, rather than an attempt to produce an ideal form from the outset.
4. Factors that condition the form and abundance of retouch need to be better defined, but are most likely to involve raw material properties, raw material availability, and the form of hafting.

Each of these related points contributed to a coherent view of prehistoric stone artefacts as a largely mechanical response to the economics and human ecology of hunter-gatherer life. In a number of guises this view developed in the 1980s and 1990s as an alternative to the stylistic explanations of archaeological assemblages (e.g. Hiscock 1994a). The history of these debates in Australia, and their most recent expression, is worth examination.

Artefact manufacture is profane

Implement types in Australia have often been distinguished from other stone artefacts on the basis of the extent of retouch and their standardisation of form. In the absence of a technical understanding of artefact manufacture early typologists saw these features as a direct reflection of the intention of the knapper (e.g. Etheridge and Whitelegge 1907: 237). The regular retouch and repeated shape of objects recognised as implements was seen to occur because these were end products, completed in accordance with some design. Early classification systems all reveal this notion (see Etheridge 1891; Howchin 1893, 1934; Kenyon and Stirling 1900; Kenyon and Mahony 1914; Noetling 1907), and the expectation that distinct classes of implements would have been made is clearly observable in correspondence from the period (see Wright 1977). In the early decades of this century typological descriptions and analyses were primarily aimed at identifying the functions of each type (see below). However, while the inferred uses of these artefacts were often profane, the image of these implements as specially and laboriously shaped for some purpose imbued each typology with the implication that those specimens retained particular meaning for their makers.

The assertion that regular and complex forms implied a standard design led some researchers to question how regular and complex forms had to be before intentionality was apparent. Consequently, as a primary division some classifications differentiated between standardised and irregular forms. For example, Noetling (1907) divided implements from Tasmania into two groups: those with bifacial retouch, which he called 'Morpholithes', and those with unifacial retouch, which he dubbed 'Amorpholithes'. The former group were considered by Noetling to be intentionally standardised hafted tools, while the latter group were seen as being unsystematically created and used as unhafted, hand-held tools. Similar divisions are found embedded in other typological systems of the day.

Debates about whether intentionality/meaning was a necessary correlate of a regularity of form, and if so how regular the form must be, are found in a number of publications prior to the mid-1930s. Two opposing viewpoints are revealed in the literature. One view was that intentional designs were poorly reflected in implement form. Kenyon (1927), for example, argued that implement form was largely determined by raw material properties; while Towle (1930) viewed retouching solely as a means of resharpening edges, rather than shaping desired forms (see below). The contrary view was displayed by Tindale (1932) in his criticism of Howchin's (1921) claim that 'crude implements' of great antiquity had been found in gibber areas of central Australia. Tindale's evaluation of these objects involved equating artificialness with the shaping that created implements that evidenced design, and the expectation of a high degree of standardisation in implement types (see Howchin 1933: 7–8). It was Tindale's more rigid notion of implement classes that was most influential during the 1940s, 1950s and 1960s.

With the pursuit of intellectual frameworks that would explain chronological change in implement types, initiated by Hale and Tindale (1930), these repeated artefact forms were taken to reveal designs which had a social significance to the maker. Increasingly during and after the 1940s, McCarthy (1947, 1948, 1949, 1953, 1958, 1963, 1964, 1967) and Tindale (1957, 1961, 1968) employed a concept of implement as not only designed end products but as items embedded with social meaning that could reveal contact between, and developments within, ethnic groups. Consequently, diffusion of traits or migration of groups into Australia from the north was often emphasised over internally generated changes, with McCarthy (1953: 257) stating that 'invention, as such, is not a feature of Aboriginal culture', and Tindale (1957: 39) concluding that '. . . probably we are dealing with culture shifts in terms of tribal displacement as well as in part changing implement fashions . . .'

In this context standardised typologies were seen to be the only way to correctly identify 'archaeological cultures' through inter-site comparisons (Tindale 1968: 628; McCarthy, Brammell and Noone 1946: 1–2; McCarthy 1958: 181). Changes to classificatory systems were discouraged, with Tindale (1968: 628–30) advocating a system of Linnaean-like nomenclature in which new classes of implements would be accepted only when a detailed illustration and description of the 'type specimen' was published. The rigidity of the classificatory systems reinforced the notion of rigidly defined and readily distinguished implement types.

Beginning in the late 1960s a number of archaeologists observed that the manufacture of flaked stone artefacts by Aboriginals and New Guineans was neither careful and standardised nor did it hold great social significance for the artisans (e.g. Gould *et al.* 1971: 163; White 1967, 1968, 1969; White and Thomas 1972). Hayden's paper on artefacts from the Western Desert was therefore one of several that began to again query the meaning of implement types, and by dedicating the paper to a consideration of these ‘surprises’ it promoted active consideration of the issue throughout the late 1970s and 1980s.

The reappearance of this debate over the nature and interpretation of typological forms has seen opinions polarised. On the one hand researchers emphasising the social/symbolic role of implements have seen implement forms as symbols or identifications (e.g. Johnson 1979: 144), symbols of social reorganisation (e.g. Bowdler 1981: 110), or stylistic phenomena (e.g. White and O’Connell 1982: 125). On the other hand, researchers heeding Hayden’s message that stone artefact manufacturing may be profane have emphasised the economic/functional aspects of implements (e.g. Bird 1985; Byrne 1980; Hiscock 1988, 1993, 1994a; Kamminga 1982; McNiven 1994). One significant consequence of the view that artefact manufacture is a profane economic activity is that artefacts that have been used (i.e. tools) will not necessarily be specially shaped to a preconceived form, and hence will be difficult to identify on the basis of morphology alone. Implications of this realisation are manifested in the other surprises described in Hayden’s paper.

Implements as a measure of function

> That special kinds of implements were shaped for particular purposes must be taken for granted . . . (Howchin 1934: 22)

Acceptance of this notion led to early classifications, particularly those by Kenyon and Stirling (1900), Kenyon (1927), and Howchin (1934), being aimed at functional descriptions based on the proposition that each implement form was indicative of the function for which it was designed. This inclination to functional descriptions is most obvious in the labels that were used, with classes being described as ‘knives’, ‘chisels’, ‘scrapers’ and so on. Later classifications, particularly McCarthy, Brammell and Noone (1946), McCarthy (1967) and Mitchell (1949), clearly constructed their types on the basis of perceived morphological distinctions in artefact assemblages, although the expectation that implement classes reflect functional classes remained.

Indications that implements were not an effective measure of site activities and artefact use were noted by a number of authors. Among the criticisms were observations that some specimens of the type were not capable of functioning in the suggested way (e.g. Kenyon 1927: 283), that the retouching which shaped the implement was not necessary for that function (e.g. Towle 1930: 11), that specimens within the same implement type may have had different uses (e.g. Howchin 1934: 22), that Aboriginals often produced tools casually and without creating them in specific shapes (e.g. Horne and Aiston 1924), and that variations in form could be explained by reference to other factors, particularly raw material properties.

Nevertheless, interpretations of function based on implement form were often very persistent. For example, although Noetling (1911) correctly explained fracture features such as bulbs of force and eraillure scars as mechanical products of the manufacturing process, these interpretations were rejected in favour of functional ones. Hence Horne (1921: 185) viewed eraillure scars on scrapers as 'thumb grips' designed to enhance use, and Legge (1927: 28) reinterpreted Noetling's hammerstones as 'pounders' for breaking shells and bones.

The hope that simple macroscopic examination of the characteristics of implement types would provide a direct insight into prehistoric tool use has continued into recent decades. For example, regional prehistories have often discussed inter-assemblage differences in implement percentages as a direct reflection of different toolkits, perhaps associated with seasonality of site occupation (e.g. White and Peterson 1969; White 1971; Allen 1974). Occasionally the underlying proposition, that most tools were implements, each type with a standard function, was tested or refined by innovative approaches, such as numerical comparisons of variations in implement types and vertebrate fauna (e.g. Clegg 1977; Bowdler 1981). Following the lead of Mulvaney and Joyce (1965) and J.P. White (1969), one attempt to link implement form and function involved detailed study of the characteristics of edges assumed to have been used (e.g. Ferguson 1980; but see Hiscock 1982). More dramatically, some researchers attempted to maintain the direct equivalence of implements and tools by denying Hayden's observations of the frequent use of unretouched flakes. For example, Cane (1984, 1992) used statements by Aboriginal informants to argue that there was a significant correspondence between archaeologists' implement classifications and Aboriginal ethno-taxonomy of tools. He concluded that since some contemporary people did not consider flakes to be useful they were in prehistoric times merely manufacturing by-products, and that formal implements constituted the bulk of stone artefacts that were used. However, the problem with that conclusion was determining a) whether the stated ethno-taxonomy is matched by the behaviour of artefact making or use, b) whether the contemporary system described by Cane is the same as precontact behaviour, and c) whether the minimal use of flakes was a widespread pattern or a regional aberration. The only obvious means of choosing between the models proposed by Hayden and Cane was to directly examine prehistoric artefacts, implements and non-implements alike, for physical evidence of usewear.

A direct test of Hayden's propositions, through usewear analyses, did not occur immediately. Initial application of usewear approaches to prehistoric Australian assemblages reveal the strength of the presumed connection between implements and artefact use. Throughout the 1970s a number of researchers, and particularly Kamminga (1977; 1978; 1980; 1981; 1982), attempted to use the new microscopy-based usewear approach to determine the functions of the implement types defined by earlier typologists. Although Kamminga (1978: 353) was aware that the analytical technique could be applied to complete assemblages, he assigned priority to understanding the functions of implement types, a strategy which fulfilled the imperatives of earlier decades, but did not test or develop the notions advanced by Hayden. It was not until the following decade that an entire assemblage was analysed by Fullagar (1982), who demonstrated that typologically recognisable implements constituted a small proportion of the artefacts which had been used prehistorically, making it reckless to rely on them alone for functional interpretations of the assemblage. For example, at the Aire Shelter II site in Victoria Mulvaney (1962) had only been able to identify 11 artefacts with macroscopic retouch or edge damage, representing 0.7% of the assemblage; but in a usewear study of the entire assemblage Fullager (1982: 75) discovered that 13.3% of the collection contained evidence for use, and that most of these artefacts were unretouched flakes. This kind of usewear research not only verified Hayden's position on the limitations of implement typology for inferences concerning function, but also stimulated investigations into the interpretation of retouching.

Retouching as rejuvenation

> It should be emphasised that this secondary retouch was done with the aim of 'resharpening' or rejuvenating a dulled working edge into a more suitable one. (Hayden 1977: 179)

The notion that retouching of flakes was not necessarily aimed at creating a standardised predetermined form had been raised by Australian researchers on a number of occasions. For example, in following Kenyon's critique, Towle (1930: 11) argued that retouch was primarily employed to treat working edges which would become blunted after 'a few strokes'. For Towle steep retouch on artefacts, including the backing on backed blades, was not intentional blunting but simply an edge which had been re-sharpened to the point of uselessness. He phrased this argument as follows:

> This edge would soon become blunted in use and, if the flake were of good material, the aboriginal workman would retouch it sufficiently to maintain its usefulness. This process would be carried on as it became necessary, until, at length, the edge would become too blunt for further treatment. The implement would then be discarded. Developed in this manner, the supposed 'chipped back knife' becomes nothing more than a discarded flake which has served its purpose in use. (Towle 1930: 6)

The application of this argument to backed blades failed to convince other researchers at the time and remains unconvincing, although the mechanism has been applied to other implement types. A similar argument was advanced by Cooper (1954) for the transition of a Tula into its slug form, with supporting evidence in both the morphology of the artefacts and detailed ethnographic observations of artefact manufacture and use (see Horne and Aiston 1924; Roth 1904). This depiction of the progressive reduction of a tula due to resharpening continues to prove useful in archaeological investigations (see Hiscock 1988; Hiscock and Veth 1991). And from the early 1950s onwards researchers in northern Australia, such as Macintosh (1951), debated whether variation in the form of bifacial and unifacial points was explained as different functional types or different phases of manufacturing (see Hiscock 1994b). These perceptions, of extensive resharpening leading to directional morphological change, have been increasingly discussed over the past two decades, as they have been overseas (e.g. Dibble 1987). Morphological transformations have also been recognised in grindstones as they are used and reduced (e.g. Cundy 1985; Smith 1985).

Since Hayden's paper an equally important, and related, issue has emerged, namely the degree to which conventional typological classifications may have confused unfinished manufacturing forms with end products that have been used. This possibility has been raised in several contexts. For example, a number of researchers, including Kamminga (1982: 85–91), Binford and O'Connell (1986), and Flenniken and White (1985) have concluded that horsehoof cores are not implements/tools but simply exhausted cores (see Akerman 1993 for an alternative view). Identical arguments have challenged the interpretation as implements of other core types (e.g. McNiven and Hiscock 1988). Another example is Hiscock's (1993) argument that in the Hunter Valley artefact forms traditionally recognised as burins or scrapers may in fact be equivalent to stages of core preparation and not end-products. These arguments revisit the question asked earlier in the century, about the correlation of regular form and intentionality, and may be seen as a necessary outcome of questioning the interpretation of retouching primarily as a means of shaping an implement to a predefined form. Consequently, one trend over the last two decades is the attempt to describe and explain the structure of the entire manufacturing process, rather than the form of the purported end product alone.

An expectation of these processes is that the different phases of manufacture and resharpening might be spatially separated across the landscape, with use and consequent resharpening increasing as artefacts are carried and used. This mechanism was discussed by O'Connell (1977), who saw it as a key factor creating assemblage variation in central Australia (see below). Since the early 1980s a number of authors pursued archaeological studies of the effects of distance to rock source on the frequency of retouch (e.g. Gould and Saggers 1985; Bird 1985; Meehan *et al.* 1985; McNiven 1993). One of the most outstanding was Byrne's (1980) simple yet powerful illustration of the increased frequency of retouch away from a silcrete quarry in Western Australia. He concluded that this was consistent with the creation and maintenance of usable edges in contexts where replacement stone was unavailable. Further studies found comparable patterns where the geological structure of the landscape allowed for estimation of access to replacement stone by distance measurements, and even highly standardised implements, such as points, display this distance-related morphological change in some contexts (Hiscock 1994b). Hence spatial analyses of assemblage variation have reinforced the proposition that heavily retouched flakes may have been worked, not to a predetermined form, but gradually as required by need. Since Hayden's paper this consideration has formed part of a broad consideration of the factors that condition retouch.

Factors that condition retouch

Publication of Hayden's paper, and articles in the same volume by O'Connell (1977) and Binford (1977), signalled the emergence of a new, and broader, cycle of discussions about the factors that determine the abundance and pattern of retouching on stone flakes, and consequently the composition of assemblages and the structure of assemblage variation. Earlier in the century extensive consideration had sometimes been given to the causes of shape differences within and between implement types. Perhaps the most common explanation offered, besides the assertion that the forms represented predetermined designs, was that the forms were reflections of raw material properties, the size of flakes being retouched, and/or the use of the item (e.g. Kenyon 1927: 282; Spencer 1914: 77; Towle 1934: 137). Mitchell (1949: 4–5, 7, 104) took this argument to the extreme, suggesting that such factors could explain all variation in implement form, removing any need to posit a deep antiquity for humans in Australia or chronological changes in Aboriginal material culture. For Mitchell, all implement and assemblage variation indicated spatial rather than temporal factors. This proposition provoked a sharp response from McCarthy (1949: 307), who concluded that

> . . . the hypothesis that material controls the form of all our implements cannot solve the problems of Australian prehistory, and it must now give way to the broader cultural interpretation. (McCarthy 1949: 307)

This cultural interpretation involved the introduction of new people or new designs of implements from outside Australia on a number of occasions (McCarthy 1949: 306, 316–17).

Re-evaluation of this approach has stemmed from a rejection of the presumed functional implication of interpreting implements solely as a manifestation of preconceived designs. As mentioned above, assemblage differences were frequently interpreted, throughout the 1970s, primarily as differences in site function, with the proportional frequency of a type being taken as a direct indication of that type's function as a site activity. A key challenge to that model was presented by Jim O'Connell (1977) in a paper that specifically aimed to testing and refining White and Peterson's (1969) explanation of assemblage variation as a reflection of seasonal differences in occupation, and hence site function. Working with sites of known season he concluded that the variation in artefact assemblages was not reflecting the pattern of site function or season of occupation, and that

> . . . a substantial amount of interassemblage variation may be the result of differences in access to material used in manufacture of tools and of particular characteristics of these materials as they affect the forms of implements. (O'Connell 1977: 280)

This proposition is supported by Hayden in the paper reproduced here. Consideration of retouching frequency and form in terms of not only raw material properties, but also the economic and logistical context in which knapping is situated, provides a more powerful framework than was available to earlier researchers interested in the relationship between implement form and rock type. Publication of '47 trips' by Binford (1977) in the same volume as the papers by Hayden and O'Connell reinforced the perceived capacity of this economic and contextual emphasis.

From the late 1970s onwards there has been increasing emphasis on issues of access and stone availability as key factors in assemblage variation (see Byrne 1980 and discussion above). Researchers have attempted to relate access to a range of characteristics of settlement systems, including the level and structure of residential mobility, structure of the environment, familiarity with the environment, and environmental and social barriers to access (e.g. Draper 1993; Hiscock 1994a; McNiven 1993, 1994; Veth 1993). This interest in depicting implement form and assemblage variability as a component of hunter-gatherer economies appears to mark a convergence between those employing analyses of artefacts to examine issues of human ecology, and those archaeologists who have been employing faunal or geomorphic evidence to define adaptive processes (e.g. Pardoe 1988, 1990, 1994; Cosgrove *et al.* 1990; Sullivan 1982).

Conclusion

In retrospect it is clear that Hayden's (1977) paper was part of the general re-expression of perspectives that had been discussed earlier in the century. The elements of this re-expression contain many of the features of the propositions espoused by New Archaeology in the 1970s (cf. Binford 1989). For example, the rejection of mentalist descriptions of implement form, and of assemblage variation, the attempt to formulate interpretative principles through ethnoarchaeological investigations, the focus on observations that were apparently anomalous in respect to traditional propositions ('surprises'), and the emphasis of economic/ecological/evolutionary mechanisms, are all features pronounced in the New Archaeology perspective. It is certainly possible to see a direct influence of this paper on some authors in the early 1980s (e.g. Byrne 1980; Hiscock 1983). However, while Hayden's paper was influential, it also epitomises a broader perspective that continues to revitalise the interpretation of Australian artefacts.

7a Stone tool functions in the Western Desert

Brian Hayden

Introduction

For relatively obscure reasons, chipped stone tools seem to have always fascinated at least some individuals in agricultural and industrial societies. Recognition of the special status of prehistoric stone artefacts has occurred among uneducated peasants, ignorant of the functions of these ‘thunderstones’, as well as among modern prehistorians and avid collectors who spend lives and fortunes on stones of the past. What lies behind this fascination? The totally alien nature of these objects to many modern men who wonder how such forms could have been fashioned out of rock and what they could have been used for may be one reason for the unusual attention given stone tools. Perhaps the fascination lies in the exotic shapes and curious forms: ‘flat-irons’, ‘willow leaves’, ‘laurel leaves’, ‘sumatraliths’, ‘short axes’, ‘tortoise cores’, ‘horsehoof cores’, ‘pounds of butter’ and others. Perhaps the unusual nature and aesthetic qualities of cryptocrystalline rocks are major factors. And perhaps it is simply the fact that they are virtually all that remains of an unknown and mysterious era of man’s history. Such outlooks and values are, of course, those of relatively contemporary Occidental men.

Whatever the cause—and there may be more than are hinted at here (see Harris 1968: 676)—one of the principal results has been the mystification of stone tools, and the treating of them as semi-sacred relics. Archaeology has only recently begun to rid itself of this set of values. For the first century of archaeology most analysts were content to sit back and indulge themselves in aesthetic and emotional mysteries of the past, and the successive transformations of those mysteries. What the tools were used for and how they were used was for the most part unfathomable, or pure reverie. And in many respects it made little difference to the archaeology of the day. People were concerned with tracing the movements of past peoples, the succession of cultures, the cultural history of artefacts, and the spiritual or aesthetic progression of mankind. For this, all that was required was the recording of changes in styles and types, and the improvements in craftsmanship.

I would maintain that it has been this mystification and relegation to the semi-sacred which are largely responsible for the otherwise unaccountable lack of first-hand research effort by archaeologists in the area of ethnographic use of stone tools (indeed, up until recently there almost appeared to be an avoidance relationship!).[1]

The same factors are probably responsible for the still pervasive and subjective feelings among archaeologists that stone tools could not simply have been used in a totally profane or simple-minded fashion. Although this is a subjective evaluation I think that most archaeologists, somewhere in the seat of their limbic systems, *feel* that most stone tools were carefully crafted, and that the stone tool maker was doing his best to make a tool worthy of his ancestors, or himself, or his group, or something else. Perhaps more importantly, archaeologists expected that prehistoric men were striving after the particular form that they crafted, and that all else was waste, or 'debitage'.

This attitude is apparent in a number of European site reports. For instance, de Lumley (1969: 47, 50) observes that:

> Some of the tools found at Terra Amata were probably made on the spot. The hut floors show evidence of tool manufacturing . . . The toolmaker's place inside the huts is easily recognized: a patch of living floor is surrounded by the litter of tool manufacture.

Later, he applies Jullian's remarks to Terra Amata: 'The toolmaker's seat is where one man carefully pursues a work that is useful to many.' The explicit implication is that stone tools were carefully made, and that considerable skill was necessary in their manufacture, without which the group would be in troubled straits.

In 1971, I was interested in finding out what use fully traditional, fully stone-using hunter-gatherers made of stone tools. Although there is probably no one who still uses the full complement of traditional stone tools today in Australia due to the wide availability of metal, there are men and women in contact with Occidental institutions who have used most of the traditional tools earlier in their lives. I went to Cundeelee mission (Western Australia) and Papunya government settlement (Northern Territory) and worked with older men and women who had used stone tools in their youth, and who could still make and use nearly the full range of traditional chipped stone tools. The people I worked with were from the Pintupi, Yankuntjara, and 'Wangkayi' dialect areas. Because of my Occidental cultural and archaeological background, I was to experience many surprises. This paper is a presentation of some of the more important of those surprises concerning the functions and uses of stone tools. The insights which result from this study will hopefully help to reorient prehistorians' attitudes and interpretations of what they are dealing with in their study of lithic remains from the past.

Because the hafted adze has been dealt with rather extensively by previous writers (W. Roth 1904; Thomson 1964; Tindale 1965; Gould *et al.* 1971), it will not generally be considered in the following discussion. Rather, the less well-documented and less well-known hand-held tools will be the primary focus of this paper.

Surprises

Values

I was certainly not the first to make the observation that stone tools were used in almost an entirely profane manner by Western Desert Aboriginals (Gould 1969: 81–3; Gould *et al.* 1971: 163), and the idea is perhaps easily accepted intellectually; however, it still came as an emotional disappointment to actually see stone tools being used in traditional ways. The feeling of 'is that all there is to it?' was uncomfortable. I was unsure of exactly what was missing, but I felt that there ought to be more to using and making stone tools.

There were a number of factors which led to this feeling of 'is that what I came all this way to see' Probably, the most immediately influential was the attitude of the Aboriginals themselves. They seemed uninterested in the stone they were using to the point of ignoring it except when the stone was no longer suited for continued use; they were predominantly interested in the work they were doing with the stone, such as making spears. A good analogy might be drawn between the amount of interest a contemporary Occidental person takes in the pencil he uses in writing the draft of a paper as opposed to the ideas and sentences of the paper. This lack of interest in stone was my first surprise.

Another surprise was the discovery that there were no master craftsmen of stone tool-making (this may not have been strictly true of the Warramunga or Walbiri where prismatic blade knives were produced for trade). No one was capable of controlling the stone medium to anywhere near the degree attained by the renowned stone knappers in the Occident, such as Bordes. Instead, there was only a moderate degree of control over the stone medium; suitable flakes for work were often picked out of almost random flakes. However, the flakes that were obtained were perfectly adequate to the technological needs of all task activities. A similar lack of detailed control was noted among the Nakako and Pitjantjatjara by Tindale (1965: 140, 160) and the Ngatatjara by Gould (Gould *et al.* 1971: 160) where 'cores' were often hurled at the ground or otherwise 'smashed' or flakes 'randomly' detached with a hammerstone. Suitable flakes were then chosen from the debris. Similar procedures were also used in the Kimberley area (Hardman 1888: 59) and in Tasmania as well (H. Ling Roth 1899: 151; Hambly 1931: 91). In an analogous vein, Thomson (1964: 407) remarks on the variability in craftsmanship between individuals in making wooden bowls, indicating frequent modest control over the working medium.

Rarity of 'tools'

But perhaps the biggest surprise, and 'disappointment', was the unbelievable lack, or rarity, or fabrication of what the archaeologist calls 'tools'. At first, I saw Aboriginals using only unretouched primary flakes for shaving and scraping wood, and unmodified blocks of stone for chopping wood. None of these would have been recognised archaeologically as 'tools'. I wondered if the tradition or knowledge of how to make retouched tools had been lost due to some epi-culture contact phenomenon. That was not the case. Some of the earlier observations on traditional uses of stone tools had described the same lack of retouched tools (Mountford 1941: 316; 1948; Tindale 1941; Gould 1969: 81–3; Gould *et al.* 1971: 163), and it was even proposed that the Pitjantjatjara possessed one of the world's most primitive technologies, since they did not have retouched tools, but only used naturally occurring forms (with the exception of the hafted adze).

What became apparent to me after considerable field observation was that retouched tools were indeed being produced, but relatively rarely. The reason for this may be explained in part by referring back to an axiom that Bordes has verbally expressed: namely that a stone flake is sharpest right after it has been removed from the core; any secondary retouch will only make it duller. Basedow (1925: 365) implied a similar notion. Thus, only in special cases were flakes retouched. Instead of retouching primary flakes, the more common reaction of all informants was to look over other primary flakes that had been struck from the core for a more suitable flake for the work at hand, or to remove several more flakes from the core until a suitable one was knocked off. Often several primary flakes might be tried out before one was found with a good working edge. Horne and Aiston (1924: 91) noted earlier a similar behaviour pattern for groups around Lake Eyre:

> Casual stones are any that have a sharp edge. They are used for scraping. Directly they are blunt they are thrown away and another picked up. Sometimes they are chipped if the stone will keep its edge long enough to warrant chipping, but usually they are not kept.

This provides important substantiation for the observations made among Western Desert groups. It might also be added that the effectiveness of working edges was sometimes difficult to gauge by superficial visual inspection due to occasional small and subtle variations of the stone surfaces forming the working edge. Pieces were selected and tried out, and sometimes discarded immediately. Of course, preliminary choice of possibly suitable pieces was done by visual inspection.

It was difficult to determine what the criteria for deciding to retouch or discard any given flake were. One criterion, I am sure, was a subjective evaluation as to how suitable the edge would be if the flake was retouched. The probability of the 'resharpening' turning out satisfactorily might also have been weighed against the relatively minor trouble it would take to remove more primary flakes from the core. But when one is seated, and getting the core means getting up because it is out of reach, this seemingly minor trouble may be a decisive factor. Moreover, some of the pieces which were retouched were discarded almost immediately because the retouch had created an unsuitable working edge. At other times, resharpened pieces would continue to be satisfactorily used. It should be emphasised that this secondary retouch was done with the aim of ‘resharpening’ or rejuvenating a dulled working edge into a more suitable one. There was no indication of any overall morphological ideal type, ‘classic’ form, or ‘perfect’ specimen, as collectors are wont to say and as archaeologists often tacitly accept in conversation. Rather, the traditional attributes of importance in the Western Desert were: effective edges (which were surprisingly variable in morphological expression), and a suitable size for holding in the hand and exerting pressure. These attributes might become more patterned by habit and tradition than one would expect, but this is only an impression. I lack quantitative data on this aspect.

Others have experienced similar surprises as well. Mountford (1941, 1948) saw stone-using activities for only a short time period and concluded that except for the hafted adze, the Pitjantjatjara might vanish and no trace of them would be left behind. In New Guinea, Peter White (1967: 409) was similarly ‘surprised’ by traditional stone tool behaviour, and originated the observation that the suitability of the edge in hand-held tools is the important variable for stone tool manufacturers and users. His excellent observations have been supported by Strathern (1969).

One other factor which may influence the frequency with which primary flakes, used as tools, are retouched is the availability of raw material. There was always spare raw material in the technological projects which I asked Western Desert individuals to work on. In traditional situations where raw material might be scarce, one could reasonably expect more primary flakes to exhibit retouch modifications. On the other hand, there was an abundance of flakes and a number of cores at most of the campsites I visited (also see Basedow 1925: 364; Thomson 1964: 406). In addition Aboriginals are known to have carried primary flakes around with them, as well as blocks of raw material (Thomson 1964: 405, plate 34; Basedow 1925: 364; Hayden, in press).

The type of raw material also appears to have an effect on the percentage of used implements which are modified. Where hard metamorphic or igneous rocks occur with naturally acute edges, I found that such rocks would be used for chopping wood, but were only modified intentionally less than 20% of the time (Figs 7a.1, 7a.3). On the other hand, opal or flint nodules do not have naturally occurring acute, sharp angles as a rule, and thus flakes must often be removed from them in order to obtain a cutting edge (Figs 7a.2, 7a.4). Therefore, about 90% of the flint and opal used in chopping wood had been modified by flake removal.

In dealing with primary flakes, sharp edges are present by definition. Of the flakes I saw used for ‘scraping’ or shaving (a more accurate descriptive term) wood, less than 25% were secondarily retouched or otherwise modified. These modified pieces did *not* always exhibit ‘scraper’ retouch (see below). It is tempting to conclude that retouched tools were made only when no suitable naturally occurring sharp-edged rocks were immediately available (or in the case of primary flakes, only when primary flakes with suitable edges were not present). However, this would be an extreme position. There is little more, if any, effort involved in resharpening, or retouching, a tool in hand than in casting around for a better replacement, and there may well be some subtle reasons which I did not perceive for choosing to retouch stones in particular circumstances.

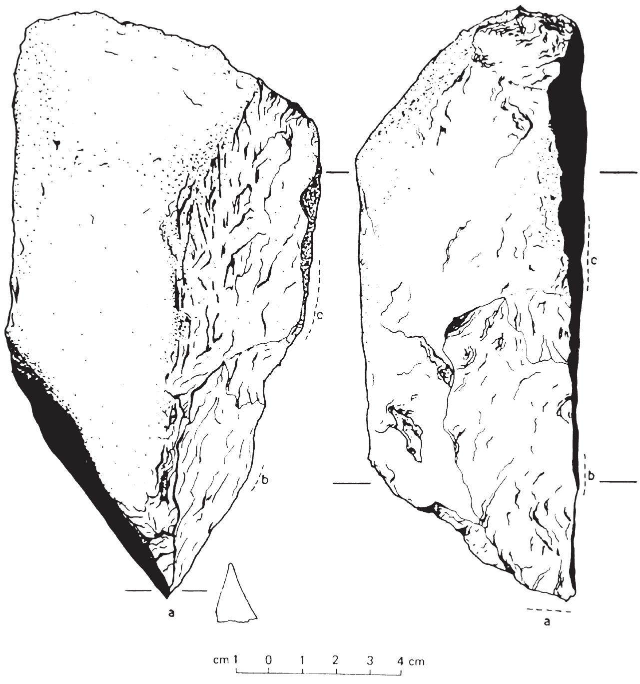

Figure 7a.1: Chopping implement made from local stone.

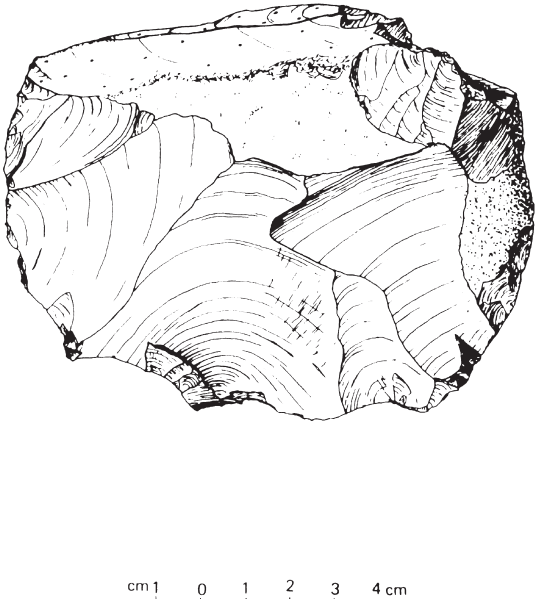

Figure 7a.2: Flaked chopping implement made of Wilson's Bluff flint.

Figure 7a.3: Using the implement illustrated in Figure 7a.1.

Figure 7a.4: Using the chopping tool illustrated in Figure 7a.2.

Obviously, adzes were nearly always retouched, if merely because unhafting and rehafting are time consuming, and once a piece is in the haft it is in self interest to get as much use from it as possible. Nevertheless, hafted adzes are sometimes removed without being retouched, either because of breakage, unsuitability or poor resharpening potential.

It thus appears that there is a basic dichotomy in the modes of stone tool use which has implications for behavioural patterns of resharpening and retouching stone tools. On one side of the dichotomy are hand-held tools which are less frequently retouched because procurement of other tools is relatively effortless (these tools are selected according to the criteria of edge effectiveness and grip); on the other side are hafted tools which are much more frequently retouched, rejuvenated or resharpened. This is because of increased replacement effort, and hafting requirements may strongly influence not only the size of the tool and attempts to reduce flakes to appropriate sizes, but also position of retouch on edges other than the working edge. This theoretical dichotomy may or may not turn out to have useful practical applications in archaeology. Certainly, hand-held tools generally seem to correspond to Binford's non-curated artefact class, and hafted tools correspond well to his curated artefact class (see p. 34). Caution should be exercised in making a one-to-one equivalence, however, since the transport and curation of unhafted flakes and 'scrapers' in bags, etc. are abundantly documented (W. Roth 1904: 20; Horne and Aiston 1924: 109; Spencer and Gillen 1927: 26).

To give some idea of the magnitude of retouched tool production under traditional conditions, I have attempted to calculate the number of *retouched* tools which a simple Western Desert nuclear family might fabricate in the course of a year. Although the estimate is inherently risky, at least it gives some idea of the magnitude of the problem with which archaeologists have to deal.

Table 7a.1 gives my estimation (based on Pintupi statements and my own estimation) of the probable number of items which would have to be replaced in a nuclear family's material repertoire per year. The group on which this is based (formerly located just south of Lake Macdonald) had a limited variety of wooden tools, but many of the estimates are probably on the generous side, especially the replacement of spear-throwers, women's hardwood bowls, and 25 spears, on a yearly basis. I have calculated the number of archaeologically recognisable tools which would result from these maintenance tasks on the basis of actual retouched tools produced in their manufacture at Papunya settlement. If unmodified hand-held flake tools were incorporated those totals would have to be at least doubled. I have been liberal with the chopping implements and assumed that they were all modified; this is undoubtedly unrealistic for most cases.

As a cross-check, I also calculated the number of retouched 'tools' produced per person, per week at campsites occupied by informants when they were using stone tools exclusively, some 30 years or more ago. These rates were 2.5 to 10 flake tools per week per person (Hayden, in press), which gives 130–520 tools per person per year. This is in very good agreement with the above estimate when one realises that one of the sites was adjacent to a quarry, so that old adzes were probably discarded there, and at the other site, several spear-throwers were manufactured by young men, thereby inflating the number of tools used and produced over a short period of time. Thus, these estimates can be considered to be on the high side. For a basic husband and wife pair with non-producing children and elder parents, it seems reasonable to think of the average

Table 7a.1: Estimate of retouched tools used yearly by an economically active nuclear family in subsistence-related activities

	Stone tools needed in woodworking		
	Chopping Implements	*Adzes*	*Hand-held flake tools*
Estimated yearly replacement of wood tools:			
1 spear-thrower	5	14	1
3 throwing or adzing sticks	6	—	3
25 spears	12	50	25
spear resharpening	—	9*	10
1 woman's bowl (hardwood)	5	—	—
4 digging sticks	12	4	—
	40	77	39
			Total: 156

Source: R.A. Gould in R.V.S. Wright (ed), *Stone Tools as Cultural Markers*

rate of retouched tool production as being around 150 per year (±50), with about 40 (±10) chopping implements being fashioned assuming almost all chopping implements were modified. Many of these tools will of course be scattered over the landscape at various locations, and it should be re-emphasised that lack of raw material may deflate these estimates considerably in some cases.

As should be apparent from the preceding discussion, many of these archaeological tools will be little more than superficially modified. Many of the functional choppers may have but one flake removed, while others may be 'used up' and be considered closer to the 'classic' type by some archaeologists (e.g. Fig. 7a.2). Western Desert archaeological tools are not generally predetermined forms, but rather mechanical results of having to create or resharpen a cutting edge one or more times. What determines the stage at which any given piece is discarded (unmodified, single flake removals, single resharpenings or multiple resharpenings) is the nature of the material, the suitability of the individual piece for potential resharpening, availability of raw material and the point at which the particular task at hand terminates. Thus, archaeological 'tools' are formed because of rejuvenation attempts, and pass through several stages. It is worth restating that habit or tradition may be an important factor in determining which pieces are perceived to have the most resharpening potential as well as the mode of resharpening most frequently used.

Functions

This leads to discussion of another surprise, or series of surprises, that I encountered during the technological projects. In the first place, I found that nearly all retouched stone tools were used in woodworking activities. This corresponds well with Long's (1971: 269) observations of Pintupi life: 'certainly the main use of stone here was for shaping and maintaining wooden tools and weapons'. An occasional retouched piece may have been used or even made for other activities such as cutting meat or skins, but there is little doubt that the vast majority were used for woodworking. This is based on information from older Pintupi individuals, as well as the lack of manufacture of such retouched tools in any other technological projects that were undertaken, such as plant food procurement and processing. The Western Desert groups with whom I worked did not traditionally use skins for any purpose, so that in a wider Australian context, skin processing may have been another important activity for which retouched tools were produced. Recourse to other Australian and world-wide ethnographic observations of hunting-gathering groups using stone tools largely supports this impression of the dominating influence of woodworking in retouched stone tool waste. For Australia, I could only find four references which indicate the use of chipped stone tools in gathering or processing plant food:

1. A reference by Jackson (1939) to chopping up fern roots with a unifacial implement. According to research done recently by Kamminga, this is almost certainly unreliable and incorrect (personal communication; see Bancroft 1894; W. Roth 1901: 10);
2. Tindale's observation (1941: 37) that Western Desert chopping implements might be used to aid in digging holes for roots. This is a unique observation, digging sticks being the implement ubiquitously preferred. It seems highly likely that this was a fortuitous occurrence, or that the detachment of roots (a woodworking function) with chopping implements was confused with the actual act of excavating;
3. Tindale's observation of using a crude hand chopper to cut off the husks of pandanus fruit (Hale and Tindale 1933: 114; 1934: 131) is also ambiguous. It is not clear whether the implement was fashioned specifically for this purpose, or whether any stone would have served, and the use of a chopper was only a matter of convenience;
4. O'Connell's recording of the former use of retouched blades as 'spoons' for eating tubers (see O'Connell 1977).

I know of no definite statement from other parts of the world to indicate that chipped stone was used for procuring or processing plant foods. While there have been notable archaeological exceptions, such as cultures using sickles, there are certainly no grounds for assertions that pebble tools of Palaeolithic age were almost invariably used for chopping up plant food (Deevey 1968: 286). On the whole, I think that the generalisation that retouched hand-held tools were used for woodworking (and possibly skin working) is a profitable starting point in analysis. Obviously, microliths are excluded from consideration here.

As to butchering, the axiom that the sharpest flake is an unretouched one would be particularly significant since sharpness is of great importance in cutting, whereas it is not necessarily important in woodworking. In practice, any waste flake is habitually used for gutting and breaking the skin in butchering. This has been amply documented by others. Such flakes seem to be rarely retouched among the Pintupi[2] as among the Ngatatjara. Gould (Gould *et al.* 1971: 156) observed that:

> These knives are discarded after only a few uses, and no effort is made to resharpen them. Thus they rarely show much in the way of secondary trimming and could be difficult for an archaeologist to recognize once the gum handle has decomposed.

For the Pitjantjara Tindale illustrates and describes the manufacture of several retouched 'knives' (1965: 114–19). Consideration of the representativeness of these examples is rendered difficult because of several factors: (1) the larger flakes were never actually seen in use and no specific use for them is given, whereas the smaller 'knives' are described as having been primarily used to incise lines in wooden implements, and (2) the original observations were made in 1933, and the 1965 article was entirely reconstructed from field notes and memory. Indeed, the retouching of these 'knives' before they were even used is rather puzzling from a functional point of view. Of further interest is Tindale's (1965: 141) implication that these knives often had one 'blunt, thick margin and a sharper, somewhat more arcuate one opposite'. These pieces, even though they are retouched in a similar fashion to scrapers and little used adzes, may well be morphologically distinguishable.

Aside from these instances of using unmodified flakes for butchering, every major ethnographic description of chipped stone tool use in Australia has mentioned woodworking of one sort or another much more frequently than any other activity. This is corroborated in Tasmania as well.

Morphology

Choppers

For the time being, the unretouched flakes will be put aside. What did the retouched tools look like that were used in the manufacture of wooden spears, throwing sticks, spear-throwers, digging sticks, bowls and other items? The choppers used for doing rough work were predominantly unifacial, although bifacial work was also present. Modification of the edges ranged from a single flake removal to flakes removed around the periphery, to good bifacial chopping tools (Fig. 7a.2), and included large flake specimens. These are more fully illustrated elsewhere (Hayden, in press). They are not substantially different from the chopping implements found throughout the Western Desert on abandoned campsites today, and are generally not too different in size and weight from the Kartan heavy duty implements (Bauer 1970). They are used for procuring wood for all wood implements, and are often used in finishing hardwood bowls, chopping out the interior of spear-throwers, thinning fighting spears, trimming branches off spear shafts, starting nocks for spear barbs, and initial shaping of spear points (e.g. Figs 7a.3, 7a.4). Such tools seem to be most often simply left at the site where the work was done after the task has been finished, an observation that Mountford also made (1941). Often they are fashioned out of quartzites and other non-cryptocrystalline rocks found locally. When good quality cryptocrystalline rocks were used they appeared to have been carried around and used as a source of raw material as well as a chopping implement, probably until the piece was exhausted (see Thomson 1964: 405; Hayden, in press). Interestingly, among both the dialectical groups with which I worked most, there was a separate name for cryptocrystalline and non-cryptocrystalline raw material suitable for making tools (*kanti* vs. *pilari* in Pintupi, and *kanti* vs. *kaltjiliri* in Yankuntjara), and on several occasions I witnessed fine-grained opal material refused in favour of coarser grained metamorphics and quartzites. Contrary to what I expected, it did not at all seem as though the finer the grain of the material the more desirable it was for using. Instead of pitying the poor craftsmen who had only metamorphics to work with for core tools, as is done from time to time (Stockton 1972: 22), perhaps one should really pity the poor craftsmen who had only cryptocrystallines to work with. The latter may be more aesthetically appealing and have better flaking properties, but edges on cryptocrystallines also tend to shatter more easily and become dulled more quickly when chopping hard woods. Perhaps the graininess of the metamorphics bites more into the grain of wood and is more effective in wood separation and detachment. Crabtree and Davis (1968: 428) have indicated such results from their experiments.

Among the Yankuntjara people that I worked with it appeared that there was a prohibition against women using cryptocrystalline rocks. One of the women I worked with said that she had never used *kanti* (flint, chert, opal, etc.), but had used *kaltjiliri*; whereas the men regularly used *kanti* for adze stones. A similar prohibition was recorded in Central Australia by Spencer and Gillen (1912: 373, 376). Pintupi women also showed a preference for using *pilari*, or non-cryptocrystalline rocks, and for using choppers, as opposed to adzes, in all their woodwork. Where a man would use an adze to hollow out or thin down hardwood, a woman would use a chopper. When women did attempt to use adzes, they were inevitably more clumsy than males. Women also employed grinding for finishing their digging sticks, sharpening the blades, and smoothing the surfaces of bowls and fighting sticks. They said that they always used a *tjiwa* (small sandstone pounding slab) for such purposes. This was first recorded in the Western Desert by Basedow (1925: 362), and later substantiated by Finlayson (1943: 79) and Thomson (1964). It was probably a relatively widespread alternative method of working wood throughout Australia (see also Horne and Aiston 1924: 93) and was also employed in parts of Tasmania. I never saw any men use grinding on their wood implements.

Thus, in terms of chipped stone tools, grainy chopping implements were used by men and women, although women tended to use such tools to the exclusion of other types. It seems that women only occasionally used hafted adzes, whether by proscription or simple preference, and used grinding to sharpen or finish many of their tools. Edge angles on these heavy duty stone chopping implements were generally high; the mode of the edge angle was 75°.

One of the implications of the above is that women do not seem to use or make any type of chipped stone tool which is unique to their sex in the Western Desert. Females will be very difficult to see archaeologically, unless via the small flat slabs and hammerstones on which lizards were pounded and leaves for pitchuri (a species of *Nicotana*) ash burned, although even here single males use the same articles. If grinding stones are present they are probably the best indicator, although absence of grinders is poor negative evidence for the absence of women.

Flake tools

As for the morphology of the retouched hand-held flake tools, here again there was a surprise. I rather expected that if the primary flakes being used to shave (not really 'scrape') down the spear shafts were to be retouched or sharpened, that the form of the retouch would be of the 'scraper' type. In fact, only about one half of the instances turned out this way. What I did not expect was that there could be alternative ways of achieving the same goal. For instance, a single flake might be removed from the edge, thereby creating an archaeologist's 'notch' (I am not referring here to minute denticulations, but to the larger types). These pieces are used exactly in the same way as the original flake except that there is a new edge to cut with, which is sometimes more effective than the former edge (Figs 7a.5, 7a.6). Such modifications when repeated yield denticulates. There was even one instance where a flake was first resharpened with a notch and then flaked back into a scraper. With my limited sample it was difficult to be sure, but I could detect no regular patterning in decisions to use either notch or scraper retouch. They appeared to occur in free variation. Even more astonishing was the finding of a very small burin in one of the ethnographic excavations, which Ngayuwa, an older Pintupi man, claimed to have made and used some 30 years before. What had he used it for? He said that he had used it for shaving down his spears. His tone seemed to question whether anyone would think of using hand-held flakes for anything else. In this case, it was the side edge of the burin which was used. True burins are occasionally found in Australia (Mulvaney 1969). Ngayuwa said that he had not learned how to make them from anyone, but that he had just thought of how to do it himself; it was about the only one he had ever made. The credibility of this story is difficult to assess; however it should be pointed out that at the settlement Ngayuwa, while in the process of knapping flakes for finishing spears, had picked out a flake with a broken edge which had a cross section much like a burin-blow edge. He

Figure 7a.5: Shaving the point of a spear with a notched flake.

Figure 7a.6: Using a flake-shaving implement in smoothing down the shaft of a spear. Note the denticulated form of resharpening along the working edge.

examined the piece carefully and placed it aside, saying that it was a good one, and he used it later as a flake shaver, and an effective one at that. Ngayuwa did not single out any other flake in such a fashion. Moreover, many of the primary flakes used at Cundeelee and Papunya had working edges close to a right angle (Fig. 7a.7). Up until this point,

Figure 7a.7: Using an obtuse-angled cutting edge to shave a spear shaft.

the extensive use of such flakes seemed a disappointing enigma. They were obviously unsuited for retouching, and never were retouched. According to my background, hunter-gatherers should have been using real 'tools' for woodworking, not broken edges of flakes or accidental right-angled edges.

Regardless of the reliability of Ngayuwa's story about the excavated burin, it did provide credible insights into possibly the major function of burins as a broad class. The slightly less than right-angled edges, strongly buttressed by the body of the flake, provide excellent shaving edges which dull slowly and are very efficient as well. This has been recognised experimentally by Crabtree and Davis (1968: 46). Many flakes with right-angled breaks or edges present the same burin cross-sectional characteristics; these are recognised and used by Western Desert Aborigines. In short, it seems as though the scraper, notch, denticulate and burin may well be stylistic variants of a single functional type. All of these can be used for shaving down and sharpening the ends of wooden shaft implements, particularly spears, throwing sticks, digging sticks, adze shafts, and even parts of Western Desert spear-throwers. There is the possibility that these differing types of retouch may have been used more differentially in various specialised contexts; for instance, notches might be used especially for sharpening the ends of spears, or notches might not be used on thick shafted implements, such as throwing sticks. However there is no ethnographic evidence for such assertions at this point; the occurrence of hand-held retouched tools was simply too infrequent for me to be able to arrive at any meaningful statements of preference or frequency.

I also observed flakes being used in a sawing motion for the fabrication of barbs on a particular type of Pintupi spear (the *karimpa*; Fig. 7a.8). These saw flakes were changed frequently, and only three out of a total of seventeen were retouched: two with notches

Figure 7a.8: Using an unmodified flake saw to undercut the sides of the *karimpa* spear barbs.

and one with a scraper retouch. One notch, near the end of a flake, was used primarily in severing cross-grain wood fibres in the barb nocks, and was not actually used in a sawing motion. The other was never used. In addition, Tindale (1965: 147) notes that small (*c.* 3 cm long) resin-backed 'knives' which have slightly serrated edges make effective saws for incising decorative lines. A surprisingly high percentage of the saws I observed in use carried an abrupt, often cortex covered, edge opposite the working edge. Tindale implies the same characteristics for his 'knives'.

Vertically oriented rocks

One of the more intriguing observations on the non-retouched artefacts concerned relatively large, flattish rocks which were embedded vertically in the sand. I saw this phenomenon twice: once in the technological projects and once in the context of an ethnographic excavation. The use was the same in both cases. The vertical slab served as a fulcrum, or pressure point, for the straightening of spears. One's hands about a metre apart would firmly grasp the spear with the part to be straightened in the middle. The centre section was then placed on the apex of the slab and force applied downward (after heating the shaft in ashes). At the settlement, this was done using a slab of flat cinder block; at the former campsite, a slab of iron rich metamorphic was used—it was about 20 cm in length and more or less like a scalene triangle in cross-section. Elsewhere in the Western Desert, wooden blocks or 'Y' uprights have been observed to serve the same function (Ackerman 1974). One of the interesting aspects of this observation is that at Isimila, Howell (1961: 121) has reported a number of hand axes set on their sides vertically in the ground. Howell was puzzled by the enigmatically positioned artefacts. On the basis of the above observation, I would argue that there is a strong probability that the vertically oriented hand axes were used as fulcrums for straightening spears.

Wear patterns

Since a primary concern was the archaeological identification of functions of stone tools, the wear pattern on all tools was examined. Here lay yet another surprise. One might ordinarily expect hard woods to abrade stone to the greatest extent, and soft woods to leave little trace of wear at all, whereas in reality stone tools used to work hard woods traditionally used by Western Desert groups showed little trace of any distinctive or diagnostic micro edge wear, while stone used to carve out the exceptionally soft and light wood of the bean tree (*Erythrina vespertilio*) for shields and bowls seemed to dissolve the edge of the stone tools in very short time. In this latter case, a very high frequency of gloss and striations occurred. These findings are more fully presented by Hayden and Kamminga (1973).

Discussion

The above results are fundamentally empirical in nature, however, it would be a mistake to let theoretical and interpretational implications go unrecognised. I would argue that in the first place, these observations set up a more viable model of the importance and role of stone tools in generalised hunter-gatherer societies of the Australian Western Desert than the more traditional models and expectations derived from Occidental archaeology. It was in fact the extremeness of Occidental archaeological assumptions, based on no more than subjective feelings of empathy with the unknown past, which led to reluctance in accepting a traditional situation as real when actually encountered. Binford (1972) has termed such discrepancies 'surprises', and the term is particularly appropriate to my own field experience. Others have previously made some of these same observations (Basedow 1925; Mountford 1941; Tindale 1941; Gould *et al.* 1971). I would like to help articulate the importance of these observations for archaeology. Moreover, with the same basic types of morphological tools found in large numbers elsewhere in the world, it way well be that insights derived from the Western Desert of Australia will have a wider application. Certainly, many of the possibilities which have been raised can be tested rather directly with archaeological techniques. For instance, when wear patterns are present on burins used for shaving shaft implements, edge damage and wear should be found on the sides, not on the point, which is often assumed to be the functional part (as the name implies). Similarly, if wear is found on notches or denticulates, or scrapers used for shaving shafts of wood implements, the wear should be found predominantly on the ventral face of the edge, and only slightly if at all on the retouched face of the edge. Also, if scrapers and notches and denticulates (and burins) are really stylistic variants of the same functional tool, then spatial distributional analysis of these different types on living floors should yield high spatial intercorrelations. Such applications might be particularly profitable with early Australian assemblages where one can probably assume that chopping implements and hand-held tools functionally replace the hafted adze.

Retouching behaviour has been divided into two types with differing determining factors: retouch on hand-held tools (possibly analogous to non-curated tools) and retouch on hafted tools (analogous to curated tools). Further, the influence of raw material on assemblages has been shown to be an important factor, and perhaps should be examined more carefully by prehistorians dealing with interassemblage variability. Indeed, it is doubtful whether the entire question of the effects of raw material on assemblages has ever been satisfactorily resolved.

Except for their grinding stones, and at burials, women seem to be exceptionally elusive in terms of archaeological visibility. With contemporary ethnological attention turning more and more toward the theoretical importance of women in hunting-gathering cultures, perhaps archaeologists should concern themselves with the nuances in material culture which may reflect their presence.

Perhaps most fundamentally, I hope to have provided what I believe is a more realistic model of the types of activities which stone tool debris represents. And even if the model is not viable for some areas, initial parameters (wear patterns, patterns of association, etc.) have been set out by which the model can be validated or invalidated. The data also make one look questioningly at some of the functional interpretations of Mousterian ‘factor IV’ tools—denticulates, notches, abrupt edge side scrapers, raclettes, and truncations—as representing plant food processing activities. In fact, chipped stone tools appear to be used very rarely for gathering or processing plant foods among hunter-gatherers.

Finally, the realisation that many archaeological types, formerly considered very different and distinct, may actually be variants of the same functional tool brings into question the entire problem of defining ‘style’ and recognising stylistic variants in archaeological contexts. I doubt that this issue has ever been dealt with directly or adequately by archaeologists, or that anyone has yet expressed the notion that stylistic variants can occur at several different levels of abstraction. It seems that in terms of contemporary aims and goals of archaeology, this would be one of the more critical conceptual areas to deal with. Perhaps this presentation of ethnographic and detailed lithic morphological data will help crystallise concepts and approaches to the problem. The issue of style is much too broad to deal with here, but hopefully the present study will have advanced the formulation of the questions in profitable forms.

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8 The archaeology of rock art in Australia

Christopher Chippindale

Rock art has been noticed in Australia by non-Aborigines since the First Fleet. The First Governor of New South Wales, Captain Arthur Phillip, reported in 1788 (Phillip 1789 [1970]: 58):

> in the neighbourhood of Botany Bay and Port Jackson, the figures of animals, of shields, and weapons, and even of men, have been seen carved upon the rocks, roughly indeed, but sufficiently well to ascertain very fully what was the object intended. Fish were often represented, and in one place the form of a large lizard was sketched out with tolerable accuracy. On the top of one of the hills, the figure of a man in the attitude usually assumed by them when they begin to dance, was executed in a still superior style.

Like their stone tools, then, the rock art of Australian Aboriginals was noticed from the start. While the stones have been the mainstay of Australian archaeology ever since, the rock art has remained more on the margin. Governor Phillip went on to remark: 'That the arts of imitation and amusement should thus in any degree precede those of necessity seems an exception to the rules laid down by theory for the progress of invention.' That comment is prescient. Archaeology—even then and certainly now—has developed means of working with stone tools: we know (or think we know) what to measure and record in worked stone objects, which aspects are central and which do not matter, what kinds of information are held in the material stone. All these are dealt with by 'theories of necessity', studies of the functional use of stone tools and of their role in subsistence that depend on ideas of rational efficiency. At the same time, we know from ethnography to recognise that stone tools have other meanings beyond the mundane; Taçon (1991) is a rare instance of symbolic aspects, the other 'power of stone', being explored archaeologically—and related rock art is a key strand of the evidence.

First, some essential points following from the defining phrase 'rock art'.

Rock art means figures made on rock, and so attached to the solid land, rather than on objects that can be carried and moved about (called often in archaeology *art mobilier*, the French for 'portable art'). Its determined and unchanging place in the landscape is important, especially in recent Australia where a sense of place and 'country' is central. Generally the figures are made by *engraving* (cutting or rubbing away the rock), to create a figure therefore *in* the surface, or by *painting*, usually in clay or ochre, to create a figure *on* the surface. Related to rock art are figures made by carving on trees (also unchanging things in the landscapes) and by arrangements of sand or other soft materials on the ground (sometimes called geoglyphs). These, by their nature not enduring, are known in recent times but not in the old archaeological record. Engravings, which may survive exposure to open weather, are found on surfaces of all kinds; paintings are confined, or survive when confined, to protected surfaces in shelters and overhangs. Flood (1997) is a thorough and good general account of Australian rock art as a whole.

‘Art’ has two kinds of meanings. One is that which Governor Phillip called ‘imitation’, the making of figures that are pictures of things. The other brings in aesthetics, as in the phrase ‘fine art’ and in the place of art and artists in our society. In regions like Arnhem Land today, Aboriginal artists paint portable pictures which are sold in a cash market, but this is a transformation of old Arnhem Land ways, where painting is to do with ceremony, with knowledge and with stories of country, which are recorded and expressed in the images on the land, the figures archaeologically classed as ‘rock art’. Within the framework the archaeologists use, categories like *hand-stencils* can be questioned (Forge 1991).

There are three main strands, then, in the varied views taken of Australian rock art in the contemporary world. There is its Aboriginal meaning, a matter of inside knowledge and often to do with what figures stand for rather than how that is expressed in ‘naturalistic’ shapes: rock art is a key link in expressing and showing people’s continuing place in country and its stories; so a figure in a West Kimberley shelter is of *nganjdjala-nganjdjala*—the capricious spirit who steals food (Layton 1992: 82)—which uninformed eyes would recognise as a human figure. There is the aesthetic appreciation of rock art as pictures that can be appreciated for their own merits without knowledge of what they stand for or why they were made: rock art is a fine art—and fine Australian rockart is as fine as any art anywhere (for a superbly illustrated survey, see Walsh 1988); an Arnhem Land Dynamic Figure is a world-class drawing (Chaloupka 1993) even to eyes who see it millennia later and without inside knowledge of what it really depicts. And there is the archaeology of rock art, in which the figures provide another class of material evidence alongside that from the ground; so a clear image of a thylacine (Tasmanian tiger) (Walsh 1994) provides a record of that creature’s existence in a certain region and period, as surely as would its bones in the trench. Each strand has its own complications, and more follow from their interaction.

Australian archaeology is thin in the ground by world standards. Aboriginal hunter-gatherer lifeways did not leave the monumental structures, the earthworks and ruined buildings, not the mass inorganic detritus, the pottery and metalwork, that are the stuff of most regional archaeologies. And the acid Australian sands and Australian weather are not kind to the survival of anything other than stone tools, difficult to characterise, from which to conjecture ancient lives. Old rock-art styles, especially in north and west Australia, show elaborate hunting kit and ceremonial dress (nearly all made of perishable materials that leave no archaeological trace there), depict people active in social interactions that include warfare and sexual relations, and include animal-headed beings and other entities of the envisioned world. Where well-dated and reticent lithics may offer ‘chronology information’, ill-dated and informative rock art may provide ‘chronology without information’. A successful integrated archaeology will bridge between the two lines of evidence to build information and chronology, but not easily for it is rare for an item recognised in the stone record to be plainly seen in the rock art image: Taçon & Brockwell for Arnhem Land (1995) and Morwood & Hobbs (1995) show how bridging is being done between rock art and underground archaeology and across also to the environmental record of landscape history. Taçon & Chippindale's study (1994) of the changing history of warfare that is documented in the region's rock art illustrates the kind of specific study which one cannot imagine being possible without this strand of archaeological evidence: a good art chronology is important.

The dimensions to the archaeological study of rock art are the same as for other archaeological materials: its distribution in space; its distribution in time; the hazards of survival and visibility that skew that ordered record; the variability in what the stuff comprises; and the information thereby encoded that can be archaeologically recovered.

Distribution in space

There is the potential for making rock art wherever rock is exposed in the land, which means most regions of Australia. Particularly known for their engravings are the Central/Western Desert region around Alice Springs (NT) and the Hawkesbury sandstone around Sydney (NSW) (Stanbury *et al.* 1990), and for their paintings the Kimberley (northeast WA) (Walsh 1993), western Arnhem Land (Top End, NT) (Chaloupka 1993) and Laura regions (north Queensland) (Trezise 1971). Layton (1992: chapter 7) is a good analytical survey of the continental picture, and the patterns discerned within and across a great regional diversity.

Distribution in time

Rock art is hard to date. It is usually on open-air surfaces, rather than buried in dateable deposits. Experience with radiocarbon and other direct dating methods applied to rock art is mixed: Watchman (1993) is a good survey in respect of paintings; McDonald *et al.* (1990) illustrates some of the puzzles; Watchman & Cole (1993) and Nelson *et al.* (1995) report dating studies of unusual rock-art material to which radiocarbon seems well suited. For the most part, a rock-art chronology is constructed from fragmentary and varied evidence—rather as prehistoric chronologies were commonly put together before direct radiometric dating simplified the task. In Arnhem Land, a notably detailed chronology has been constructed from a whole variety of direct and indirect sources of evidence: Chippindale & Taçon (1993) show chronological reasoning from individual panels, and Taçon & Brockwell (1995) how this fits a larger regional picture—both studies building on Chaloupka's (1977, 1985) and Brandl's (1973) earlier work. Rosenfeld (1993) is an up-to-date sketch of the long-term picture, as we best now know it.

Survival and visibility

Rock art images cannot be older than the age of the geological surfaces which bear them, so the first controlling factor is the durability of those surfaces, a subject not yet researched. Fine red ochre in Australia's most ancient archaeological sites hints at its use, perhaps for rock art, from first settlement. In some regions, the oldest surviving paintings are red or purple-red ochre (mulberry), considered often to be some thousands of years old; yellow ochre endures less, and the clays used as white pigments are transient. In any area, and even within any one panel, survival is very variable, depending on the local protection or exposure of painted surfaces. Clarke & North (1991) give a good regional analysis of pigment and its durability. Some engravings, now covered with thick mineral skins and crusts, have the appearance of great antiquity; others are on soft, flaking surfaces of no great age. It is thought likely that it is the engravings which will more often endure over the very long term.

Variability

Australian rock art is visually very varied, each region having its distinctive elements. Maynard (1979) set the elements into a pan-Australian framework with the imagery developing over the long term into a more sophisticated naturalism. The continental picture, if there is just one, looks more complex now, but a pan-Australian element—the 'Panaramitee' repertoire of engraved motifs—is recognised as an archaic rock art horizon that runs across the whole landmass, Tasmania included (and see Rosenfeld 1991). Nobbs (1984) on South Australian archaic engravings shows how such an analytical entity is defined.

Information encoded

The archaeology of art (Layton 1991) on the one hand, and the conventional methods with which archaeologists study and classify the varying shapes of artefacts on the other, provide the starting points to the archaeological study of rock art. Neither is quite appropriate, and nor is the specialised approach of western aesthetics and art history. The *achaeological* study of Aboriginal rock art is undeveloped; some elements of what is needed are seen in Clegg (1994), a remarkable set of original studies addressing basics like 'How do we know what this is a picture of?', or 'Which is the most important figure when there are several on a panel?' Officer (1991), defining just what is an anthropomorph (meaning an image recognised as 'of human shape'), explores one special case among these open questions.

Modern Aboriginal knowledge of the art—whether the images themselves are ancient or not—is often a useful starting point for a research study (see papers in Morwood & Hobbs 1992), as well as important itself. Layton (1992), in what is now the standard book on Australian rock art, concentrates on the anthropological approach to the whole and larger story.

Only a fraction of the rock art of Australia has been recorded archaeologically, and only a fraction of that fraction published or analysed. Its conservation and preservation (Lambert 1989) is difficult— how does one 'manage' the environment and climate experienced by a great painted rock face, set within the side of a grand cliff in open remote country?

Rock art, with its graphic proof of people present in their country, is of special concern to Aborigines concerned with their own land—often more so than the reticent and hidden traces of archaeology underground. Paintings have their stories, and it is remembered which ancestral beings left their image on the rock. The imagery of late Arnhem Land rock painting with its X-ray conventions continues and thrives in the work of the region's celebrated artists who paint in natural white, red, yellow and black pigments on panels of bark and, nowadays, on fine paper (Dyer 1994). The iconography of the modern acrylic dot paintings from the desert centre relates to that of the region's rock engravings. The re-painting of some rock art images, renewed when they fade, as proof of living connection to country has been controversial (Ward 1992; Walsh 1992). Rock art, like other aspects of Aboriginal archaeology, has a role in the contested visions of the Australian landscape that face each other in our historically divided society.

Neighbours to Australian rock art are figures from Irian Jaya, arising from ancient links north across what is now the Arafura Sea. Rock art of the Pacific islands seems rather of another tradition. In archaeological research, Australian rock art has a special place, for its quantity and varied range, for its high aesthetic quality and the detailed record of ancient Australians it encompasses, and for the rich knowledge of its recent and present meaning in the ethnographic record of Aboriginal understanding up to and including present knowledge, some of it not of a nature to be divulged beyond those individuals and communities entitled to know. The rock art of no other region or continent offers so much, a common handicap to studying rock art being an absence of any ethnographic or ethnohistoric insight relating to it. Robert Bednarik, energetic editor of the journal *Rock Art Research* (published from Australia on the subject of rock art world-wide), takes a view of rock art and its chronology in Australia and other regions outside Europe which upsets much common knowledge on where and when aspects of human genius first are manifestly visible. Rock-art research today is a lively field where several agendas are followed.

The reprinted paper, typical of good recent research, illustrates these themes, and the special opportunity Australia can offer to relate not just two kinds of field record, the paint on the rock and the artefacts in the ground, but two whole domains of knowledge, the one deriving from Aboriginal insight, the other from archaeological method. The fieldwork was done in the Top End of the Northern Territory, in collaboration with Wardaman people in whose land the study areas lie. Wardaman 'art' (as we call it) is *buwarraja*, created not by people but by the creator-beings of the Dreaming who imbued the land with their essence. In the shelters, the Lightning Brothers left images relating to their famous fights on the rock surfaces, great striped figures painted in red and white. Excavation in the rockshelters beneath the surfaces has documented the history of occupation; ochre, lithics, charcoal and ochred fragments of stone cortex have enormously increased in quantity in a recent period extending back to 900–1400 years ago; there is a small or nil presence before. A sensible and sensitive discussion recognises the implications of this archaeological finding without seeking to make one frame of knowledge true over and in opposition to the other.

8a Of lightning brothers and white cockatoos: dating the antiquity of signifying systems in the Northern Territory, Australia

Bruno David, Ian McNiven, Val Attenbrow, Josephine Flood & Jackie Collins

Introduction

A number of authors have argued that the late Holocene was a period of widespread change in Aboriginal Australia (e.g. Lourandos 1983; David 1991). Such changes may have involved a major restructuring of sociopolitical systems, such as the beginnings of ceremonially based, extractive networks geared to the large-scale management of resources (including eels) in western Victoria (Lourandos 1983; 1991). In north Queensland, David (1991) has argued that, during the last 3000 years or so, systems of land tenure and/or regional interaction networks may have changed. However, as far as we know, no researcher has attempted systematically to relate changes observed in the archaeological record with broader concerns relating to past belief systems. Recognising the difficulties of such a program, we address this issue with an investigation of the archaeology of a number of locations (Yiwarlarlay, Mennge-ya and Garnawala) in what is today Wardaman country.

Wardaman country

Wardaman country is renowned archaeologically for its vast body of rock art, which to the local Wardaman people is visual proof of the Dreaming itself. To archaeologists, such paintings were created some time in the past—they have a definable antiquity. Given their importance in Wardaman society today, and their identity as signifiers and signified of the belief system we know of as the Dreaming, investigations of their antiquity may shed important light on the beginnings of the modern belief system itself. In essence we are looking for patterns, and we begin by asking whether or not the paintings which today express the identity of the land to Wardaman people were all initially undertaken within a time-specific and identifiable time frame. If this is the case, then it is possible that we are identifying the antiquity of the modern ontological system itself, or at least its expression, largely as we know it today. Knowing which of these two options we are observing, however, may be a major archaeological problem which we may not be able to solve.

Wardaman country is located to the southwest of Katherine, Northern Territory (Fig. 8a.1). Wardaman people generally recognise matritotems (the *ngurlu*), assign subsection partly, though not exclusively, through the mother, and practice a matri-focal system of parent-child relationship. There exists a matrifiliative complementary relationship to land, with patrifiliation being primary.

During the recent past, Wardaman country was divided into various estates, each of which reckoned a cosmological identity with specific Dreaming beings. Some of these were travelling beings (such as *Gorondolni*, the Rainbow Serpent), while others concerned specific parts of the landscape only (e.g. *Gandawaq*, the moon, at Jalijbang). While the entire landscape thereby gained its identity and was made discontinuous by its affiliations with specific Dreaming beings and events, it was united into a cosmological whole by its common participation in a unified system of land and law expressed in the Dreaming. In this sense, the land is a humanised landscape (Rigsby 1981), and the way in which the various estates are broken up and interlinked at various levels reflects the pattern of Wardaman land tenure and land use.

The land's Dreaming identities are central to the local belief system. It is in the Dreaming that Wardaman ontology is centred. Dreaming realities are expressed everywhere—in the mountains, rivers, trees and rock outcrops. As Merlan (1989a: 4–9) notes:

> The Wardaman use the word *laglan* 'country, place, site' (and also camp) to refer to tracts of country and places within them to which they claim attachment, as in the phrase *nganinggin laglan* 'my country'. Each such country is composed of many different sites, at least some principal ones of which are associated with estate-linked *buwarraja*, that is, creator figures or 'dreamings' which are saliently or exclusively identified with that particular country. An example is the association of *girribug* 'pheasant coucal' with a particular country . . . of which the Willeroo homestead and some neighbouring places are focal sites. In addition to these particular estate-linked and bounded dreamings, through each country there pass at least some mythological paths of other, long-range dreamings, many of which . . . happen to come from the west and northwest, as far away as Port Keats and Western Australia. Thus each country, or 'estate' (see Maddock 1982) is defined by a particular constellation of far-travelled and more local dreamings and sites.

In short, the landscape consists of a complex patchwork of landed Dreamings criss-crossed by non-local, travelling ones, both of which give identity to the land and link Wardaman country with neighbouring lands. Individual places identified as of specific

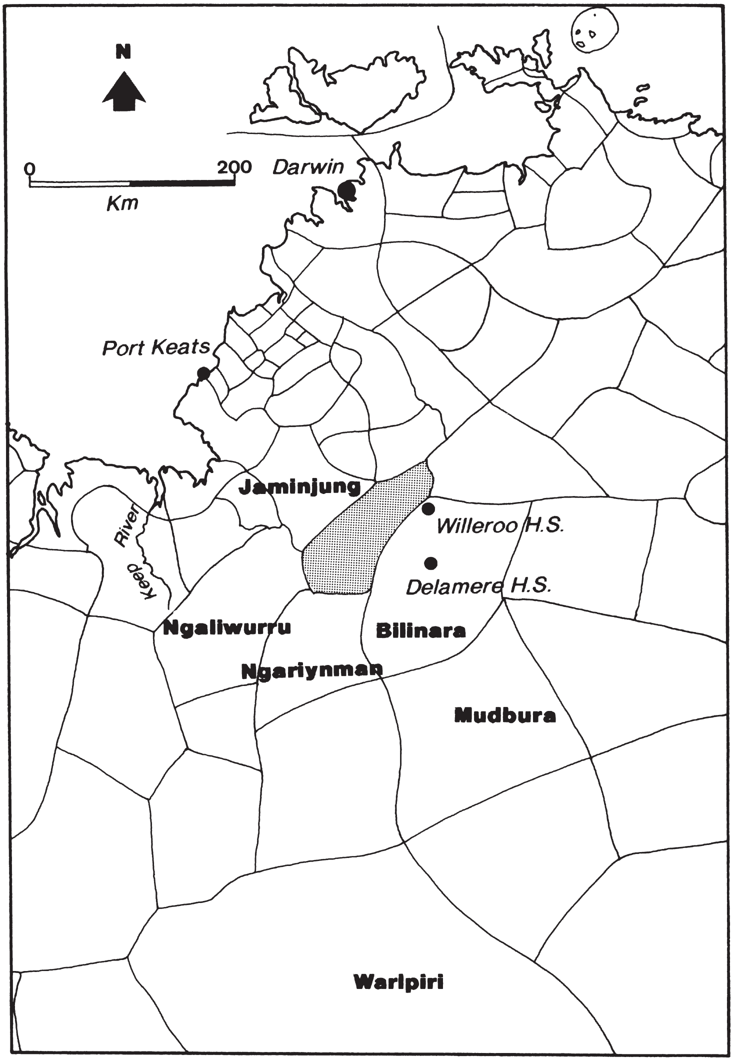

Figure 8a.1: Wardaman country (after Tindale 1974).

significance to Wardaman people take many forms, from features such as water-holes or hills to smaller objects such as rocks or prominent trees, including individual or complexes of rockshelters. It is with the latter that we will be specifically dealing in this paper, for it is here that rock art is most commonly found.

Much of the 'art' located in Wardaman country is *buwarraja* and was never created by people, but is (rather than represents) the Dreaming beings which sit in the rock (cf. Merlan 1989b; Frost *et al.* 1992). Such sites are imbued with the essence of

Dreaming beings, whose identities often reflect the identities of the land in which the site (and hence paintings) occurs. In this way, the rockshelters at Nimji and Murning, near the Yingalarri water-hole, have important associations with *gulirrida* (peewees), and as such many painted figures at these sites are *gulirrida* to local Wardaman people.

The identities of the paintings in Wardaman country express the narrative tradition, which is itself firmly embedded in the Dreaming. Given the archaeological observability of the paintings which express the current belief system, we have attempted to trace back archaeologically the history of the paintings themselves—that is, to date their antiquity and to document how they have changed through time. In doing so, we hope to arrive at some understanding of the dynamics of the belief system which has resulted in the creation of the observable rock art and/or in the way in which that belief system has been expressed in the past.

Yiwarlarlay

Yiwarlarlay is the home of the Lightning Brothers, Yagjagbula and Jabirringgi (Fig. 8a.2). The former is young and handsome, whereas Jabirringgi is older and rather unattractive. Ganayanda is married to Jabirringgi. Every day, one of the two brothers goes hunting, bringing back the day's catch to Yiwarlarlay. One day, as Jabirringgi returns from the hunt, he hears his brother with Ganayanda in a secluded fissure in the rock. In anger, he throws a spear at Yagjagbula, who evades it. The two brothers take up positions on the surrounding plains, whence a fight erupts, creating lightning in the skies. The frogs come up from the south, as does the rain, who watch the brothers fight. Eventually, Yagjagbula wins the fight by knocking Jabirringgi's head-dress off with his boomerang.

Some of these events are visible in the rockshelters at Yiwarlarlay in the form of paintings, although knowledge of the story is necessary for their appropriate interpretation (Fig. 8a.3). Two of these shelters—Yiwarlarlay 1 and Delamere 3—were excavated in 1989 by the authors. Their results are summarised below.

Yiwarlarlay 1

This rockshelter houses the Lightning Brothers. At Yiwarlarlay 1, there is no evidence that any of the paintings pre-date the arrival of Europeans (both the stratified ochre and the pieces of painted, exfoliated rock wall come from recent levels) (David *et al.* 1990; 1991) (Fig. 8a.4). David *et al.* (1990: 83) concluded that the paintings at Yiwarlarlay 1, and other similar paintings elsewhere in Wardaman country, may have been an attempt by local people to highlight the identity of the land by painting

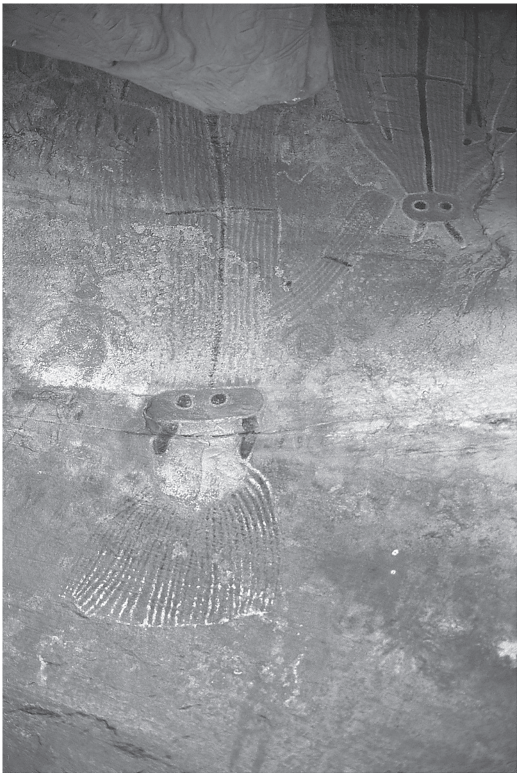

Figure 8a.2: Yagjagbula and Jabirringgi, the Lightning Brothers at Yiwarlarlay 1.

Figure 8a.3: Dreaming (*buwarraja*) fauna associated with the Lightning Brothers, Yiwarlarlay 1.

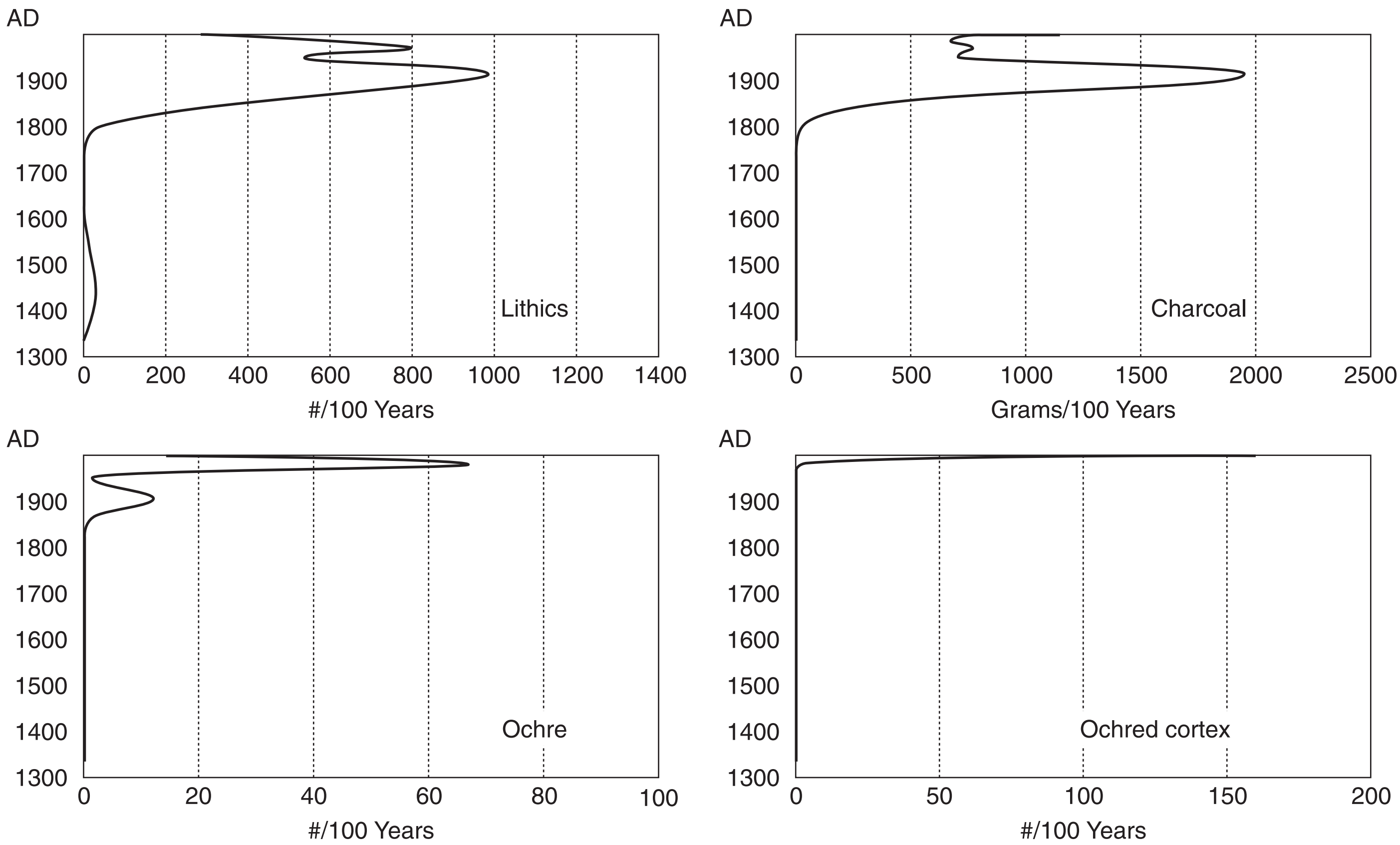

Figure 8a.4: Yiwarlarlay 1: deposition rates of materials from excavation square E20. All deposition rates are per sq. m per 100 years.

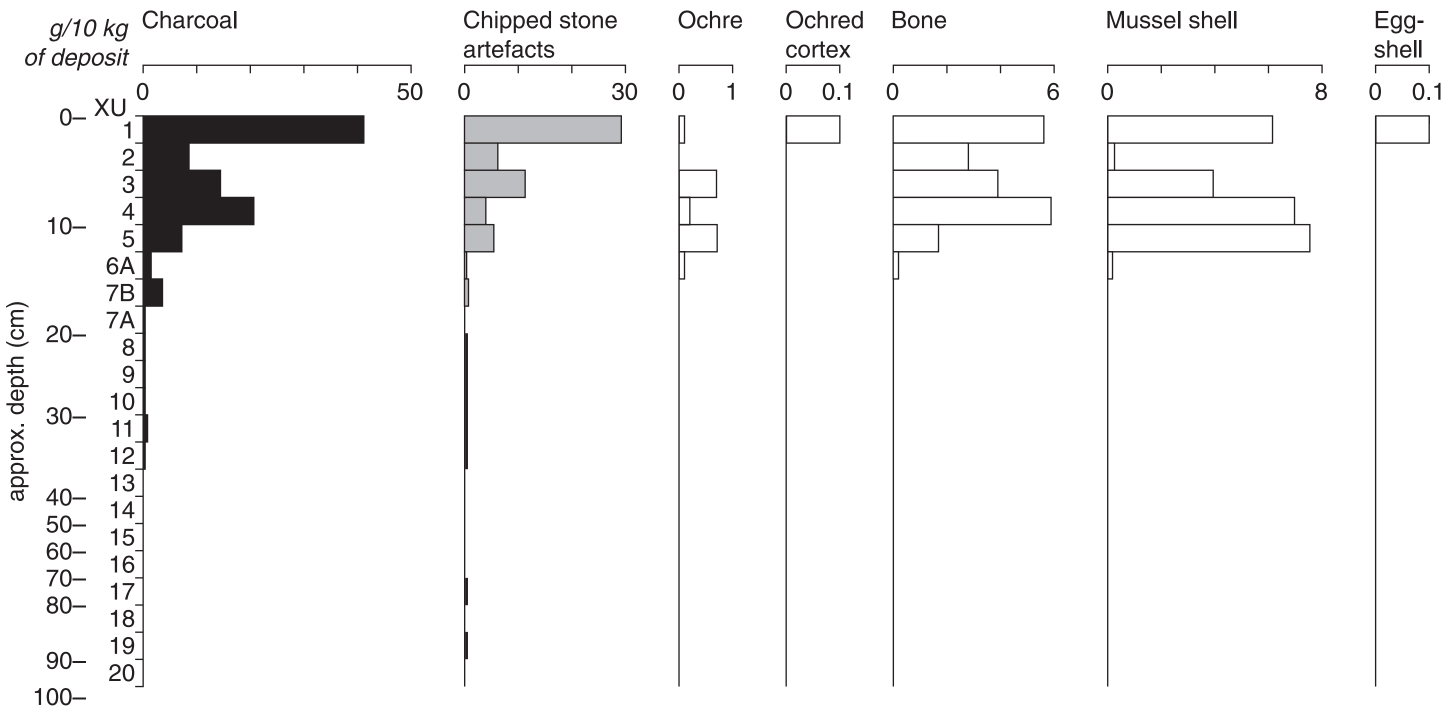

Figure 8a.5: Delamere 3: quantities of excavated materials by excavation unit, square G16 (after McNiven *et al.* 1992).

the local Dreaming beings on rock walls. This may have resulted from the dislocation of traditional people following initial European incursions into Wardaman country, restricting the local people's access to their traditional territories and restricting also their ability to fulfil their required Dreaming obligations in some places. Hence, recent changes in artistic expressions observed at Yiwarlarlay 1 and other places may have been related to changes in access to land in Wardaman country, changes which necessitated broader re-adjustments in the way Law and the Dreaming were articulated. These issues are beyond the scope of this paper, but have been explored further elsewhere (David *et al.* 1991).

Delamere 3

Excavations at Delamere 3, located at Yiwarlarlay opposite Yiwarlarlay 1, have shown that paintings here were only created during the last 380±60 years or so (Fig. 8a.5) (McNiven *et al.* 1992). The appearance of *in situ* ochres at Delamere 3 corresponds in time with a significant increase in the densities of other cultural materials, such as stone artefacts, bones and mussel shells.

Mennge-ya

Mennge-ya, 'at the white cockatoo', is a Dreaming place located at Jalijbang. Innisvale Station (Fig. 8a.6). At nearby Wynbarr, old man White Cockatoo has a number of wives who forage in the area for kapok ('native cotton') to feed their husband. One of these foraging places is Mennge-ya, where two wives can be seen 'sitting' in the rock. A few other Dreaming beings can also be seen near the two white cockatoos at Mennge-ya, but it is the latter that are visually dominant.

We excavated in 1989 below the painted panel, whose figures include two large striped anthropomorphs (the white cockatoos), a zoomorph (a crocodile) and smaller anthropomorphs (Attenbrow *et al.* in press). Artefacts near the base of the sequence are dated at 2109±60 BP (NZA-1624), below a major cultural change and large increase in the discard of stone artefacts at *c.* 2000 BP: in the lower Levels 4–5 there are five large ochre pebbles, reminiscent of those found today in an adjacent creekbed, and 11 small non-angular pieces which appear to be parts from larger blocks. Above the break in the sequence, in Levels 1–3, there are four ochre pebbles, 209 pieces and two fragments (both in the uppermost Level 1) with distinct striations and/or bevelled surfaces that are evidence of their use. We think the pebbles were carried into the shelter by humans, perhaps for painting, perhaps for other uses: similar ochreous pebbles were used as hearth-stones in the Jalijbang 2 shelter near by (David *et al.* 1992). We think the smaller pieces had been used for painting, as some of them have use-striations

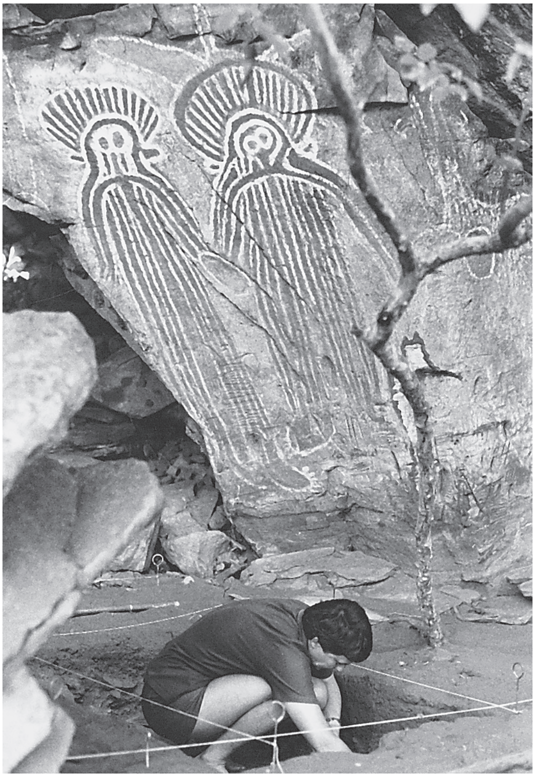

Figure 8a.6: Mennge-ya, showing White Cockatoos (*menngen*) and excavation squares.

and bevelling, while others are tiny pieces resembling those typically produced during painting activity.

The striped anthropomorphs in the rockshelter, which are of very fresh appearance, show evidence of retouching. The painted crocodile underlies other paintings and, on the basis of superimpositions and degrees of fading, appears to be among the earliest paintings at the site.

A five-fold increase in quantities of ochre in Level 4 indicates a major increase in painting activity around 2080±90 BP, with peak pigment densities occurring between 1400 BP and 100 BP. It is difficult to relate the excavated ochres to the paintings currently visible at Mennge-ya, but the following chronology, based on the sequence of superimpositions, is proposed. The striped anthropomorphs are relatively recent (probably dating to post-contact or immediately pre-contact times), and may therefore correspond with the most recent peak in ochre from the deposits. That is, they were painted during the last 380±60 years, while Level 1 or the top few centimetres of Level 2 were accumulating. The painting of the faded crocodile, which underlies the striped figures, may have taken place around 500 years ago, during accumulation of Level 3 or the lower half of Level 2. The other paintings at the site exhibit similar levels of disintegration to the faded crocodile, and may therefore be roughly contemporaneous with it (Fig. 8a.7).

Garnawala

In the Dreaming, two sisters are chased from Port Keats (northwest of Wardaman country) by Gorondolni, the Rainbow Serpent. They are followed by a diver duck and a flying fox, who are in turn followed by numerous animals—kangaroos, emus, peewees, dingoes, etc. The beings pass through Garnawala on their way southeast. At the Yingalarri water-hole, Gorondolni plays his didgeridoo. The diver duck approaches the Rainbow Serpent (who is not paying close attention to what is happening around him). The diver duck drags a spear along the ground between his toes, and when he gets close to the Rainbow Serpent spears him. Some of the actors in this story travel through Garnawala, although the major events described take place about 15 km to the southeast, near the Yingalarri (Mulvaney's (1975) Ingaladdi) water-hole. The Dreaming beings at one site at Garnawala (Garnawala 1) include two elderly beings—*djangural*—who observe the events as they unfold near the Yingalarri water-hole. In the process, they watch over young *yirmi-nyonong* (Fig. 8a.8). In this way, the Garnawala sites are linked with the Rainbow Serpent story, which is itself a long Dreaming story linking numerous localities in Wardaman country and beyond.

Stanner (1961: 238) recorded three versions of this story at port Keats during the 1930s. His 'Marithiel' version is reproduced here:

> Lerwin. The Rainbow Serpent, had no wife. Amanggal, The Little Flying Fox, had two wives. Lerwin stole one of the women while Amanggal was looking for food. When he discovered the loss, Amanggal pursued Lerwin to a far country and slew him with a stone-tipped spear. Lerwin cried out in pain, jumped into deep water, and was transformed into a serpent. Amanggal flew into the sky . . .

Garnawala contains numerous sandstone outcrops, many of which contain large galleries of paintings. At one of these—Garnawala 1—the authors undertook excavations in 1990 beneath a large painted frieze containing the *djangural* and *yirmi-nyonong* mentioned in the Rainbow Serpent story (see above) (Fig. 8a.8). Although sorting and analysis of the Garnawala excavated material has not been completed, preliminary findings are as follows.

Occupation at Garnawala 1 began shortly before 5240±70 BP (Wk-1764). Cultural materials in the early levels are relatively sparse, and do not appear to show any evidence of painting activity at the site. A major stratigraphic break, dated to 860±65 BP (Wk-1763), indicates a major change in the types and quantities of cultural materials deposited at the site. Above this date, amounts of stone artefacts, hearths, burnt stones, mussel shells and other food refuse increase dramatically. At this time also we find the

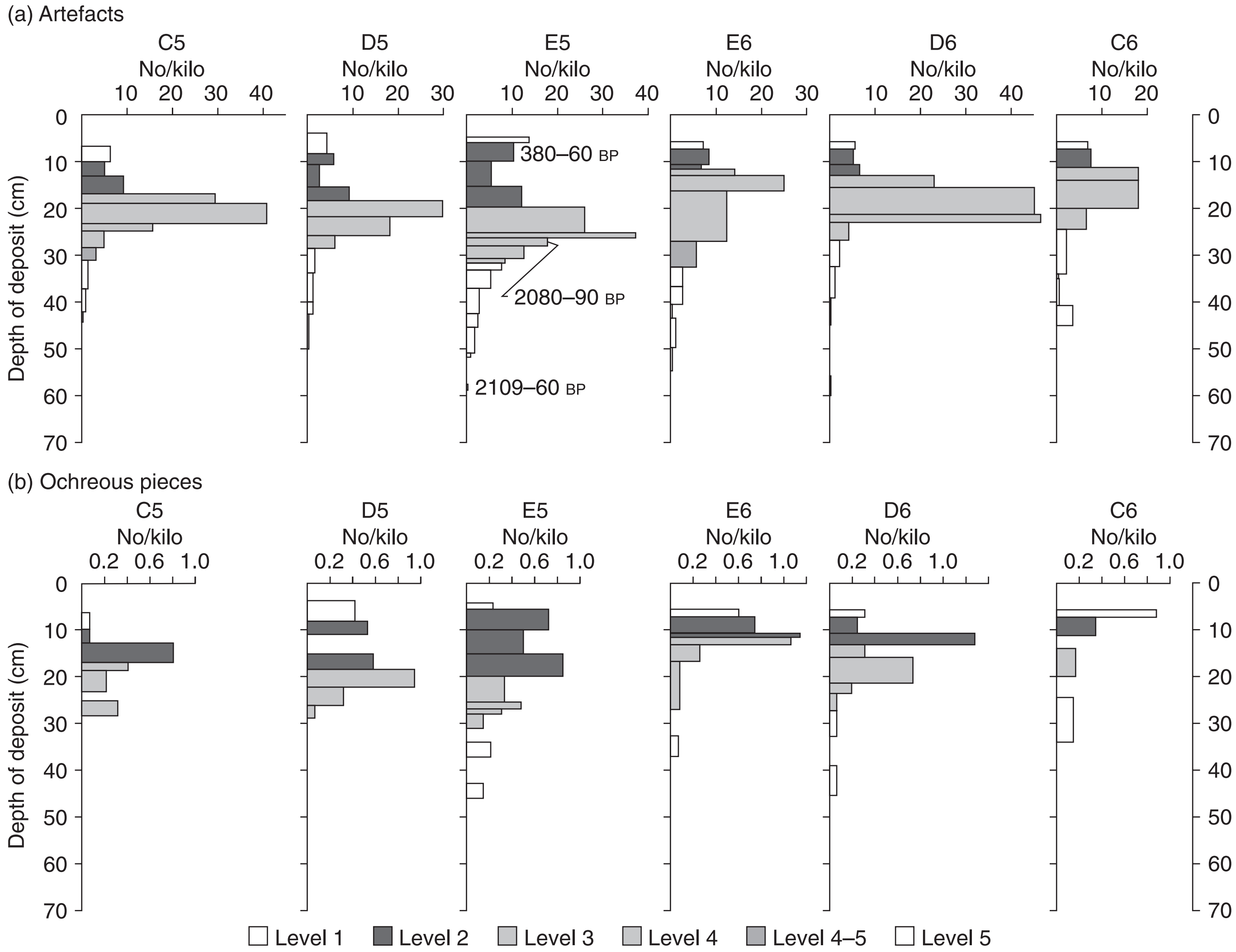

Figure 8a.7: Mennge-ya: quantities of stone artefacts (A) and ochre fragments (B) by excavation unit from the excavated squares.

Figure 8a.8: Garnawala 1, showing *djangural*, *yirmi-nyonong* and the 1990 excavation.

first evidence of intensive artistic activity at the site. Over 500 introduced fragments of ochre have been excavated, consisting mostly of red, yellow and white pigments. Preliminary results of the analysis shows that the beginnings of painting at the site dates to approximately 860 BP, a pattern of change well associated with the stratigraphic change noted above. A radiocarbon date of 939±91 BP (NZA-1323), obtained from below the stratigraphic break, re-enforces our confidence in the fine-grained dating of this change at Garnawala 1.

Discussion

At Yiwarlarlay 1, Delamere 3, Mennge-ya and Garnawala 1, evidence for painting activity does not begin until the late Holocene, and is concentrated especially at various times during the last 1400 years or so in spite of evidence for earlier occupation of the sites. At Yiwarlarlay 1, Arndt (1962: 169) stated that Kulumput, a local Wardaman elder, claimed

> the Lightning Brothers originally 'camped' on the Victoria River, where several neighbouring tribes were free to visit them. When the country and the people were divided between rival pastoral interests it was no longer practical for the Wardaman people to visit the Lightning Place. The Wardaman elders at Delamere Station decided that the Lightning Brothers could 'camp' at the Rain Place near the homestead, so that they

> could be seen by the rising generation. A contemporary of Kulumput, Emu Jack, 'dreamed' (visualised) the design and did the painting. The task was delayed by station and tribal duties and was not finished until he was in bush-retirement prior to his death 'near the end of the Japanese war [World War 2, 1939–45]'.

Two points salient to the current discussions can be identified from the above passage and from the archaeological work recently undertaken at the site:

1. the paintings of the Lightning Brothers and other figures at Yiwarlarlay 1 express local Dreaming beliefs; and
2. the identity of Yiwarlarlay 1 as a Lightning Brothers place dates to the post-contact period, and was stimulated by changes in patterns of land tenure and access to tracts of land.

At Yiwarlarlay 1, the major archaeological changes thus took place at the same time as widespread alterations in social conditions (including access to land) during proto-historical times. The rock art at all sites investigated appears to date largely, if not entirely, to the last 1400 years or so. The appearance or intensification of painting activity during this time was accompanied by major increases in the deposition rates of stone artefacts and food debris in the excavated sites, which may indicate that these changes were broad in scope. Because of their systemic nature, we would argue that these changes signify alterations in social circumstances at this time. The fact that increases in stone artefacts at Mennge-ya preceded the major increase in ochre deposition rates may also indicate that sociocultural changes were probably under way by the time rock painting became widespread. Given that today the rock art is closely linked with territorial concerns throughout Wardaman country, we therefore propose that:

1. rock art became more widespread in Wardaman country sometime during the late Holocene. We can trace back the beginnings of modern artistic expressions to this time;
2. the beginnings of this move probably related to a new system of land management initiated sometime around or shortly before 1400–900 years ago;
3. these changes indicate new strategies of territorial behaviour which, we argue, took place in response to population increases and/or changes in intensities of inter-personal relations.

Conclusion

Given the current position of rock paintings in Wardaman ontology, the appearance or intensification of rock painting activity *c.* 1400–900 years ago implies that there have been significant changes in systems of land tenure and, possibly, the Dreaming at that time. These include:

1. a change in *world view* (ontology), including ways of perceiving the land and the 'Dreaming'; and/or
2. a change in the *practice* of existing beliefs. An example of this is the change in the location of the Lightning Brothers' place during the early contact period that resulted from the cessation of access to the 'original' Lightning place; and/or
3. a change in the way the belief system was *expressed* (communicated). In this case world views did not change, but people began to express them in rock painting, indicating a new way of expressing the land's identity.

The paintings in each of the excavated sites today related to the local Dreaming beings which give identity to the land. These identities also reflect current systems of land affiliation and land tenure. The rock art and its associated narrative tradition expresses people-land-Dreaming relations in such a way that the art and oral traditions mutually re-enforce the Law expressed in the Dreaming and in the land. The archaeological evidence indicates that, prior to 1400–900 years ago, the 'Dreaming' of the time was not as systematically expressed through rock painting as it is today. Because of the nature and recurrence of the archaeological changes noted above, we would thus argue that broad, regional changes in the management and expression of territorial affairs took place during the late Holocene.

It is difficult at this stage to identify which of the three possible options enumerated above is or are likely to be correct. Nevertheless, other changes are also associated with those in rock painting, including increases in intensities of stone artefact deposition rates that may indicate increases in intensities of site use. The latter implies to us that the late Holocene may have witnessed a population explosion in northern Australia. David (1991) has argued for a similar phenomenon for southeastern Cape York Peninsula (to the northeast of Wardaman country), where he suggested that a late Holocene regionalisation of rock art may reflect increases in the sizes of interacting populations, increases in conflict and a subsequent regionalisation of social groups. This scenario may also be relevant to the present study.

Having said this, let us not forget that to Wardaman people the land, the sites and the 'art' which we are discussing are timeless and ever-present. They are expressions of events which are operationalised in the Dreaming. Our archaeological perceptions of these places should never undermine the fact that to others they may be timeless, Dreaming actualities. This acknowledgement is a fundamental aspect of our research in Wardaman country.

Acknowledgements

We would like to thank the many Wardaman people who showed us and allowed us to record and excavate some of their sites. Special thanks to the late Ruby Alison, Riley Birdun, July Blutcher, Daisy Gimin, Lily Gingina, Queenie Ngabijiji, Tarpot Ngamunagami, Elsie Raymond, Oliver Raymond, Barbara Raymond, Michael Raymond, Lindsay Raymond and Tilley Raymond. The Dreaming stories presented here were recounted on-site by the above individuals, and this paper was read and approved for publication by them.

We also gratefully thank Francesca Merlan for commenting no aspects of an earlier draft of this paper and for permission to quote an unpublished report, the AIATSIS, the Australian Museum and the Australian Heritage Commission for funding the radiocarbon dates, and the Earthwatch Corps for helping us in the field.

9 A reader's guide

Tim Murray

There is a broad literature on the archaeology of Australia which reflects both the national and international significance of the subject. Many of the papers reprinted in this collection were first published either in the United Kingdom or in the USA, two countries where archaeologists have had a long-standing interest in the archaeology of foraging societies such as those which existed in Australia until the time of dispossession. Since then Australian anthropology, and latterly Australian archaeology, have provided significant comparative information to scholars based in Europe and in North America so that they could more readily pursue their own inquiries into the varieties of human experience, and to develop models of social and cultural evolution. Over the same period the study of human remains collected in Australia have made a similarly significant contribution to the development of physical anthropology on a world-wide scale.

Outside the confines of professional interest in the archaeology of Australia there has been a growing interest in seeking an understanding of the role archaeology plays in Australian society. There is ample evidence that there is considerable public interest in archaeological discoveries made in Australia (one need only reflect on the announcement for very early dates from Jimnium in the Kimberleys, see Fullagar *et al.* 1996). Nonetheless, with some notable exceptions, there has been very little overt discussion of the role such discoveries play in enhancing public understanding of the human history of the continent, or what role they should play in our drive for national identity (see e.g. Mulvaney 1981; Murray 1996a, 1996b).

At the present time most of what passes for discussion of these matters has been devoted to exploring the complex and ever-changing relationship between archaeologists and indigenous peoples, particularly with reference to 'ownership' of the past, and to the conflicts which have erupted over the reburial of skeletal remains and the contents of archaeological deposits (see e.g. Bowdler 1992; MacBryde 1992; Mulvaney 1991; Allen 1987; Murray 1996c, 1996d, 1996e; Murray and Allen 1995). These explorations have raised significant ethical and intellectual issues which go right to the heart of notions of heritage and community. An obvious point of concern here is the role such explorations should play in the quest for reconciliation between Aboriginal people and the broader Australian society.

More general discussions about the place of archaeology in defining space, place and time in Australian society have tended to be very few and far between. John Mulvaney made a great contribution here (see e.g. Bonyhardy and Griffiths 1996), and in recent times the pathbreaking work of Tom Griffiths (1995) has helped focus popular attention on more subtle contributions made by archaeologists and, at an earlier time, antiquarians to our understanding of these issues. But having said this, it is nonetheless true that few Australian historians have sought an active engagement with archaeologists exploring the archaeology of Aboriginal Australia, even though since Manning Clark it has been commonplace for general Australian histories to include discussion of the history of Australia before the European invasion. The only book-length treatment is Geoffrey Blainey's *Triumph of the Nomads. A History of Ancient Australia* (1975), but compilations such as *Seeing the First Australians* (Donaldson and Donaldson 1985) also have enduring value.

General prehistories

The only up-to-date general prehistory is Flood's *Archaeology of the Dreamtime* (1995), now its third edition. Other general prehistories, such as Mulvaney (1975), Mulvaney and White (1987) and White and O'Connell (1982) which were more intellectually satisfying in their time are now seriously out of date both in information and perspective. Although somewhat idiosyncratic in its coverage, Noel Butlin's *Economics and the Dreamtime* (1993) is a challenging synthesis, which follows his influential excursion into prehistoric demography, *Our Original Aggression* (1983). Harry Lourandos has also recently published a new prehistory of Australia (1997).

Major compilations

One of the major drawbacks of Flood's account of Aboriginal prehistory is a lack of a sense of debate among practitioners, which is best exemplified in problem-oriented compilations such as Allen and O'Connell (1995). Other excellent examples of this genre which remain relevant to contemporary practice are Allen *et al.* (1977); Dodson (1992); Meehan and Jones (1988); and Spriggs *et al.* (1993).

At another level reference works such as *The Encyclopedia of Aboriginal Australia* (Horton 1994) and the *Australian Encyclopedia* present useful synopses and surveys of aspects of the field.

For technical aspects of undertaking archaeological research see Graham Connah (ed.) 1982 *Australian Field Archaeology: A Guide to Techniques*. Aboriginal Studies Press: Canberra.

History of Australian archaeology

Until recent years Australian archaeologists have not been particularly fond of self-reflection about the history of research or the social and cultural implications of the knowledge they produce. A major exception to this generalisation is the work of John Mulvaney, who has published the most thoughtful discussions of the history of Australian archaeology. Throughout his career Mulvaney sought to position the history of

Australian archaeology within the context of studies of Aboriginal Australia, which have been undertaken both in Australia and overseas (Mulvaney 1958, 1964, 1981, 1985, 1986, 1988, 1990). The best points of access to Mulvaney's work is a compilation of his writings (Mulvaney 1990), and the papers in his feschrift which contain two historical surveys (Golson 1986; McBryde 1986).

David Horton (1991) has also produced a useful compilation of source material for a history of Australian archaeology (in the form of reprinting selections from 'classic' papers). During the 1980s debate was joined for a short time about the influence of archaeologists from Cambridge University on Australian archaeology (Murray and White 1981). One useful outcome was the beginnings of a fitful discussion of the identity, or distinctiveness, of Australian archaeology which began well (Thomas 1982; Murray and White 1982) and then faded. More general discussion of the identity of Australian archaeology has recently reappeared (Meehan and Jones 1988; Murray 1992a, 1992b).

Significant journals and series

The Australian Archaeological Association (AAA) is the largest grouping of archaeologists and those interested in archaeological matters. It has an annual conference and publishes a journal (*Australian Archaeology*) twice a year. The proceedings of the annual conference are often published. (Address: Department of Archaeology and Anthropology, ANU Canberra ACT 0200.)

The Australian Association for Historical Archaeology (ASHA) has a primary focus on the archaeology of the last 200 years. The Association publishes *The Australasian Journal of Historical Archaeology* once a year and a Bulletin and Technical Reports at more regular intervals. It also publishes more general surveys and conference proceedings. (Address: Box 220, Holme Building, University of Sydney NSW 2006.)

The Australian Rock Art Research Association (AURA) publishes *Rock Art Research*, and regularly holds symposia and congresses. (Address: PO Box 216, Caulfield South VIC 3162.)

The Archaeological and Anthropological Society of Victoria publishes *The Artefact*. (Address: PO Box 328C, Melbourne VIC 3001.)

The Australian Institute of Aboriginal and Torres Straits Islander Studies publishes *Aboriginal Studies*. Some archaeology occasionally appears in *Aboriginal History* and (very occasionally) in *Australian Historical Studies*.

Archaeology in Oceania (AO) has no formal link with any archaeological society, but has been regarded as one of the more influential journals in the field.

It has become increasingly common for the proceedings of conferences to be published. The Archaeometry and the Women in Archaeology conferences both have a number of collections in print. However, the most significant series of monographs and compilations has been *Terra Australis* and its companion series *Research Papers in Archaeology and Natural History* both of which are published through the Australian National University. Newer entries into the market such as *Tempus* (Anthropology

Museum, University of Queensland) and *Queensland Archaeological Research* will be joined by electronic journals and series based around the various Departments of Archaeology in Australia. This is a rapidly expanding form of publication and is best accessed via the home pages of the institutions concerned. Full e-mail and internet addresses can be obtained by approaching the Departments listed below.

University departments

Unpublished Bachelors and doctoral theses are a major source of information about the archaeology of Aboriginal Australia. Some of these are listed in the journal *Australian Archaeology*, and many are available through interlibrary loan. For a complete list contact:

Department of Anthropology
Northern Territory University
PO Box 40146
CASUARINA NT 0811

Department of Anthropology & Archaeology
James Cook University of North Queensland
Townsville QLD 4811

Department of Sociology and Anthropology
University of Queensland
Brisbane 4072 QLD

Department of Archaeology & Palaeoanthropology
University of New England
Armidale NSW 2351

Department of Prehistoric and Historical Archaeology
A14
University of Sydney NSW 2006

Division of Archaeology and Natural History
The Research School of Pacific and Asian Studies
The Australian National University
Canberra ACT 0200

Department of Archaeology and Anthropology
The Australian National University
Canberra ACT 0200

Department of Archaeology
La Trobe University
Bundoora VIC 3083

Discipline of Visual Arts and Archaeology
Flinders University
GPO Box 2100
Adelaide SA 5100

Centre for Archaeology
The University of Western Australia
Nedlands WA 6009

Aboriginal people and archaeologists on heritage

I have already explained why the Aboriginal voice is only rarely heard in this book. Even in the case of discussion about the relationships between archaeologists and Aboriginal people, with few notable exceptions this is a discussion between archaeologists. This will (and should) change. The following references give some of the flavour of contemporary debate among archaeologists and between archaeologists and Aboriginal people about matters of heritage and identity: Birckhead *et al.* (1992); Burke *et al.* (1994); Davidson *et al.* (1995); Flood (1989); Fourmile (1989); Greer (1996); Langford (1983); McBryde (1985, 1992); Mulvaney and Murphy (1996); Murray (1992, 1996a, b, e, f); Pearson and Sullivan (1995); Ross (1996); Schrire (1985); Smith and Clarke (1996); Sullivan (1985, 1996); Tasmanian Aboriginal Centre (1991); Tasmanian Aboriginal Lands Council (1996); and Ucko (1983).

Apart from prospecting this literature, in the short term the best way of gaining an understanding of the wide variety of Aboriginal viewpoints on these issues is to contact relevant State and Federal heritage agencies. They will be delighted to refer you to the Aboriginal bodies responsible for developing and implementing Aboriginal heritage policies.

Government agencies

State and Federal heritage agencies are also a significant source of professional publication in the fields of heritage and applied archaeology. The HERA database (maintained by the Australian Heritage Commission) is available on line, on CD-ROM, and in printed form and is a good source of leads which will improve access to a sometimes disparate literature.

ACT Heritage Unit
Environment and Conservation Bureau
Department of the Environment Land and Planning
PO Box 1119
Tuggeranong ACT 2901

Australian Heritage Commission
GPO Box 1567
Canberra ACT 2601

AIATSIS
(Australian Institute of Aboriginal and Torres Strait Islander Studies)
GPO Box 553
Canberra ACT 2601

Australian Cultural Development Office
GPO Box 1920
Canberra ACT 2601

Cultural Heritage
Queensland Department of Environment and Heritage
PO Box 155
Brisbane QLD 4002

Aboriginal Areas Protection Authority
PO Box 1844
Darwin NT 0801

Heritage Team
Department of State Aboriginal Affairs
PO Box 3140
Adelaide SA 5001

Department of Aboriginal Sites
3rd Floor
35 Havelock Street
West Perth WA 6005

Resources, Wildlife and Heritage
Parks and Wildlife Service Tasmania
Department of Environment and Land Management
GPO Box 44A
Hobart TAS 7001

Aboriginal Heritage Branch
NSW National Parks and Wildlife Service
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Notes

3 The pattern of continental occupation

1. Earlier seed grinding implements dating to ~30 000 BP are described from the Cuddie Springs site on the eastern edge of the arid zone near Brewarrina (Furby *et al.* 1993).

4b The fifth continent

1. Excepting the dugout 'lippa lippa' of the Arnhemland coast, which is a recent introduction from Macassan visitors and which needs iron tools to manufacture, and the outriggers of Cape York derived from Melanesia.

5a Holocene environments and prehistoric site patterning in the Victorian mallee

1. All SUA dates cited are likely to be recalculated by the laboratory. In this case final values may be less than values cited by Macumber (1980) (M. Barbetti, pers. comm.).
2. This value includes a –340 year revision by the laboratory. May 1981. The date will not be further revised.

5b Perspectives on 'trends toward social complexity in prehistoric Australia and Papua New Guinea'

1. This chapter has been modified from the paper delivered to the Australian Archaeological Association in Tallebudgera, Queensland, November 27, 1984, but still retains the form of the orally delivered version. Although my remarks at Tallebudgera were presented as a discussion to the panel 'Trends toward social complexity in prehistoric Australia and Papua New Guinea', Peter White and I agreed that they might better introduce the papers published in AO rather than attempt to summarise them. I am grateful to Peter and Tim Murray (co-organisers of the Tallebudgera panel) for inviting me to participate in the proceedings. Also, I am pleased to acknowledge the Fulbright Commission and Australian-American Educational Foundation for the grant of a Senior Scholarship to work in the Department of Anthropology, University of Sydney for a year. Finally, I thank many friends in Sydney and throughout Australia for patiently discussing their work and ideas with me and so furthering my understanding of a set of important data and the appropriate theories used to interpret them.
2. Actually, I was only told this anecdote which, for all I know, may be purely apochryphal. At any rate, I do not have any particular person in mind as the hero of the story.

3. But see Lourandos (1983: 92): 'By all indications intensification of social and economic relations would appear to have been increasingly taking place during the Holocene period on the Australian mainland, the process being nipped in the bud by the coming of the Europeans.' For a comment on Lourandos' (and others') use of the term 'intensification', see later.
4. I list separately the monographs I reviewed specifically for this paper (this list I distributed at the AAA). In the following sections of this paper, I refer mainly to these monographs either directly (see the list of cited references) or implicity. I have not tried to document my opinions much beyond the exercise of culling information from this list of studies but, of course, it is hard to exclude knowledge gained from a variety of other sources. In a planned, longer form of this paper, which will be combined with collateral evolutionary studies, more extensive citations will be incorporated.

Adams, R. McC. 1981, *Heartland of Cities*. University of Chicago Press, Chicago.
Blanton, R., Kowalewski, S., Feinman, G. and Appel, J. 1981, *Ancient Mesoamerica: A Comparison of Change in Three Regions*. Cambridge University Press, Cambridge.
Chang, K-C. 1983, *Art, Myth, and Ritual: The Path to Political Authority in Ancient China*. Harvard University Press, Cambridge.
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Flannery, K. and Marcus, J. (eds) 1983, *The Cloud People: Divergent Evolution of the Zapotec and Mixtec Civilizations*. Academic, New York,
Hirth, K. (ed) 1984, *Trade and Exchange in Early Mesoamerica*. University of New Mexico Press, Albuquerque.
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Keightley, D. (ed) 1983, *The Origins of Chinese Civilization*. University of California Press, Berkeley.
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Nissen, H-J. 1983, *Grundzuege einer Geschichte der Fruehzeit des Vorderen Orients*. Wissenschaftliche Buchgesellschaft, Darmstadt.
Vértesalji, P.P. 1984, *Babylonien zur Kupfersteinzeit*. Dr Ludwig Reichert, Wiesbaden.
Vogt, E. and Leventhal, R. (eds) 1983, *Prehistoric Settlement Patterns*. University of New Mexico Press, Cambridge.
Yoffee, N. and Cowgill G.L. (eds) nd, *The Collapse of Ancient States and Civilizations*.

5c The cemetery as symbol

1. The name 'Murray Black Collection' refers only coincidentally to prehistoric skeletons of the River Murray Aborigines and is certainly not meant in a pejorative sense. These samples were excavated by Mr George Murray Black, of Gippsland, between the years 1929 and 1951.

2. The burial from the buff yellow dune core (B.79) is said by the authors to be 'more heavily built than other human remains on the site, and is extensively mineralised. Of the exposed burials at Snaggy Bend this individual may be the most ancient' (1985: 75).

   Morphological dating is not intrinsically a poor method of determining the age of a specimen, but when dealing with geologically modern, intra-specific variation, we must be careful to avoid the self-fulfilling classificatory exercise that lumps all the 'robust' crania from all over the continent and through a considerable time span into one morphotype.

   As an illustration, I had the opportunity to examine a humeral fragment from B.79 in the field. It was carbonate encrusted and mineralised, and lay to the side of the (now reburied) cranium. This portion of the right upper arm bone measures 15 mm and 20 mm in minimum and maximum midshaft diameters. By comparison, two clearly male individuals on the same site measure 16 and 24 mm (B.3), and 19 and 24 mm (B.33, left humerus). B.79 is not significantly different from South Australian male or female means for either measurement: 16.2 and 20.7 mm for males, 12.9 and 17.3 mm for females (Van Dongen 1963). This sample is from Swanport mainly and is thought to be later Holocene in age. In further contrast, a mid-Holocene sample made up of Robinvale IV, Snaggy Bend 3 and 33 Mossgiel and Nitchie averages 18.7 mm in minor diameter and 23.6 mm in the major axis.

   Finally, of the published older material, Tandou (Freedman and Lofgren 1983) measures 16 and 21 mm in minor and major axes. This specimen is probably on the order of 5000 years older than Snaggy Bend B.79.

   While the skull may have appeared rugged or robust, some minimal quantitative evidence from the post-cranial skeleton suggests the opposite: Snaggy Bend B.79 is smaller than both older and later individuals from the same region. In fact, it does not significantly exceed a more recent female sample from the lower tracts of the River Murray. This sortie into skeletal measurement does not attempt to address rugosity or robusticity in prehistoric populations. It is simply important to be explicit in our criteria for determining the age of any archaeological specimen.
3. Just as *population* can only be defined operationally, endogamy and exogamy are subjective terms. I have assumed that the postulated lineal descent groups—lineages or clans—are exogamous. Aggregations of these lineages, organised as bands, tribes, linguistic groups or in some other way, are the groups I am suggesting were endogamous.

7a Stone tool functions in the Western Desert

1. This is not to imply that no interest was taken in the use and manufacture of stone tools, although such observations are surprisingly rare. In Australia valuable data were recorded by Roth (1904), Spencer and Gillen (1899, 1912), Aiston (1928; Horne and Aiston 1914), Basedow (1925), Love (1942), Mountford (1941), Tindale (1941, 1965), Thomson (1964), and Gould (Gould, Koster and Sontz 1971). These observations are invaluable; but almost invariably, except for Gould, none of these observers was an archaeologist by training, and with the further exception of Tindale, none was involved in archaeology in any professional way.
2. In Africa, MacCalman and Grobbelaar (1965) observed a group of stone using Ova-Tjimba. The parallels to the Australian situation in terms of retouching stone tools are striking. For instance, in the butchering of an ungulate, only unmodified flakes were selected from core knappings and used.

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